Breastfeeding and the Risk of Postneonatal Death in the United States

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ABSTRACT. *Objective.* **Breastfed infants in the United States have lower rates of morbidity, especially from infectious disease, but there are few contemporary studies in the developed world of the effect of breastfeeding on postneonatal mortality. We evaluated the effect of breastfeeding on postneonatal mortality in United States using 1988 National Maternal and Infant Health Survey (NMIHS) data.**

Methods. **Nationally representative samples of 1204 infants who died between 28 days and 1 year from causes other than congenital anomaly or malignant tumor (cases of postneonatal death) and 7740 children who were still alive at 1 year (controls) were included. We calculated overall and cause-specific odds ratios for ever/never breastfeeding among all children, conducted race and birth weight–specific analyses, and looked for duration–response effects.**

Results. **Overall, children who were ever breastfed had 0.79 (95% confidence interval [CI]: 0.67–0.93) times the risk of never breastfed children for dying in the postneonatal period. Longer breastfeeding was associated with lower risk. Odds ratios by cause of death varied from 0.59 (95% CI: 0.38–0.94) for injuries to 0.84 (95% CI: 0.67–1.05) for sudden infant death syndrome.**

Conclusions. **Breastfeeding is associated with a reduction in risk for postneonatal death. This large data set allowed robust estimates and control of confounding, but the effects of breast milk and breastfeeding cannot be separated completely from other characteristics of the mother and child. Assuming causality, however, promoting breastfeeding has the potential to save or delay ~720 postneonatal deaths in the United States each year.** ***Pediatrics*** **2004;113:e435–e439. URL: http://www.pediatrics.org/cgi/content/full/113/5/e435;** ***breastfeeding, infant mortality, cause of death, risk, logistic models.***

ABBREVIATIONS. SIDS, sudden infant death syndrome; NMIHS, National Maternal and Infant Health Survey; OR, odds ratio; CI, confidence interval.

In developing countries, breastfeeding protects against diarrhea[1] and respiratory diseases,[2] important causes of infant death.[3–9] In contemporary developed countries, however, where infectious diseases account for a smaller portion of infant mortality,[10] what effect, if any, breastfeeding has on mortality is not clear. There is a large literature on the benefits to the child and the mother of breastfeeding,[11] but almost all contemporary US data concern morbidity or are of a specific cause of death, such as sudden infant death syndrome (SIDS).[12–14] The only US study of all-cause mortality and feeding method since the introduction of modern infant formulas in the late 1950s is an analysis of the 1988 and 1995 cycles of the National Survey of Family Growth[15]; Forste et al reported that breastfed children had substantially lower risk of dying between 1 month and 1 year, but they did not attempt control of confounding beyond race and birth weight. In Great Britain in the 1970s, Carpenter et al[16] found that the infants of mothers who declared an intention to breastfeed had lower mortality from "preventable" causes, largely infectious diseases, trauma, and SIDS, out to 2 years of age. Knowing whether breastfed children have a survival advantage is important in its own right. In addition, developing policy or recommendations concerning potentially lethal hazards from breastfeeding, such as exposure to human immunodeficiency virus[17] or chemical carcinogens in milk,[18] requires some estimate of the mortality benefit as well.

Studying the salutary effects of breastfeeding presents some widely recognized problems in inference. In addition to the control of confounding by parity, maternal age, birth weight, and other factors that are plausibly associated both with the decision to breastfeed and the welfare of the infant, there is a special problem with reverse causality. Because infants who are sick from birth may be unable to breastfeed and children who become ill later may stop, breastfeeding infants may seem healthier because illness, especially mortal illness, prevents breastfeeding rather than because breastfeeding prevents illness. The recommended methods for dealing with this problem are to exclude deaths that occur in the neonatal period and to assign feeding category by how the child was fed at some time before death occurred.[19] In addition, infants who die from congenital anomalies or malignant tumors (~15% of all postneonatal deaths in the United States in the late 1980s) may have been unable to initiate breastfeeding,[20,21] and it is unlikely that their deaths are preventable by breastfeeding and they thus should be excluded. These tactics do not exclude reverse causality completely, but they should minimize its effects.

