Racial/Ethnic Disparities in Breastfeeding and Potential Mediation by Hospital Practices

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THESIS

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TABLE OF CONTENTS

TABLES OF CONTENTS (continued)

CHAPTER **PAGE**

LIST OF TABLES

LIST OF TABLES (continued)

LIST OF TABLES (continued)

LIST OF FIGURES

FIGURE PAGE

LIST OF ABBREVIATIONS

AAP	American Academy of Pediatrics
BF	Breastfeeding
BFHI	Baby-Friendly Hospital Initiative
CDC	Centers for Disease Control and Prevention
CDE	Controlled Direct Effect
CI	Confidence Interval
DE	Direct Effect
Exc BF	Exclusive Breastfeeding
FPL	Federal Poverty Line
HP2020	Healthy People 2020
IFPS	Infant Feeding Practices Survey
IE	Indirect Effect
IL	Illinois
NDE	Natural Direct Effect
NICU	Neonatal Intensive Care Unit
NIE	Natural Indirect Effect
OR	Odds Ratio
PRAMS	Pregnancy Risk Assessment Monitoring System
SES	Socioeconomic Status
SIDS	Sudden Infant Death Syndrome
TE	Total Effect
US	United States
WHO	World Health Organization
WIC	Special Supplemental Nutrition Program for Women, Infants, and Children

SUMMARY

Breastfeeding is the healthiest way to feed an infant and is related in a dose-response manner to many maternal and child health outcomes. Despite this, many women do not breastfeed, do so for only short periods of time, or supplement with formula. There is also persistent variation in breastfeeding across racial/ethnic subgroups in the United States, but little public health research has focused on understanding how and why these disparities arise.

This study used secondary data from the 2004–2008 Illinois Pregnancy Risk Assessment Monitoring System (PRAMS) to address three main aims: (1) understanding the interplay of race/ethnicity and socioeconomic variables (measured by education and income) in predicting breastfeeding initiation, duration, and exclusivity; (2) determining whether Baby-Friendly Hospital Initiative (BFHI) practices are associated with breastfeeding duration and exclusivity and whether this association varies by race/ethnicity; and (3) applying counterfactual mediation analysis to assess whether differential experience of BFHI practices across race/ethnicity contributes to observed disparities in breastfeeding exclusivity. Breastfeeding outcomes were defined as seven negative outcomes: never breastfeeding and, among breastfeeding initiators, termination of breastfeeding before two, six, and 12 weeks after delivery, and termination of exclusive breastfeeding before two, six, and 12 weeks after delivery.

After adjusting for covariates, significant Black-White and Hispanic-White disparities existed for never breastfeeding and exclusive breastfeeding termination before two, six, and 12 weeks. There were not significant racial/ethnic disparities in any breastfeeding termination after controlling for covariates. The disparities for breastfeeding initiation were modified by income level and demonstrated that merely controlling for socioeconomic variables would have masked the complexities of this relationship.

This study showed that several BFHI practices were independently and cumulatively associated with any and exclusive breastfeeding termination. Specifically, not receiving a formula gift pack, breastfeeding in the first hour, feeding the infant only breast milk in the hospital, and not giving a pacifier

were associated with reductions in any breastfeeding termination, while not receiving a formula gift pack, breastfeeding in the first hour, telling mothers to feed on demand, not giving a pacifier, and giving a support phone number were all associated with reductions in exclusive breastfeeding termination. This study also showed that overall high hospital breastfeeding support was strongly associated with reduced odds of any and exclusive breastfeeding termination during the first three months, though high hospital support was generally more protective against breastfeeding termination for White and Black women than Hispanic women.

This analysis culminates in the use of mediation analysis to assess whether racial/ethnic disparities in breastfeeding exclusivity could be partially explained by differential access to BFHI practices. The data showed that only two of six practices (not receiving a formula gift pack and breastfeeding in the first hour) were significant independent mediators of the Black-White and the Hispanic-White disparities in exclusive breastfeeding termination, but the magnitude of this mediation was very small. Although there was little mediation effect of any individual practices, high overall hospital support for breastfeeding was a significant mediator of the Black-White disparity in exclusive breastfeeding at every time point and was a stronger mediator than any individual BFHI practice. On the other hand, high levels of hospital breastfeeding support did not mediate the Hispanic-White disparities in exclusive breastfeeding.

The mediation analysis shows that, while breastfeeding-supportive hospital practices are effective at improving overall breastfeeding outcomes, equalizing access to such practices may not substantially change the observed racial/ethnic disparities in breastfeeding. Instead, more targeted public health interventions may be necessary for improving exclusive breastfeeding rates among Black and Hispanic women. Recommendations for future research include application of mediation analysis to understand the specific causes of racial/ethnic disparities in breastfeeding to develop targeted interventions, and the further application of mediation analysis to evaluate the effect of breastfeeding interventions.

I. INTRODUCTION AND SPECIFIC AIMS

Breastfeeding is related to many positive maternal and child health outcomes (1–16) and the American Academy of Pediatrics (AAP) recommends infants be breastfed for at least the first year and be fed only breast milk for the first six months of life (17). In spite of this, many women in the United States do not breastfeed their infants, breastfeed only for short periods of time, or supplement breastfeeding with formula feeding (18). There is persistent variation in breastfeeding across racial/ethnic subgroups in the United States, but little public health research has focused on understanding how and why these disparities arise.

There is also evidence mothers with lower educational attainment and lower household incomes are less likely to breastfeed than their more educated and wealthier counterparts (19–21). It is common practice in social epidemiology to adjust for either race/ethnicity or dimensions of SES to try to discern the independent effect of the other factor. While race/ethnicity and SES may have independent effects on some health outcomes (22), the meaning of these independent effects may not always be useful and does not reflect the true correlation and interplay of these factors in society (23). Few studies have examined how race/ethnicity and SES collide to influence breastfeeding behaviors (24).

In general, the epidemiologic approach to research on disparities in breastfeeding tends to focus on describing differences between racial/ethnic groups, not explaining why differences are present (25–27). The public health literature on disparities in breastfeeding tends to focus on descriptive, rather than explanatory, analyses. More research is needed on why disparities in breastfeeding exist, how they are created and maintained, and how interventions may impact the disparities. Better information about the causes and influences of disparities in breastfeeding will help with planning and implementing public health programs and policies to effectively reduce these disparities (26).

More information is also needed about the impact of interventions on racial/ethnic disparities in breastfeeding. Evidence demonstrating the overall effectiveness of an intervention does not necessarily

mean that the intervention will reduce the *racial/ethnic disparity* in that outcome (28), as differential access to or differential effectiveness of the intervention by race/ethnicity could lead to increases in disparities after the program is implemented (28, 29). The BFHI practices are an example of a population-based public health strategy that shows promise for improving overall breastfeeding outcomes, but very few studies have considered potential differential experience or differential effectiveness of these practices. No studies have examined how the observed racial/ethnic disparities in breastfeeding in the United States might be changed by increasing implementation of these practices.

This study seeks to achieve three specific aims:

1. Describe the racial/ethnic disparities in breastfeeding initiation, duration, and exclusivity in Illinois while demonstrating how alternate scenarios of accounting for socioeconomic status (SES) impact the magnitude, interpretation, and implications of the results.
2. Evaluate the effectiveness of BFHI practices at improving breastfeeding duration and exclusivity for Illinois women, with particular focus on assessing whether practices are differentially effective across race/ethnicity.
3. Apply counterfactual-based mediation analysis to determine whether differential experience of BFHI practices may be contributing to observed racial/ethnic disparities in breastfeeding duration and exclusivity.

In achieving these aims, this study will expand the understanding of the interplay of race and SES in breastfeeding behaviors, document the effectiveness of hospital support at improving breastfeeding, and demonstrate the potential for maternity care practices to contribute to the goal of eliminating disparities in breastfeeding. Additionally, this study will provide a framework and model for routine use of mediation analysis in racial/ethnic disparities research.

II. BACKGROUND

A. Breastfeeding in the United States and Illinois

Breastfeeding is the healthiest way to feed an infant and confers many health benefits to both mother and baby (1). Infants who are breastfed have lower rates of diarrheal disease (2), lower respiratory tract infections (3), eczema (4), acute ear infections (3, 5), asthma (3,6), childhood leukemia (7), child obesity (8,9), sudden infant death syndrome (SIDS) (3, 11), celiac disease (10), and early-onset inflammatory bowel disease (12). Women who breastfeed their infants have lower rates of breast and ovarian cancer (1, 3, 13), and type-2 diabetes mellitus (15). Breastfeeding also improves maternal-infant bonding (1, 14) and is associated with decreased postpartum depression among new mothers (16). Many studies have also documented the dose-response relationship of breastfeeding with many of child and maternal health outcomes, including acute ear infections (3, 5), overweight/obesity (8, 9), SIDS (3, 11), and acute leukemia (7) in children, and ovarian cancer (3) and type-2 diabetes (15) among women. Because of these numerous benefits to women and children, the AAP recommends that a woman breastfeed her infant for at least the first year of life and that she feed her infant only breast milk during the first six months of life (17).

Despite these known breastfeeding benefits and professional recommendations, many women in the United States do not breastfeed their infants, breastfeed only for short periods of time, or supplement breastfeeding with formula feeding. In 2010, 76.5% of US infants were breastfed, 27.0% were breastfed for the recommended duration of at least one year, and only 16.4% were exclusively breastfed for the recommended duration of six months. The 2010 breastfeeding rates in Illinois were lower than the nation for all outcomes: 75.2% of infants were ever breastfed, 21.0% were breastfed for at least one year, and only 11.1% were exclusively breastfed for at least six months (18). Suboptimal breastfeeding, including women who stop breastfeeding early or do not exclusively breastfeed, has been estimated to cost $2.2 billion annually in direct medical expenses due to increased infant and child morbidity (30).

There is also wide variation in breastfeeding rates across several demographic characteristics. In 2008, 80.0% of Hispanic infants in the United States were ever breastfed, compared to 75.2% of non-Hispanic White infants and only 58.9% of non-Hispanic Black infants. Non-Hispanic Black infants are also less likely to be breastfed for a full year, with only 12.5% continuing for the recommended time frame compared to 24.3% of non-Hispanic White and 26.3% of Hispanic infants (31). Infants born to young mothers, mothers with lower educational attainment, or mothers with lower household incomes are also less likely to be breastfed than the infants born to their older, more educated, and wealthier counterparts (20). During 2000–2008, there was progress toward reducing the difference in breastfeeding initiation rates between non-Hispanic Black and non-Hispanic White infants, with the absolute difference falling from 24.4 percentage points in 2000 to 16.3 percentage points in 2008. During the same time period, however, the racial/ethnic disparity in breastfeeding to 12 months between non-Hispanic White and Black infants increased from 10.8 percentage points to 11.8 percentage points (31). There have been fewer studies on racial/ethnic disparities in exclusive breastfeeding, and results have been inconsistent with respect to the direction and magnitude of disparities (32–34). Clearly there is still a long way to go to ensure that all women and infants are equally benefiting from breastfeeding.

Even among women who want to breastfeed, many do not achieve their breastfeeding goals. Sixty percent of women in the United States who intended to breastfeed their infants reported that they did not breastfeed as long as they wanted to (35). Another study showed that among women who intended to breastfeed for at least two months, 21% had stopped before that time point (36). Exclusive breastfeeding is even harder to achieve, even among women with strong intentions to breastfeed. According to one study, 61% of women intended to exclusively breastfeed their infants, but 16% of these women had already ceased exclusive breastfeeding at one week postpartum (37). Another study showed that among women who intended to exclusively breastfeed for at least three months, only about half were exclusively breastfeeding at one month postpartum (38). The quick drop-off in breastfeeding (even among those who want to breastfeed) demonstrates that it is not sufficient to focus public health resources on the initial

decision to begin breastfeeding, but that priority must also be given to the protection and support of breastfeeding to help women overcome barriers (35).

Women who do not breastfeed as long as they had intended commonly cite reasons related to lactation difficulties (e.g., poor latch, pain, or breast infection) and nutritional concerns (e.g., perceived low milk supply or poor infant weight gain) (35, 36). These problems can generally be prevented and eliminated with adequate breastfeeding support, management, and education. More than half of women who reported stopping breastfeeding earlier than desired cited insufficient milk supply as a reason, despite the fact other studies have shown that the vast majority of women are biologically capable of producing sufficient milk (35). One study showed that first-time mothers who have concerns related to infant breastfeeding difficulties during the first week postpartum are more likely to stop breastfeeding altogether or supplement with formula before two months. Likewise, the perception of insufficient milk supply among first-time mothers during the first two weeks postpartum is related to shorter duration of overall and exclusive breastfeeding (36). If provided with the appropriate support, resources, and tools, women should be able to overcome these problems and successfully breastfeed their infant as long as they would like to do so.

Healthy People 2020 (HP2020), the US national public health targets, established several objectives for breastfeeding: (a) at least 81.9% of infants will ever be breastfed; (b) at least 60.6% of infants will be breastfed for at least six months; (c) at least 34.1% of infants will be breastfed for at least 12 months; (d) at least 46.2% of infants will be exclusively breastfed for at least three months; and (e) at least 25.5% of infants will be exclusively breastfed for at least six months (39). *Healthy People* also states overarching goals of achieving health equity (defined as the "the attainment of the highest level of health for all people") and eliminating health disparities (40).

The HP2020 national objectives reveal the extent to which the United States currently falls short in breastfeeding. While breastfeeding initiation rates are close to the HP2020 objectives for some subgroups, even the subgroups with the highest breastfeeding rates have not yet achieved the HP2020 objectives for breastfeeding duration and exclusivity. Black American infants, in particular, have

breastfeeding rates far from the national objectives. The presence of wide disparities in breastfeeding by demographic variables is troubling because not all infants and women are equally benefiting from breastfeeding. To promote health equity, it is imperative to improve breastfeeding in the groups with lower rates.

B. Improving Breastfeeding through Maternity Care Practices

The most recent AAP Breastfeeding Policy Statement (2012) introduced a conceptual change for how physicians should view infant feeding choices (17). As summarized by Eidelman (2012), the new policy acknowledges that "an individual mother's decision to breastfeed her infant should not be conceived as a lifestyle choice, but rather as a critical and basic health decision for her infant's immediate and long-term welfare" (41, pg. 203). Physicians and other healthcare providers, therefore, have a responsibility to enable patients to make a feeding decision that will best benefit their infants' and their own health. Because nearly all births in the United States occur in hospitals, hospital practices and policies have the potential to influence the feeding behaviors of more than 11,000 infants each day (42).

Affirming the importance of the hospital's role for supporting breastfeeding, HP2020 added two new objectives that focus on maternity care practices: (a) reducing the proportion of breastfed newborns who receive formula supplementation within the first two days of life to 14.2%; and (b) increasing the proportion of births that occur in facilities that provide recommended care for lactating mothers and their babies to 8.1% (39). There is substantial progress needed in the United States and in Illinois to accomplish these objectives. In 2010, 24.2% of breastfed infants in the United States and 27.2% in Illinois were fed formula within the first two days of life. In the first half of 2013, 7.2% of US births were in birthing facilities with recommended breastfeeding-related care; only 2.5% of Illinois births were in facilities meeting this standard (18).

1. Baby-Friendly Hospital Initiative and the Ten Steps

In 1991, the World Health Organization (WHO) and United Nations Children's Fund launched the BFHI, a global movement aimed at making hospital environments more supportive of

breastfeeding. The "Ten Steps to Successful Breastfeeding" make up the core of the BFHI, summarizing the maternity care practices necessary for encouraging and supporting new mothers in breastfeeding (43). The Ten Steps cover relevant policies and practices over the full spectrum of care for pregnant women and new mothers (see Table I). The first two steps relate to the policies and systematic support necessary for improving a hospital's approach toward breastfeeding (44). The remaining eight steps are practices that are directly experienced by women and infants under the care of the hospital. These steps reflect the provision of a continuum of breastfeeding education and support during pregnancy (Step 3), the hospital stay (Steps 4–9), and post-hospital discharge (Step 10) (44, 45).

TABLE I

BABY-FRIENDLY HOSPITAL INITIATIVE: TEN STEPS TO SUCCESSFUL BREASTFEEDING

1. Have a written breastfeeding policy that is routinely communicated to all healthcare staff.
2. Train all healthcare staff in skills necessary to implement this policy.
3. Inform all pregnant women about the benefits and management of breastfeeding.
4. Help mothers initiate breastfeeding within a half hour of birth.[a]
5. Show mothers how to breastfeed and how to maintain lactation, even if they should be separated from their infants.
6. Give newborn infants no food or drink other than breast milk, unless medically indicated.
7. Practice rooming-in by allowing mothers and infants to remain together 24 hours a day.
8. Encourage breastfeeding on demand.
9. Give no artificial teats, pacifiers, dummies, or soothers to breastfeeding infants.[b]
10. Foster the establishment of breastfeeding support groups and refer mothers to them on discharge from the hospital or clinic.

[a] In the United States, this is often revised to say "within an hour of birth."

[b] The AAP adds a caveat that they do not recommend a complete restriction on pacifier use because of the evidence that pacifiers reduce the risk of SIDS and have an analgesic effect for infants undergoing medical procedures. They instead recommend delayed introduction of pacifiers for breastfeeding infants until breastfeeding is established (usually 3–4 weeks) (46).

Since the BFHI began in 1991, more than 15,000 hospitals throughout the world have been Baby-Friendly designated, but the vast majority of them are in developing countries (47). There are many barriers that may prevent hospitals from achieving Baby-Friendly designation, such as leadership priorities, organizational readiness-to-change, and financial costs (48). It was not until 2009 that the AAP endorsed the Ten Steps to Successful Breastfeeding, and began to incorporate the Ten Steps into curricula for medical residents (46). As of June 2014, 187 US hospitals were Baby-Friendly certified across 44 states and the District of Columbia (49). In Illinois, there were only four maternity care hospitals (out of 120) that were Baby-Friendly designated as of June 2014 (50).

While the vast majority of US hospitals and Illinois are not yet Baby-Friendly certified, some steps of the BFHI are being routinely integrated into maternity care practices. Table II summarizes the percent of hospitals in the United States and Illinois complying with each of the Ten Steps in 2011 (51). Hospitals in the nation and in Illinois generally had high compliance (>80%) with Steps 3, 5, and 8; breastfeeding education in prenatal education classes, teaching breastfeeding techniques, and encouraging on demand breastfeeding. The other seven steps, however, were practiced by far fewer of the hospitals surveyed, demonstrating that the bulk of recommended breastfeeding-related practices are not yet a regular part of maternity care. Of particular note were the very low compliance rates for having a model breastfeeding policy (Step 1: 19.3% in United States and 11.2% in Illinois), limiting supplemental feedings (Step 6: 23.0% in United States and 19.4% in Illinois), and providing discharge support (Step 10: 28.4% in United States and 22.9% in Illinois). For nine out of the ten steps, compliance with the Baby-Friendly steps was lower in Illinois hospitals than the national average; Illinois surpassed the national average only in prenatal breastfeeding education (with 98.2% of Illinois hospitals meeting this standard).

Overall, in 2011, only 43.5% of US hospitals and 32.3% of Illinois hospitals were implementing at least six of the recommended ten practices (52). The extent to which hospitals comply with the Ten Steps of the BFHI varies by hospital size, with larger hospitals implementing more of the recommended practices, but teaching status or neonatal intensive care unit (NICU) level were not related to hospital

adherence to the Ten Steps (42, 53, 54). The evidence shows that many hospitals are clearly still implementing practices that are not supportive of breastfeeding.

TABLE II

PERCENT OF UNITED STATES AND ILLINOIS HOSPITALS COMPLYING WITH THE TEN STEPS TO SUCCESSFUL BREASTFEEDING, 2011

Step #	**Practice**	**% US Facilities**	**% IL Facilities**
1	Model Policy	19.3	11.2
2	Assess Staff Competency	54.9	53.2
3	Prenatal Education	92.8	98.2
4	Early Initiation	56.5	49.1
5	Teach Breastfeeding Techniques	90.8	88.9
6	Limit Supplements	23.0	19.4
7	Rooming-In	37.0	28.7
8	Teach Feeding Cues (on-demand feeding)	84.8	81.5
9	Limit Pacifiers	36.3	26.6
10	Discharge Support	28.4	22.9

Individual women may also report varying levels of breastfeeding-supportive practices during their hospital stay. Only a few studies have estimated the national prevalence of the Ten Steps as experienced by women in the hospital and these estimates are shown in Table III. All of these studies have the limitation of not being representative of all women in the United States, but instead representing subgroups of women with higher propensities toward breastfeeding (37, 55, 56). For each practices, the percent of women reporting experiencing the practice fell in the range of 40%–70%, indicating that many important breastfeeding-supportive practices are not yet regularly incorporated into the care provided to new mothers and infants, even among women of higher socioeconomic status and those who intend to breastfeed (55, 56).

TABLE III

PERCENT OF BREASTFEEDING MOTHERS EXPERIENCING THE TEN STEPS IN THE DELIVERY HOSPITAL

	Author and Publication Year	DeGirolamo 2008	DeClerq 2009	DeClerq 2013
	Survey Used	Infant Feeding Practices Survey II	Listening to Mothers II	Listening to Mothers III
	Infant Birth Year	2000–2001	2005	2011–2012
	Study Population	Mostly non-Hispanic White women with higher education and income than national average	Nationally representative of singleton births among women who intended to exclusively breastfeed	Nationally representative of singleton births among women who intended to breastfeed
BFHI Step #[a]	**Maternity Care Practice**	**%**	**%**	**%**
4	Initiate Breastfeeding within 1 Hour of Delivery	60	77	NM
5	Teach Breastfeeding Techniques	NM	66	64
6	No Supplemental Feedings	52	63	63
7	Rooming-In	57	NM	62
8	Encourage On-Demand Feeding	66	77	66
9	No Pacifier Given	44	58	60
10	Provide Discharge Support	72	65	51

NM = not measured.

[a] Steps 1–3 not measured by any study in table

a. **Evidence for Baby Friendly: Breastfeeding duration**

Extensive evidence from many international randomized controlled trials supports the Ten Steps as important for enabling continued breastfeeding. This evidence, which was used by WHO to develop and support the Ten Steps, only extends to studies as late as the 1990s and includes few studies on US populations (43). Only one randomized controlled trial has tested the effectiveness of Baby-Friendly designation on breastfeeding duration; this study was performed in Belarus during 1996–1997 (57). The body of evidence, therefore, may not accurately represent the relationship between maternity care practices and breastfeeding promotion for contemporary American women.

Additionally, the evidence for Baby-Friendly practices is mostly comprised of studies examining specific individual hospital practices without accounting for the other practices a woman may have experienced. Because many hospital practices are related to each other, studies that account for other practices and establish the independent and cumulative effects of various practices are needed. To better describe the effect of the implementation of the Ten Steps in US hospitals on the length of breastfeeding duration, a review of US studies examining the association of breastfeeding duration with multiple hospital practices simultaneously was undertaken by this research. Five studies were found that studied the impact of multiple maternity care practices on breastfeeding duration for American women and are summarized in Table IV.

Two studies by DiGirolamo et al. (2001, 2008) used data from various iterations of the Infant Feeding Practices Survey (IFPS) to examine the individual and cumulative effects of BFHI practices on early termination of breastfeeding (56, 58). In these studies, early breastfeeding initiation (Step 4), not giving supplemental feedings (Step 6), and not giving a pacifier (Step 9) were associated with increased odds of breastfeeding for at least six weeks after adjusting for confounders and the other hospital practices. Rooming-in (Step 7), encouraging women to feed on demand (Step 8), and providing information for breastfeeding support upon discharge (Step 10) were not associated with early breastfeeding termination in this study.

TABLE IV

LITERATURE REVIEW: ASSOCIATION OF THE TEN STEPS[a] AND BREASTFEEDING DURATION AMONG WOMEN WHO INITIATED BREASTFEEDING

							Is Practice[a] Significantly Associated with Breastfeeding Duration ? (in adjusted analyses, if applicable)					
Author	**Publica-tion Year**	**Data Source**	**Infant Birth Year(s)**	**Sample Population**	**Breast-feeding Duration Measure**	**Adjusted for Con-founders?**	**Step 4**: first hour initiation	**Step 6**: No supplements	**Step 7**: rooming-in	**Step 8**: on demand feeding	**Step 9**: no pacifier given	**Step 10**: discharge support
DiGirolamo	2001	IFPS	1993	95% NH White women, higher education and income than nation	≥6 wks	Yes	Yes	Yes	No	No	No	n/s
DiGirolamo	2008	IFPS-II	2000–2001	85% NH White women, higher education and income than nation	≥6 wks	Yes	Yes	Yes	No	No	Yes	No
Murray	2009	PRAMS	2002–2003	Colorado	≥8 wks	No	Yes	Yes	Yes	No	Yes	Yes
Ahluwalia	2012	PRAMS	2004–2006	11 states (CO, FL, IL, LA, ME, NE, NJ, NY, OR, VT, WV) and New York City	≥10 wks	Yes	Yes	Yes	No	Yes	Yes	Yes
Nickel	2013	IFPS-II	2000–2001	85% NH White women, higher education and income than nation	Continuous through 12 mos (survival analysis)	Yes	No	Yes	No	No	No	No

[a] Steps 1–3, and 5 were not examined by any of the studies
n/s = not studied

A study of Colorado mothers using PRAMS data found that breastfeeding in the first hour after birth (Step 4), feeding the infant only breast milk (Step 6), rooming-in (Step 7), not giving the infant a pacifier (Step 9), and the hospital providing a phone number to call for breastfeeding support (Step 10) were all associated with increased breastfeeding rates at eight weeks postpartum. Telling the mother to breastfeed on demand (Step 8) was the only practice not associated with breastfeeding continuation for at least eight weeks (59). This study, however, did not adjust for confounding factors.

Ahluwalia et al. (2012) recently analyzed PRAMS data from 12 states to examine the effects of six BFHI practices on breastfeeding continuation for at least 10 weeks. Five of the recommended hospital practices showed a statistically significant positive association with breastfeeding continuation: helping mothers initiate breastfeeding within the first hour after birth (Step 4), giving the newborn breast milk only (Step 6), encouraging mothers to breastfeed on demand (Step 8), not giving the baby a pacifier (Step 9), and providing a telephone number for breastfeeding support (Step 10). The practice with the strongest positive association was giving breast milk only. Rooming-in (Step 7) was the only examined practice not significantly associated with breastfeeding for at least 10 weeks (60).

A 2013 study by Nickel et al. examined the effect of several hospital practices on the duration of breastfeeding using survival analysis, enabling the study of breastfeeding as a continuous duration outcome (61). When the practices were examined individually, only giving supplemental feedings in-hospital or receiving a hospital formula gift pack (a liberal interpretation of noncompliance with Step 6) was associated with reduced breastfeeding duration. This practice was associated with a 21% decrease in breastfeeding duration, or a 10.5-week reduction in overall length of breastfeeding. While other practices were not significantly related to breastfeeding duration individually, combinations of certain practices were related. For instance, the combination of on-demand feeding with not giving supplemental feedings or a formula gift pack (Step 6), rooming-in (Step 7), or no pacifiers (Step 9) were significantly associated with longer breastfeeding duration. This suggests that combinations of key practices, rather than any one hospital practice alone, may be important for improving breastfeeding duration.