We use 1988 US National Maternal and Infant Health Survey (NMIHS) data to analyze the association between breastfeeding and postneonatal death using a case-control approach. We do not consider neonatal death (a liveborn child who dies before 28 days), because breastfeeding information was not gathered on the children who died so young and

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most such deaths are attributable to preterm birth or congenital anomalies; we also excluded deaths from congenital anomaly or malignancy occurring in the postneonatal period.

METHODS

Subjects

The 1988 NMIHS is a nationally representative stratified systematic sample of 9953 women who had live births, 3309 who had late fetal deaths (28 weeks' gestation or more, including term stillborn), and 5332 who had infant deaths (a liveborn child who died by 1 year of age) in 1988.[22] These live births and infant deaths were from 48 states (none from Montana or South Dakota), the District of Columbia, and New York City. Black infants were oversampled in all 3 components of the NMIHS, and very low birth weight (<1500 g) and moderately low birth weight (1500–2499 g) infants were oversampled in the live birth component. Vital events to unmarried mothers in Arizona, Kansas, and North Dakota were excluded.[22] Only live births and infant deaths are included in our analysis. The final sample for analysis, containing 1204 postneonatal deaths (cases) and 7740 live births (still alive and ≥1 year old at survey; controls), is shown in Fig 1.

Mothers answered a mailed questionnaire on characteristics of the parents, previous and subsequent pregnancies, prenatal care and health habits, and the infant's health. Information from the birth certificate and death certificate was also included in the data set. Women whose infant died before 1 month or did not live with her at any time after birth were not asked the breastfeeding questions. The answer "yes" or "no" to the question, "Did you ever breastfeed this infant?" was defined as "ever breastfed" or "never breastfed" in the analysis. The duration of breastfeeding is from the answer to the question, "How old was your infant when you stopped breastfeeding?"

Causes of postneonatal death (*International Classification of Diseases, Ninth Revision*) were obtained from death certificates. For some analyses, we divided the deaths into 4 categories: infections, injuries, SIDS, and others (Table 1).

Statistics

We used logistic regression to calculate the odds ratio (OR) of ever having breastfed to never having breastfed for postneonatal death. We first considered all postneonatal deaths as cases and the live births as controls. We then duplicated the analysis using cases from each of 4 cause-of-death categories, whereas the controls remained unchanged. Race and birth weight are so strongly related to breastfeeding that we did analyses separately by race and birth weight category. Covariates included mother's age, education, smoking during pregnancy, and participation in the federal nutritional support program for Women, Infants, and Children; and infant's gender, race, birth weight, congenital malformation reported at birth, live birth order, and single or multiple birth. The race- and birth weight–specific analyses did not include race or birth weight terms. We also did proportional hazard models to calculate the hazard ratio for ever breastfeeding in cases only to determine whether breastfeeding delayed death even among infants who died.

We were interested in determining whether prolonged breastfeeding had greater effects. Because these are case-control data, however, we cannot simply put breastfeeding duration in the logistic model, because the opportunity for the case infants to breastfeed extends only to their age at death, whereas the controls can breastfeed for up to 1 year. So, unless the case infants died very late in the infancy (clearly not true in this study), their opportunity for prolonged breastfeeding was significantly compromised. We addressed this problem by doing an analysis using the model described above but limiting the case group to those who had survived 3 months or more and using 3 months of breastfeeding versus <3 months or none in place of the ever/never breastfed variable. This equalizes the opportunity to breastfeed at 3 months in the cases and controls, at a cost of reduced sample size among the cases. We then can compare the OR of ever breastfeeding with the OR of breastfeeding for 3 months or more.