In addition to the study of individual practices, several studies examined the cumulative effect of the number of practices experienced in the hospital. In the 2001 DiGirolamo study, infants who experienced zero practices were seven times more likely to stop breastfeeding by six weeks compared to those experiencing all five measured practices. If all the infants in the sample had experienced all five practices in the study, only 6% would have stopped breastfeeding by six weeks instead of the observed 17% (58). The 2008 DiGirolamo study produced similar results: infants experiencing zero or one step were seven times more likely to stop breastfeeding by six weeks compared to infants experiencing all six measured steps (56). Nickel et al. (2013) also demonstrated the significant effect of multiple hospital practices on overall breastfeeding duration. Compared to women exposed to six BFHI steps, those exposed to four or five steps had a nine-week reduction in breastfeeding and those exposed to only two or three steps had a 12-week reduction in breastfeeding duration (61). Given that the steps seem to work together in an additive manner to improve breastfeeding duration, it is important for hospitals to adopt as many BFHI steps as possible, even if they are not yet ready to apply for full BFHI designation.

b. **Evidence for Baby-Friendly: Breastfeeding exclusivity**

The impact of BFHI practices on breastfeeding exclusivity in the United States has been less extensively studied than the impact on breastfeeding duration. A survey of 2001 US Baby-Friendly hospitals showed that in-hospital exclusive breastfeeding rates were much higher than the national average, but practices were not individually examined (62). One study showed a dose-relationship between the length of early skin-to-skin time between mother and baby (a component of Step 4) and the odds of exclusive breastfeeding during the hospital stay (43, 63) The literature is also sparse when it comes to examining the lasting impact of the specific Ten Steps maternity care practices on exclusive breastfeeding beyond the hospital stay or early breastfeeding periods.

Two recent studies focus on how recommended maternity care practices are associated with achieving intentions to exclusively breastfeed. DeClerq et al. (2009) studied the effect of seven hospital practices on exclusive breastfeeding at one week postpartum among women who intended to exclusively breastfeed. They found that first-time mothers (primiparas) who experienced each of the six measured

practices (Step 4: hospital helped get started breastfeeding; Step 5: hospital showed how to position baby to avoid soreness; Step 6: hospital did not provide supplemental feedings; Step 8: hospital encouraged to breastfeed on demand; Step 9: hospital did not give a pacifier; and Step 10: hospital told mother about community support resources) were more likely to be exclusively breastfeeding at one-week postpartum than those not experiencing each practice. Among mothers with other children (multiparas), the only steps significantly associated with increased exclusive breastfeeding rates at one-week postpartum were: encouraging to breastfeed on demand (Step 8) and not providing supplemental feedings (Step 6). Additionally, for both primiparas and multiparas, there was a significant dose-response relationship in the number of recommended practices experienced and the percent of mothers fulfilling their expectation to exclusively breastfeed for at least one week. The observed dose-response effect was stronger, however, for primiparas than multiparas (37).

Secondly, Perrine et al (2012) studied the effect of six specific BFHI practices on the likelihood that a woman would achieve her intended duration of exclusive breastfeeding. When the analyses controlled for all hospital practices simultaneously, only no supplemental feedings (Step 6) was associated positively with improved achievement of exclusive breastfeeding intentions. The cumulative effects of the practices were also studied; of women who experienced zero or one of the practices, only 23.4% achieved their goal, compared to 46.9% of women who experienced all six practices, and the intermediate categories followed a dose-response trend (38).

While these studies show promising results, they are limited because they address only women who intended to exclusively breastfeed their infants. More generalized studies are needed on the exclusive breastfeeding behaviors of all women, as hospitals have a responsibility to inform and support women to make the healthiest feeding choice for their infant (41). Women who may have entered the hospital uncertain about their feeding intentions or intending to use formula should still be given the encouragement and support they need to potentially overcome their hesitations for and barriers to exclusive breastfeeding. More research is also needed to understand how maternity care practices might influence a woman’s exclusive breastfeeding behavior weeks to months after hospital discharge.

2. **Hospital discharge formula gift packs**

Infant formula began to be produced in the late 19th century and dozens of breast milk substitutes were available by the early 1900s. In 1929, the American Medical Association created a committee to approve the safety of infant formulas and restricted the advertising of formula to medical personnel only. However, in 1988, the formula industry began to advertise directly to the general public, despite the opposition of the medical profession. Shortly thereafter, the AAP released a statement opposing such public marketing, listing concerns that the advertisements would have a negative effect on breastfeeding and interfere with physician's advice on infant feeding (64). Nevertheless, this marketing continued and it became common practice for formula manufacturers to provide US hospitals with free formula for their nurseries and gift packs for new mothers upon hospital discharge (65).

In 1981, the International Code of Marketing of Breast-milk Substitutes was developed by the WHO to protect and promote breastfeeding by ensuring that formula and other substitutes for breast milk are appropriately marketed and distributed in the healthcare setting (66). Compliance with this Code is required (as part of Step 1) to receive Baby-Friendly designation, including a requirement that "no pregnant women, mothers, or their families are given marketing materials or samples or gift packs by the facility that include breast-milk substitutes, bottles/teats, pacifiers, other infant feeding equipment or coupons" (67, pg. 38). Despite this strong recommendation, the distribution of hospital discharge formula gift packs is pervasive in US hospitals. In a study of mostly non-Hispanic White women, 83.7% of new mothers reported receiving a discharge pack with free formula or formula coupons (56). This number may be even higher among women of low SES and among minorities. Among participants in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) in Los Angeles County, 87.3% reported receiving a hospital formula discharge gift pack (68). Distribution of formula to mothers within the hospital setting may send a mixed message to mothers who may be verbally encouraged by hospital staff to breastfeed, but see formula used extensively in the hospital setting. Reiff (1985) found that hospital modeling of formula use had a greater influence on mothers' early infant feeding choices than verbal instructions about infant feeding from hospital staff (69).

The evidence for the effect of hospital discharge formula gift packs on breastfeeding points toward a reduction in exclusive breastfeeding duration, but not overall duration. A Cochrane review of nine randomized controlled trials from the 1980s and 1990s showed that giving hospital discharge packs with either formula samples or formula promotional material reduce exclusive breastfeeding at various time points up to 10 weeks postpartum, but there was no significant effect on general breastfeeding duration (70). More recent observational studies have generally confirmed these findings, though one study in New Jersey found that receipt of formula in the hospital discharge pack was associated with reduced overall breastfeeding during the first 10 weeks, but not exclusive breastfeeding (56, 59, 68, 71, 72).

3. **Gaps in current research on racial/ethnic disparities**

A WHO report on the effectiveness of the BFHI Ten Steps acknowledges that the hospital environment is only one of the many factors influencing a woman's decision and ability to breastfeed. Given the wide array of socioeconomic and cultural factors that influence breastfeeding, the report states that "it would not be surprising therefore to learn that implementation of the 'Ten Steps' affects breastfeeding differently in different communities" (43, pg. 2). Little is known about how the effectiveness of maternity care practices varies across different racial/ethnic and ethnic groups in the United States and how disparities in breastfeeding are affected by this potential variation in implementation.

Even if an intervention is successful at improving overall breastfeeding outcomes, it will not necessarily mean that the intervention will reduce *racial/ethnic disparities* in those outcomes. Two scenarios could cause disparities to increase after implementation of an overall effective intervention: differential access and differential effectiveness. Differential access occurs when not all subgroups experience the intervention equally; if the disadvantaged population was less likely to experience an effective intervention, the extent of the health disparity would increase (28, 29). The second scenario occurs when the overall effect of an intervention masks differential effectiveness. Vulnerable populations

may benefit less from population-based health interventions than advantaged populations, so the disparity between the groups could increase even if they experience equal access to the intervention (28).

The vast majority of the literature evaluating the effect of BFHI practices on breastfeeding does not address their differential experience or differential effectiveness, nor whether the implementation of these practices could lead to changes in racial/ethnic disparities. Some have suggested that hospitals serving low-wealth and minority populations may be more hesitant to implement BFHI practices because of underlying assumptions that these populations are not interested in breastfeeding, but data are not available to support such suspicions (48). Virtually no studies have examined whether the experience of BFHI practices varies by race/ethnicity. One study demonstrated that non-Hispanic Black and Hispanic women were less likely to feed their infant only breast milk in the hospital (Step 6) (68).

Only one known study has examined potential differential effectiveness of BFHI practices across racial/ethnic groups in the United States. Ahluwalia et al. (2012) examined the race/ethnicity-specific effects of six BFHI practices on breastfeeding continuation for at least 10 weeks. The only BFHI practice to consistently improve breastfeeding duration across non-Hispanic White, non-Hispanic Black, and Hispanic women was not giving formula supplements in the hospital (Step 6). The other practices showed varying levels of effectiveness by race/ethnicity, though the statistical significance of the effect modification by race/ethnicity was not tested. The results seem to generally imply, however, that the magnitude of the association between many BFHI practices and breastfeeding duration is stronger among non-Hispanic Black women and weaker among Hispanic women compared to non-Hispanic White women (60). The potential differential effectiveness demonstrated by this study has implications for how racial/ethnic disparities in breastfeeding duration would be affected by implementation of the practice. Further validation of these results and the extension of this study to exclusive breastfeeding are needed.

Understanding whether and how BFHI practices are influencing racial/ethnic disparities has important implications for practice recommendations and program implementation. If gaps exist in the experience of BFHI practices by race/ethnicity, this may spark accelerated implementation of the Ten Steps in hospitals serving disadvantages populations. Understanding which practices may be most

effective in various subpopulations would also help target breastfeeding interventions to the populations with the lowest breastfeeding rates. Evidence is needed to understand how differential receipt and effectiveness of maternity care practices will together enable (or impede) reduction of racial/ethnic disparities in breastfeeding duration and exclusivity.

C. **Mediation Analysis Methods**

The application of mediation methods to racial/ethnic disparities in breastfeeding may help clarify whether specific interventions impact the observed disparities. Mediation refers to a chain of events, or the process by which one antecedent variable affects an intermediate (mediating) variable, which, in turn, affects an outcome variable (73). Most epidemiologic research focuses on answering questions of whether an exposure variable, *A*, is related to an outcome variable, *Y*, and whether this relationship is a causal association. Mediation involves adding a third variable, *M*, to this process in such a way that *M* is conceptualized as being in the pathway between *A* and *Y*. Some or all of the effect of *A* on *Y* occurs because *A* causes *M*, and *M*, in turn, causes *Y* (73, 74). The mediator helps to clarify the relationship between the exposure and outcome by exploring the pathways through which the exposure exerts its effect on the outcome (75). Specific methods have been developed to help explain the mechanisms by which these variables affect each other and this family of statistical methods is called mediation analysis. In mediation analysis, the goal is to decompose the overall relationship of the independent and dependent variables, or the total effect (TE), into two paths: the indirect effect (IE) and the direct effect (DE). The IE represents the effect of *A* on *Y* that occurs because of the effect of *A* on the mediating variable, *M*, and *M*'s subsequent effect on *Y*. The DE links *A* and *Y* through all potential pathways that do not operate through *M* (73, 74).

Mediation analysis methods have a long history in the social sciences, where they were heavily influenced by the work of Baron and Kenny (73, 75, 76). Over the last several decades, many new forms of mediation analysis methods have been developed to accommodate increasingly complex regression models and data types (75). Despite this, mediation methods have been slow to breach epidemiologic

research and their full potential and utility for the field have yet to be realized. In particular, the application of mediation methods to the evaluation of interventions may be especially relevant for maternal and child health and breastfeeding research.

1. **Differentiating mediation from other third-variable relationships**

Before conducting mediation analysis, it is important to establish that the proposed mediator variable is, in fact, a truly intermediate variable in the exposure-outcome process. There are several other ways in which a third variable may be related to *A* and *Y*, and mediation analysis would not be appropriate in these situations. For instance, a third variable, *Z*, may be associated with both the exposure and the outcome, but not be part of the causal chain between them. In this confounding situation, *Z* could cause both *A* and *Y*, or *Z* could be noncausally associated with either or both of the variables. In this case, estimating the *A* → *Y* relationship without accounting for *Z* would lead to an incorrect inference, but this variable does not function as a stage in any indirect path between *A* and *Y* (73, 74). Alternatively, a third variable may cause the *A* → *Y* association to differ at various levels of *Z*. This is an example of effect modification or interaction, also called moderation by the social science literature (73). In interaction, *Z* does not operate within the causal mechanism, but it affects the magnitude of the causal association of exposure and outcome. Figure 1 depicts how the third variable relates to *A* and *Y* in mediation, confounding, and interaction systems to demonstrate their different conceptualizations.

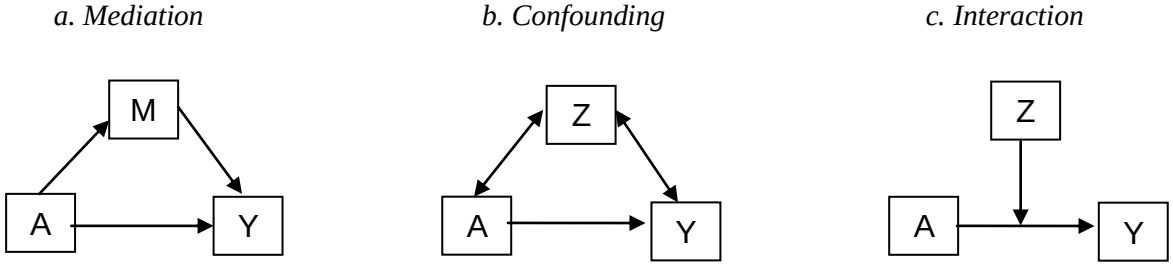


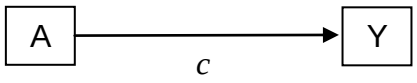
Figure 1. Types of relationships a third variable may have with an exposure and outcome.

The traditional epidemiologic processes for assessing confounding and mediation are algebraically equivalent and differ only in theoretical approach. In both situations, the effect of the third variable (either the mediating or confounding effect) is assessed by calculating the difference between two estimates of the $A \rightarrow Y$ relationship: one predicting Y only from A and the second predicting Y from both A and M or Z. The statistics alone cannot determine whether mediation or confounding is present, as they are mathematically equivalent. The true distinction between mediation and confounding is that mediation involves an explicitly defined causal relationship among the variables and defines a priori a precise mechanism of $A \rightarrow M \rightarrow Y$. Under the mediation hypothesis, both the DE and IE may be of interest and the role of the mediator can be quantified. The confounding hypothesis, on the other hand, seeks only to adjust for the confounder to obtain an unbiased estimate of the $A \rightarrow Y$ relationship (74). Therefore, the features that separate mediation from confounding are a strong theory for causal order and an interest in both the DE and IE. Once a strong case for the third variable to be a mediator is made, one can proceed with appropriate mediation analysis methods.

2. **Historical development of mediation methods**

The earliest formal approach to assessing the presence of mediation—the causal steps criteria—were developed by Baron and Kenny in 1986 (73, 75, 76). In this approach, the relationships between the *A*, *M,* and *Y* variables are specified as in Figure 2.

a. Total effect of an exposure on an outcome



b. Direct and indirect effects of an exposure on an outcome

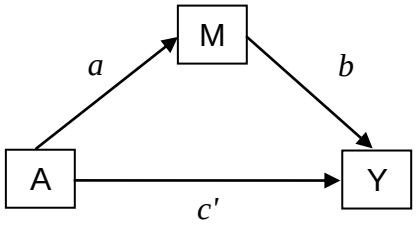


Figure 2. General mediation framework.

The coefficients for the relationships between the variables are estimated using three regression models shown in equations #1–3:

(1) $Y = i_i + cA + e_1$

(2) $Y = i_2 + c'A + bM + e_2$

(3) $M = i_3 + aA + e_3$

Where i_i, i_2, and i_3 are intercepts,

Y is the dependent variable,

A is the independent variable,

M is the mediator variable,

a is the coefficient relating A and M,

b is the coefficient relating M and Y,

c is the coefficient relating A and Y (not adjusted for M),

c' is the coefficient relating A and Y adjusted for M, and

e_1, e_2, and e_3 represent the residual errors for each equation

After the coefficients are estimated, the causal steps approach assesses whether mediation is present by applying four statistical criteria:

1. *c* must be significant in equation 1 (a significant effect of A on Y without adjusting for M),
2. *a* must be significant in equation 2 (a significant effect of A on M),
3. *b* must be significant in equation 3 (a significant effect of M on Y), and
4. *c* must be greater (in absolute value) than *c'*.

If all four criteria are present, mediation is considered to be present. The causal steps approach was developed as a method to identify the presence of mediation, but it does not directly address the magnitudes and interpretation of the effects. Based on Baron and Kenny's approach, other researchers have developed several methods for quantifying the mediating effect (73, 76).

The Baron and Kenny model serves as the foundation for statistical methods developed to quantify the role of the mediator in a causal process. In the traditional mediation analysis approach, the magnitude of the mediating effect (the IE) may be calculated either by the difference in coefficients or product of coefficients methods. The difference in coefficients method determines the value of the IE by subtracting the coefficient relating *A* and *Y* after adjusting for *M* from the coefficient relating *A* and *Y* without adjusting for M, or *c–c'*. The product of coefficients method determines the value of the IE by multiplying the coefficients for the $A \rightarrow M$ and the $M \rightarrow Y$ paths, or *ab* (73). MacKinnon et al. (1985) demonstrated the equivalence of *c–c'* and *ab* for models involving a continuous outcome variable (77).

Sobel (1982, 1986) developed a test for the significance of the IE by estimating the standard error of the product of the coefficients (*ab*) (78, 79). The ratio of *ab* to its standard error is used as a test statistic employing the standard normal distribution to determine whether the IE significantly differs from zero (73). This test, however, requires the assumption that the sampling distribution of the IE is normal. The distribution of *ab* has been shown to have asymmetric tendencies, so this test may not be appropriate and is no longer the preferred method for determining the significance of the IE. (76)

A commonly used effect size measure in mediation analysis is the proportion mediated, which represents the percent of the TE accounted for by the IE. It is calculated by dividing the IE (either *ab* or

c–*c'*) by the TE (*c*) (73, 80). This measure can often be misleading or confusing, however, since it does not have the properties of a true proportion. For instance, *c* can be smaller than *ab*, leading to a proportion greater than 1. As another example, *c* and *ab* can have different signs, leading to a negative proportion (76). This measure is not recommended in these situations because it does not have a meaningful interpretation. Additionally, the proportion mediated has been shown to be highly unstable for small sample sizes (<2,000) and has performed well only when all variables in the mediation model were continuous (77).

3. **Limitations of traditional mediation analysis methods**

There are many assumptions inherent in the traditional mediation model: (1) uncorrelated error terms in the three regression equations, (2) no *A*–*M* interaction, (3) no misspecification of causal order or direction, (4) no misspecification due to unmeasured confounders, and (5) no misspecification due to imperfect measurement. However, even when these assumptions are met, simulation studies have shown that the power to detect mediated effects using the causal steps criteria is very low, as is the power for tests based on the normal distribution of the IE estimators produced by either the difference or product methods (73, 76).

Traditional mediation analysis methods have been limited by their ability to decompose the TE into direct and indirect effects in settings involving nonlinear relationships. The two estimates of the IE calculated by the product and difference methods are not equivalent for nonlinear regression models, such as log-linear, logistic, or survival models (73). The validity of the product and differences methods depends on the assumption that the residual error terms of the regression equations are uncorrelated. Because regression equations involving dichotomous or categorical outcomes have fixed residuals, this assumption is not met and the regression coefficients depend on other independent variables and the scaling of the equations (81, 82). In this situation, the effects cannot be properly estimated and a transformation of the results is needed to yield valid estimates (73). Winship and Mare (1983) developed a method of standardizing the regression coefficients to solve this problem and allow correct IE estimation (83). While this produces estimates properly scaled to each other (thus producing equivalent IE

estimates via the product and difference methods), the effect estimates themselves have no meaningful interpretation. So, while the significance of the standardized IE can be tested, its magnitude cannot be interpreted as a meaningful value.

The traditional mediation analysis model also has limited practical value because it cannot accommodate models involving exposure-mediator interaction. When the exposure and mediator interact to produce the outcome, the traditional method of calculating a single IE is meaningless because the IE would vary across levels of the mediator variable. The traditional mediation model does not accommodate this interaction nor does it provide statistical methods for calculating the DE and IE in the presence of such interaction (84). Because of these weaknesses, researchers have sought to develop more flexible models and equations for estimating and assessing the direct and indirect effects. Recent work has used the counterfactual framework to extend traditional mediation analysis methods to a more general form that can accommodate nonlinear models and interactions (75, 81, 84).

4. **Counterfactual approach to mediation**

Neyman and Fisher originated the *counterfactual* and *potential-outcome* frameworks in the early 20th century, which formalize notions of cause and effect from randomized experiments (85). Rubin then extended the notion of potential outcomes to causal effects and his work forms the basis for modern epidemiologic thought around causality (86). The counterfactual framework is premised on examining how an individual's outcome would have changed if he/she experienced two different exposure statuses. For a dichotomous exposure, this would play out as a comparison between the outcome an individual would have experienced if they were exposed to a risk factor and the outcome *that same individual* would have experienced if they were unexposed. In practice, only one of these outcomes is factual and observable, according to the true exposure status of the individual and the other outcome is hypothetical, counter to fact, and cannot be observed (87). Because the various potential outcomes within an individual are not all observable, the true causal effects within individuals are unknowable. The statistical solution to this problem is to best approximate the population average causal effect using observed data for the exposed and unexposed groups (87–89). The ability to compare exposed and

unexposed groups is rooted in the concept of exchangeability, which says that the two groups would have had the same outcome if, counter to fact, they had had the same exposure (90, 91).

Pearl (2001) first derived a general estimation formula for mediation based on the counterfactual framework to overcome the limitations of traditional methods in dealing with discrete outcomes and interactions (92). This formula is based on general structural equation models defining the three variables in the mediation model as arbitrary functions of their antecedent variables and residual errors. Pearl (2012) later derived formulas for the estimation of the total, direct, and indirect effects in terms of counterfactual statements (81).

The TE is the simplest effect to define and estimate since it does not require the specification of the mediator. The TE is the change in outcome resulting from a change in exposure from the baseline value of a^* to the endpoint of a. If the exposure is dichotomous, a^* is equivalent to being unexposed and a is equivalent to being exposed. In terms of the counterfactual model, the TE is defined by equation 4. The difference in the two expected values of Y is equivalent to the regression slope of A on Y estimated by ordinary least squares regression (81, 92).

(4) $$TE_{a^*,a} = E(Y|A = a) - E(Y|A = a^*)$$

The controlled direct effect (CDE) is the most basic form for estimating the DE by fixing the mediator at a set level. Controlling for M simulates blocking the mediating pathway by preventing A from transmitting its effect to Y through M. The CDE can therefore be interpreted as the effect of A on Y occurring through all paths not involving the mediator. The counterfactual definition for the CDE is given in equation 5. When exposure-mediator interaction is present, however, the value of the CDE will vary according to the level of the mediator. One could choose to report multiple CDEs for each potential value of m, but there will not be one measure that will adequately describe the effect of the exposure (81, 92).

(5) $$CDE_{a*,a|m}(Y) = E(Y|A = a, M = m) - E(Y|A = a^*, M = m)$$

In settings involving exposure-mediator interaction, it is more meaningful to describe the natural direct effect (NDE), which is the expected change in the outcome resulting from a change in exposure

from a^* to a while keeping the mediator at whatever level would have been obtained if the exposure were a^*. Under the counterfactual approach, the NDE can be estimated by equation 6. The NDE is a weighted average of the CDE, using the pre-transition distribution of the mediator $P(m|a^*)$ as the weighting function. In the presence of exposure-mediator interaction, the NDE still retains its meaning and specifies the DE at the observed mean mediator value for the unexposed (81, 92). When exposure-mediator interaction is not present, the CDE and NDE are mathematically equivalent (84).

(6) $$NDE_{a^*,a}(Y) = \sum_z [E(Y|a,m) - E(Y|a^*,m)]P(m|a^*)$$

In practice, the IE has no equivalent controlled interpretation parallel to the CDE because it is not possible to selectively control for variables that would disable only the direct link between *A* and *Y* (81). Instead, a natural indirect effect (NIE) can be defined as the expected change in outcome by holding exposure constant at a^* and changing the mediator value from what it would have naturally been if the exposure were a^* to what it would have been if the exposure were *a*. Equation 7 estimates the NIE (81, 92):

(7) $$NIE_{a^*,a}(Y) = \sum_z \{E(Y|a^*,m) * [P(m|a) - P(m|a^*)]\}$$

a. **Effect decomposition for dichotomous outcomes**

To further extend mediation analysis to epidemiology, VanderWeele and Vansteelandt (2010) applied Pearl's approach to develop estimation equations for effect decomposition on the odds ratio (OR) scale (84). Pearl's general formulas for the total, direct, and indirect effects are given in terms of risk differences estimated from linear regression, but the same counterfactual notions inherent in these formulas can be applied to logistic regression, yielding OR forms of the equations (84, 92).

The TE, CDE, NDE, and NIE on the OR scale are shown respectively in equations 8–11. In the notation of VanderWeele, Y_a and M_a represent the values of the outcome and mediator, respectively, that would have been observed had the exposure been set to level *a*. The outcome that would have been observed had the exposure been set to *a* and the mediator been set to *m* is denoted by Y_{am} (84). These

equations also condition the odds of the outcome on a set of confounders, *c*, for which control is made in the logistic regression models. On the OR scale, the TE decomposes into the product of the NDE and NIE, and this decomposition holds even in the presence of exposure-mediator interaction (75, 84).

(8) $$\mathrm{OR}^{\mathrm{TE}}_{a,a^*|c} = \frac{P(Y_a = 1|c) \,/\, \{1 - P((Y_a = 1|c)\}}{P(Y_{a^*} = 1|c) \,/\, \{1 - P((Y_{a^*} = 1|c)\}}$$

(9) $$\mathrm{OR}^{\mathrm{CDE}}_{a,a^*|c}(m) = \frac{P(Y_{am} = 1|c) \,/\, \{1 - P((Y_{am} = 1|c)\}}{P(Y_{a^*m} = 1|c) \,/\, \{1 - P((Y_{a^*m} = 1|c)\}}$$

(10) $$\mathrm{OR}^{\mathrm{NDE}}_{a,a^*|c}(a^*) = \frac{P\left(Y_{aM_{a^*}} = 1\middle|c\right) \,/\, \{1 - P(\left(Y_{aM_{a^*}} = 1\middle|c\right)\}}{P\left(Y_{a^*M_{a^*}} = 1\middle|c\right) \,/\, \{1 - P(\left(Y_{a^*M_{a^*}} = 1\middle|c\right)\}}$$

(11) $$\mathrm{OR}^{\mathrm{NIE}}_{a,a^*|c}(a) = \frac{P\left(Y_{aM_a} = 1\middle|c\right) \,/\, \{1 - P(\left(Y_{aM_a} = 1\middle|c\right)\}}{P\left(Y_{aM_{a^*}} = 1\middle|c\right) \,/\, \{1 - P(\left(Y_{aM_{a^*}} = 1\middle|c\right)\}}$$

In practice, the estimation of the CDE, NDE, and NIE requires the estimation of two regression models, shown in equations 12 and 13. The coefficients from these regression models are used to quantify the CDE, NDE, and NIE on the OR scale as shown in equations 14–16. These equations are also directly transferrable to the risk ratio scale if log-linear regression is used for equations 12–13 instead of logistic regression (75).

(12) $$\text{logit}\,(P(Y = 1|a, m, c) = \theta_0 + \theta_1 a + \theta_2 m + \theta_3 am + \theta_4' c$$

(13) $$\text{logit}\,(P(M = 1|a, c) = \beta_0 + \beta_1 a + \beta_2' c$$

(14) $$OR_{CDE} = exp(\theta_1 + \theta_3 m)(a - a^*)$$

(15) $$OR_{NDE} = \frac{\exp(\theta_1 a)\{1 + \exp(\theta_2 + \theta_3 a + \beta_0 + \beta_1 a^* + \beta_2' c)\}}{\exp(\theta_1 a^*)\{1 + \exp(\theta_2 + \theta_3 a^* + \beta_0 + \beta_1 a^* + \beta_2' c)\}}$$

(16) $$\mathrm{OR}_{NIE} = \frac{\{1 + \exp(\beta_0 + \beta_1 a^* + \beta_2' c)\}\{1 + \exp(\theta_2 + \theta_3 a + \beta_0 + \beta_1 a + \beta_2' c)\}}{\{1 + \exp(\beta_0 + \beta_1 a + \beta_2' c)\}\{1 + \exp(\theta_2 + \theta_3 a + \beta_0 + \beta_1 a^* + \beta_2' c)\}}$$

Note that the regression model represented by equation 12 and the subsequent effect formulas in equations 14–16 all account for exposure-mediator interaction, represented by θ_3 (75, 84). If interaction is

not present, θ_3 can be set to zero and all of the terms involving this coefficient will drop out of the equations. In this case, the OR for the CDE and NDE will be equivalent (75).

b. **Identification assumptions**

Specific identifiability assumptions must be met for the effects from a mediation analysis to have valid causal interpretations (75, 84, 92). To produce an unbiased CDE, two assumptions must be met: (1) there is no exposure—outcome confounding, conditional on a set of controlled covariates, *c*; and (2) there is no mediator—outcome confounding, conditional on *A* and *c*. Furthermore, the estimation of the NDE and NIE requires two more assumptions in addition to those in place for the CDE: (1) There is no exposure—mediator confounding, conditional on *c*; and (2) there is no effect of A that confounds the mediator—outcome relationship, conditional on *c*.

Figure 3 demonstrates graphically how these assumptions operate in practice. Meeting assumptions 1–3 requires respectively controlling for all confounding variables Z_1, Z_2, and Z_3. In practice, one does not need to distinguish which covariates are confounders for each pathway, but simply control for a group of covariates that includes all potential confounders of any of the specified relationships. Additionally, the fourth assumption requires that there is no causal relationship between *A* and Z_2 (75). If there are any Z_2 variables caused by the exposure, even the measurement and control for this variable does not allow for the valid identification of the NDE and IE (84).

These identification assumptions cannot be tested from the data, but must be evaluated based on subject knowledge and conceptual understanding of the pathways involved in the causal mechanism (84). In any observational study, there will likely be several unmeasured confounding variables not captured in the analysis, thus threatening the validity of the results via residual confounding. While the complete control of every conceivable confounder is not practical or feasible, it is possible to evaluate how unmeasured confounding may affect the mediation analysis results by using sensitivity analysis (93).

The potential impact of unmeasured confounders of any of the relationships in the mediation model can be tested using sensitivity analysis. Control for mediator-outcome confounding is of particular interest in sensitivity analysis because it is the type of confounding most often ignored. To evaluate the

effect of unmeasured confounding on the DE and IE estimates, bias terms are calculated and applied to the estimated regression coefficients to obtain bias-corrected coefficients. The NDE and NIE can then be recalculated using the bias-corrected estimates to obtain the bias-corrected effect estimates. The details of these sensitivity analysis methods have been described by VanderWeele (93, 94).

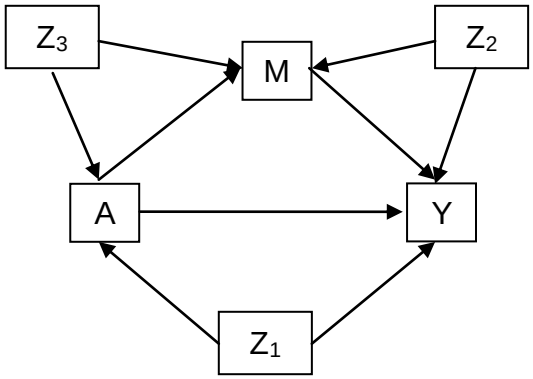


Figure 3. Confounding variables affecting identification of direct and indirect effects.

III. RACIAL/ETHNIC DISPARITIES IN BREASTFEEDING INITIATION, DURATION, AND EXCLUSIVITY: DOES SOCIOECONOMIC STATUS MATTER?

Breastfeeding is the healthiest way to feed an infant and confers many health benefits to both mother and baby, including lower rates of diarrheal disease (2), lower respiratory tract infections (3), eczema (4), acute ear infections (3, 5), asthma (3, 6), childhood leukemia (7), child obesity (8,9), SIDS (3, 11), celiac disease (10), early-onset inflammatory bowel disease (12), breast and ovarian cancer (1, 3, 13), and type-2 diabetes mellitus (15). Additionally, many studies have documented the dose-response relationship of breastfeeding with many of the child and maternal health outcomes previously listed (3, 5, 7–9, 11, 15). Because of these health benefits, the AAP recommends that infants be breastfed for at least the first year of life and fed only breast milk during the first six months of life (17).

Despite these known breastfeeding benefits and professional recommendations, many women in the United States do not breastfeed their infants, breastfeed only for short periods of time, or supplement breastfeeding with formula feeding. In 2010, 76.5% of US infants were breastfed, 27.0% were breastfed for the recommended duration of at least one year, and only 16.4% were exclusively breastfed for the recommended duration of six months and breastfeeding rates in Illinois were lower than the nation for all outcomes (18).

In addition, these overall rates mask wide variation in breastfeeding across racial/ethnic subgroups. In 2008, 80.0% of Hispanic infants in the United States were ever breastfed, compared to 75.2% of non-Hispanic White infants and only 58.9% of non-Hispanic Black infants. Non-Hispanic Black infants are also less likely to be breastfed for a full year, with only 12.5% still breastfeeding at 12 months, compared to 24.3% of non-Hispanic White and 26.3% of Hispanic infants (31). Studies have shown that the Black-White disparity in breastfeeding initiation is decreasing, but the disparity in breastfeeding continuation has persisted (24, 31). Some studies have also shown lower rates of exclusive breastfeeding among non-Hispanic Black infants than those of other races/ethnicities (32, 33), but other studies have shown no differences in exclusive breastfeeding across racial/ethnic groups (34). Furthermore, infants

born to mothers with lower educational attainment and lower household incomes are less likely to be breastfed than the infants born to their more educated and wealthier counterparts (19–21), and these socioeconomic differences persist within racial/ethnic subgroups (19).

It is common practice in social epidemiology to adjust for either race/ethnicity or dimensions of SES to try to discern the independent effect of the other factor on health outcomes. While race/ethnicity and SES (measured through income, education, employment, or other factors) may have independent effects on some health outcomes (22), the meaning of these independent effects may not always be useful. Because the social structure of the United States produces strong correlations of race/ethnicity and SES, defining their independent effects does not reflect the true relationship and interplay of these factors in society (23). For instance, some measures of SES are not equivalent across racial/ethnic groups. Minority communities tend to receive poorer quality education for any given education level, have lower purchasing power at a given income, and lower income return on education (26, 95). While some have suggested a reduced emphasis on racial/ethnic disparities and an increased emphasis on SES-based disparities, this approach is not an appropriate substitute because SES does not fully explain racial/ethnic disparities and there is evidence of interaction between these two factors (26). This suggests there is still inherent value in examining racial/ethnic disparities to try to understand the processes resulting in unequal health outcomes, but that researchers need to be clear and explicit about how to deal with the entanglement of race/ethnicity and SES.

In the case of breastfeeding, there are many differences between racial/ethnic groups that could potentially contribute to differences in behaviors. The decisions to begin and continue breastfeeding are not simple, but are affected by a wide variety of factors, including: social support, self-efficacy, economic resources (e.g., paid maternity leave, affordability of breast pumps and formula), social norms, historical race relations leading to cultural preferences, experience of discrimination, and many others (25, 26, 35, 96–98). Some of these influences may be tightly linked to SES, while others may operate independently. Few studies have examined the interplay of race/ethnicity and SES in influencing breastfeeding behaviors

(24). The purpose of this study is to quantify the extent of racial/ethnic disparities in breastfeeding initiation, duration, and exclusivity among Illinois women, while examining how the results and practical interpretation of the disparities change according to whether and how SES is considered in the analysis.

A. Methods

1. Data source

This study used 2004–2008 data from the IL-PRAMS, sponsored by Centers for Disease Control and Prevention (CDC). The IL-PRAMS is a mail and phone survey of women who recently delivered a live-born infant. Birth certificates serve as the sampling frame and most women complete the survey three to six months after delivery. The PRAMS asks women about their attitudes, behaviors, and experiences before, during, and after pregnancy. Various demographic and medical fields from the infant's birth certificate are linked to the completed surveys (99). The IL-PRAMS uses stratified sampling by infant birth weight (<2500 grams and ≥2500 grams) to ensure adequate sample sizes of high-risk populations (99, 100). The IL-PRAMS had response rates ranging from 72.0% to 79.1% during 2004–2008, meeting the minimum required response rates set by CDC (101).

2. Variable definition

Three types of breastfeeding outcomes are of interest in this study: breastfeeding initiation, breastfeeding duration, and breastfeeding exclusivity. Breastfeeding initiation was defined as a yes/no variable based on a woman's self-report of whether she ever breastfed her infant. The negative response (never breastfed) was the outcome of interest.

Among women who initiated breastfeeding, breastfeeding duration and exclusivity were determined at three time points: two, six, and 12 weeks after delivery. The early time point represents a critical period in early breastfeeding establishment, and the later times are typical points when a working mother would return to employment. Additionally, 12 weeks is the latest common time point that can reasonably be used to assess breastfeeding from PRAMS for most survey participants because of the

timing of the survey. For all analyses, the duration and exclusivity outcomes are framed as the prevalence of *termination* of breastfeeding before each time point.

The length of overall breastfeeding duration is reported by the mother on the PRAMS survey. If the infant was still breastfeeding at the time of the survey, the duration of breastfeeding was censored at infant's age (in weeks) at the survey time. Duration was dichotomized for the three time points of interest and any observations censored prior to the time point were excluded from analyses of that outcome.

Exclusive breastfeeding is defined as the length of time the infant was fed only breast milk and no other food or drinks. The mother reports to PRAMS the age of the infant (in weeks) when she first fed the baby any food/liquid other than breast milk. If the infant had not yet received any foods or liquids other than breast milk at the time of the survey, the length of exclusive breastfeeding was censored at the infant's age at the survey time. Exclusive breastfeeding duration was then dichotomized according to the three time points of interest. For those missing information on first food, information on cessation of any breastfeeding was used to determine whether exclusive breastfeeding had ceased by each time point. For example, if a woman had stopped overall breastfeeding by two weeks, it was assumed she was not exclusively breastfeeding at two, six, or 12 weeks. Finally, any observations censored prior to the time point of interest were excluded from analysis of each outcome.

Birth certificate variables on maternal race and maternal Hispanic origin were combined to create a race/ethnicity variable of three categories: non-Hispanic White (hereafter "White"), non-Hispanic Black (hereafter "Black"), and Hispanic. Women of other race/ethnicities were excluded due to small sample sizes in IL-PRAMS. White women serve as the reference group for comparisons across race/ethnicity.

Several variables were examined as potential confounders in the analysis: infant sex, maternal age (<20, 20–24, 25–29, 30+ years old), marital status (married versus not married), parity (primiparous [1 child] versus multiparous [>1 child]), plurality (singleton versus multiple birth), smoking during the last three months of pregnancy (yes versus no), infant NICU admission (yes versus no), length of mother's

hospital stay (<4 days versus ≥4 days), delivery method (cesarean section versus vaginal), and low birth weight (<2500 grams versus ≥2500 grams).

Two measures of SES were defined to explore their impact on the racial/ethnic disparities of interest: maternal education and income level. Both education and income were included in this study because they may capture distinct SES elements, as shown by a 2001 study of postpartum women in California (102). Maternal education was defined as the number of years of school, categorized as less than high school, high school diploma, or more than high school. Income was defined by approximating low-income status (<200% federal poverty level—FPL) via ascertaining participation in social service programs and absolute income. Income as a percent of the FPL was not available from PRAMS. Women were considered low income if they met any of the following conditions:

- Medicaid was payer for prenatal care and/or delivery (Illinois eligibility = 200% FPL)
- WIC participant (Illinois eligibility = 185% FPL)
- Temporary Assistance for Needy Families cash assistance recipient
- Food Stamps recipient
- For married women: Household income <$35,000. For unmarried women: Household income <$25,000. (In 2008, 200% FPL was $28,000 for a household size of 2 and $35,200 for a household size of 3 (103). It can be assumed that each married woman has a household of at least three people and each unmarried woman has a household size of at least two.)

3. **Analytic sample**

To ensure that all women in the study had the opportunity to breastfeed, only women whose infant was alive and living with them at the time of the survey were included in the analysis. Additionally, only White, Black, and Hispanic women and those with valid values for all covariates were included in the sample.

4. **Statistical methods**

The distributions of the covariates were examined according to racial/ethnic group. Differences in proportions across the categorical covariates were tested for significance via χ^2 tests. The prevalence of each of the seven breastfeeding outcomes was described by race/ethnicity alone and in combination with income status. Statistical differences by race/ethnicity were assessed for significance via χ^2 tests.

Logistic regression was used to obtain the ORs and 95% confidence intervals (CIs) for the association of race/ethnicity with each breastfeeding outcome. For each of the breastfeeding outcomes, four models were run: (1) a crude model including only race/ethnicity; (2) a model adjusting for all of the covariates except education and income; (3) a model adjusting for all of the covariates, including education and income; and (4) a model adjusting for all of the covariates and an interaction term for race/ethnicity and income level. When the interaction term in model #4 was statistically significant, the stratum-specific racial/ethnic disparities were estimated using contrast statements.

The IL-PRAMS data are weighted to account for stratum-specific sampling fractions, nonresponse, and noncoverage (100). The sampling strata and weights were used in all analyses to account for the complex sample design and the combination of multiple data years. The SAS version 9.3 (SAS Institute, Cary, North Carolina) was used for all data management and statistical analyses.

B. Results

A total of 8,572 women completed the 2004–2008 IL-PRAMS surveys between two and eight months after their infant's delivery. Of this group, 427 (weighted 3.5%) were excluded because the infant was not alive or not living with the mother at the survey time, 315 (weighted 3.4%) were excluded because the mother was not White, Black, or Hispanic, and 339 (weighted 3.9%) were excluded due to missing values on any study covariates. The final analytic sample included 7,491 White, Black, or Hispanic women whose infant was still alive, lived with them at the time of the survey, and had valid

values on all covariates for the analysis (weighted 89.3% of original survey sample). The weighted analytic sample represented more than 740,000 Illinois births during 2004–2008.

Of the analytic sample, only 0.1% of respondents were missing information on breastfeeding initiation. Of women who initiated breastfeeding, breastfeeding duration and exclusivity information was generally complete, with only 1.9% to 4.0% missing for each of the six duration/exclusivity outcomes.

There were significant differences in maternal age, education, marital status, income level, parity, smoking in the last three months of pregnancy, length of hospital stay, method of delivery, plurality, infant NICU admission, and infant birth weight between White, Black, and Hispanic women (see Table V). Of particular note, more than 85% of Black and Hispanic women were low-income, compared to only 38% of White women.

TABLE V

SAMPLE CHARACTERISTICS OF ILLINOIS NEW MOTHERS

	Whites	**Blacks**	**Hispanics**	**χ2 test p value**
Sample Size	4,271	1,357	1,863	
Weighted Sample Size	415,829	146,211	186,430	
Percent of Population	55.6	19.5	24.9	
	Weighted column %	**Weighted column %**	**Weighted column %**	
Maternal Age				
<20 years old	6.0	19.6	12.9	<0.01
20–24 years old	16.6	27.8	28.4	
25–29 years old	30.3	26.3	28.9	
≥30 years old	47.1	26.3	29.8	
Maternal Education				
<High School	7.7	23.1	47.4	<0.01
High School diploma	20.9	32.4	32.0	
>High School	71.4	44.5	20.6	
% Married	77.6	27.8	50.0	<0.01
% Low Income	38.4	86.4	89.6	<0.01
% Primiparous	41.9	40.7	35.4	<0.01
% Smoked During Last 3 Months of Pregnancy	14.1	13.1	2.6	<0.01
% Maternal Hospital Stay ≥4 days	19.2	32.5	19.8	<0.01
% Cesarean Section	29.8	31.5	25.9	<0.01
% Plural Birth	2.2	1.5	0.9	<0.01
% Male Infants	50.6	53.1	51.3	0.32
% Infants Admitted to NICU	9.4	14.0	13.5	<0.01
% Low Birth Weight Infants (<2500g)	5.6	10.5	5.8	<0.01

Table VI gives the rates of the seven breastfeeding outcomes for each racial/ethnic group. The rates of all breastfeeding outcomes were significantly different across race/ethnicity. Black women were more likely to never breastfeed than White or Hispanic women. Among breastfeeding initiators, Black women had the highest breastfeeding termination rates at two, six, and 12 weeks, with more than 50% of breastfeeding Black women stopping breastfeeding by the time their infant was 12 weeks old compared to approximately 38% of Whites and 45% of Hispanics. For exclusive breastfeeding, Black and Hispanic women had similar rates of termination by 12 weeks (>80%) and were more likely to terminate than Whites (67%).

TABLE VI

PERCENT OF ILLINOIS WOMEN WHO NEVER BREASTFED AND PERCENT OF BREASTFEEDING WOMEN WHO STOPPED ANY AND EXCLUSIVE BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS, BY RACE/ETHNICITY

	Whites			**Blacks**			**Hispanics**			**$\chi 2$ test**
	n	**% (95% CI)**		**n**	**% (95% CI)**		**n**	**% (95% CI)**		**p value**
Never Breastfed	4,269	22.9	(21.6–24.3)	1,355	41.8	(38.8–44.9)	1,860	14.1	(12.5–15.8)	<0.01
Any Breastfeeding (among initiators)										
Stopped before 2 weeks	3,246	6.7	(5.8–7.7)	785	9.9	(7.5–12.3)	1,536	5.6	(4.3–6.8)	<0.01
Stopped before 6 weeks	3,246	22.6	(21.0–24.1)	785	33.4	(29.6–37.1)	1,536	27.9	(25.5–30.3)	<0.01
Stopped before 12 weeks	3,084	37.8	(35.9–39.6)	768	51.0	(46.9–55.0)	1,499	44.8	(42.1–47.5)	<0.01
Exclusive Breastfeeding (among initiators)										
Stopped before 2 weeks	3,239	37.0	(35.2–38.8)	785	51.4	(47.3–55.4)	1,508	52.2	(49.5–54.9)	<0.01
Stopped before 6 weeks	3,250	53.4	(51.6–55.2)	790	68.5	(64.8–72.3)	1,522	68.2	(65.7–70.7)	<0.01
Stopped before 12 weeks	3,180	67.3	(65.5–69.0)	791	81.3	(78.2–84.5)	1,517	79.0	(76.8–81.2)	<0.01

Table VII further stratifies the seven breastfeeding outcomes by race/ethnicity and income level, demonstrating how the breastfeeding patterns change when these two characteristics are considered together. For breastfeeding initiation, among low-income women, Blacks were more likely to never breastfeed than Whites or Hispanics, but among higher-income women, Blacks were the least likely to never breastfeed. For breastfeeding duration among low-income women, Hispanics had the lowest termination rates at all three time points. In contrast, for higher-income women, there were no significant differences across the racial/ethnic groups in breastfeeding termination at any of the three time points. For exclusive breastfeeding, Whites had the lowest termination rates at all three time points among both low-income and higher-income women.

TABLE VII

PERCENT OF ILLINOIS WOMEN WHO NEVER BREASTFED AND PERCENT OF BREASTFEEDING WOMEN WHO STOPPED ANY AND EXCLUSIVE BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS, BY RACE/ETHNICITY AND INCOME STATUS

A. AMONG LOW-INCOME WOMEN

	Whites			Blacks			Hispanics			$\chi 2$ test
	n	% (95% CI)		n	% (95% CI)		n	% (95% CI)		p value
Never Breastfed	1,651	34.7	(32.3–37.2)	1,157	47.2	(43.9–50.5)	1,652	14.5	(12.7–16.3)	<0.01
Any Breastfeeding (among initiators)										
Stopped before 2 weeks	1,071	11.2	(9.2–13.2)	607	12.0	(9.0–15.0)	1353	5.9	(4.5–7.2)	<0.01
Stopped before 6 weeks	1,071	34.2	(31.1–37.2)	607	38.0	(33.6–42.5)	1353	28.4	(25.8–30.9)	<0.01
Stopped before 12 weeks	1,035	51.5	(48.2–54.8)	597	56.3	(51.7–60.9)	1321	45.5	(42.6–48.4)	<0.01
Exclusive Breastfeeding (among initiators)										
Stopped before 2 weeks	1,062	40.5	(37.3–43.7)	608	53.0	(48.4–57.4)	1324	53.3	(50.4–56.1)	<0.01
Stopped before 6 weeks	1,071	59.8	(56.6–62.9)	613	70.3	(66.0–74.5)	1338	68.7	(66.1–71.4)	<0.01
Stopped before 12 weeks	1,061	74.3	(71.5–77.2)	615	83.5	(80.1–86.9)	1334	79.5	(77.2–81.8)	<0.01

B. AMONG HIGHER-INCOME WOMEN

	Whites			Blacks			Hispanics			$\chi 2$ test
	n	% (95% CI)		n	% (95% CI)		n	% (95% CI)		p value
Never Breastfed	2,618	15.6	(14.1 –17.0)	198	7.7	(3.6–11.9)	208	11.4	(6.7–16.1)	<0.01
Any Breastfeeding (among initiators)										
Stopped before 2 weeks	2,175	4.6	(3.7–5.5)	178	2.4	(0.0–4.9)	183	3.2	(0.5–5.9)	0.34
Stopped before 6 weeks	2,175	17.0	(15.3–18.6)	178	16.5	(10.5–22.5)	183	23.8	(17.0–30.5)	0.11
Stopped before 12 weeks	2,049	30.9	(28.8–33.1)	171	31.2	(23.5–39.0)	178	39.0	(31.2–46.8)	0.14
Exclusive Breastfeeding (among initiators)										
Stopped before 2 weeks	2,177	35.3	(33.1–37.4)	177	45.4	(37.2–53.7)	184	43.9	(36.1–51.7)	<0.01
Stopped before 6 weeks	2,179	50.3	(48.1–52.3)	177	62.2	(54.1–70.3)	184	63.9	(56.4–71.5)	<0.01
Stopped before 12 weeks	2,119	63.8	(61.6–66.0)	176	73.2	(65.8–80.7)	183	75.0	(68.2–81.8)	<0.01

Table VIII shows the Black-White and Hispanic-White disparities in the seven breastfeeding outcomes under different scenarios of dealing with covariates and the SES measures of education and income. The crude Black-White disparity in never breastfeeding (OR=2.42, 95% CI: 2.09–2.80) was substantially reduced after adjustment for non-SES covariates (OR=1.51, 95% CI: 1.29–1.80). Additional adjustment for education and income reduced the disparity further, but the presence of significant interaction between race/ethnicity and income indicates that income-stratified OR measures are more appropriate for interpretation. Qualitative interaction was present so that among low-income women, Blacks had 58% higher odds of never breastfeeding than Whites, but among higher-income women, Blacks had 54% *lower* odds of never breastfeeding than White women.

Among breastfeeding initiators, the magnitudes of the Black-White disparities in breastfeeding termination at two, six, and 12 weeks were reduced substantially after adjustment for non-SES covariates to the point where the disparities were essentially nonsignificant (see Table VIII). Additional adjustment for education and income rendered disparities in breastfeeding termination at all three time points nonsignificant, though this additional adjustment caused only a small change in OR magnitude. There was not significant interaction of race/ethnicity and income for overall breastfeeding termination at any of the three time points.

Among breastfeeding initiators, the magnitudes of the Black-White disparities in exclusive breastfeeding termination were reduced substantially after adjustment for non-SES covariates (see Table VIII). Still, in covariate-adjusted models, Black women had approximately 70% higher odds of terminating any breastfeeding before two, six, and 12 weeks than Whites. The magnitudes of these disparities were only minimally changed after further adjustment for education and income. There was not significant interaction of race and income for exclusive breastfeeding termination at any of the three time points.

The crude OR for the Hispanic-White disparity in never breastfeeding was 0.55 (95% CI: 0.47–0.65), indicating Hispanics had lower odds of never breastfeeding than Whites (see Table VIII). Adjustment for covariates demonstrated negative confounding, where the magnitude of the disparity

widened to OR=0.43 (95% CI: 0.36–0.52). Further adjustment for education and income also widened the magnitude of the Hispanic-White disparity, but the presence of significant race/ethnicity-income interaction indicates income-stratified OR measures are most appropriate. Compared to their White counterparts, low-income Hispanics had 81% lower odds of never breastfeeding and higher-income Hispanics had 45% lower odds of never breastfeeding.

Among breastfeeding initiators, the Hispanic-White disparities in breastfeeding termination were different for the two-week time point compared to the six- and 12-week time points (see Table VIII). In the crude analysis, Hispanic and White women had approximately equal odds of stopping breastfeeding before two weeks. After adjustment for non-SES covariates, however, the OR for the Hispanic-White disparity was 0.63, indicating that Hispanics had 37% lower odds of stopping breastfeeding before two weeks than Whites. Further adjustment for SES factors widened the Hispanic-White disparity so that Hispanics had 53% lower odds than Whites of stopping breastfeeding before two weeks postpartum. Interaction between race and income was not present for the two-week breastfeeding termination outcome. Breastfeeding termination before six and 12 weeks, however, showed different patterns. In crude analysis, Hispanic women had higher odds than White women of stopping breastfeeding before six or 12 weeks, but adjustment for covariates (with or without SES variables) rendered these differences nonsignificant. Interaction between race/ethnicity and income was present at six and 12 weeks, so income-stratified ORs are more appropriate for interpretation. While these income-stratified ORs for six and 12 weeks are mostly not statistically significant, they display qualitative interaction; among low-income women, Hispanics have *lower* odds of breastfeeding termination than Whites, but among higher-income women, Hispanics have *higher* odds than Whites.

Among breastfeeding initiators, the crude analysis of Hispanic-White disparities in exclusive breastfeeding termination showed that Hispanics had approximately 85% higher odds than Whites of terminating exclusive breastfeeding at all three time points. After adjustment for the array of non-SES covariates, the magnitude of these disparities was changed by less than 10% (see Table VIII). Further

adjustment for education and income, however, diminished the extent of the disparities substantially. After adjustment for covariates and SES, the Hispanic-White disparities in exclusive breastfeeding termination at two, six, and 12 weeks were: OR=1.60 (95% CI: 1.35–1.90), OR=1.56 (95% CI: 1.30–1.87), and OR=1.44 (95% CI: 1.17–1.77), respectively. There was not significant interaction of race/ethnicity and income for exclusive breastfeeding termination at any of the three time points.