We used SAS 8.2 (SAS, Inc, Cary, NC) for preliminary tabula-

TABLE 1. Causes of Postneonatal Death in the 1988 NMIHS (Excluding Malignancy and Congenital Anomalies)

Postneonatal Death Causes and ICD-9 Codes	*n*
Infections (*n* = 255)	
001–139 Infectious and parasitic diseases	47
240–279 Endocrine, nutritional, and metabolic diseases and immunity disorders	1
320–389 Meningitis and other diseases of the nervous system and sense organs	56
460–519 Diseases of the respiratory system	124
520–579 Diseases of the digestive system	27
Injuries (*n* = 126)	
E800–E999 Injury and poisoning	126
SIDS (*n* = 591)	
798.0 Sudden infant death syndrome	591
Others and unknown (*n* = 232)	
280–289 Diseases of the blood and blood-forming organs	3
760–779 Certain conditions originating in the perinatal period	30
780–797, 798.1–799 Symptoms, signs, and all other ill-defined conditions	87
Unknown	112

ICD-9 indicates *International Classification of Diseases, Ninth Revision.*

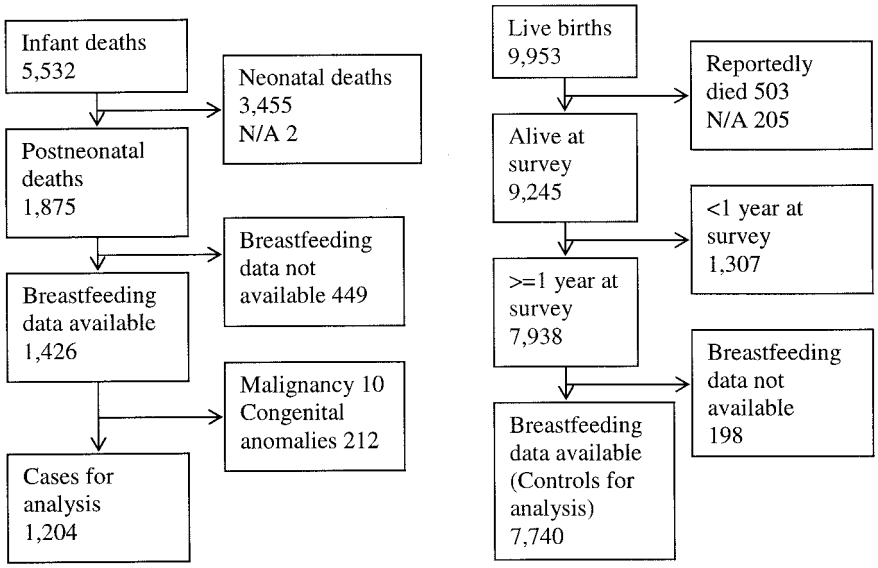

Fig 1. Samples for analysis from the 1988 National Maternal and Infant Survey. Of 449 subjects without breastfeeding information in infant death group, 323 answered "No" to the question, "Was the baby at home with mother at any time after delivery?", 78 had no available answer, and another 48 answered "Yes" but did not provide information regarding breastfeeding. Of 198 subjects without breastfeeding information in the live birth group, 100 answered "No" to the question, 37 had no available answer, and another 61 answered "Yes" but did not provide information regarding breastfeeding. N/A denotes not available.

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tion and descriptive analysis and, because of the oversampling of black and low birth weight infants, SUDAAN 8.0.2 (Research Triangle Institute, Research Triangle Park, NC) to reweight the sample for the overall estimates and to calculate the ORs and 95% confidence intervals (CIs) in the final models. SUDAAN adjustment gives an estimate of the number of people in the US population with a given characteristic in that year; it uses different weights depending on the degree to which a given group was oversampled by design. For example, on the basis of the sampling frequencies, the sample of 7740 live births represents 3 186 497 live births in 1988, and the sample of 1204 postneonatal deaths represents 9145 deaths as a result of causes other than malignancy or congenital anomaly.

RESULTS

As seen in Table 2, after adjustment with SUDAAN, cases and controls differ on all covariables of interest. Mothers of the children who died are younger, are less educated, and smoke more often during pregnancy. The children who died had a higher birth order and were more often male, black, and of low birth weight. There remained an excess of children with congenital anomalies among the cases, although children who died by 28 days or who died of their congenital anomaly or a malignant tumor were excluded. Age at death is shown in Fig 2. Most children who died did so before they had completed 4 months of life.

After adjustment for sampling strategy with SUDAAN, 53% of control infants were ever breastfed, compared with 38% of cases. Logistic regression models showed an OR of 0.79 (95% CI: 0.67–0.93) for ever breastfed (Table 3). Race-specific analyses gave similar estimates for the OR, although the proportion ever breastfed was much lower in black infants. For the low birth weight infants, the OR of ever breastfed was 0.97 (95% CI: 0.64–1.47). The estimates from logistic models changed only slightly by category of cause of death (Table 3). The overall risk estimate changes little even when we include deaths as a result of an underlying congenital anomaly (n = 212) or malignant tumor (n = 10); the OR for overall postneonatal death was 0.74 (95% CI: 0.63–0.87). Among cases only, a proportional hazard model showed that the risk of death at any specific time was marginally lower in the ever breastfed infants (hazard ratio: 0.91; 95% CI: 0.79–1.06).

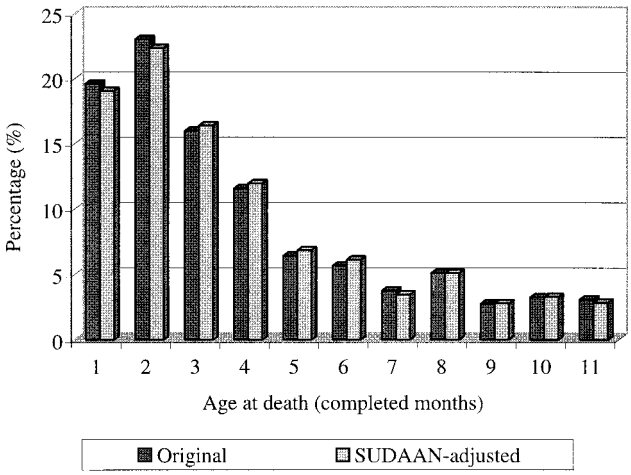

Fig 2. Age at death for postneonatal deaths.

In addition to the covariates adjusted in the models, we examined possible confounding from cesarean section. We found no difference in SUDAAN adjusted proportion of cesarean section between cases (20.2%) and controls (18.3%). Cesarean section did not affect the percentage of ever breastfeeding in either cases or controls and had no effect on the estimate of the strength of the effect of breastfeeding when it was included in the models. For duration of breastfeeding, comparing cases who survived 3 months (n = 691 in original sample and n = 5363 after adjustment with SUDAAN) and all controls, 3 months or more of breastfeeding showed an OR of 0.62 (95% CI: 0.46–0.82), less (ie, more protective) than the OR for ever/never breastfed (0.79; 95% CI: 0.67–0.93).

DISCUSSION

Breastfed children have a decreased risk of postneonatal death in the United States, although infectious diseases, those most plausibly prevented by

TABLE 2. Characteristics of Case (Postneonatal Death) and Control (Live Birth) infants

Variables	Original Samples		SUDAAN-Adjusted	
	Live Birth (n = 7740)	Postneonatal Death (n = 1204)	Live Birth (N = 3186497)	Postneonatal Death (N = 9145)
Mother's age (y; mean ± SEM)*	25.6 ± 0.06	23.9 ± 0.2	26.3 ± 0.02	24.4 ± 0.2
Mother's education (y; mean ± SEM)*	12.3 ± 0.03	11.7 ± 0.1	12.6 ± 0.04	11.8 ± 0.1
Male gender*	50.3	59.8	52.0	59.2
Race*				
White	44.4	50.0	77.2	68.3
Black	52.2	46.6	17.2	27.3
Others	3.4	3.4	5.5	4.4
Live birth order*				
1	40.7	30.8	41.7	32.1
2	30.9	32.3	32.8	32.5
≥3	28.4	36.9	25.5	35.4
Plurality single*	95.2	95.0	98.0	95.4
Birth weight <2500 g*	26.0	23.8	13.0	21.9
Congenital malformation reported at birth*	1.2	2.8	0.8	2.9
Maternal smoking during pregnancy*	22.9	38.6	21.7	39.8
WIC after delivery*	50.2	55.7	36.8	49.2

n indicates sample size; N, US population estimate; SEM, standard error of the mean; WIC, the nutrition program for Women, Infants, and Children.
* $P < .01$ (SUDAAN-adjusted).