TABLE VIII

ODDS RATIOS AND 95% CONFIDENCE INTERVALS FOR RACIAL/ETHNIC DISPARITIES IN NEVER BREASTFEEDING AND IN ANY AND EXCLUSIVE BREASTFEEDING TERMINATION BEFORE TWO, SIX, AND 12 WEEKS

A. BLACK VERSUS WHITE ILLINOIS WOMEN

	Crude OR_1 (95% CI) [a]	Adjusted Model 1: no SES OR_2 (95% CI) [b]	Adjusted Model 2: SES OR_3 (95% CI) [c]	income interaction term p-value	Stratified Model: among low income OR_{4a} (95% CI) [d]	Stratified Model: among higher income OR_{4b} (95% CI) [d]
Never Breastfed	2.42 (2.09–2.80)	1.52 (1.29–1.80)	1.39 (1.17–1.65)	<0.01	1.58 (1.30–1.91)	0.46 (0.26–0.82)
Any Breastfeeding (among initiators)						
Stopped before 2 weeks	1.53 (1.12–2.08)	1.10 (0.78–1.54)	0.99 (0.70–1.39)	0.25	-	-
Stopped before 6 weeks	1.72 (1.42–2.08)	1.26 (1.02–1.57)	1.16 (0.93–1.45)	0.58	-	-
Stopped before 12 weeks	1.71 (1.43–2.05)	1.24 (1.01–1.51)	1.17 (0.96–1.44)	0.63	-	-
Exclusive Breastfeeding (among initiators)						
Stopped before 2 weeks	1.80 (1.51–2.15)	1.69 (1.40–2.05)	1.60 (1.31–1.95)	0.67	-	-
Stopped before 6 weeks	1.90 (1.57–2.30)	1.73 (1.41–2.12)	1.64 (1.33–2.03)	0.98	-	-
Stopped before 12 weeks	2.12 (1.70–2.64)	1.77 (1.40–2.24)	1.71 (1.34–2.17)	0.66	-	-

B. HISPANIC VERSUS WHITE ILLINOIS WOMEN

	Crude OR_1 (95% CI) [a]	Adjusted Model 1: no SES OR_2 (95% CI) [b]	Adjusted Model 2: SES OR_3 (95% CI) [c]	income interaction term p-value	Stratified Model: among low income OR_{4a} (95% CI) [d]	Stratified Model: among higher income OR_{4b} (95% CI) [d]
Never Breastfed	0.55 (0.47–0.65)	0.43 (0.36–0.52)	0.29 (0.24–0.36)	0.02	0.29 (0.23–0.36)	0.55 (0.33–0.90)
Any Breastfeeding (among initiators)						
Stopped before 2 weeks	0.82 (0.62–1.07)	0.63 (0.47–0.86)	0.47 (0.33–0.66)	0.63	-	-
Stopped before 6 weeks	1.33 (1.14–1.54)	1.08 (0.91–1.29)	0.86 (0.71–1.05)	0.02	0.78 (0.63–0.98)	1.39 (0.94–2.06)
Stopped before 12 weeks	1.34 (1.17–1.53)	1.07 (0.92–1.25)	0.89 (0.74–1.07)	0.03	0.81 (0.66–1.00)	1.28 (0.90–1.81)
Exclusive Breastfeeding (among initiators)						
Stopped before 2 weeks	1.86 (1.63–2.13)	1.94 (1.68–2.24)	1.60 (1.35–1.90)	0.43	-	-
Stopped before 6 weeks	1.87 (1.63–2.15)	1.88 (1.61–2.18)	1.56 (1.30–1.87)	0.51	-	-
Stopped before 12 weeks	1.83 (1.57–2.14)	1.68 (1.42–1.99)	1.44 (1.17–1.77)	0.48	-	-

ODDS RATIOS AND 95% CONFIDENCE INTERVALS FOR RACIAL/ETHNIC DISPARITIES IN NEVER BREASTFEEDING AND IN ANY AND EXCLUSIVE BREASTFEEDING TERMINATION BEFORE TWO, SIX, AND 12 WEEKS

[a] model not adjusted for any covariates

[b] model adjusted for: infant sex, maternal age, marital status, parity, plurality, smoking during last three months of pregnancy, infant NICU admission, length of maternal hospital stay, delivery method, and low birth weight.

[c] model adjusted for all covariates in OR_1, plus maternal education and dichotomous income index

[d] model adjusted for all covariates in OR_2, plus race*income interaction term added to test effect modification. For outcomes with a statistically significant race*income interaction term ($p<.05$), stratified results are shown for the racial/ethnic disparity within each income level

Table IX further summarizes the relationship of the SES variables with disparities in the seven breastfeeding outcomes. For breastfeeding initiation, income was a significant effect modifier of both the Black-White and Hispanic-White disparities, though it operated differently for the two disparities. For breastfeeding duration, income was an effect modifier of the Hispanic-White disparities at six and 12 weeks, but not for any of the Black-White disparities. When education and income were added to the model as covariates, they acted as confounders of the Hispanic-White disparities in all seven outcomes, but were not confounders of any of the Black-White disparities.

TABLE IX

SUMMARY OF THE RELATIONSHIP OF EDUCATION AND INCOME
WITH RACIAL/ETHNIC DISPARITIES IN BREASTFEEDING OUTCOMES

	Black-White		**Hispanic-White**	
	Is Income an Effect Modifier?	**Are Education & Income Confounders?**	**Is Income an Effect Modifier?**	**Are Education & Income Confounders?**
Never Breastfed	YES	no	YES	YES
Any Breastfeeding *(among initiators)*				
Stopped before 2 weeks	no	no	no	YES
Stopped before 6 weeks	no	no	YES	YES
Stopped before 12 weeks	no	no	YES	YES
Exclusive Breastfeeding *(among initiators)*				
Stopped before 2 weeks	no	no	no	YES
Stopped before 6 weeks	no	no	no	YES
Stopped before 12 weeks	no	no	no	YES

C. **Discussion**

This study provides new information about the intersection of race/ethnicity and SES in breastfeeding by comparing ways of dealing with SES variables in an analysis. Socioeconomic status played many different roles across the Black-White and Hispanic-White disparities in the seven breastfeeding outcomes, including: effect modifier, positive confounder, and negative confounder. This demonstrates that researchers seeking to study racial/ethnic disparities in breastfeeding need to explicitly decide whether and how to account for SES in their study, as the magnitude and direction of a disparity may change as a result of this decision. This decision of whether and how to account for SES also has implications for the interpretations of racial/ethnic disparities in breastfeeding. When the SES variables were left out of the model intentionally, the racial/ethnic disparities in breastfeeding represent differences in breastfeeding arising from causes including differences in education and income across groups. In contrast, when control is made for education and income, the remaining racial/ethnic disparities represent the differences arising from pathways not involving education and income; such disparity measures describe what the racial/ethnic disparity would be if the two populations had the same distribution of the SES variables (27). For example, the OR for the Hispanic-White disparity in exclusive termination before two weeks was 1.94 before control for education and income, but 1.60 after control for these variables. This demonstrates that after adjustment for covariates, Hispanic women were 94% more likely than White women to stop exclusively breastfeeding before two weeks when the disparity included components caused by or correlated with education and income. However, if Hispanic women had the same education and income distributions as White women, they would have 60% higher odds than Whites to stop exclusively breastfeeding before two weeks. So while education and income level differences may account for some of the racial/ethnic disparities in exclusive breastfeeding, there is still a significant disparity not related to those two SES variables.

While race/ethnicity and SES may both be significant predictors of breastfeeding and thus have independent effects, the meaning of these independent effects may not always be useful (22, 23). As

shown in this study, education and income vary greatly across the three racial/ethnic groups studied, where <15% of Black and Hispanic women were higher-income, compared to more than 60% of Whites. Holding education and income constant in an analysis, therefore, does not resemble the true social reality in the United States, nor reflect their interplay in society (23).

In this study, education and income did not act as confounders of the Black-White disparities in breastfeeding, implying that the mechanisms leading to Black-White disparities in breastfeeding are not connected to the specific measured constructs of education or income in this study (at least not after also controlling for the array of other covariates). In contrast, education and income were confounders of the Hispanic-White disparities in all examined breastfeeding outcomes and acted either as negative or positive confounders, depending on the outcome of interest. This implies that population differences in education and income do contribute to the Hispanic-White disparities in breastfeeding, but in some cases, equalizing the education and income distributions across the subgroups resulted in wider disparities.

Furthermore, this study demonstrated that race/ethnicity and income may interact to produce unexpected relationships in breastfeeding. Not only did the Black-White and the Hispanic-White disparities significantly differ across income level for certain breastfeeding outcomes, but the direction of the disparity changed direction across income level. Researchers therefore need to consider assessing interaction between race/ethnicity and SES when examining racial/ethnic disparities, as this may provide important information for tailoring interventions and defining high-risk populations. Even if a researcher decides not to control for SES so that a measure of racial/ethnic disparity captures components due to SES, it is still important to assess for effect modification by SES. Ignoring SES or merely controlling for SES variables could mask complexities inherent in the data and may lead to incorrect conclusions about the nature of the disparities.

This study also demonstrates that Illinois women are generally not meeting recommendations for breastfeeding, particularly with respect to duration and exclusivity. Compared to breastfeeding initiation, fewer studies have been published on disparities in breastfeeding duration and exclusivity, despite

evidence that breastfeeding is related in a dose-response manner to several child and maternal health outcomes (5, 7, 8, 11, 15). This study found a rapid drop-off in breastfeeding during the first few weeks after delivery for all subgroups of breastfeeding initiators. Of the women who started breastfeeding, approximately one-third of Whites and one-half of Blacks and Hispanics stopped breastfeeding before 12 weeks. Approximately the same proportions of women also stopped exclusive breastfeeding before their infant was only two weeks old. The quick drop-off in breastfeeding demonstrates that it is not sufficient to focus public health resources only on the initial decision to begin breastfeeding, but that priority must also be given to the protection and support of prolonged and exclusive breastfeeding (35).

Furthermore, a unique aspect of this study is that it examined breastfeeding duration and exclusivity outcomes *only among breastfeeding initiators*. This enabled the estimations of racial/ethnic disparities to more clearly describe patterns in stopping breastfeeding itself rather than being convoluted by the inclusion of women who never breastfed. Interestingly, this study showed that the adjusted Black-White and Hispanic-White disparities in breastfeeding duration were nonsignificant or minimal *among initiators*. Breastfeeding duration disparities based on the whole population of new mothers, such as those described in several CDC reports (20, 31), may therefore be an artifact of the disparity in initiation and not truly due to shorter breastfeeding duration for a certain subgroup. On the other hand, there were significant disparities in exclusive breastfeeding duration among initiators, suggesting that initiation alone does not account for the differences in exclusive breastfeeding. To reduce disparities in exclusive breastfeeding, public health interventions may need to specifically address factors like attitudes toward breastfeeding, subjective norms, perceived behavioral control, and breastfeeding difficulties or concerns (14, 36, 104, 105).

This study has several limitations that need to be considered. First, only three racial/ethnic groups (White, Black, and Hispanic) were compared and other/multirace women were excluded from analysis. This categorization may not necessarily represent homogeneous subgroups with shared social and cultural experiences related to breastfeeding. Heterogeneity of breastfeeding behaviors by nativity, for example,

has been shown to exist within all three of the racial/ethnic groups in this study (106, 107). Previous studies have also demonstrated that for Hispanic women, acculturation is an important predictor of breastfeeding behaviors (106, 108–110). Variables related to nativity, culture, or language proficiency were not available in PRAMS so further stratification was not possible. The only acculturation-related variable available from IL-PRAMS is survey language (English versus Spanish). Appendix A compares the population characteristics and breastfeeding behaviors of Hispanic Illinois women by survey language, but these results were not incorporated into the larger study.

Secondly, PRAMS data are self-reported several months after delivery and may be subject to recall or reporting errors. A recent study showed that the validity of the self-reported breastfeeding initiation data in PRAMS is generally high (111), but there have been no studies on the validity of PRAMS data on breastfeeding duration and exclusivity. From other studies, it is known that women tend to overestimate their durations of any and exclusive breastfeeding (112–115), and this misclassification is likely to be true for this study. The true rate of breastfeeding termination at each time point, therefore, may be higher than reported here. Furthermore, if misclassification was differential across racial/ethnic groups, the disparities measures could be biased in either direction.

Thirdly, education and income were the only available measurements of SES captured by PRAMS. The categorization of these variables into only two or three categories leaves considerable risk variation within groups and masks the complexities of their relation to disparities in breastfeeding. There may also be misclassification of these variables due to missing values or reporting errors; if misclassification is differential by race/ethnicity, this could lead to biased estimates (95). Income level, in particular in this study, may be misclassified because it was determined very indirectly; the validity of this measure could not be assessed because alternate measurements were not available in PRAMS. Additionally, while one would ideally want to fully control for SES to obtain the pure cultural effect of race/ethnicity on breastfeeding behaviors, the simplified measurements of education and income in this study are unlikely to fully capture SES. There is likely residual confounding by other SES elements, such

as wealth or occupation, so even estimates adjusted for education and income cannot rule out the contribution of other SES variables to the disparities in breastfeeding (95, 102, 116).

Finally, this study used logistic regression to model the breastfeeding outcomes when log-linear regression would have been the ideal choice because most of the outcomes were not rare. Stata version 11 (StataCorp, College Station, Texas) was used to attempt estimation of log-linear regression models while accounting for the PRAMS complex sample design, but adjusted models would not converge. The ORs presented in this study, therefore, are overestimates of the relative risks. Relative measures of disparities are also influenced by the overall prevalence of the outcome; rare outcomes will show larger relative disparities and non-rare outcomes will demonstrate smaller disparities (117). In the future, marginal standardization could be used to estimate the adjusted absolute disparities (for an example, see Appendix B). While this method has its own limitations and is also subject to the prevalence of the outcome (so that absolute differences will appear smaller for rare outcomes and larger for non-rare outcomes), they provide a different perspective and can be useful for program and policy development (117).

Overall, this study demonstrates the complexity of attempting to disentangle the race/ethnicity- and SES-based differences in breastfeeding. The ultimate purpose of studying racial/ethnic disparities is not merely to describe differences, but to inform program and policy development to foster equity (26, 29). In the case of breastfeeding, modeling the racial/ethnic disparities in a way that does not adjust for SES may be more useful for describing real-world population-based differences between groups rather than adjusting away the SES effects. However, for researchers wishing to determine the extent of the disparity that operates through pathways not involving SES (perhaps giving a disparity measure due to cultural norms, discrimination, and other race/ethnicity-specific issues), comprehensively controlling for SES would be important. It would also be wise to assess for interaction between race/ethnicity and income or other measures of SES, as this may provide important information for tailoring interventions. This study demonstrates the importance of explicitly defining a priori what aspects of a racial/ethnic disparity are truly of interest so that the analytic approach can be tailored as necessary.

IV. BABY-FRIENDLY HOSPITAL PRACTICES IN ILLINOIS: ARE THEY EFFECTIVE AT PREVENTING EARLY BREASTFEEDING TERMINATION?

Breastfeeding confers many health benefits to both mother and baby (1–16), and has been shown to be related in a dose-response manner to many health outcomes (3, 5, 7–9, 11, 15). As a result, the AAP recommends that infants be breastfed for at least the first year of life and fed only breast milk during the first six months of life (17). Despite these recommendations, many women in the United States do not breastfeed their infants, breastfeed only for short periods of time, or supplement breastfeeding with formula feeding. In 2010, while 76.5% of US infants were breastfed, only 27.0% were breastfed for the recommended duration of at least one year and only 16.4% were exclusively breastfed for the recommended duration of six months. The breastfeeding rates in Illinois for 2012 were even lower: 75.2% of infants were ever breastfed, 21.0% were breastfed for at least one year, and 11.1% were exclusively breastfed for at least six months (18). Suboptimal breastfeeding, including women who stop breastfeeding early or supplement with formula, has been estimated to cost $2.2 billion annually in direct medical expenses due to increased infant and child morbidity (30).

Many women find breastfeeding to be difficult and stop breastfeeding earlier than expected. In the United States, 60% of women who intended to breastfeed did not achieve their desired duration (35), and only 50% of women who prenatally intended to exclusively breastfeed were still doing so at one-month postpartum (38). Women who stop breastfeeding early most commonly cite reasons related to lactation difficulties (e.g., poor latch, pain, or breast infection) and nutritional concerns (e.g., perceived low milk supply or poor infant weight gain) (35, 36). These concerns can generally be prevented and eliminated with adequate breastfeeding support, management, and education.

In 1991, the WHO and the United Nations Children's Fund launched the BFHI, a global movement aimed at making hospital environments more supportive of breastfeeding. The "Ten Steps to Successful Breastfeeding" outline core hospital practices necessary for encouraging breastfeeding (43).

The Ten Steps cover institutional policies and a continuum of breastfeeding education/support during pregnancy, the delivery hospitalization, and post-hospital discharge (44, 45).

Extensive evidence from many international randomized controlled trials during the 1970s–1990s supports the Ten Steps as important for enabling continued breastfeeding (43), but this evidence may not accurately represent the relationship between hospital practices and breastfeeding for contemporary American women. A few studies have demonstrated the effects of BFHI practices on breastfeeding duration in the United States, though many have been limited by lack of generalizability, insufficient control for confounding, and inability to examine multiple practices simultaneously. In general, there is some evidence that no formula supplements in the hospital, breastfeeding within the first hour, and no pacifiers in hospital are protective against breastfeeding termination during the first few months. Evidence has been less consistent, however, for the effects of other specific steps (56, 58–61). Some studies have also shown a dose-response relationship between the number of BFHI practices experienced and breastfeeding duration outcomes (56, 58, 61).

There is even less evidence on the impact of BFHI practices on breastfeeding exclusivity in the United States. No studies were found that examined hospital practices and breastfeeding exclusivity at the individual level in the general population of new mothers; instead, only studies looked at hospital-level practices or subset analyses to women who intended to exclusively breastfeed (37, 38, 62). Overall, more evidence is needed on how BFHI practices may influence women's decisions to continue breastfeeding and to exclusively breastfeed.

The hospital environment is only one of the many factors influencing a woman's decision and ability to breastfeed. Given the wide array of socioeconomic and cultural factors that influence breastfeeding, a WHO report states that "it would not be surprising therefore to learn that implementation of the 'Ten Steps' affects breastfeeding differently in different communities" (67). Little is known about how the effectiveness of maternity care practices varies across different racial/ethnic groups in the United States. Only one known study has examined the effectiveness of hospital practices by race/ethnicity and

demonstrated that some practices may have higher effectiveness for non-Hispanic Blacks than non-Hispanic Whites or Hispanics (60). This study seeks to quantify the effect of BFHI practices on breastfeeding duration and exclusivity for Illinois women, and determine whether there is differential effectiveness of these practices by race/ethnicity.

A. Methods

1. Data source

This study used 2004–2008 data from IL-PRAMS, as described in chapter III.

2. Variable definition

This study includes six outcomes: termination of any and exclusive breastfeeding before two, six, and 12 weeks. The time points were chosen to measure continuation through the critical time period for early breastfeeding establishment (two weeks) (36) and typical time points of returning to employment (six and 12 weeks). The methods for categorizing breastfeeding duration and exclusivity at each time point were discussed in chapter III. In regression models, the duration outcomes were modeled as terminating versus continuing breastfeeding to the time point of interest.

Birth certificate variables on maternal race and Hispanic origin were combined to create a race/ethnicity variable of three categories: non-Hispanic White (hereafter "White"), non-Hispanic Black (hereafter "Black"), and Hispanic.

The experience of ten specific hospital practices were defined in a standard set of yes/no questions (118); seven of these practices related directly to Baby-Friendly-recommended practices (67):

- No formula gift pack given (not a distinct step in the *Ten Steps to Successful Breastfeeding*, but a required component of the Step #1 comprehensive breastfeeding policy)
- BFHI Step #4: Breastfeeding in first hour after delivery
- BFHI Step #6: Baby fed only breast milk in hospital
- BFHI Step #7: Rooming-in

- BFHI Step #8: Mother told by hospital staff to breastfeed on demand
- BFHI Step #9: No pacifier given in hospital
- BFHI Step #10: Hospital gave mother a breastfeeding support phone number

The direction of the yes/no questions in relation to BFHI recommendations varies across the practices, but the comparisons of interest were all framed as the Baby-Friendly practice versus the non-recommended practice. Because the baby being given only breast milk in hospital (Step #6) is a measure of very short-term breastfeeding exclusivity, it was highly correlated with the exclusive breastfeeding outcomes and therefore only examined in relation to any breastfeeding termination.

Three additional hospital practices were included in the PRAMS questions, but were excluded from analysis. "I breastfed my baby in the hospital" is a breastfeeding initiation outcome and could not be temporally examined in relation to the other practices. Two other practices, "The hospital gave me information on breastfeeding" and "The hospital helped me learn to breastfeed," were too vague to determine whether specific BFHI recommendations were met.

In addition to the analysis of the seven individual BFHI practices, the total number of BFHI practices experienced was determined for each woman and could range from zero to seven overall breastfeeding-support practices and zero to six exclusive breastfeeding-support practices (after exclusion of practice of giving the infant only breast milk in the hospital). The mean number of breastfeeding-support practices (mean=4.2) was then used to set the cut-point for a dichotomous indicator measuring high (≥5 BFHI practices) versus low (<5 BFHI practices) hospital breastfeeding support. For exclusive breastfeeding support, the cut-point was reduced by one practice (≥4 versus <4 BFHI practices).

Finally, several variables were examined as confounders in the analysis: infant sex, maternal age (<20, 20–24, 25–29, 30+ years old), marital status (married versus not married), parity (primiparous [1 child] versus multiparous [>1 child]), plurality (singleton versus multiple birth), prenatal smoking during the last three months of pregnancy (yes versus no), infant NICU admission (yes versus no), length of

mother's hospital stay (<4 days versus ≥4 days), delivery method (vaginal versus cesarean section), and low birth weight (<2500 grams versus ≥2500 grams).

Because this study intended to examine differential effectiveness of hospital practices by race/ethnicity, and because race/ethnicity and class are closely intertwined in the United States (see chapter III for discussion), measures of SES, such as maternal education and income, were not included in this analysis.

3. **Analytic sample**

To ensure that all women in the study had the opportunity to breastfeed, only women whose infant was alive and living with them at the time of the survey were included in the analysis. Additionally, only White, Black, and Hispanic women with valid values for all covariates were included in the sample. Finally, only women who reported ever breastfeeding their infant were included in the analytic sample for this study. While the relationship of maternity care practices to breastfeeding initiation may be of interest, cross-sectional nature of the PRAMS survey does not allow for establishment of temporality between these events.

4. **Statistical methods**

The prevalence of each hospital practice and level of breastfeeding support were calculated for the analytic sample. The rates of overall and exclusive breastfeeding termination before two, six, and 12 weeks were compared by experience of BFHI practices, testing significance using χ^2 tests.

Logistic regression was used to obtain the ORs and 95% CIs for the association of each hospital practice with each breastfeeding outcome, while adjusting for covariates and all other hospital practices simultaneously. Separate logistic models examined the dichotomous level of hospital breastfeeding support as the independent variable of interest. For all models, the interaction of race/ethnicity and each hospital practice was tested to determine whether any practices were differentially effective across racial/ethnic subgroups. If the p-value for the interaction term in the model was <0.05 for any of the

racial/ethnic subgroups, race/ethnicity-stratified estimates of the association of the hospital practice and breastfeeding outcome were generated using contrast statements.

The sampling strata and weights were used to account for the complex sample design of the PRAMS survey and the combination of multiple data years. Domain analyses were used to limit the results to the analytic sample while retaining the sample design and ensuring correct standard errors. The SAS version 9.3 (SAS Institute, Cary, North Carolina) was used for all data management and statistical analyses.

B. **Results**

A total of 8,572 women completed the 2004–2008 IL-PRAMS surveys between two and eight months after their infant's delivery. Of this group, 427 (weighted 3.5%) were excluded because the infant was not alive or not living with the mother at the survey time, 315 (weighted 3.4%) were excluded because the mother was not White, Black, or Hispanic, and 339 (weighted 3.9%) were excluded due to missing values on study covariates. Of the 7,491 PRAMS respondents eligible for the study, 1,811 (weighed 24.5%) were excluded because they never breastfed. The final analytic sample included 5,680 breastfeeding initiators, representing more than 560,000 Illinois births during 2004–2008.

The percent of breastfeeding Illinois women who experienced each BFHI practice is shown in Table X. Giving a support phone number, rooming-in, and encouraging women to breastfeed on demand were most common, with more than 80% of breastfeeding Illinois women experiencing these practices in the delivery hospital. Many recommended practices, however, were experienced by a minority of breastfeeding Illinois women, including not receiving a pacifier and feeding the infant only breast milk in-hospital. Only about 14% of breastfeeding Illinois women reported not receiving a formula gift pack from the hospital.

TABLE X

PERCENT OF BREASTFEEDING ILLINOIS WOMEN WHO EXPERIENCED BABY-FRIENDLY HOSPITAL INITIATIVE PRACTICES

BFHI Practice (n=5,680)	**n**	**weighted % (95% CI)**	
No Formula Gift Pack Given	800	14.1	(13.1–15.0)
Breastfeeding in First Hour	3,093	60.9	(59.6–62.3)
Only Breast Milk in Hospital	2,399	44.5	(43.1–45.9)
Rooming-In	4,272	83.1	(82.1–84.1)
Encouraged to Feed On Demand	4,400	81.4	(80.3–82.5)
No Pacifier Given in Hospital	2,570	48.2	(46.8–49.6)
Gave a Support Phone Number	4,828	84.9	(83.8–85.9)
Number of BFHI Practices (n=5,680)	**n**	**weighted % (95% CI)**	
0 steps	51	0.5	(0.3–0.7)
1 step	370	3.7	(3.3–4.2)
2 steps	719	9.8	(9.0–10.6)
3 steps	1,030	17.7	(16.6–18.8)
4 steps	1,233	23.5	(22.3–24.7)
5 steps	1,294	25.4	(24.1–26.6)
6 steps	819	16.3	(15.3–17.4)
7 steps	164	3.2	(2.7–3.6)
High Hospital Breastfeeding Support (≥5 practices)	2,277	44.8	(43.4–46.2)

In crude analyses (see Table XI), five of the seven BFHI practices (breastfeeding in the first hour after delivery, only feeding breast milk in the hospital, encouragement to feed on demand, no pacifier given, and giving a support phone number) were significantly associated with lower rates of breastfeeding termination before two weeks. At six and 12 weeks, all seven BFHI practices were significantly associated with lower rates of breastfeeding termination. Additionally, women who experienced high levels of hospital breastfeeding support (≥5 practices) were less likely to stop breastfeeding before two, six, and 12 weeks.

TABLE XI.

PERCENT OF BREASTFEEDING ILLINOIS WOMEN WHO TERMINATED ANY BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS, BY EXPERIENCE OF BABY-FRIENDLY HOSPITAL INITIATIVE PRACTICES

BFHI Practice	BF <2 Weeks (n=5,567) weighted % (95% CI)		χ2 p-value	BF <6 Weeks (n=5,567) weighted % (95% CI)		χ2 p-value	BF <12 Weeks (n=5,351) weighted % (95% CI)		χ2 p-value
Formula Gift Pack Given									
No (Baby Friendly)	5.6	(3.9–7.3)	0.15	20.7	(17.6–23.8)	<0.01	34.2	(30.5–37.8)	<0.01
Yes	7.1	(6.3–7.9)		26.5	(25.1–27.8)		43.0	(41.4–44.5)	
Breastfeeding in First Hour									
Yes (Baby Friendly)	5.6	(4.7–6.4)	<0.01	20.2	(18.7–21.7)	<0.01	35.8	(34.0–37.6)	<0.01
No	8.9	(7.6–10.2)		34.2	(32.0–36.3)		51.1	(48.8–53.4)	
Only Breast Milk in Hospital									
Yes (Baby Friendly)	2.7	(2.0–3.4)	<0.01	13.4	(11.9–14.8)	<0.01	27.2	(25.3–29.1)	<0.01
No	10.3	(9.1–11.5)		35.7	(33.9–37.6)		53.4	(51.5–55.3)	
Rooming-In									
Yes (Baby Friendly)	6.8	(6.0–7.7)	0.79	24.5	(23.1–25.8)	<0.01	40.2	(38.6–41.8)	<0.01
No	7.1	(5.4–8.8)		31.4	(28.4–34.4)		49.5	(46.3–52.8)	
Encouraged to Feed On Demand									
Yes (Baby Friendly)	5.7	(5.0–6.5)	<0.01	23.8	(22.4–25.1)	<0.01	39.8	(38.2–41.4)	<0.01
No	12.1	(9.9–14.2)		34.0	(31.0–37.1)		50.6	(47.3–53.9)	
Pacifier Given in Hospital									
No (Baby Friendly)	5.2	(4.2–6.1)	<0.01	20.7	(19.0–22.4)	<0.01	35.7	(33.7–37.8)	<0.01
Yes	8.5	(7.4–9.6)		30.3	(28.5–32.1)		47.3	(45.3–49.3)	
Gave a Support Phone Number									
Yes (Baby Friendly)	6.4	(5.6–7.1)	<0.01	24.5	(23.2–25.8)	<0.01	40.7	(39.1–42.2)	<0.01
No	9.9	(7.7–12.2)		32.2	(28.8–35.7)		48.0	(44.2–51.8)	
Level of Hospital Breastfeeding Support									
High: ≥5 practices	3.3	(2.6–4.1)	<0.01	14.8	(13.3–16.4)	<0.01	29.2	(27.2–31.2)	<0.01
Low: <5 practices	9.8	(8.7–11.0)		34.6	(32.7–36.4)		52.0	(50.0–53.9)	

All examined BFHI practices were also significantly associated with lower rates of exclusive breastfeeding termination before two, six, and 12 weeks (see Table XII). Women who experienced high levels of exclusive breastfeeding support in the hospital (≥4 practices) were less likely to stop exclusive breastfeeding by each time point.

TABLE XII

PERCENT OF BREASTFEEDING ILLINOIS WOMEN WHO TERMINATED EXCLUSIVE BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS, BY EXPERIENCE OF BABY-FRIENDLY HOSPITAL INITIATIVE PRACTICES [a]

BFHI Practice*	**Exc BF <2 Weeks (n=5,532)**			**Exc BF <6 Weeks (n=5,562)**			**Exc BF <12 Weeks (n=5,488)**		
	weighted % (95% CI)		**χ2 p-value**	**weighted % (95% CI)**		**χ2 p-value**	**weighted % (95% CI)**		**χ2 p-value**
Formula Gift Pack Given									
No (Baby Friendly)	31.3	(27.8–34.8)	<0.01	47.7	(44.0–51.5)	<0.01	63.1	(59.4–66.8)	<0.01
Yes	45.3	(43.8–46.9)		61.7	(60.3–63.2)		74.3	(72.9–75.6)	
Breastfeeding in First Hour									
Yes (Baby Friendly)	36.3	(34.6–38.1)	<0.01	53.3	(51.5–55.1)	<0.01	67.4	(65.6–69.1)	<0.01
No	54.4	(52.2–56.6)		69.9	(67.9–72.0)		81.1	(79.3–82.9)	
Rooming-In									
Yes (Baby Friendly)	41.8	(40.2–43.3)	<0.01	58.2	(56.7–59.8)	<0.01	71.4	(70.0–72.9)	<0.01
No	51.2	(48.0–54.4)		67.4	(64.4–70.4)		79.2	(76.5–81.8)	
Encouraged to Feed On Demand									
Yes (Baby Friendly)	41.1	(39.5–42.6)	<0.01	57.9	(56.3–59.4)	<0.01	71.1	(69.6–72.5)	<0.01
No	53.5	(50.3–56.7)		68.3	(65.3–71.3)		80.2	(77.6–82.8)	
Pacifier Given in Hospital									
No (Baby Friendly)	37.4	(35.4–39.4)	<0.01	54.0	(52.0–56.1)	<0.01	67.5	(65.5–69.5)	<0.01
Yes	48.9	(47.0–50.9)		65.1	(63.3–67.0)		77.5	(75.9–79.2)	
Gave a Support Phone Number									
Yes (Baby Friendly)	41.9	(40.4–43.4)	<0.01	58.5	(57.0–60.0)	<0.01	71.6	(70.2–73.0)	<0.01
No	51.5	(47.8–55.3)		67.1	(63.6–70.6)		79.4	(76.4–82.4)	
Level of Hospital Support for Exclusive Breastfeeding*									
High: ≥4 practices	35.9	(34.2–37.7)	<0.01	53.2	(51.4–55)	<0.01	67.4	(65.7–69.1)	<0.01
Low: <4 practices	55.8	(53.5–58.1)		70.8	(68.7–72.9)		81.6	(79.8–83.4)	

[a] practice of giving only breast milk in hospital is not included in analyses of exclusive breastfeeding because the practice itself is a measure of early exclusive breastfeeding and highly correlated with the outcomes

Table XIII shows the association of BFHI practices and breastfeeding termination at each time point after controlling for covariates and all other practices. Three BFHI practices significantly reduced the odds of breastfeeding termination before two weeks: feeding the infant only breast milk in the hospital, encouraging to feed on demand, and not giving a pacifier. At six weeks, only breast milk, no pacifier, and breastfeeding in the first hour after delivery were significantly protective against breastfeeding termination. At 12 weeks, four specific BFHI practices reduced the odds of breastfeeding termination: no formula gift pack, breastfeeding in the first hour, feeding only breast milk (though the strength of this association varied by race/ethnicity), and not giving a pacifier. Rooming-in and giving a support phone number were not significantly associated with breastfeeding termination at any of the three time points.

In addition, the overall level of hospital breastfeeding support was strongly related to breastfeeding termination at all three time points (see Table XIII). High breastfeeding support (≥5 BFHI practices) reduced the odds of breastfeeding termination before two weeks by nearly 70%. Significant interaction between high support and race/ethnicity was present at six and 12 weeks; the protective effect of high hospital breastfeeding support was weaker for Hispanic women than for White or Black women, but still significant for all racial/ethnic groups.

TABLE XIII

ADJUSTED ODDS RATIOS AND 95% CONFIDENCE INTERVALS FOR TERMINATION OF ANY BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS, AMONG BREASTFEEDING ILLINOIS WOMEN, BY EXPERIENCE OF BABY-FRIENDLY HOSPITAL INITIATIVE PRACTICES

BFHI Practice	**BF <2 Weeks (n=5,567)**		**BF <6 Weeks (n=5,567)**		**BF <12 Weeks (n=5,351)**	
	aOR[a]	**(95% CI)**	**aOR**[a]	**(95% CI)**	**aOR**[a]	**(95% CI)**
No Formula Gift Pack Given	0.86	(0.59–1.25)	0.83	(0.67–1.03)	0.80	(0.66–0.97)
Breastfeeding in First Hour	0.95	(0.73–1.23)	0.75	(0.64–0.88)	0.82	(0.70–0.95)
Only Breast Milk in Hospital	0.26	(0.19–0.36)	0.32	(0.27–0.38)	*	
Among Whites					0.35	(0.30–0.42)
Among Blacks					0.25	(0.17–0.36)
Among Hispanics					0.50	(0.38–0.65)
Rooming-In	1.29	(0.87–1.92)	0.91	(0.73–1.13)	0.87	(0.71–1.06)
Encouraged to Feed On Demand	0.55	(0.42–0.72)	0.88	(0.73–1.06)	0.95	(0.80–1.13)
No Pacifier Given in Hospital	0.70	(0.54–0.90)	0.71	(0.61–0.83)	0.74	(0.65–0.84)
Gave a Support Phone Number	0.79	(0.57–1.08)	0.89	(0.72–1.09)	0.95	(0.78–1.15)
High Hospital Support for Breastfeeding (≥5 vs. <5 BFHI practices)	0.32	(0.24–0.43)	*		*	
Among Whites			0.31	(0.25–0.38)	0.38	(0.32–0.46)
Among Blacks			0.33	(0.21–0.50)	0.30	(0.21–0.45)
Among Hispanics			0.47	(0.35–0.62)	0.54	(0.42–0.70)

[a] Adjusted for maternal race/ethnicity, infant sex, maternal age, marital status, parity, plurality, smoking during last three months of pregnancy, infant NICU admission, length of maternal hospital stay, delivery method, and low birth weight. Models for individual practices also controlled for all other practices simultaneously.

* Because at least one component of the race/ethnicity*practice interaction term was statistically significant, an overall adjusted OR is not available. Instead, race-specific estimates are displayed.

Table XIV shows the association of BFHI practices and exclusive breastfeeding termination at the three time points, after simultaneously controlling for all other practices. Five of the six BFHI practices (all but rooming-in) were significantly associated with reductions in breastfeeding termination at all three time points, though some practices were only effective among certain racial/ethnic subgroups. No formula gift pack and breastfeeding in the first hour were associated with reduced exclusive breastfeeding termination across time for all racial/ethnic groups (no interaction with race/ethnicity). Encouraging mothers to feed on demand was most effective at preventing breastfeeding termination at two and 12 weeks among Black women. Not giving a pacifier was associated with reduced odds of breastfeeding termination at two weeks for White and Black women only. Giving a support phone number was associated with reduced odds of breastfeeding termination at two weeks for White and Hispanic women only. Rooming-in was the one practice that was generally not associated with exclusive breastfeeding termination, with the exception of at six weeks for White women only.

The level of exclusive breastfeeding hospital support was also related to breastfeeding termination across time, but varied across racial/ethnic subgroups (see Table XIV). High exclusive breastfeeding support (≥4 BFHI practices) reduced the odds of breastfeeding termination at each time point by approximately 45%–60% for White women, 55%–60% for Black women, and 20%–30% for Hispanic women. The association of high exclusive breastfeeding support was significant for all subgroups across time, except at 12 weeks for Hispanic women.

TABLE XIV

ADJUSTED ODDS RATIOS AND 95% CONFIDENCE INTERVALS FOR TERMINATION OF EXCLUSIVE BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS, AMONG BREASTFEEDING ILLINOIS WOMEN, BY EXPERIENCE OF BABY-FRIENDLY HOSPITAL INITIATIVE PRACTICES

BFHI Practice [b]	**Exc BF <2 Weeks (n=5,532)**		**Exc BF <6 Weeks (n=5,562)**		**Exc BF <12 Weeks (n=5,488)**	
	aOR[a]	**(95% CI)**	**aOR**[a]	**(95% CI)**	**aOR**[a]	**(95% CI)**
No Formula Gift Pack Given	0.56	(0.47–0.68)	0.60	(0.50–0.71)	0.64	(0.53–0.76)
Breastfeeding in First Hour	0.59	(0.51–0.67)	0.61	(0.53–0.71)	0.64	(0.54–0.75)
Rooming-In	0.89	(0.74–1.08)	*		0.86	(0.70–1.07)
Among Whites			0.78	(0.62–0.97)		
Among Blacks			0.71	(0.45–1.12)		
Among Hispanics			1.14	(0.82–1.60)		
Encouraged to Feed On-Demand	*		0.81	(0.69–0.97)	*	
Among Whites	0.70	(0.56–0.87)			0.90	(0.70–1.15)
Among Blacks	0.52	(0.34–0.79)			0.40	(0.21–0.73)
Among Hispanics	1.03	(0.78–1.36)			0.91	(0.64–1.29)
No Pacifier Given in Hospital	*		0.67	(0.59–0.76)	0.65	(0.57–0.75)
Among Whites	0.59	(0.50–0.70)				
Among Blacks	0.58	(0.41–0.82)				
Among Hispanics	0.86	(0.68–1.08)				
Gave a Support Phone Number	*		0.81	(0.67–0.98)	0.80	(0.64–0.99)
Among Whites	0.70	(0.53–0.92)				
Among Blacks	1.34	(0.80–2.25)				
Among Hispanics	0.75	(0.58–0.98)				

ADJUSTED ODDS RATIOS AND 95% CONFIDENCE INTERVALS FOR TERMINATION OF EXCLUSIVE BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS, AMONG BREASTFEEDING ILLINOIS WOMEN, BY EXPERIENCE OF BABY-FRIENDLY HOSPITAL INITIATIVE PRACTICES

BFHI Practice [b]	**Exc BF <2 Weeks (n=5,532)**		**Exc BF <6 Weeks (n=5,562)**		**Exc BF <12 Weeks (n=5,488)**	
	aOR[a]	**(95% CI)**	**aOR**[a]	**(95% CI)**	**aOR**[a]	**(95% CI)**
High Hospital Support for Exclusive Breastfeeding (≥4 vs. <4 BFHI practices)	*		*		*	
Among Whites	0.41	(0.35–0.49)	0.49	(0.41–0.58)	0.54	(0.44–0.66)
Among Blacks	0.41	(0.29–0.57)	0.45	(0.31–0.65)	0.41	(0.26–0.64)
Among Hispanics	0.78	(0.62–0.97)	0.71	(0.56–0.91)	0.75	(0.57–1.00)

[a] Adjusted for maternal race/ethnicity, infant sex, maternal age, marital status, parity, plurality, smoking during last three months of pregnancy, infant NICU admission, length of maternal hospital stay, delivery method, and low birth weight. Models for individual practices also controlled for all other practices simultaneously.

[b] Practice of giving only breast milk in hospital is not included in analyses of exclusive breastfeeding because the practice is highly correlated with the outcome

* Because at least one component of the race/ethnicity*practice interaction term was statistically significant, an overall adjusted OR is not available. Instead, race-specific estimates are displayed.

TABLE XV

SUMMARY OF EFFECTIVENESS OF BABY-FRIENDLY HOPSITAL INITIATIVE PRACTICES AT PREVENTING ANY AND EXCLUSIVE BREASTFEEDING TERMINATION AMONG BREASTFEEDING ILLINOIS WOMEN

BFHI Practice	**Overall BF Termination**			**Exclusive BF Termination**		
	2 wks	**6 wks**	**12 wks**	**2 wks**	**6 wks**	**12 wks**
No Formula Gift Pack Given	-	-	A	A	A	A
Breastfeeding in First Hour	-	A	A	A	A	A
Only Breast Milk in Hospital	A	A	A	*n/a*	*n/a*	*n/a*
Rooming-In	-	-	-	-	w	-
Encouraged to Feed On Demand	A	-	-	w,b	A	b
No Pacifier Given in Hospital	A	A	A	w,b	A	A
Gave a Support Phone Number	-	-	-	w,h	A	A
High Hospital Support for Breastfeeding	A	A	A	A	A	w,b

A = practice associated with significantly reduced odds of outcome for *all three racial/ethnic groups*
w = practice associated with significantly reduced odds of outcome for *White women*
b = practice associated with significantly reduced odds of outcome for *Black women*
h = practice associated with significantly reduced odds of outcome for *Hispanic women*
n/a = association not examined because practice is highly correlated with outcome (see methods)

C. **Discussion**

This study sought to examine the association between seven specific BFHI practices and termination of any and exclusive breastfeeding for Illinois women. This study generally found that more BFHI practices were protective against exclusive breastfeeding termination than against overall breastfeeding termination, but an index measuring high hospital support for breastfeeding was strongly protective against all breastfeeding termination outcomes. A particular focus of this study was assessing potential differential effectiveness of BFHI practices by race/ethnicity, which was shown to be present for some practices. The protective effect of high hospital breastfeeding support showed significant differential effectiveness by race/ethnicity for nearly all of the breastfeeding termination outcomes; the protective effect of high hospital support was generally weakest for Hispanic women across the breastfeeding outcomes. The results of this study provide more detailed information about the effect of

BFHI practices on breastfeeding behaviors and evidence supporting further implementation of BFHI recommendations in Illinois hospitals.

Consistent with the literature, this study showed that hospital formula gift packs are associated with exclusive breastfeeding termination, but generally not overall breastfeeding termination (56, 68, 70, 71). Alarmingly, the provision of formula discharge gift packs is very pervasive in Illinois, with more than 85% of breastfeeding women receiving a gift pack. Removing formula gift packs from the hospital is an important part of the comprehensive breastfeeding-supportive policy required for Baby-Friendly designation (66, 67). Putting an end to this practice, however, requires strong leadership and initiative, since many hospitals currently benefit from free formula and funding from formula manufacturers (71).

Breastfeeding within the first hour was associated with breastfeeding termination before six and 12 weeks, and exclusive breastfeeding at all three time points. Other studies have documented the relationship of early initiation of breastfeeding to breastfeeding termination during weeks six through 10 (56, 58–60). Not breastfeeding in the first hour after delivery has been hypothesized to be related to infant feeding difficulties, delayed lactogenesis, and lower breastfeeding confidence (43, 119). The compounding of these problems over time may explain why early initiation is related to overall breastfeeding at only the later time points. If early feeding is related to improved lactation, this would also explain why women who breastfed in the first hour were more likely to maintain prolonged exclusive breastfeeding over time.

Many studies have documented the impact of no in-hospital supplemental feedings on prolonged breastfeeding duration independent of other hospital practices (56, 58, 59, 61), and this study confirms these findings. In fact, of all the individual practices examined in this study, feeding the infant only breast milk was the strongest individual protective factor against breastfeeding termination before all three time points. Additionally, this study demonstrates that the effect of only feeding the infant breast milk in the hospital on preventing breastfeeding termination was strongest for Black women and weakest for Hispanic women, similar to the findings of Ahluwalia et al. (2012) (60). Providing supplemental feedings to infants during the first few days of life can lead to problematic feeding behaviors and decreased milk

production (due to reduced frequency of feeding) (43). The HP2020 calls for formula supplementation of breastfed infants in the first two days of life to be reduced to 14.2% (39). Given that more than half of breastfed infants in this study received food other than breast milk in the hospital, there is much room for improvement in Illinois hospitals.

When other hospital practices were taken into account, rooming-in did not confer an additional benefit for sustaining breastfeeding, consistent with other studies (56, 58, 60, 61). The results for exclusive breastfeeding termination also generally showed a lack of an independent effect of rooming-in, though results were not consistent across time. While rooming-in alone may not provide an independent benefit for breastfeeding duration or exclusivity, it was moderately correlated with other BFHI practices and may indirectly benefit breastfeeding by facilitating other positive breastfeeding practices (e.g., avoidance of in-hospital formula supplementation). A lack of independent effect, therefore, should not be interpreted as a cause for discarding the practice of rooming-in. Instead, it is important to consider the benefits of rooming-in in the context of overall hospital breastfeeding support and other beneficial breastfeeding practices.

This study found that encouraging mothers to breastfeed on demand reduced the odds of overall breastfeeding termination at two weeks only. Several other studies have also shown a lack of relationship between encouraging on-demand feeding with breastfeeding duration after accounting for other hospital practices (56, 58, 59, 61). For exclusive breastfeeding, the effects of this practice were not consistent across race/ethnicity or across time. In general, the results suggest encouraging on-demand feeding may be most effective at promoting exclusive breastfeeding among Black women and less effective among Hispanic women. Different maternal interpretations of feeding cues and the ability to recognize demand may account for the inconsistent results.

Pacifier use had a significant effect independent of the other hospital practices on breastfeeding termination before all three time points, as also demonstrated by several other studies (56, 59, 60). Furthermore, no pacifier use was generally related to reduced odds of exclusive breastfeeding termination across time, with the exception of two-week exclusive breastfeeding among Hispanic women. Early

pacifier use is thought to interfere with breastfeeding by reducing suckling time at the breast (therefore negatively affecting milk supply) and by leading to latching problems (43).

Hospital provision of a phone number for post-discharge breastfeeding support was not associated with termination of any breastfeeding at two, six, or 12 weeks. Other studies in the literature have varied in their reports of the effectiveness of post-discharge support services on breastfeeding duration (56, 59–61). This study did find, however, that the provision of a support phone number generally decreased the odds of exclusive breastfeeding termination at all three time points. The increased demands of exclusive breastfeeding may explain why the provision of post-discharge support was more important for exclusive breastfeeding than for overall breastfeeding. The PRAMS question on this practice does not ask what type of post-discharge support was available for request through the phone number, which could have varied widely in type (professional or peer-based) or format (group meetings, individual visits, or phone-based support) of assistance. Information about whether women used the phone number to access support resources is also unavailable in this survey, so this study examines only whether the provision of the phone number, not its use, was related to breastfeeding termination.

A few studies have reported a dose-response relationship between the number of hospital practices and breastfeeding duration (56, 58, 61). This study showed that high levels of hospital breastfeeding support were strongly associated with reduced odds of any and exclusive breastfeeding termination during the first three months. Effect modification by race/ethnicity was present at nearly all time points and, when present, showed that the effect of high hospital support on breastfeeding termination was stronger for White and Black women than Hispanic women. Given that Hispanic women show preferences for mixed breastfeeding and formula feeding (“los dos”) and that they rely extensively on familial support (especially mothers and other female relatives) (120, 121), Hispanic women may be less likely to be impacted by hospital practices than other women. Furthermore, the cumulative number of hospital practices on breastfeeding termination showed evidence of quadratic trends (results not shown), indicating the benefit of a one-practice increase is not consistent across all levels of breastfeeding support. Instead, the protective effect of experiencing more BFHI practices appears to build synergistically as

women experience more and more supportive practices. The BFHI practices are meant to be delivered as a package of breastfeeding support and this study supports considering the combined effect of the practices in assessing breastfeeding support.

While this study has several strengths, including the use of population-based data, the ability to assess exclusive breastfeeding in addition to overall breastfeeding duration, and the ability to mutually control for multiple BFHI practices to assess the independent effects of each practice, it has several limitations that need to be considered. First, only three racial/ethnic groups (White, Black, and Hispanic) were compared and other/multirace women were excluded from analysis. This categorization may not necessarily represent homogeneous subgroups who share social and cultural experiences related to breastfeeding, such as norms, attitudes, social support, or economic resources. Heterogeneity by nativity, for example, has been shown to exist within all three of these racial/ethnic groups with respect to breastfeeding behaviors (106, 107). Variables related to nativity, culture, or primary language were not available in PRAMS so further division by subgroups was not possible.

Secondly, PRAMS data are self-reported several months after delivery and may therefore be subject to recall or reporting errors. Most women complete the PRAMS three-to-six months postpartum and may not accurately remember their experiences during the hospital stay or the early postpartum period. Other studies have shown that women tend to overestimate their duration of any and exclusive breastfeeding, and this misclassification is likely to be true for this study (112–115). The true prevalence of breastfeeding termination at each time point, therefore, may be higher than reported here, which would result in an underestimation of the true effectiveness of the BFHI practices. Additionally, women could have misreported their hospital experiences, which could have biased the results in either direction.

Finally, this study uses dichotomous variables for breastfeeding and hospital practice variables, which may not be ideal measurements of these constructs. Survival analysis may be a better method of examining any and exclusive breastfeeding over time. Additionally, this study used logistic regression when log-linear regression would have been the ideal choice because the breastfeeding termination outcomes were non-rare. Stata version 11 (StataCorp, College Station, Texas) was used to attempt

estimation of log-linear regression models while accounting for the PRAMS complex sample design, but adjusted models would not converge. Therefore, the ORs presented in this study are overestimates of the relative risks. Furthermore, the more common outcomes (e.g., exclusive breastfeeding termination at 12 weeks) will be overestimated to a greater extent that the less common outcomes (e.g., any breastfeeding termination at two weeks). This precludes the comparison of effect sizes across outcomes without transforming the ORs to relative risks. In addition, the use of relative measures to assess BFHI practice effectiveness does not take into account the magnitude of the problem in the population and the prevention potential of the practices. Future studies could use adjusted population attributable fractions to quantify the population impact of implementing various hospital practices.

Overall, this study demonstrates the independent and combined effects of various BFHI practices on breastfeeding duration and exclusivity among Illinois women who started breastfeeding. Since the BFHI began nearly twenty-five years ago, more than 15,000 hospitals throughout the world have been designated as Baby Friendly, but implementation has been slow in the United States (47). As of April 2014, only 177 US hospitals were Baby-Friendly certified (50). While many other hospitals in Illinois have expressed intent to pursue Baby-Friendly designation because of efforts like the *Healthy Places* project (122) and the *Illinois Breastfeeding Blueprint* (123), only four Illinois hospitals (out of 120) are currently Baby-Friendly certified (50). Given that the Ten Steps of the BFHI seem to work together synergistically to improve breastfeeding duration and exclusivity, it is important for hospitals to adopt as many BFHI practices as possible, even if they are not yet ready to apply for full BFHI designation. Hospitals wishing to take intermediate steps to support breastfeeding and exclusive breastfeeding should focus on implementing practices with the strongest individual effects: supporting breastfeeding within the first hour, reducing formula supplementation in-hospital, not giving pacifiers, and ceasing distribution of formula gift packs. Continuing improvements in hospital breastfeeding support has the potential to dramatically improve breastfeeding duration and exclusivity for Illinois women and likely women across the United States.

V. CAN BABY-FRIENDLY HOSPITAL PRACTICES REDUCE RACIAL/ETHNIC DISPARITIES IN EXCLUSIVE BREASTFEEDING? AN ASSESSMENT USING MEDIATION ANALYSIS

Breastfeeding is the healthiest way to feed an infant (1–16) and is related in a dose-response manner to many maternal and child health outcomes (3, 5, 7–9, 11, 15). The AAP recommends that infants be exclusively breastfed, or fed only breast milk, during the first six months of life (17). Many women, however, experience difficulties during the first few weeks of breastfeeding that lead them to stop altogether or supplement with formula (36). In 2010, only 16.4% of US infants and 11.1% of Illinois infants were exclusively breastfed for the recommended duration of six months (18). Suboptimal breastfeeding, including women who stop early or supplement with formula, has been estimated to cost $2.2 billion annually in direct medical expenses due to increased infant and child morbidity (30).

There is also wide variation in breastfeeding across racial/ethnic subgroups in the United States. Generally, Hispanic women are most likely to initiate and continue breastfeeding and non-Hispanic Black women are the least likely to do so (31). There have been fewer studies, however, on racial/ethnic disparities in exclusive breastfeeding, and results have been inconsistent with respect to the direction and magnitude of disparities (32–34). *Healthy People 2020* includes overarching goals of achieving health equity and eliminating health disparities (40); to achieve such goals, public health research must move from simply documenting these disparities to explaining how and why they arise (26, 29).

More research needs to explicitly consider what influences disparities so that effective interventions can be developed to reduce them. The causes of disparities in breastfeeding in the United States are still relatively unknown and traditional epidemiologic methods do not allow us to address the formation or maintenance of disparities (27). Considering the causal pathways of how an individual mother makes decisions about infant feeding is conceptually different from examining the causal pathways that generate and maintain racial/ethnic differences in breastfeeding (29). Better information

about the causes of disparities in breastfeeding will help with planning and implementing public health programs and policies that may help reduce these disparities.

In 1985, Rose distinguished between the causes of disease in individuals and the causes of disease incidence in populations, demonstrating that population-based prevention strategies were more likely than targeted interventions to positively impact overall health outcomes (124). Recent discussions in the literature have centered on whether population-based approaches appropriately address social inequalities and whether such approaches might actually increase health disparities (28, 125). Two scenarios could cause disparities to increase after implementation of an overall effective intervention: differential access and differential effectiveness. Differential access occurs when not all subgroups experience the intervention equally; if the disadvantaged population was less likely to receive an effective intervention, the extent of the health disparity would increase (28, 29). The second scenario occurs when the overall effect of an intervention masks differential effectiveness. Vulnerable populations may benefit less from population-based health interventions than advantaged populations and if this occurs, the disparity between the groups could increases even after equal access to the intervention (28).

The "Ten Steps to Successful Breastfeeding" are examples of evidence-based population strategies to encourage and support breastfeeding (43). Because nearly all births in the United States occur in hospitals, hospital practices and policies have the potential to influence the feeding behaviors of more than 11,000 infants each day (42). One study from Belarus found an increase in socioeconomic disparities in breastfeeding exclusivity at three months after Baby-Friendly implementation (126), but no known studies have evaluated the impact of BFHI practices on breastfeeding disparities in the United States. In general, there has been little research on breastfeeding interventions that address differential experience, differential effectiveness, and how such interventions might lead to changes in breastfeeding outcomes.