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TABLE 3. Percentage of Ever Breastfed and ORs for Postneonatal Deaths

	Original Samples					SUDAAN-Adjusted				
	Live births		Postneonatal Deaths		Adjusted* OR (95% CI)	Live Births		Postneonatal Deaths		Adjusted* OR (95% CI)
	n	% Ever Breastfed	*n*	% Ever Breastfed	Ever/Never Breastfed	*N*	% Ever Breastfed	*N*	% Ever Breastfed	Ever/never Breastfed
Total	7740	39.7	1204	31.2	0.76 (0.65–0.88)	3 186 497	53.4	9145	37.7	0.79 (0.67–0.93)
Race specific										
Black	4038	26.0	561	16.0	0.67 (0.52–0.87)	549 360	27.9	2498	16.8	0.69 (0.53–0.90)
Nonblack	3702	54.7	643	44.3	0.79 (0.65–0.96)	2 637 137	58.7	6647	45.6	0.81 (0.66–0.98)
Birth weight specific										
<2500 g	2015	27.7	287	24.7	0.87 (0.60–1.37)	200 443	36.1	2002	31.4	0.97 (0.64–1.47)
≥2500 g	5725	43.9	917	33.2	0.73 (0.61–0.86)	2 986 054	54.5	7143	39.5	0.76 (0.64–0.91)
Death cause specific										
Infections	-	-	255	30.2	0.75 (0.55–1.02)	-	-	1914	37.0	0.76 (0.54–1.07)
Injuries	-	-	126	28.6	0.67 (0.43–1.05)	-	-	971	31.9	0.59 (0.38–0.94)
SIDS	-	-	591	31.6	0.77 (0.63–0.95)	-	-	4514	38.3	0.84 (0.67–1.05)
Others	-	-	232	32.3	0.76 (0.54–1.07)	-	-	1745	40.4	0.81 (0.56–1.16)

* Adjusted for mother's age, education, and smoking during pregnancy and infant's gender, race (except for race subgroup analyses), birth weight (except for birth weight subgroup analyses), congenital malformation reported at birth, live birth order, plurality, and WIC status.

breastfeeding, no longer contribute substantially to postneonatal mortality. Longer breastfeeding was associated with lower risk of postneonatal death. There is little heterogeneity of this effect among the different causes of death, at least with the coarse groupings that we used. Even among cases only, those who were ever breastfed live marginally longer. This is a very large data set, representative of the US population, albeit in 1988. Because postneonatal mortality in the United States has declined from 3.6/1000 in 1986 to 2.3/1000 in 2000, a prospective study now would need to enroll >60 000 newborns and follow them for 1 year to approach the power and precision of these data. Familiar confounders can be accounted for in the analysis, and the oversampling among black infants and premature infants allows reasonably precise and robust estimates for them specifically. We see a more modest benefit than Forste, who observed a remarkably strong protective OR of 0.2 in a model with only race, birth weight, and breastfeeding. We examined the 1995 National Survey of Family Growth data, which formed half of the basis of the Forste analysis (1988 and 1995). An estimate adjusting for most covariables used in our study gives an OR of ~0.7 for breastfeeding and all-cause postneonatal mortality, comparable to our estimate in this study. Carpenter did not give an OR for his UK study, but it is possible to estimate an OR of 0.4 from the published data. Because Carpenter used a set of causes of death (what he termed "preventable death") that are not readily extractable from *International Classification of Diseases, Ninth Revision* coded death certificates and deaths out to 2 years of age, we cannot compare our estimate directly with his.

Is it plausible that breastfeeding protects not only against infectious disease mortality, through familiar immune enhancing mechanisms, but also against SIDS, accidental death, and others? Although a satisfactory mechanism has not yet been proposed, the protection from SIDS has been seen in several studies and is under investigation. For accidental death, Carpenter also observed lower risk, and although the association may represent something as simple as physical proximity, it deserves additional study, having been seen both in his data and in ours.