A mediation approach is well-suited for addressing questions about whether public health interventions have the potential to change health disparities. Mediation provides both a framework and accompanying statistical methods for exploring causal relationships (127) and decomposes a total effect

into direct and indirect effects (73). In the context of disparities, mediation analysis can decompose a total racial/ethnic disparity into a "mediated disparity measure," which represents a disparity caused by a pathway involving a specific intermediary variable, and a "direct disparity measure," which represents the portion of the disparity caused by all pathways not involving the intermediary variable (27). When the intermediary variable of interest is a health intervention, like BFHI practices, such methods can explore whether observed disparities are mediated (i.e., caused) by differential experience of the intervention.

This study examines whether racial/ethnic groups vary in their access to BFHI practices and in the association of BFHI practices with exclusive breastfeeding termination. This study also used counterfactual mediation methods to determine whether differential access to BFHI practices contribute to observed racial/ethnic disparities in exclusive breastfeeding.

A. Methods

1. Data source and analytic sample.

This study used 2004–2008data from the IL-PRAMS, as described in chapter III. The same exclusion criteria described in chapter IV were applied so that the final analytic sample included only non-Hispanic White, non-Hispanic Black, and Hispanic women who initiated breastfeeding.

2. Mediation analysis framework

This study uses a mediation model to assess whether racial/ethnic disparities in exclusive breastfeeding termination are partially attributable to differential access to and effectiveness of BFHI practices. Using a mediation framework requires the a priori conceptualization of study variables into one of four categories (outcome, exposure, mediator, or covariate), which each have corresponding statistical approaches. In this study, race/ethnicity will be considered the exposure (*A*) of interest, exclusive breastfeeding termination at three time points will be the outcome variables (*Y*), and BFHI practices will be the mediator (*M*) variables. Figure 4 illustrates the hypothesized relationships of these variables.

The $A \rightarrow M$ path represents differential access to the BFHI practice: whether one racial/ethnic subgroup is more likely than another to experience the practice. The $M \rightarrow Y$ path represents the effect of

BFHI hospital practices on exclusive breastfeeding termination, which may be modified by *A*, (depicted by dashed line). The *A*–*M* interaction on *Y* represents potential differential effectiveness of the hospital practices by race/ethnicity. Together, both differential experience and differential effectiveness of BFHI practices may influence observed racial/ethnic disparities in exclusive breastfeeding.

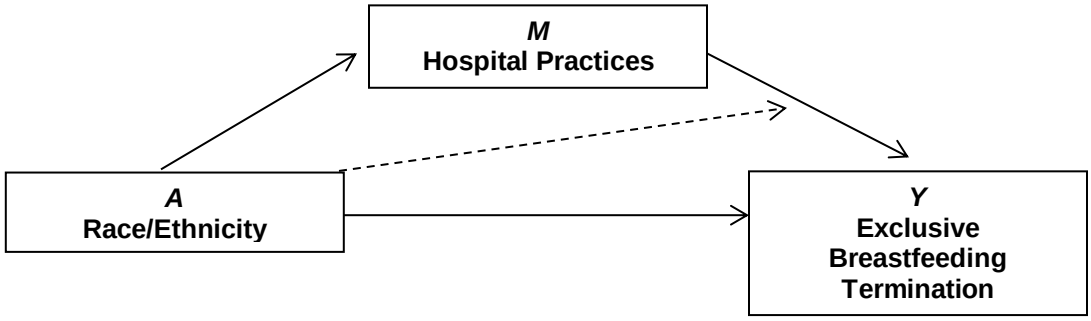


Figure 4. Mediation and moderation framework for the influence of hospital practices on racial/ethnic disparities in breastfeeding exclusivity.

3. **Variable definition**

The outcomes of interest were termination of exclusive breastfeeding before two, six, and 12 weeks, as discussed in chapter III. In regression models, the duration outcomes were modeled as those terminating versus continuing breastfeeding to the time point. Any breastfeeding termination (regardless of exclusivity) at different time points was not examined in the mediation analysis because there were not significant Black-White or Hispanic-White disparities in these outcomes after adjusting for covariates (see chapter III).

The exposure of interest was maternal race/ethnicity, for which I used birth certificate variables on maternal race and Hispanic origin to classify women as non-Hispanic White (hereafter "White"), non-Hispanic Black (hereafter "Black"), or Hispanic. White women served as the reference group.

The mediators in this study were six specific BFHI practices, as described in chapter IV: no formula gift pack given, breastfeeding in first hour after delivery, rooming-in, mother told by hospital staff to breastfeed on demand, no pacifier given in hospital, and hospital gave mother a breastfeeding support phone number. The comparisons in regression models were all framed as the Baby-Friendly practice versus the non-recommended practice. In addition to the analysis of each of the six individual BFHI practices, the practices were combined to represent high or low overall hospital breastfeeding support (≥4 versus <4 BFHI practices), as described in chapter IV.

Finally, several variables were examined as covariates: infant sex, maternal age (<20, 20–24, 25–29, 30+ years old), marital status (married versus not married), parity (primiparous [1 child] versus multiparous [>1 child]), plurality (singleton versus multiple birth), prenatal smoking during the last three months of pregnancy (yes versus no), infant neonatal intensive care unit (NICU) admission (yes versus no), length of mother's hospital stay (<4 days versus ≥4 days), delivery method (vaginal versus cesarean section), and low birth weight (<2500 grams versus ≥2500 grams). Because race/ethnicity and class are closely intertwined in the United States (see chapter III for further discussion), measures of SES, such as maternal education and income, were not included in this analysis as covariates. By purposefully not controlling for SES, the racial/ethnic disparities measured in this study will include the disparity components caused by different SES distributions across racial/ethnic groups.

4. **Statistical methods**

The prevalence of breastfeeding termination at two, six, and 12 weeks was estimated for each racial/ethnic group and χ^2 tests were used to assess whether differences across race/ethnicity were significant. Logistic regression was used to estimate the crude and covariate-adjusted ORs and 95% CI for the racial/ethnic disparities in exclusive breastfeeding at each time point. The adjusted ORs estimate the racial/ethnic disparities in exclusive breastfeeding termination, without adjustment for any BFHI practices ($A \rightarrow Y$ path without controlling for M or other BFHI practices).

The prevalence and 95% CI of each of the six BFHI practices were estimated for each racial/ethnic group. Differences in proportions were tested for significance via χ^2 tests to crudely assess

differential access to the practices by race/ethnicity. Logistic regression was then used to estimate the OR and 95% CI for the racial/ethnic disparities in BFHI practice experience, adjusting for covariates and all other BFHI practices (*A* → *M* path).

This study previously assessed the association of BFHI practices with exclusive breastfeeding termination and produced race/ethnicity stratified results if race/ethnicity-practice interaction was present (*M* → *Y* path, including potential *A*–*M* interaction) (see chapter IV). These results, based on logistic regression models including adjustment for covariates and all other hospital practices, were summarized in plain language.

A macro for SAS by Valeri and VanderWeele (2013) was used to conduct the mediation analysis, estimating the ORs and 95% CIs for the TE, NDE, and NIE for each mediator-outcome combination (75). Separate analyses were conducted for Black-White and Hispanic-White disparities because the mediation macro cannot accommodate three-level variables. For each disparity, two regression equations were estimated for each combination of outcome and mediator, as shown in equations 17 and 18:

$$\text{(17)} \qquad \text{logit}\,(P(Y=1|a,m,c) = \theta_0 + \theta_1(Race) + \theta_2(Mediating\ Hosp\ Practice) + \theta_3(Race * HospPrac) + \theta_4' c$$

$$\text{(18)} \qquad \text{logit}\,(P(M=1|a,c) = \beta_0 + \beta_1(Race) + \beta_2' c$$

For both equations, *c* represents the group of covariates for which adjustment is made, including the full covariate list and all BFHI practices other than the one being examined as mediator. In situations where prior analyses showed no evidence of differential effectiveness by race/ethnicity (see Chapter IV), no *A*–*M* interaction was assumed and θ_3 was set to zero in the macro.

The coefficients from these models were used to estimate the NDE and NIE in the formulas denoted by equations 19 and 20 (75, 128). The TE was estimated as the product of the NDE and NIE. Confidence intervals for all effect estimates were determined via the delta method of calculating standard errors (75).

$$(19) \qquad OR_{NDE} = \frac{\exp(\theta_1)\{1 + \exp(\theta_2 + \theta_3 + \beta_0 + \beta_2' c)\}}{1 + \exp(\theta_2 + \beta_0 + \beta_2' c)}$$

$$(20) \qquad OR_{NIE} = \frac{\{1 + \exp(\beta_0 + \beta_2' c)\}\{1 + \exp(\theta_2 + \theta_3 + \beta_0 + \beta_1 + \beta_2' c)\}}{\{1 + \exp(\beta_0 + \beta_1 + \beta_2' c)\}\{1 + \exp(\theta_2 + \theta_3 + \beta_0 + \beta_2' c)\}}$$

The NDE and NIE are based on the counterfactual framework in which, contrary to fact, each individual is compared to him/herself under different conditions. Because race/ethnicity is the exposure in this study, traditional interpretations of the counterfactual model (i.e., what would have happened to a Black/Hispanic woman if she had been White) are not appropriate (27, 87). Instead, an alternate population-based interpretation considers what would have happened to minority women if their BFHI practice experiences were the same as Whites. In the context of disparities, the NDE and NIE can respectively be interpreted as direct disparity and mediated disparity measures, emphasizing a population-based rather than individual interpretation of the effects (27). In this study, the NDE and NIE interpretations based on this population-focused application of the counterfactual framework are:

NDE = the OR for the racial/ethnic disparity (Blacks/Hispanics versus Whites) in terminating exclusive breastfeeding if Black/Hispanic women were to receive the BFHI practice at the same prevalence as observed for White women.

NIE = *among Black/Hispanic women*, the OR of terminating exclusive breastfeeding at their observed prevalence of the BFHI practice versus if they had experienced the same prevalence of the BFHI practice as White women.

The regression models used to estimate the NDE and NIE in this study were adjusted for covariates and for other BFHI practices so the effect estimates are conditional on all such variables.

The IL-PRAMS data are weighted to account for stratum-specific sampling fractions, nonresponse, and noncoverage (100). The sampling strata and weights were used in all analyses, including modification of the SAS mediation macro (75), to account for the complex sample design of the

PRAMS survey and the combination of multiple data years. Version 9.3 of SAS (SAS Institute, Cary, North Carolina) was used for all data management and statistical analyses.

B. **Results**

A total of 8,572 women completed the 2004–2008 IL-PRAMS surveys between two and eight months after their infant's delivery. Of this group, 427 (weighted 3.5%) were excluded because the infant was not alive or not living with the mother at the survey time, 315 (weighted 3.4%) were excluded because the mother was not White, Black, or Hispanic, and 339 (weighted 3.9%) were excluded due to missing values on study covariates. Of the 7,491 PRAMS respondents eligible for the study, 1,811 (weighed 24.5%) were excluded because they never breastfed. The final analytic sample included 5,680 breastfeeding initiators, representing more than 560,000 Illinois births during 2004–2008.

Early exclusive breastfeeding termination was high for all Illinois breastfeeding women; by two-weeks postpartum, one-third of Whites and one-half of Black and Hispanic women who began breastfeeding had already stopped doing so exclusively (see Table XVI). Black and Hispanic women were more likely than Whites to stop exclusive breastfeeding before two, six, and 12 weeks and these differences remained statistically significant after adjusting for covariates (see Table XVII).

TABLE XVI

PERCENT OF BREASTFEEDING ILLINOIS WOMEN WHO STOPPED EXCLUSIVE BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS

Exclusive Breastfeeding Termination	**White**		**Black**		**Hispanic**	
	%	**(95% CI)**	**%**	**(95% CI)**	**%**	**(95% CI)**
before 2 weeks † ‡	37.0	(35.2–38.8)	51.4	(47.3–55.4)	52.2	(49.5–54.9)
before 6 weeks † ‡	53.4	(51.6–55.2)	68.5	(64.8–72.3)	68.2	(65.7–70.7)
before 12 weeks † ‡	67.3	(65.5–69.0)	81.3	(78.2–84.5)	79.0	(76.8–81.2)

† $p>.05$ for difference in proportions across Blacks and Whites
‡ $p>.05$ for difference in proportions across Hispanics and Whites

TABLE XVII

ADJUSTED ODDS RATIOS AND 95% CONFIDENCE INTERVALS FOR TERMINATION OF EXCLUSIVE BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS AMONG BREASTFEEDING ILLINOIS WOMEN

Exclusive Breastfeeding Termination	**Black vs. White**			**Hispanic vs. White**		
	n	**OR**[a]	**(95% CI)**	**n**	**OR**[a]	**(95% CI)**
before 2 weeks	4,024	1.69	(1.40–2.05)	4,747	1.94	(1.68–2.24)
before 6 weeks	4,040	1.73	(1.41–2.12)	4,772	1.88	(1.61–2.18)
before 12 weeks	3.971	1.77	(1.40–2.24)	4,697	1.68	(1.42–1.99)

[a] adjusted for: infant sex, maternal age, marital status, parity, plurality, smoking during last three months of pregnancy, infant NICU admission, length of maternal hospital stay, delivery method, and low birth weight.

Table XVIII shows the percent of breastfeeding women who experienced each BFHI practice and overall high hospital breastfeeding support. Compared to White women, Black and Hispanic women were significantly more likely to receive a formula gift pack, and they were less likely to breastfeed in the first hour, to be encouraged to breastfeed on demand, and to be given a support phone number. Additionally, Hispanic women were more likely than Whites to report their infant was given a pacifier in the hospital. High breastfeeding support was more common for White women (67.7%, 95% CI: 66.1–69.4) than for either Black (50.5%, 95% CI: 46.6–54.5) or Hispanic (57.7%, 95% CI: 55.1–60.3) women.

TABLE XVIII

PERCENT OF BREASTFEEDING ILLINOIS WOMEN WHO EXPERIENCED BABY-FRIENDLY HOSPITAL INITIATIVE PRACTICES, BY RACE/ETHNICITY

BFHI Practice	White n=3,292 % (95% CI)		Black n=802 % (95% CI)		Hispanic n=1,586 % (95% CI)	
No Formula Gift Pack Given † ‡	16.6	(15.2–18.0)	9.8	(7.5–12.2)	11.2	(9.6–12.9)
Breastfeeding in First Hour † ‡	66.0	(64.3–67.7)	51.0	(47.0–54.9)	56.0	(53.4–58.6)
Rooming-In	82.8	(81.5–84.1)	83.3	(80.7–85.9)	83.6	(81.8–85.5)
Encouraged to Feed On Demand † ‡	85.0	(83.7–86.2)	75.4	(72.1–78.7)	77.4	(75.3–79.6)
No Pacifier Given in Hospital ‡	46.5	(44.7–48.4)	42.7	(38.8–46.6)	54.4	(51.8–57.0)
Gave a Support Phone Number † ‡	90.7	(89.6–91.7)	84.9	(82.1–87.8)	73.1	(70.8–75.5)
High Hospital Exclusive BF Support *(≥4 BFHI practices)* † ‡	67.7	(66.1–69.4)	50.5	(46.6–54.5)	57.7	(55.1–60.3)

† p>.05 for difference in proportions across Blacks and Whites
‡ p>.05 for difference in proportions across Hispanics and Whites

Table XIX shows the disparities in the experience of BFHI practices after adjustment for covariates and other hospital practices. Both Black and Hispanic women had significantly lower odds than White women of experiencing three BFHI practices: no formula gift pack, breastfeeding in the first hour, and receiving a support phone number. However, both Blacks and Hispanics were more likely than Whites to experience rooming-in. Additionally, compared to Whites, Black women were less likely to be encouraged to breastfeed on demand and Hispanic women were more likely to report their infant did not receive a pacifier in the hospital. After adjustment for covariates, Black women had about 40% lower odds than White women (OR=0.59, 95% CI: 0.48–0.73) and Hispanic women had about 33% lower odds than White women (OR=0.67, 95% CI: 0.57–0.79) of experiencing high hospital breastfeeding support.

TABLE XIX

ADJUSTED ODDS RATIOS AND 95% CONFIDENCE INTERVALS FOR RACIAL/ETHNIC DISPARITIES IN EXPERIENCING BABY-FRIENDLY HOSPITAL INITIATIVE PRACTICES AMONG BREASTFEEDING ILLINOIS WOMEN

Experience of BFHI Practice (Yes vs. No)	**Black vs. White n=4,094**		**Hispanic vs. White n=4,878**	
	aOR[a]	**(95% CI)**	**aOR[a]**	**(95% CI)**
No Formula Gift Pack Given	0.50	(0.37–0.68)	0.49	(0.40–0.61)
Breastfeeding in First Hour	0.64	(0.51–0.80)	0.64	(0.54–0.76)
Rooming-In	1.88	(1.39–2.54)	1.49	(1.20–1.86)
Encouraged to Feed On Demand	0.68	(0.54–0.87)	0.86	(0.71–1.05)
No Pacifier Given in Hospital	1.02	(0.84–1.25)	1.51	(1.30–1.75)
Gave a Support Phone Number	0.65	(0.49–0.86)	0.32	(0.26–0.39)
High Hospital Exclusive BF Support (≥4 vs. <4 BFHI practices)	0.59	(0.48–0.73)	0.67	(0.57–0.79)

[a] adjusted for: infant sex, maternal age, marital status, parity, plurality, smoking during last three months of pregnancy, infant NICU admission, length of maternal hospital stay, delivery method, and all other hospital practices simultaneously

Table XX gives an overview of the findings previously presented regarding the effectiveness of BFHI practices at reducing early exclusive breastfeeding termination (see Chapter IV). Most practices other than rooming-in were associated with significant reductions in exclusive breastfeeding termination across the three time points. There were a few individual practices that were differentially effective by race/ethnicity. The only BFHI practice variable demonstrating consistent interaction with race/ethnicity was overall high hospital breastfeeding support for the Hispanic-White comparison. Experiencing at least four BFHI practices was less effective at preventing exclusive breastfeeding termination among Hispanics than Whites at all three time points.

TABLE XX

SUMMARY OF BABY-FRIENDLY HOSPITAL INITIATIVE PRACTICE EFFECTIVENESS AT PREVENTING EXCLUSIVE BREASTFEEDING TERMINATION BEFORE TWO, SIX, AND 12 WEEKS AMONG BREASTFEEDING ILLINOIS WOMEN [a]

BFHI Practice	**differential effectiveness: Blacks versus Whites?**	**differential effectiveness: Hispanics versus Whites?**	**practice effective at preventing Exc BF termination?**
No Formula Gift Pack Given			
Exc BF Termination Before 2 weeks	no	no	YES
Exc BF Termination Before 6 weeks	no	no	YES
Exc BF Termination Before 12 weeks	no	no	YES
Breastfeeding in First Hour			
Exc BF Termination Before 2 weeks	no	no	YES
Exc BF Termination Before 6 weeks	no	no	YES
Exc BF Termination Before 12 weeks	no	no	YES
Rooming-In			
Exc BF Termination Before 2 weeks	no	no	no
Exc BF Termination Before 6 weeks	no	YES	Whites only
Exc BF Termination Before 12 weeks	no	no	no
Encouraged to Feed On Demand			
Exc BF Termination Before 2 weeks	no	YES	Whites & Blacks only
Exc BF Termination Before 6 weeks	no	no	YES
Exc BF Termination Before 12 weeks	YES	no	Blacks only
No Pacifier Given in Hospital			
Exc BF Termination Before 2 weeks	no	YES	Whites & Blacks only
Exc BF Termination Before 6 weeks	no	no	YES
Exc BF Termination Before 12 weeks	no	no	YES
Gave a Support Phone Number			
Exc BF Termination Before 2 weeks	YES	no	Whites & Hispanics only
Exc BF Termination Before 6 weeks	no	no	YES
Exc BF Termination Before 12 weeks	no	no	YES
High Hospital Exclusive Breastfeeding Support (≥4 vs. <4 BFHI practices)			
Exc BF Termination Before 2 weeks	no	YES	YES
Exc BF Termination Before 6 weeks	no	YES	YES
Exc BF Termination Before 12 weeks	no	YES	Whites & Blacks only

[a] full model results are shown in Table XIV in chapter IV. Models adjusted for: infant sex, maternal age, marital status, parity, plurality, smoking during last three months of pregnancy, infant NICU admission, length of maternal hospital stay, delivery method, low birth weight, and all other BFHI practices simultaneously

Table XXI presents the results of the mediation analyses and displays the direct, mediated (through BFHI-practices) and total racial/ethnic disparities estimates for exclusive breastfeeding termination at two, six, and 12 weeks. The TE in this table includes adjustment for all hospital practices other than the mediating BFHI practice of interest. No formula gift pack and breastfeeding in the first hour each significantly mediated the Black-White and Hispanic-White disparities in exclusive breastfeeding termination at all three time points. Not giving a pacifier was also a significant mediator of the Hispanic-White disparity in exclusive breastfeeding termination at two weeks only. The magnitude of the mediation effect, however, was very small in each circumstance (NIE ORs range: 1.03 to 1.06), so the qualitative difference between the TE and NDE effect estimates is minimal.

For the Hispanic-White disparity in exclusive breastfeeding termination before six weeks, no pacifier use in the hospital was a significant mediator in the opposite direction from the other results (OR_{NIE}=0.96, 95% CI: 0.95–0.98). In this case, the NIE and the NDE operated in different directions and the NDE was larger than the TE—an example of inconsistent mediation or suppression.[74] In this example, after controlling for covariates and other BFHI practices, the Hispanic-White disparity in exclusive breastfeeding termination before six weeks would slightly increase if access to no pacifier use were equalized across the two racial/ethnic groups.

The largest indirect effects for the Black-White disparities in exclusive breastfeeding termination were due to high hospital breastfeeding support. For example, the total Black-White racial disparity in exclusive breastfeeding termination before two weeks before accounting for overall hospital support was OR_{TE}=1.71 (95% CI: 1.37–2.13), indicating that Blacks had 71% higher odds of terminating exclusive breastfeeding. After accounting for high hospital support, the direct racial disparity was OR_{TE}=1.54 (95% CI: 1.25–1.90), indicating that even if Blacks experienced high hospital support at the same level as Whites, they would still have 54% higher odds than Whites of exclusive breastfeeding termination before two weeks. The mediating disparity measure (OR_{NIE}=1.11, 95% CI: 1.04–1.17) indicates that the odds of exclusive breastfeeding termination among Blacks are elevated by approximately 11% because they do not experience high hospital support at the same prevalence as Whites. For each time point, these results

demonstrate that the total Black-White disparity in exclusive breastfeeding termination is partially explained by differential experience of high hospital support. If provision of hospital support were equal across Blacks and Whites, disparities in exclusive breastfeeding would be slightly reduced. High hospital support, on the other hand, was not a significant mediator of the Hispanic-White disparity in exclusive breastfeeding termination at any of the three time points.

TABLE XXI

DIRECT, MEDIATED (THROUGH BFHI PRACTICES), AND TOTAL RACIAL/ETHNIC DISPARITY ESTIMATES FOR EXC BF TERMINATION AMONG BREASTFEEDING ILLINOIS WOMEN

A. BLACKS VS. WHITES

Mediator BFHI Practice (Yes vs. No)	**Direct Disparity (Natural Direct Effect)** Racial disparity after controlling for mediator		**Mediated Disparity (Natural Indirect Effect)** Excess Exc BF termination among Blacks due to differential experience of mediator		**Total Disparity (Total Effect)** Racial disparity without control for mediator	
	OR_{NDE} [a] (95% CI)		**OR_{NIE} [a] (95% CI)**		**OR_{TE} [a] (95% CI)**	
Exclusive BF Termination Before 2 Weeks (n=4,024)						
No Formula Gift Pack Given	1.54	(1.25–1.88)	**1.03**	**(1.02–1.05)**	1.59	(1.30–1.95)
Breastfeeding in First Hour	1.54	(1.25–1.88)	**1.05**	**(1.02–1.09)**	1.62	(1.32–1.98)
Rooming-In	1.54	(1.25–1.88)	0.99	(0.97–1.00)	1.51	(1.24–1.85)
Encouraged to Feed On Demand	1.54	(1.25–1.88)	1.02	(1.00–1.04)	1.57	(1.28–1.92)
No Pacifier Given in Hospital	1.54	(1.25–1.88)	1.00	(0.97–1.02)	1.53	(1.25–1.88)
Gave a Support Phone Number	1.58	(1.29–1.94)	0.99	(0.98–1.01)	1.57	(1.28–1.92)
High Hospital Exclusive BF Support (≥4 vs. <4 BFHI practices)	1.54	(1.25–1.90)	**1.11**	**(1.04–1.17)**	1.71	(1.37–2.13)
Exclusive BF Termination Before 6 Weeks (n=4,040)						
No Formula Gift Pack Given	1.62	(1.31–2.00)	**1.03**	**(1.02–1.05)**	1.67	(1.35–2.07)
Breastfeeding in First Hour	1.62	(1.31–2.00)	**1.05**	**(1.01–1.08)**	1.69	(1.37–2.09)
Rooming-In	1.62	(1.31–2.00)	0.98	(0.96–1.00)	1.59	(1.28–1.96)
Encouraged to Feed On Demand	1.62	(1.31–2.00)	1.01	(1.00–1.02)	1.64	(1.33–2.02)
No Pacifier Given in Hospital	1.62	(1.31–2.00)	1.00	(0.98–1.02)	1.62	(1.31–2.00)
Gave a Support Phone Number	1.62	(1.31–2.00)	1.01	(1.00–1.02)	1.63	(1.32–2.02)
High Hospital Exclusive BF Support (≥4 vs. <4 BFHI practices)	1.66	(1.33–2.07)	**1.10**	**(1.04–1.16)**	1.82	(1.43–2.31)
Exclusive BF Termination Before 12 Weeks (n=3,971)						
No Formula Gift Pack Given	1.65	(1.29–2.10)	**1.03**	**(1.01–1.04)**	1.69	(1.33–2.16)
Breastfeeding in First Hour	1.65	(1.29–2.10)	**1.03**	**(1.01–1.06)**	1.70	(1.34–2.17)
Rooming-In	1.65	(1.29–2.10)	0.98	(0.96–1.00)	1.62	(1.27–2.06)
Encouraged to Feed On Demand	1.78	(1.37–2.32)	1.05	(0.99–1.11)	1.87	(1.41–2.48)
No Pacifier Given in Hospital	1.65	(1.29–2.10)	1.00	(0.98–1.02)	1.64	(1.29–2.09)
Gave a Support Phone Number	1.65	(1.29–2.10)	1.01	(1.00–1.03)	1.66	(1.31–2.12)
High Hospital Exc BF Support (≥4 vs. <4 BFHI practices)	1.78	(1.36–2.33)	**1.11**	**(1.04–1.18)**	1.97	(1.47–2.65)

DIRECT, MEDIATED (THROUGH BFHI PRACTICES), AND TOTAL RACIAL/ETHNIC DISPARITY ESTIMATES FOR EXC BF TERMINATION AMONG BREASTFEEDING ILLINOIS WOMEN

B. HISPANICS VS. WHITES

Mediator BFHI Practice (Yes vs. No)	**Direct Disparity (Natural Direct Effect)** Racial disparity after controlling for mediator		**Mediated Disparity (Natural Indirect Effect)** Excess Exc BF termination among Blacks due to differential experience of mediator		**Total Disparity (Total Effect)** Racial disparity without control for mediator	
	OR_{NDE} [a] (95% CI)		**OR_{NIE} [a] (95% CI)**		**OR_{TE} [a] (95% CI)**	
Exclusive BF Termination Before 2 Weeks (n = 4,024)						
No Formula Gift Pack Given	1.81	(1.55–2.12)	**1.03**	**(1.02–1.05)**	1.88	(1.60–2.19)
Breastfeeding in First Hour	1.81	(1.55–2.12)	**1.06**	**(1.03–1.09)**	1.92	(1.64–2.25)
Rooming-In	1.81	(1.55–2.12)	0.99	(0.98–1.01)	1.80	(1.54–2.11)
Encouraged to Feed On Demand	1.82	(1.56–2.13)	1.00	(0.99–1.01)	1.82	(1.56–2.13)
No Pacifier Given in Hospital	1.71	(1.45–2.01)	0.98	(0.96–1.01)	1.68	(1.44–1.97)
Gave a Support Phone Number	1.81	(1.55–2.12)	**1.05**	**(1.01–1.09)**	1.90	(1.63–2.22)
High Hospital Exclusive BF Support (≥4 vs. <4 BFHI practices)	1.76	(1.51–2.05)	1.02	(1.00–1.05)	1.80	(1.54–2.11)
Exclusive BF Termination Before 6 Weeks (n = 4,040)						
No Formula Gift Pack Given	1.75	(1.49–2.06)	**1.03**	**(1.02–1.05)**	1.81	(1.54–2.13)
Breastfeeding in First Hour	1.75	(1.49–2.06)	**1.05**	**(1.03–1.08)**	1.85	(1.57–2.17)
Rooming-In	1.73	(1.47–2.03)	1.01	(0.99–1.02)	1.74	(1.48–2.05)
Encouraged to Feed On Demand	1.75	(1.49–2.06)	1.00	(1.00–1.01)	1.76	(1.50–2.07)
No Pacifier Given in Hospital	1.75	(1.49–2.06)	**0.96**	**(0.95–0.98)**	1.69	(1.44–1.99)
Gave a Support Phone Number	1.75	(1.49–2.06)	1.03	(1.00–1.07)	1.81	(1.55–2.12)
High Hospital Exclusive BF Support (≥4 vs. <4 BFHI practices)	1.70	(1.45 – 2.00)	1.03	(1.00–1.06)	1.76	(1.49–2.07)
Exclusive BF Termination Before 12 Weeks (n = 3,971)						
No Formula Gift Pack Given	1.59	(1.33–1.90)	**1.03**	**(1.01–1.04)**	1.63	(1.36–1.95)
Breastfeeding in First Hour	1.59	(1.33–1.90)	**1.05**	**(1.02–1.08)**	1.67	(1.40–2.01)
Rooming-In	1.59	(1.33–1.90)	0.99	(0.98–1.00)	1.57	(1.31–1.88)
Encouraged to Feed On Demand	1.59	(1.33–1.90)	1.00	(1.00–1.01)	1.59	(1.33–1.91)
No Pacifier Given in Hospital	1.59	(1.33–1.90)	0.96	(0.94–0.98)	1.53	(1.27–1.83)
Gave a Support Phone Number	1.59	(1.33–1.90)	1.02	(0.99–1.06)	1.63	(1.36–1.94)
High Hospital Exc BF Support (≥4 vs. <4 BFHI practices)	1.53	(1.27–1.83)	1.03	(1.00–1.06)	1.57	(1.30–1.89)

[a] Adjusted for infant sex, maternal age, marital status, parity, plurality, maternal prenatal smoking, maternal pre-pregnancy BMI, infant NICU admission, length of maternal hospital stay, delivery method, year of delivery. ORs concerning individual BFHI practices are also adjusted for all other BFHI practices simultaneously.

Significant mediated disparity measures (natural indirect effects) are bolded

Table XXII summarizes the results of the mediation analysis by identifying which practices significantly mediate the disparities for each exclusive breastfeeding termination outcome.

TABLE XXII

SUMMARY OF MEDIATION OF RACIAL/ETHNIC DISPARITIES IN EXCLUSIVE BREASTFEEDING TERMINATION BY BABY-FRIENDLY HOPSITAL INITIATIVE PRACTICES

BFHI Practice	**Exclusive BF Termination**		
	2 wks	**6 wks**	**12 wks**
No Formula Gift Pack Given	B H	B H	B H
Breastfeeding in First Hour	B H	B H	B H
Rooming-In	-	-	-
Encouraged to Feed On Demand	-	-	-
No Pacifier Given in Hospital	-	H	-
Gave a Support Phone Number	H	-	-
High Hospital Support for Breastfeeding	B	B	B

B = practice is a significant mediator of Black-White disparity
H = practice is a significant mediator of Hispanic-White disparity

C. **Discussion**

This study sought to apply mediation analysis in a new way to evaluate whether hospital breastfeeding-support practices contribute to racial/ethnic disparities in exclusive breastfeeding. Because reducing disparities is a critical public health goal, it is important to understanding how programs and policies may impact not only individual-level outcomes, but also disparities (28). An effective population-based intervention will not necessarily improve disparities, as differential experience and differential effectiveness can widen the gaps between two population subgroups (28, 29). This study applied a counterfactual mediation framework to examine whether differential experience of hospital breastfeeding-support practices accounts for some of the observed racial/ethnic disparities in exclusive breastfeeding

termination. Such methods considered what would happen to the disparity measures if access to the BFHI practices were equalized across race/ethnicity while preserving the observed effectiveness of the practices (which may or may not have been differential by race/ethnicity).

High overall hospital support for breastfeeding, as defined as the experience of at least four of six BFHI practices, was a significant mediator of the Black-White disparity in exclusive breastfeeding at every time point. Though it was not a very strong mediator, high hospital support was a stronger mediator than any individual BFHI practice. Like the evidence on the effectiveness of improving individual breastfeeding outcomes, reducing gaps in the combined experience of BFHI practices may be more important for reducing the Black-White disparities in exclusive breastfeeding than any individual practice alone (56, 58, 61). On the other hand, high levels of hospital breastfeeding support did not mediate the Hispanic-White disparities in exclusive breastfeeding, perhaps partially due to the fact that high hospital support was less effective at preventing exclusive breastfeeding for Hispanic women than for White women.

Individual BFHI practices were, at best, very weak mediators of the racial/ethnic disparities in exclusive breastfeeding termination. Only two of six practices (not receiving a formula gift pack and breastfeeding in the first hour) were significant mediators of the Black-White and the Hispanic-White disparities in exclusive breastfeeding termination consistently across the time points, but the magnitude of mediation by these practices was very small. If Black and Hispanic women were to experience these two hospital practices at the same level as White women, their odds of exclusive breastfeeding termination at each time point would generally be reduced by only 3%–5%. The other BFHI practices generally did not mediate either the Black-White or Hispanic-White disparities in exclusive breastfeeding termination.

There was one example of significant inconsistent mediation in this study, where the NDE and NIE operated in opposite directions, resulting in an NDE larger than the TE (74). For the Hispanic-White disparity in exclusive breastfeeding termination before six weeks, no pacifier use was a significant suppressor that resulted in an OR_{NIE} less than 1.0. This means that, among Hispanic women, the odds of exclusive breastfeeding termination before six weeks is lower under their observed prevalence of pacifier

receipt than what would have occurred if they received a pacifier at the same prevalence as Whites. Because of this, the Hispanic-White disparity in exclusive breastfeeding termination before six weeks would slightly increase (from OR_{TE}=1.69 to OR_{NDE}=1.75) if pacifier receipt were equalized across the two racial/ethnic subgroups. This occurs because Hispanic women were significantly more likely than Whites to not receive a pacifier in-hospital; so bringing the Hispanic prevalence down to be the same as Whites would remove some of the advantage Hispanics have with regard to this practice. While this suppression is statistically significant, the magnitude of the mediation is very small and this practice was not a significant mediator of the Hispanic-White disparities at either two or 12 weeks.

The BFHI practices may have exhibited little-to-no mediating effects on disparities in exclusive breastfeeding for several reasons. First, if there was not a significant difference in the experience of the hospital practices across racial/ethnic groups, such practices would not contribute to the observed total racial/ethnic disparities (28, 29). In this study, no pacifier use (for Blacks versus Whites) and encouragement of on-demand feeding (for Hispanics versus Whites) were examples of BFHI practices that were equally accessed across race/ethnicity and thus did not mediate the disparities of interest. The second scenario potentially leading to no mediation occurs if the BFHI practice is not effective or only minimally effective at preventing exclusive breastfeeding termination. Noneffective practices generally would not affect racial/ethnic disparities in an outcome, even if there were wide differences in access to those practices across groups (29). In this study, rooming-in was generally not associated with exclusive breastfeeding termination. So, although Blacks and Hispanics were more likely to experience this practice, it did not mediate racial/ethnic disparities in the outcomes. Therefore, both the effectiveness and the differential levels of access to an intervention must be considered in predicting how that intervention may affect a health outcome.

These results demonstrate that while breastfeeding-supportive hospital practices may be effective at improving overall breastfeeding outcomes, equalizing access to such practices may not substantially change the observed racial/ethnic disparities in breastfeeding. Instead, more targeted public health interventions may be necessary for improving exclusive breastfeeding rates among Black and Hispanic

women relative to White women (28, 125). More research on the specific causes of the racial/ethnic disparities in breastfeeding, particularly in exclusive breastfeeding, are necessary to develop such targeted interventions.

This study did not have information about delivery hospital, so a hospital-level analysis was not possible. It is not known, therefore, whether minority women experienced BFHI practices less frequently because they were less likely to deliver at hospitals implementing the practices or because there was differential experience within hospitals. This study also could not address whether a woman's breastfeeding intentions may have affected her experience of hospital breastfeeding support. Future research should seek to understand the real-world implementation of BFHI practices and how facility and patient factors interact to affect a woman's experience of hospital breastfeeding support.

The counterfactual mediation framework used in this study represents just one way to examine how disparities may change due to a public health intervention, but provides advantages over other methods. A simplified mediation analysis can be conducted by comparing regression models with and without control for the intervention (mediator). The disadvantage of this method is that it only estimates the total and direct effects (no indirect effect), thus taking the focus off the mediating intervention itself (84). Furthermore, this method cannot estimate a single direct racial/ethnic disparity estimate if exposure-mediator interaction (representing differential effectiveness by race/ethnicity) is present (127). The counterfactual mediation methods used in this study estimate the natural direct *and* indirect effects, even in the presence of exposure-mediator interaction (80). Another alternate strategy would be stratified analysis, as used in a 2014 study of BFHI implementation in Belarus (126). This strategy compares the observed racial/ethnic disparities among intervention and nonintervention groups to determine whether the disparities change across intervention experience. The disadvantage of the stratification strategy is that the disparity measures are not population-based, instead providing separate disparity estimates according to intervention status. In contrast, the counterfactual mediation methods described in this study show how the disparity in the whole population would change as a result of changes to the mediator.

In general, decisions about which covariates to include in a mediation analysis are critical because mediation requires strict assumptions of unmeasured confounding (of the $A \rightarrow Y$, $M \rightarrow Y$, and $A \rightarrow M$ relationships) for causal interpretations to be valid. In addition, there should be no unmeasured confounding of the $M \rightarrow Y$ relationship by a factor that is caused by A (75). While VanderWeele (2010, 2013) suggests the use of sensitivity analysis procedures to assess the impact of potential unmeasured confounding on the mediation results (93, 94), such sensitivity analyses were not undertaken for this study because the results showed such weak mediating effects.

This study has several limitations that need to be considered. First, only three racial/ethnic groups (White, Black, and Hispanic) were compared and other/multirace women were excluded from analysis. This categorization may not necessarily represent homogeneous subgroups who share social and cultural experiences related to breastfeeding, such as norms, attitudes, social support, or economic resources. Heterogeneity by nativity, for example, has been shown to exist within all three of these racial/ethnic groups with respect to breastfeeding behaviors (106, 107). Variables related to nativity, culture, or primary language were not available in PRAMS so further division by subgroups was not possible.

Secondly, this study includes only new mothers in Illinois and may therefore not be generalizable to the general US population. The Illinois and national populations may vary within racial/ethnic subgroups, as in the case of distinct sociocultural groups of Black women who identify as African-Americans (26) and variations in country of origin for Hispanic women giving birth (129). Additionally, the factors leading to disparities in exclusive breastfeeding and in BFHI practices may be different in Illinois than for the nation. Future studies could replicate the work of this study for a more representative population of the entire United States.

Thirdly, PRAMS data are self-reported and may therefore be subject to recall or reporting errors. The PRAMS survey is administered three to six months after the woman delivered her infant, so she may not vividly remember her experiences during the hospital stay or the first few months after delivery. Other studies have shown that women tend to overestimate their duration of exclusive breastfeeding, probably due to a combination of poor recall, social desirability, and varying interpretations about the relevance of

certain infant feeding behaviors (e.g., using formula "just once in a while") (112–114). Women may also misreport the hospital practices they experienced during their delivery stay due to recall errors. If misclassification of BFHI practices was non-differential with respect to race/ethnicity and exclusive breastfeeding, the NIE would be underestimated and the NDE overestimated (130, 131). If misclassification of the BFHI practices were differential, the NDE and NIE estimates could be biased in either direction (130).

Finally, this study uses dichotomous variables for the outcomes and mediators, which may not be ideal measurements of these constructs. The hospital practices (mediators) in this study were also dichotomized because the related PRAMS questions offer only yes/no responses (132), though they may be better described in ordinal levels of experience. VanderWeele (2012) showed that dichotomizing the mediator variable masks finer levels of inherent gradation, resulting in an overestimation of the NDE and an underestimation of the NIE (133). Exclusive breastfeeding may be more accurately described by a survival model, but the mediation analysis methods available for survival analysis are very limited and did not fit well with the requirements of this study (134, 135). Additionally, this study used logistic regression when log-linear regression would have been the ideal choice. The dichotomous outcomes are not rare and the ORs presented in this study, therefore, are overestimates of the relative risks. Stata version 11 (StataCorp, College Station, Texas) was used to attempt estimation of log-linear regression models while accounting for the PRAMS complex sample design, but adjusted models would not converge.

Counterfactual mediation analysis is an important methodological tool that can be applied to identify the factors that influence disparities in breastfeeding and to evaluate whether interventions are appropriately addressing those factors. Overall, this study has demonstrated the application of counterfactual mediation analysis to address research questions about how racial/ethnic disparities change due to population-based public health interventions. This study showed that differential access to BFHI practices accounts for little-to-none of the observed racial/ethnic disparities in exclusive breastfeeding termination. While BFHI practices are important for improving population-based breastfeeding outcomes,

their implementation will not help to solve the problem of persistent disparities in breastfeeding. More research about the factors that cause and maintain disparities in breastfeeding is necessary to inform targeted interventions for Black and Hispanic women, which may be more successful at reducing disparities than hospital-based population approaches.

VI. DISCUSSION

A. Synthesis of Findings

This study sought to achieve several goals: (1) to expand the understanding of the interplay of race/ethnicity and SES in breastfeeding behaviors; (2) to document the effectiveness of BFHI practices at improving breastfeeding duration and exclusivity; and (3) to demonstrate the potential for BFHI practices to contribute to the goal of eliminating disparities in breastfeeding.

Socioeconomic status (as measured by education and income) can play many different roles in racial/ethnic disparities in breastfeeding initiation, duration, and exclusivity. In this study, education and income were not confounders of the Black-White disparities in the seven breastfeeding outcomes, but they were confounders of the Hispanic-White disparities for all outcomes. Furthermore, the direction of confounding by education and income of the Hispanic-White disparities changed depending on the breastfeeding outcome of interest. Additionally, race/ethnicity and income may interact in unexpected ways to produce complex relationships with an outcome. For several breastfeeding outcomes, the Black-White and/or the Hispanic-White disparities were significantly different across income level. If income level had been ignored or simply included as a covariate (rather than an interaction term), such complexities would have been masked and could lead to incorrect conclusions about the nature of the disparities. This underscores the important of making a priori decisions about whether and how to account for SES in studies of disparities in breastfeeding, as the magnitude and direction of a disparity will be impacted.

The choice about whether or not to include measures of SES in an analysis depends on the goals of the researcher (27). Since the interpretations of a racial/ethnic disparity depend on the variables included as covariates in the model, the decision about how to treat SES is not an arbitrary one. In the context of breastfeeding, race/ethnicity does not represent any form of biologic variability, as research has demonstrated that nearly all women are biologically capable of producing sufficient milk for breastfeeding (35). Instead, race/ethnicity is used as a descriptor of the collective social/cultural factors

that influence women's breastfeeding behaviors, which may include: social support (98), cultural norms (104, 120), partner and family views (105, 120, 121), self-efficacy (96, 98, 136), healthcare provider support (35), and socioeconomic factors like employment (35, 97), just to name a few. All of these factors may contribute to the overall disparity of interest; isolation of any one factor's contribution would require adequate control for all other factors. Even if one wanted to remove the impact of SES from the disparity measure, it is unlikely that any one study would be able to appropriately control for all elements of SES, so residual confounding by other SES factors would likely remain (27, 137).

In this study, several BFHI practices were shown to be independently and cumulatively associated with any and exclusive breastfeeding termination. Specifically, not receiving a formula gift pack, breastfeeding in the first hour, feeding the infant only breast milk in the hospital, and not giving a pacifier were associated with reductions in any and exclusive breastfeeding termination, while giving a support phone number was associated only with reductions in exclusive breastfeeding termination. Rooming-in was generally not associated with the outcomes and encouraging mother to breastfeed on demand showed inconsistent results. Of the individual practices examined in this study, feeding the infant only breast milk was the strongest protective practice against any breastfeeding termination, while not giving a formula gift pack and breastfeeding in the first hour were the strongest protective practices against exclusive breastfeeding termination. This study also showed that high overall hospital breastfeeding support was strongly associated with reduced odds of any and exclusive breastfeeding termination during the first three months. Effect modification by race/ethnicity was present at nearly all time points and when present, showed that the effect of high hospital support on breastfeeding termination was stronger for White and Black women than Hispanic women. The findings of this study were generally consistent with those from previous studies (56, 58–61, 68, 70, 71), but this research is unique in considering exclusive breastfeeding termination as an outcome.

In an ideal setting, BFHI practices are intended to be delivered as a package of breastfeeding support. This study supports considering the combined effect of the practices in assessing breastfeeding support, as have other studies (56, 58, 61). High overall hospital breastfeeding support was highly

protective against any and exclusive breastfeeding termination. Furthermore, the examination of the effect of the cumulative number of hospital practices on breastfeeding termination showed evidence of quadratic trends, indicating that the benefit of a one-practice increase in BFHI practices is not consistent across all levels of breastfeeding support. Instead, the more BFHI practices a woman was already experiencing, the more protection against breastfeeding termination conferred by one additional practice. Put another way, the protective effect of experiencing more BFHI practices appears to build synergistically as women experience more and more supportive practices.

This analysis culminates in the use of mediation analysis to assess whether racial/ethnic disparities in breastfeeding exclusivity could be partially explained by differential access to BFHI practices. The data showed that two practices (not receiving a formula gift pack and breastfeeding in the first hour) were consistently significant mediators of the Black-White and the Hispanic-White disparities in exclusive breastfeeding termination, but the magnitude of this mediation was very small. If Black and Hispanic women were to experience these two hospital practices at the same level as White women, their odds of exclusive breastfeeding termination at each time point would generally be reduced by only 3%–5%. The other individual BFHI practices generally did not mediate either the Black-White or Hispanic-White disparities in exclusive breastfeeding termination. Although there was little mediation effect of any individual practices, high overall hospital support for breastfeeding was a significant mediator of the Black-White disparity in exclusive breastfeeding at every time point and was a stronger mediator than any individual BFHI practice. On the other hand, high levels of hospital breastfeeding support did not mediate the Hispanic-White disparities in exclusive breastfeeding.

The mediation analysis in this study shows that ensuring Black women receive the same level of hospital breastfeeding support as White women would very slightly reduce the magnitude of the Black-White disparities in exclusive breastfeeding termination, but a significant disparity would still remain. For Hispanic women, ensuring equal hospital breastfeeding support to Whites would make no significant changes in the magnitude of the Hispanic-White disparity in exclusive breastfeeding. These results demonstrate that while breastfeeding-supportive hospital practices are effective at improving overall

breastfeeding outcomes, equalizing access to such practices may not substantially change the observed racial/ethnic disparities in breastfeeding. Instead, more targeted public health interventions may be necessary for improving exclusive breastfeeding rates among Black and Hispanic women (28, 125). More research on the specific causes of the racial/ethnic disparities in breastfeeding, particularly in exclusive breastfeeding, are necessary to develop such targeted interventions.

B. **Implications for Practice**

By demonstrating racial/ethnic and income disparities in breastfeeding initiation, duration, and exclusivity in Illinois, this study informs which groups should be targeted for public health interventions to improve breastfeeding outcomes. Low-income Black women and low-income White women were the only two subgroups of Illinois women who were not meeting the *HP2020* objective for breastfeeding initiation in 2004–2008 (39). While Black women are usually targeted in breastfeeding interventions because they have the lowest prevalence of breastfeeding nationally, this study shows that low-income White women in Illinois could also benefit from additional public health interventions. Furthermore, this study showed that Hispanic women, who are usually viewed as having some of the best breastfeeding behaviors, were actually more similar to Black women when it came to exclusive breastfeeding. Therefore, different subgroups of women can use enhanced education and support for different elements of breastfeeding, whether it be making the initial decision to breastfeed or enabling women to continue to breastfeed and to do so exclusively.

This study also demonstrated a rapid drop-off in any and exclusive breastfeeding among breastfeeding initiators, suggesting it is not sufficient to focus public health resources only on the initial decision to begin breastfeeding. Priority must also be given to the protection and support of prolonged and exclusive breastfeeding (35). This study showed that many BFHI practices are associated with longer durations of any and exclusive breastfeeding. More universal implementation of BFHI practices may help women sustain breastfeeding over longer periods of time. As of June 2014, only four Illinois hospitals were Baby-Friendly certified (50). Many other hospitals in Illinois have expressed their intent to pursue

Baby-Friendly designation because of efforts like the *Healthy Places* project (122) and the *Illinois Breastfeeding Blueprint* (123), and public health professionals should continue to encourage and facilitate progress in this area. Given that the Ten Steps of the BFHI seem to work together synergistically to improve breastfeeding duration and exclusivity, it is important for hospitals to adopt as many BFHI practices as possible, even if they are not yet ready to apply for full BFHI designation. Hospitals wishing to take intermediate steps to support breastfeeding and exclusive breastfeeding should focus on implementing practices with the strongest individual effects: supporting breastfeeding within the first hour, reducing formula supplementation in-hospital, not giving pacifiers, and ceasing distribution of formula gift packs. Continuation of improvements in hospital breastfeeding support has the potential to dramatically improve breastfeeding duration and exclusivity for Illinois women and likely women across the United States.

As demonstrated in the mediation analysis of exclusive breastfeeding, population-based interventions like BFHI practices that are effective overall will not necessarily reduce breastfeeding disparities (28). Instead, public health professionals may need to use targeted approaches to improve breastfeeding behaviors among the groups of women with the lowest rates. Many types of breastfeeding interventions that have demonstrated positive impacts on breastfeeding initiation, duration, and exclusivity could be applied to targeted population groups. Examples of such interventions include: prenatal education in a variety settings (138), peer counseling (139–141), doula care (142–144), early prenatal participation in the WIC program (145), education involving the baby's father (146), education involving the baby's grandmother (particularly for adolescent mothers) (147), and school-based education for adolescents (138, 148). Participatory approaches to developing breastfeeding interventions may also be useful for ensuring that such interventions are relevant for the target population (28). Tailored programs that focus on vulnerable populations should be a complementary strategy to population-based approaches (125). Together these strategies can improve breastfeeding outcomes for the whole population and reduce the gaps between racial/ethnic subgroups.

C. **Implications for Research**

Because reducing disparities is a critical public health goal, it is important to understand how programs and policies may impact not only individual-level outcomes, but also disparities (28). A mediation approach, because it provides both a framework and accompanying statistical methods for exploring relationships involving intermediary variables, is well-suited for addressing questions about whether public health interventions have the potential to change health disparities (127). This study has demonstrated the application of mediation analysis to a specific research question about the impact of BFHI practices on racial/ethnic disparities in exclusive breastfeeding termination, but it also provides an example of how mediation methods can more broadly be incorporated into research on disparities in maternal and child health outcomes. Specifically, this study demonstrates how mediation methods can enable evaluation of programs and policies from a disparities perspective.

The findings of this study showed that many BFHI practices were associated with reduced odds of any and exclusive breastfeeding termination. These practices, however, were nonsignificant or very weak mediators of the observed racial/ethnic disparities in exclusive breastfeeding termination—meaning that little to none of the observed disparities in exclusive breastfeeding were due to differential access to BFHI practices. So, BFHI practices may be important for improving population-based breastfeeding outcomes, but their implementation will not ameliorate the problem of persistent disparities in exclusive breastfeeding termination. In general, more research about the factors that cause and maintain disparities in breastfeeding is necessary to inform targeted and tailored interventions (29), as such interventions may be more successful at reducing disparities than hospital-based or more general population approaches (28).

Mediation analysis can also be applied to test specific hypotheses about the factors that contribute to racial/ethnic disparities in breastfeeding (127). Many variables may influence breastfeeding behaviors and research should more specifically address whether and how these factors play a role in the establishment of disparities in breastfeeding. Because mediation analysis requires explicit definitions of directional relationships between variables, epidemiologists should articulate the theoretical model

guiding their research before applying such methods. For further research on disparities in breastfeeding, behavioral theories such as the Theory of Reasoned Action (149), Theory of Planned Behavior (150), and Self-Efficacy Theory (151) may be useful for generating hypotheses about the variables that mediate racial/ethnic disparities in breastfeeding outcomes. These theories have already been applied in many studies of breastfeeding (14, 96, 104, 136, 152–154) and provide different perspectives on important intermediary variables. For example, under Self-Efficacy Theory, proposed mediators of the racial/ethnic disparities in breastfeeding might include measures of self-efficacy itself, but also the antecedents of self-efficacy: personal accomplishments, vicarious experience, verbal persuasion, and psychological states (96). Once a theoretical model is developed, researchers can use mediation analysis to test the contribution of specific intermediary variables to the disparities in breastfeeding, thus informing the development of targeted public health interventions for disadvantaged populations.

Once such targeted interventions are developed, mediation analysis can also be used to evaluate whether such interventions are working as expected on the targeted mediator variables (127). In this scenario, the exposure would be participation in the intervention, the outcome would be a breastfeeding behavior of interest, and the mediator variable would be the specific intermediary variable that the intervention sought to affect (e.g., self-efficacy level). In this model, the indirect path would represent the effect of the program operating through the specific target variable and the direct path would represent the effect of the program operating through all other pathways. If the program is effective overall but shows little-to-no indirect path, it would demonstrate that the program is not working as intended. Further extensions of path analysis and structural equations models could be applied to examine multiple mediators simultaneously.

In addition to the wider application of mediation analysis, this study highlights several other promising areas for research on racial/ethnic disparities in breastfeeding. This study did not have information about delivery hospital, so a hospital-level analysis was not possible. It is not known, therefore, whether minority women experienced BFHI practices less frequently because they were less likely to deliver at hospitals implementing the practices or because there was differential experience

within hospitals. Future research could examine the possibility of linking data from PRAMS to the Maternity Practices in Infant Nutrition and Care questionnaire, which surveys hospitals about their implementation of breastfeeding-supportive practices (44). Such a linkage would allow for the possibility of comparing the provision of breastfeeding support as reported by individual women and by facilities. Multilevel analysis could also be used to separate the variation in experience of hospital practices due to individual factors versus hospital factors. This would help to better describe many of the patient and facility factors that predict experience of breastfeeding-supportive practices during the delivery hospitalization.

This study also could not address whether a woman's breastfeeding intentions may have affected her experience of hospital breastfeeding support. The Ten Steps are intended to be provided nearly universally to all women delivering in a hospital, not targeted to specific populations. The true implementation of these practices, however, especially among non-Baby-Friendly facilities could vary greatly across on patient characteristics. For example, women with strong intentions to exclusively breastfeed may have been more likely to experience or to remember experiencing BFHI practices than women who intended to supplement breastfeeding with formula, either because of patient demand for certain practices (e.g., no pacifier or rooming-in), the level of willingness of hospital staff to intervene to promote the BFHI practice, or other factors. Future research should seek to understand the real-world implementation of BFHI practices and how facility and patient factors may interact to affect a woman's experience of hospital breastfeeding support.