To some extent, the policy implications of demonstrating benefits of breastfeeding depend on whether the benefits will be achieved by persuading a mother to breastfeed when she otherwise might not have. Strictly, though, causality is difficult to demonstrate for any specific part of the interaction between the breastfeeding mother and her child. It may be that breastfeeding represents a package of skills, abilities, and emotional attachments that mark families whose infants survive and that it is these factors that produce the benefits seen, rather than breastfeeding or breast milk per se. We cannot randomize breastfeeding, although it is possible to randomize breast milk: Lucas et al[23] conducted an ingenious study in which premature infants who were fed their mother's milk from a bottle did better on follow-up testing than children who were fed formula.

Reverse causality, produced by the motivation or enthusiasm that marks a healthier child who can breastfeed or by specific characteristics of the child's illness, such as cleft palate and breathlessness during sucking, that prevent breastfeeding might produce an artificial benefit of breastfeeding. Eliminating deaths in the first month and deaths from congenital anomaly or malignant tumor, where infants who are unable to breastfeed are concentrated, and using the initial feeding method to categorize feeding should diminish but perhaps not eliminate this problem. However, excluding these deaths also excludes the chance to examine whether breastfeeding has any effects on these deaths, especially those who are not fatally ill at birth. In a prospective study, it might be possible to include neonatal deaths if careful attention were paid to the reason that a child was breastfed or not. We do not have such data; however, we can eliminate from the analysis any child, case or control, who was admitted to the neonatal intensive care unit. This yields a similar but less precisely estimated OR of 0.83 (95% CI: 0.67–1.03).

The NMIHS data are from cases and controls and depend on interviews done after the child had sur-

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vived or not. There thus is opportunity for recall bias, if women report their feeding methods differently depending on whether the child survived. To produce the results that we see would require substantial underreporting of breastfeeding by mothers of children who died, which does not seem likely. For the analysis of duration, the case control data do not allow direct estimation of a duration effect, because the cases and controls have different opportunities to breastfeed for longer periods. When we limit the analysis to cases who survived at least 3 months and look at the effect of 3 months or more of breastfeeding, however, we see an increase in the protective effect, consistent with the idea that longer breastfeeding is more protective.

If more US mothers can be persuaded to breastfeed and indeed it is breastfeeding that accounts for the benefits, then the United States might improve its poor ranking among industrialized countries for postneonatal death. In 1986, 2 years before these data were collected, the United States ranked 16th (3.6/1000) in postneonatal death, well below Finland (first; 1.8/1000) and Sweden (second; 2.0/1000).[24] The US breastfeeding prevalence in 1986 was 57% at birth and 22% at 6 months,[25] whereas in Finland and Sweden, the prevalence at 6 months then was still ~60% and 50%, respectively.[26] Although the United States still trails the Nordic countries both in breastfeeding and in postneonatal mortality, the US rate of postneonatal death has fallen steadily between the late 1980s and now, and breastfeeding has increased. In 2001, 70% of mothers left the hospital breastfeeding, and 33% were still breastfeeding at 6 months.[25] If we assume that the risk structure has not changed as the overall rates have fallen, then the overall postneonatal mortality rate, a weighted average of the rate among those who were breastfed and those who were not, consists of 70% of children who are breastfed when they leave the hospital and who have a rate of 2.1 per 1000, and 30% of children who are not breastfed and have a rate of 2.7. If all children were breastfed, then it should prevent 1.8 postneonatal deaths per 10 000 live births. Because there are ~4 million births per year,[27] 720 postneonatal deaths might be prevented or delayed each year at little cost or risk. The benefit would be concentrated among young, less educated mothers who participate in Women, Infants, and Children and now have a relatively low rate of breastfeeding. The case for breastfeeding is already very strong, but this benefit on such a basic outcome might still increase encouragement of and support for breastfeeding in US children.

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Breastfeeding and the Risk of Postneonatal Death in the United States

Aimin Chen and Walter J. Rogan

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The online version of this article, along with updated information and services, is located on the World Wide Web at:
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