D. **Limitations and Considerations**

While this study used rigorous methods to carry out the study aims, there are several limitations that need to be taken into consideration when interpreting the findings.

1. **Race/ethnicity construct**

This study compared breastfeeding behaviors for women of three racial/ethnic groups: non-Hispanic Whites, non-Hispanic Blacks and Hispanics. The definition of these racial/ethnic groups,

however, may not accurately capture the desired race/ethnicity construct, which this study uses as a proxy for common social norms, cultural preferences, experience of discrimination, economic and social capital, and other shared experiences that may affect breastfeeding behaviors. The definition of the racial/ethnic groups comes from the race and ethnicity questions on the birth certificate, which were historically developed through perceived homogeneity based on external physical features. In reality, racial/ethnic groups defined by such classification systems may not truly be homogenous, as in the case of foreign-born versus native-born women (155). Several studies have shown that children with foreign-born parents were more likely to be breastfed and be breastfed for longer durations than their respective counterparts with US-born parents, regardless of race/ethnicity (106, 107).

Acculturation, or level of assimilation into the dominant culture, is one factor that has been used to describe heterogeneity in Latino health. Common proxy measures for acculturation in health research include: primary language, place of birth (nativity), generational immigration status (e.g., children with immigrant parents), citizenship status, and length of time in the United States (156). Previous studies have demonstrated that Hispanics who are more acculturated to US society are less likely to initiate and continue breastfeeding and to exclusively breastfeed at hospital discharge than Hispanics with lower acculturation levels, even after controlling for demographic variables (106, 108–110). The only acculturation-related variable that was available from IL-PRAMS is survey language (English versus Spanish). Appendix A includes several tables that compare the population characteristics and breastfeeding behaviors of Hispanic Illinois women by survey language. In this sample, Hispanic women who took the Spanish survey were more likely to initiate breastfeeding than Hispanic women who took the English survey, even after adjusting for confounders. In contrast to what has been shown in other studies, survey language was minimally related to any breastfeeding termination before 12 weeks, but not at earlier time points or at any time point for exclusive breastfeeding. Survey language represents, at best, only a very crude measure of acculturation for Hispanic women and results may have varied if other acculturation measures had been available in IL-PRAMS. Because language did not appear to

substantially alter any or exclusive breastfeeding duration in this population, further sub-analyses of Hispanic women by survey language were not conducted.

Research has shown that there is also considerable heterogeneity with regard to cultural, historical, and social factors within the population of Black persons who identify as African American, but health research has generally not incorporated such population distinctions (26). In this study, which examines only Illinois residents, variation in the Black population may be less pronounced than in a national study. Indicators of distinct cultural and ethnic differences within the Black population were not available in IL-PRAMS. Therefore, Black women were analyzed as one racial/ethnic group, even though they may also show inherent differences by country of birth and region of origin (e.g., Caribbean versus Africa) (26).

An additional limitation of the race/ethnicity categorization used in this study is that women were grouped into only one race/ethnicity category (though some women may identify themselves as multiple races). So, even the simple measurement of race/ethnicity may not accurately capture the sociocultural experiences and breastfeeding preferences of women in this study.

2. **Pregnancy Risk Assessment Monitoring System questionnaire**

The PRAMS is a cross-sectional survey, so this study is also limited in its ability to draw assumptions about the causal relationships of the variables. Assumptions about temporality are fundamental to mediation analysis and a strong case must be made for the direction of the relationships in the framework. By dichotomizing the breastfeeding duration and exclusivity outcomes at two, six, and 12 weeks, it can be assumed that these outcomes occurred after the experience of the hospital practices for the vast majority of women (since most women are discharged less than four days after delivery). This limitation, however, precludes the assessment of how maternity care practices influence racial/ethnic disparities in breastfeeding initiation because it could not be known whether breastfeeding initiation occurred first (and therefore influenced receipt of hospital practices).

Adequacy of prenatal care was missing for 6%–8% of the weighted sample (depending on the subsample) and was therefore not included as a covariate in the analysis. For the results of aim one (see

chapter III), a sensitivity analysis was conducted to consider how the addition of adequacy of prenatal care utilization impacted the results; adequate or better prenatal care was associated with only one breastfeeding outcome: any breastfeeding for at least 12 weeks. Additionally, the addition of prenatal care adequacy to the adjusted models did not substantially change the estimates of the Black-White or Hispanic-White disparities in any of the breastfeeding outcomes, indicating that prenatal care did not act as a confounder when adjustment was made for other covariates related to maternal and infant characteristics.

There are many variables related to breastfeeding that were not available in IL-PRAMS. Women's prenatal employment status, paid maternity leave, and timing of return to work have been shown to be important predictors of breastfeeding initiation, duration, and exclusivity (157–163). More than 50% of women with infants under one year of age participate in the labor force (164), but only about 41% of working women receive any paid maternity leave benefits and only for an average of about three weeks (165). Paid maternity leave may be particularly relevant as a predictor of breastfeeding behaviors for less-advantaged populations (159, 166). Other important variables such as self-efficacy (96) and breastfeeding (152, 153) may also act as mediators in the racial/ethnic disparities in breastfeeding, but were not measured by PRAMS. Further research should include these types of potential explanatory variables when developing studies to determine factors influencing disparities in breastfeeding.

This study also did not have information about delivery hospital, so a hospital-level or multilevel analysis was not possible. It is not known whether the different experience of BFHI practices by minority women was because they were less likely to deliver at hospitals implementing BFHI practices or because they were less likely to experience the BFHI practices within hospitals (or both). Future research could examine the possibility of linking data from PRAMS to the Maternity Practices in Infant Nutrition and Care questionnaire, which surveys hospitals about their implementation of breastfeeding-supportive practices (44). Such a linkage would allow for the possibility of comparing the provision of breastfeeding support as reported by individual women and by facilities. Multilevel analysis could also be used to separate the variation in experience of hospital practices due to individual factors versus hospital factors.

In this study, the analyses relating to hospital practices included breastfeeding women, who should all have experienced breastfeeding-supportive practices in the hospital according to AAP and WHO standards. This study could not, however, address whether a woman's breastfeeding intentions or attitudes may have affected her in-hospital experience of breastfeeding support. Future research should seek to understand the real-world implementation of BFHI practices and how hospital and patient factors interact to affect a woman's breastfeeding support.

3. **External validity**

While this survey is representative of the population of White, Black, and Hispanic Illinois women who recently delivered a live birth, the results are not generalizable to the broader population of US women. While the racial/ethnic distribution of live births in Illinois is similar to that of the nation (for 2012 births: Illinois maternal race/ethnicity was 54% White, 17% Black, 22% Hispanic compared to 54% White, 15% Black, and 23% Hispanic for the nation) (129), there may be differences between the state and nation within racial/ethnic subgroups. For the Black population that identifies as African American, previous research has identified nine distinct sociocultural subgroups that roughly correspond to geographic area (26). The Black women residing in Illinois, therefore, may not be representative of the Black women in the nation or in other regions of the United States. There is also wide variation in the Hispanic population across the United States with respect to country of origin. For example, in 2012, 83% of Hispanic births in Illinois were to women of Mexican origin, compared to only 61% of national births (129). Illinois' Hispanic women, therefore, are unlikely to represent the experiences of the broader Hispanic population throughout the country. Future studies could replicate this study using a more representative population of the entire United States.

Additionally, the factors contributing to racial/ethnic disparities in breastfeeding and hospital practices may not be consistent across the United States. Past studies have shown that there is wide regional variation in the delivery of BFHI practices within maternity-care hospitals (42). Up to this point in time, Illinois have been very slow to adopt BFHI practices. For nine practices of the Ten Steps, the percent of Illinois delivery hospitals complying with Baby-Friendly recommendations in 2011 were lower

than the national average (51). Additionally, 32.3% of Illinois hospitals complied in 2011 with at least six of the recommended Ten Steps, compared to 43.5% of US hospitals (52). Only four (out of 120) birthing hospitals in Illinois are currently Baby-Friendly certified (50), and in the first half of 2013, only 2.5% of Illinois births occurred in Baby-Friendly hospitals (compared to 7.2% in the nation) (18). Therefore, the experience of BFHI practices by Illinois women may not represent the hospital experiences of all women in the country.

4. **Dichotomous variables**

Dichotomous variables are not the preferred measure for a process like breastfeeding, which could be more accurately described by a survival model. This study, however, is limited by the availability of data in the PRAMS dataset. The PRAMS survey is usually completed by women three to six months after they delivered their infant, so collection of information about breastfeeding duration is censored at the time point the survey is completed. Additionally, the women who complete the survey early (e.g., three months) may be different than the women completing the survey later in time (e.g., six months) due to factors influencing the ease of contacting women and obtaining their cooperation. Because of this, the latest time point usually examined for breastfeeding in the PRAMS survey is 12 weeks, as later responses may not be representative of the whole population.

Survival analysis (censored for all participants at 12 weeks) was considered as an alternative to dichotomizing the breastfeeding outcomes. However, the mediation analysis methods available for survival analysis are limited and did not fit well with the requirements of this study. The traditional method of conducting survival mediation analysis is similar to the traditional mediation analysis method for other models; Cox proportional hazard models are fit with and without the mediator and the hazard ratios from these two models are compared to determine if mediation is present. This is equivalent to obtaining the total effect and controlled direct effect and determining if they are different. Indirect effects are not estimated by this method. Others have shown, however, that the mathematical structure of Cox models does not allow for the assumption of proportional hazards to be met in both models with and without the mediator, so it is not consistent to compare the results in this way. Lange and Hansen (2011)

developed a method of conducting survival mediation analysis for additive hazards models that overcomes these issues and allow for the estimation of NDEs, IEs, and their CIs. This method, however, produces estimates only on the risk difference scale and it cannot accommodate binary mediators (134). VanderWeele (2011) showed that this method is transferrable to Cox models with a rare outcome (135). Because breastfeeding termination is a non-rare outcome and because the mediators in this analysis were all binary, the Lange method applied to either additive hazards models or Cox models was not feasible for this study.

The values of the mediators (hospital practices) in this study were also dichotomized. In reality, the experience of these hospital practices may be better captured by an ordinal scale allowing varied levels of experiences. For instance, not providing any supplemental feedings (Step 6) could be instead be characterized as the percent of hospital feedings where the infant received formula. VanderWeele (2012) showed that, in most contexts, dichotomizing the mediator variable to mask finer levels of inherent gradation results in an overestimation of the DE and an underestimation of the IE (133). To explain why this occurs, an illustration using the example of supplemental feeding is useful. If we were interested in the true effect of supplemental feeding as a mediator, a continuous or ordinal variable may better capture how often or how much the infant was fed formula in the hospital. By dichotomizing to any versus no formula, infants who received breast milk 90% of the time and those who received formula 0% of the time are all grouped together as not experiencing the BFHI practice. The observed DE, therefore, actually captures a portion of the effect of the mediator for those infants who had an intermediate level of formula feeding masked by the dichotomization. The observed IE, therefore, will be diluted and underestimated because the DE gets credit for some of the effect that actually occurs through the mediator. Other hospital practices considered in this study may be subject to this same limitation and the results presented may underestimate the true IE and overestimate the true DE.

The PRAMS survey asks only yes/no questions about the hospital practices women experienced during their delivery hospitalizations. The dichotomization of hospital practices is done out of convenience and a concern for reducing misclassification due to reporting errors. This is the simplest

question format and reduces the time and effort required to complete the survey. Additionally, asking women to report hospital practices on finer gradations (e.g., frequency) could increase recall bias. Because the survey occurs months after the hospital stay, women may not accurately remember the details of their hospital stay to report the practices at finer levels than yes or no. Furthermore, many of the hospital practice questions in PRAMS have been framed to reflect practice recommendations set forth by the WHO in the BFHI. The standards to which Baby-Friendly hospitals are held accountable reflect this all-or-nothing framing of the hospital practices (e.g., no formula supplementation, rooming-in 24 hours per day) (67). While dichotomous versions of the mediators may not be the most precise for examining the true effect of hospital practices, the dichotomous versions of the mediators asked on PRAMS represent a compromise between reducing reporting errors, reducing survey burden, and equating practices closely with best practice standards.

Because any and exclusive breastfeeding termination at the time points in this study are not rare outcomes, log-linear regression would be the preferred modeling method to estimate relative risks for all analyses. Stata version 11 (StataCorp, College Station, Texas) was used to attempt estimation of log-linear regression models while accounting for the complex sample design of the PRAMS survey. For all breastfeeding outcomes examined in aim one (see chapter III), the adjusted log-linear regression models that adjusting for all covariates would not converge and logistic regression was used as an alternative model. Because the outcomes are non-rare, the ORs presented in this study are overestimates of the relative risks. The prevalence of exclusive breastfeeding termination increases over time, so the ORs presented for the outcomes at 12 weeks will be overestimated to a greater degree than the estimates for outcomes at two weeks. The variable extent of overestimation also precludes comparing the magnitude of the results for models across the various time points.

5. **Potential measurement error**

Because the PRAMS survey relies on the self-report of women's breastfeeding behaviors and experiences during the hospital stay, there is a potential for misclassification due to recall or reporting

biases. The PRAMS survey is administered two to eight months after delivery, so a woman may not vividly remember her experiences during the hospital stay or the first few weeks/months postpartum.

Given that various measures of breastfeeding are the outcome variables for all parts of this study, understanding the validity of the PRAMS measures of breastfeeding is important. A recent study showed that the validity of the self-reported data in PRAMS on breastfeeding initiation is generally high; when compared to birth certificates, PRAMS had a sensitivity of 94.3% and a specificity of 76.0% for breastfeeding initiation (111). While questions on breastfeeding initiation seem fairly straight forward, some women may interpret ever breastfeeding differently than medical or lactation professional. Some women, for instance, may not count themselves as having breastfed if they only did so one or two times, or if they failed in early attempts to breastfeed. Such interpretation differences may account for the differences between PRAMS and birth certificates shown in the study described above.

No studies have specifically considered the validity of PRAMS data on breastfeeding duration or exclusivity, but other studies on maternal breastfeeding recall shed some light on potential reporting errors in PRAMS. One study of maternal recall of timing of weaning compared mother's reports of breastfeeding status at six months to prospectively collected data during the first 12 weeks after delivery. The correlation of the maternal report to the prospectively collected data was only moderate (r=.49) and most women tended to overestimate the duration they breastfed their infant (115). Another study found that at six months after delivery, 88% of women were able to accurately recall their breastfeeding duration within one month of the clinical record. Women in this study also tended to slightly overestimate breastfeeding duration (by a mean of 0.7 weeks) (113). The exact reasons for such overestimation are not well known, but both poor recall and social desirability may be at work (114).

Maternal recall of exclusive breastfeeding is generally less accurate than their recall of initiation or overall duration. A South African study showed that the six- to nine-month recall of women for exclusive breastfeeding status at five time points had relatively high sensitivities but low specificities, and that both validity measures improved for later time points compared to earlier ones. This study also showed that only 28% of women recalled their true exclusive breastfeeding duration within one week

when asked about it at six to nine months after delivery; 57% of women overestimated by more than one week and 15% underestimated by more than one week (112). A different study showed that only 58% of American women could accurately recall their length of exclusive breastfeeding duration within one month when surveyed six months after delivery (113). In addition to the recall and social desirability issues that may lead to misclassification of all breastfeeding outcomes, reporting exclusive breastfeeding may be less accurate because of issues with personal interpretation of the importance and relevance of infant feeding behaviors. For example, women who supplement with formula during the first few days and then go on to exclusively breastfeed may or may not ignore the early supplementation when reporting their infant feeding practices (114). The PRAMS question about food introduction reads "*How old was your baby the first time you fed him or her anything besides breast milk?*" (132) and may have led to confusion about whether to report what she personally fed the baby versus what others fed him/her. In this study, of the 3,065 women reporting they were still exclusively breastfeeding at two weeks, only 2,012 women (weighted percent=67.2%) reported their baby was fed only breast milk in the hospital. This suggests many women may have felt that hospital-provided formula supplements didn't count toward their personal breastfeeding behaviors. The tendency to ignore hospital supplements when reporting exclusive breastfeeding duration was more common among Black and Hispanic women than White women; of women reporting exclusive breastfeeding at two weeks, 76.1% of Whites, 54.9% of Blacks, and 49.6% of Hispanics reported their baby received only breast milk in the delivery hospital. To be consistent with standard measurements of exclusive breastfeeding, *any* formula supplementation or other foods, regardless of frequency or timing, would qualify as exclusive breastfeeding termination.

Given the findings described above, it is likely that the measurement of breastfeeding initiation in this study is fairly accurate. There may be some reporting errors due to interpretation of ever breastfeeding, but the high sensitivity of the PRAMS question indicates that the sub-analyses of only breastfeeding initiators would likely be minimally affected by misclassification. On the other hand, it is likely that the measurement of any and exclusive breastfeeding termination at two, six, and 12 weeks includes some substantial reporting errors that could impact the results of this study. The true rates of any

and exclusive breastfeeding termination at each time point may actually be higher than reported because of the tendency to overestimate duration. Overestimation may occur due to inaccurate recall, but could also be related to social desirability (114) or rounding (e.g., reporting two months rather than seven weeks). If any and exclusive breastfeeding termination are underestimated in this study, the true effectiveness of hospital practices at preventing breastfeeding termination would be underestimated.

Few studies have examined whether differential misclassification of breastfeeding occurs by demographic factors, such as education, income, or race/ethnicity. A study in Brazil found that women who were more educated and wealthier tended to overestimate breastfeeding duration, but poorer and less educated women did not tend to misreport breastfeeding duration in one direction more than another (167). If this same misclassification pattern holds for contemporary American women, the racial/ethnic disparities in breastfeeding termination in this study may be overestimated due to the reporting bias.

No studies were found on the validity of the self-reported BFHI practice data from PRAMS. Women may not accurately remember their experiences of these practices during the hospital stay, or they may interpret the questions about these practices differently. Misclassification of the hospital practice variables could have occurred and the extent and direction of such recall and reporting biases are unknown. In the mediation analysis, if mediator misclassification was non-differential (with respect to race/ethnicity and exclusive breastfeeding termination), the mediator-outcome relationship can be weakened and the indirect effect underestimated (130, 131). While methods for correcting this bias are available for continuous mediators, misclassification bias corrections are not available for binary mediators (168). In situations involving exposure-mediator interaction, it is less clear how non-differential measurement error of the mediator would bias the total, direct, and indirect effects (130). If misclassification of the hospital practices were differential, the DE and IE estimates could be biased in either direction, depending on how the misclassification was related to the exposure and/or outcome. The TE, however, remains unbiased under any form of mediator misclassification (non-differential or differential) because the mediator value is not incorporated into the total effect estimation (130).

6. **Mediation assumptions of unmeasured confounding**

In general, decisions about which variables to include as covariates in a mediation analysis are even more critical than in other observational studies because mediation requires strict assumptions regarding unmeasured confounding for causal interpretations to be valid. For this study, the required assumptions of no unmeasured confounding involve the associations of race/ethnicity and exclusive breastfeeding termination ($A \rightarrow Y$), BFHI practices and exclusive breastfeeding termination ($M \rightarrow Y$), and race/ethnicity and BFHI practices ($A \rightarrow M$). In addition, there should be no confounding (either measured or unmeasured) of the association of BFHI practices and exclusive breastfeeding termination by a factor that is caused by race/ethnicity ($M \rightarrow Y$ caused by A). (75) These assumptions may not hold for this analysis, thus affecting the ability to draw valid causal conclusions. While VanderWeele (2010, 2013) suggests the use of sensitivity analysis procedures to assess the impact of potential unmeasured confounding on the mediation results (93, 94), such sensitivity analyses were not undertaken for this study because the mediation effects were nonsignificant or very weak. While it is probable that unmeasured confounding exists in this study, most mediation analyses showed little-to-no mediating effect and accounting for unmeasured confounders would almost certainly render all mediation findings nonsignificant.

VII. CONCLUSIONS

This study used a population-based dataset of new mothers in Illinois to enhance research on disparities in breastfeeding by: (1) demonstrating the complex and varying ways that race/ethnicity and SES may together influence breastfeeding behaviors and describing the subgroups at highest risk for poor breastfeeding initiation, duration, and exclusivity; (2) assessing the effectiveness of Baby-Friendly hospital practices for improving breastfeeding duration and exclusivity, with a special focus on differential effectiveness across racial/ethnic groups; and (3) demonstrating an application of mediation methods to evaluating the impact of differential experience of Baby-Friendly hospital practices on disparities in breastfeeding exclusivity. In doing so, this study has sought to inform the development of public health programs and policies that will successfully contribute to reduced disparities in breastfeeding, thus enabling all women and children to more equally benefit from breastfeeding.

Future research in breastfeeding demands more attention to understanding the factors that generate and maintain disparities in breastfeeding initiation, duration, and exclusivity. Mediation methods are well suited for assessing causal pathways, and could be applied to research on disparities in breastfeeding by: (1) identifying and quantifying differences between racial/ethnic subgroups that account for their disparities in breastfeeding; (2) evaluating whether breastfeeding interventions are making an impact on breastfeeding outcomes through their expected intermediary variables; and (3) determining whether equitable implementation of population-based interventions could contribute to reductions in observed disparities. Such research is likely to demonstrate the need for both population-based and targeted interventions as complementary approaches for improving breastfeeding overall and eliminating racial/ethnic disparities.

APPENDICES

APPENDIX A:

Tables Comparing Hispanic New Mothers in Illinois by Survey Language

TABLE XXIII

CHARACTERISTICS OF HISPANIC NEW MOTHERS IN ILLINOIS, BY SURVEY LANGUAGE

	English Survey	**Spanish Survey**	**χ2 test p value**
Sample Size	699	1,164	
Weighted Sample Size	68,474	117,956	
Percent of Population	36.7%	63.3%	
	column %	***column %***	
Maternal Age			
<20 years old	17.9	10.1	<0.01
20–24 years old	27.7	28.8	
25–29 years old	26.3	30.4	
≥30 years old	28.1	30.7	
Maternal Education			
<High School	27.4	59.0	<0.01
High School diploma	34.3	30.7	
>High School	38.4	10.3	
% Married	50.1	49.9	0.91
% Low Income	77.6	96.6	<0.01
% Primiparous	41.6	31.7	<0.01
% Smoked During Last 3 Months of Pregnancy	5.8	0.7	<0.01
% Maternal Hospital Stay ≥4 days	21.6	18.8	0.15
% Cesarean Section	28.4	24.5	0.08
% Plural Birth	1.1	0.7	0.25
% Male Infants	51.8	51.0	0.77
% Infants Admitted to NICU	11.6	14.6	0.06
% Low Birth Weight Infants (<2500g)	6.4	5.5	0.17

APPENDIX A (continued)

TABLE XXIV

PERCENT OF HISPANIC ILLINOIS WOMEN WHO NEVER BREASTFED AND PERCENT OF BREASTFEEDING HISPANIC WOMEN WHO STOPPED ANY AND EXCLUSIVE BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS, BY SURVEY LANGUAGE

	English Survey		**Spanish Survey**		**χ2 test**
	n	**% (95% CI)**	**n**	**% (95% CI)**	**p value**
Never Breastfed	699	19.3 (16.1–22.4)	1161	11.2 (9.2–13.1)	<0.01
Any Breastfeeding (*among initiators)*					
Stopped before 2 weeks	544	7.0 (4.7–9.3)	992	4.8 (3.4–6.2)	0.08
Stopped before 6 weeks	544	31.8 (27.6–36.0)	992	25.7 (22.8–28.6)	0.02
Stopped before 12 weeks	529	49.6 (45.0–54.2)	970	42.2 (38.9–45.6)	0.01
Exclusive Breastfeeding *(among initiators)*					
Stopped before 2 weeks	547	50.3 (45.8–54.8)	961	53.3 (50.0–56.7)	0.29
Stopped before 6 weeks	548	69.2 (65.1–73.4)	974	67.6 (64.5–70.8)	0.54
Stopped before 12 weeks	546	80.0 (76.4–83.6)	971	78.5 (75.7–81.2)	0.51

APPENDIX A (continued)

TABLE XXV

CRUDE AND ADJUSTED ODDS RATIOS AND 95% CONFIDENCE INTERVALS FOR NEVER BREASTFEEDING AND TERMINATION OF ANY AND EXCLUSIVE BREASTFEEDING BEFORE TWO, SIX, AND 12 WEEKS BY SURVEY LANGUAGE, AMONG HISPANIC BREASTFEEDING ILLINOIS WOMEN

Breastfeeding Outcome		**English Survey vs. Spanish Survey**			
	n	**OR**	**(95% CI)**	**aOR**[a]	**(95% CI)**
Never Breastfed	1,860	1.90	(1.43–2.51)	1.79	(1.33–2.41)
Any Breastfeeding (*among initiators)*					
Stopped before 2 weeks	1,536	1.51	(0.95–2.41)	1.46	(0.89–2.40)
Stopped before 6 weeks	1,536	1.35	(1.05–1.72)	1.26	(0.97–1.63)
Stopped before 12 weeks	1,499	1.35	(1.07–1.69)	1.28	(1.01–1.63)
Exclusive Breastfeeding *(among initiators)*					
Stopped before 2 weeks	1,508	0.89	(0.71–1.11)	0.83	(0.66–1.05)
Stopped before 6 weeks	1,522	1.08	(0.85–1.37)	1.02	(0.80–1.31)
Stopped before 12 weeks	1,517	1.10	(0.83–1.45)	1.04	(0.78–1.39)

[a] adjusted for: infant sex, maternal age, marital status, parity, plurality, smoking during last three months of pregnancy, infant NICU admission, length of maternal hospital stay, delivery method, and low birth weight.

APPENDIX B

Example Results of Marginal Standardization

TABLE XXVI

PREDICTED MARGINAL PREVALENCE ESTIMATES [a] OF NEVER BREASTFEEDING AND ANY AND EXCLUSIVE BREASTFEEDING TERMINATION BEFORE TWO, SIX, AND 12 WEEKS, BY RACE/ETHNICITY

	Whites	Blacks	Hispanics	Absolute Disparity	
	% (95% CI)	% (95% CI)	% (95% CI)	Black-White	Hispanic-White
Never Breastfed	25.8 (24.3–27.4)	33.6 (30.9–36.2)	14.1 (12.4–15.7)	7.8%	-11.7%
Any Breastfeeding (*among initiators)*					
Stopped before 2 weeks	7.6 (6.5–8.7)	8.2 (6.1–10.3)	5.0 (3.9–6.1)	0.6%	-2.6%
Stopped before 6 weeks	24.5 (22.9–26.2)	28.8 (25.3–32.3)	26.0 (23.6–28.3)	4.3%	1.5%
Stopped before 12 weeks	40.6 (38.7–42.5)	45.3 (41.4–49.3)	42.1 (39.4–44.8)	4.7%	1.5%
Exclusive Breastfeeding *(among initiators)*					
Stopped before 2 weeks	37.0 (35.2–38.9)	49.6 (45.5–53.8)	53.0 (50.2–55.7)	12.6%	16.0%
Stopped before 6 weeks	53.9 (52.0–55.8)	66.5 (62.6–70.5)	68.3 (65.8–70.9)	12.6%	14.4%
Stopped before 12 weeks	68.5 (66.8–70.3)	79.1 (75.6–82.5)	78.3 (75.9–80.6)	10.6%	9.8%

[a] adjusted for: infant sex, maternal age, marital status, parity, plurality, smoking during last three months of pregnancy, infant NICU admission, length of maternal hospital stay, delivery method, and low birth weight.

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