Improved estimates of the benefits of breastfeeding using sibling comparisons to reduce selection bias

Reilly, Siobhan and Evenhouse, Eirik

December 2005

Author information:

Eirik Evenhouse, Ph.D. (Corresponding author)
Department of Economics, Mills College
5000 MacArthur Boulevard
Oakland CA 946113
510-430-2338

Siobhan Reilly, Ph.D.
Department of Economics, Mills College
5000 MacArthur Boulevard
Oakland CA 946113
510-430-2346
sreilly@mills.edu

Manuscript: HSR-04-0155.R3

Improved Estimates of the Benefits of Breastfeeding Using Sibling Comparisons to Reduce Selection Bias

Eirik Evenhouse
Department of Economics
Mills College

Siobhan Reilly
Department of Economics
Mills College

Acknowledgments

For assistance and advice, we thank Dan Dohan, Donna Ginther, Hal Luft, Charles Mullin, Steve Raphael, members of the Writing Seminar at the UCSF Institute for Health Policy Studies, and two anonymous referees. We also thank participants at the 2005 meeting of the Population Association of America, the 2004 Add Health Users Conference, and the 2003 meeting of the Western Economics Association. Evenhouse acknowledges the financial support of the Agency for Healthcare Research and Quality (under grant number 5 T32 HS00086), and Reilly that of the Meg Quigley Women's Studies Fellowship Program. The views presented here are those of the authors, and not necessarily shared by either funding agency or its employees.

This research uses data from Add Health, a program project designed by J. Richard Udry, Peter S. Bearman, and Kathleen Mullan Harris, and funded by a grant P01-HD31921 from the National Institute of Child Health and Human Development, with cooperative funding from 17 other agencies. Special acknowledgment is due Ronald R. Rindfuss and Barbara Entwisle for assistance in the original design. Persons interested in obtaining data files from Add Health should contact Add Health, Carolina Population Center, 123 W. Franklin Street, Chapel Hill, NC 27516-2524 (**addhealth@unc.edu**).

Disclosures

Both authors report no conflict of interest, financial or otherwise.

Disclaimers

The views presented here are those of the authors, and not necessarily shared by either funding agency or its employees.

Improved Estimates of the Benefits of Breastfeeding Using Sibling Comparisons to Reduce Selection Bias

Eirik Evenhouse and Siobhan Reilly

Abstract

Objective: Better measurement of the health and cognitive benefits of breastfeeding by using sibling comparisons to reduce sample selection bias.

Data: We use data on the breastfeeding history, physical and emotional health, academic performance, cognitive ability, and demographic characteristics of 16,903 adolescents from the first (1994) wave of the National Longitudinal Study of Adolescent Health. The sample includes 2,734 sibling pairs.

Study Design: We examine the relationship between breastfeeding history and fifteen indicators of physical health, emotional health, and cognitive ability, using ordinary least squares and logit regression. For each indicator, we estimate, in addition to the usual between-family model, a within-family model to see whether differences in siblings' outcomes are associated with differences in the siblings' breastfeeding histories.

Principal Findings: Nearly all of the correlations found in the between-family model become statistically insignificant in the within-family model. The notable exception is a persistent positive correlation between breastfeeding and cognitive ability. These findings hold whether breastfeeding is measured in terms of duration or as a Yes/No variable.

Conclusions: This study provides persuasive evidence of a causal connection between breastfeeding and intelligence. However, it also suggests that non-experimental studies of breastfeeding overstate some of its other long-term benefits, even if controls are included for race, ethnicity, income, and education.

Despite an enormous literature demonstrating better health and cognitive outcomes among breastfed children, the effects of breastfeeding are uncertain. This is because the vast majority of studies share a common weakness: they are non-experimental. Their Achilles heel is selection bias. If a variable influences both the decision to breastfeed and the child outcome being studied, then omitting it produces a spurious correlation between breastfeeding and the outcome. For example, worse outcomes among children of younger, less educated, lower-income, and African-American mothers may correlate with their lower breastfeeding rates but be owed partly to disadvantages that cannot be captured in the regressions.

In this study, we use sibling comparisons to reduce selection bias. Sibling comparisons are a potentially valuable tool for controlling for unobserved but relevant attributes of children's family and social environments. Differences between two siblings in health or cognitive outcomes that are correlated with differences in their breastfeeding histories are not attributable to any unobserved maternal or household characteristics that affect both children symmetrically.

There are very few sibling analyses of infant feeding. A PubMed search on October 14, 2004 yielded none. We are aware of only two sibling analyses of breastfeeding, both focused on obesity. Using Add Health, Nelson *et al.* (2003) look at breastfeeding and adolescent obesity. Anderson *et al.* (2003), using the National Longitudinal Survey of Youth (NLSY), look at the impact of maternal employment on child obesity, but control for breastfeeding as a factor that can differ between siblings and might influence body weight. In both studies, the correlation between breastfeeding and obesity is negative in the conventional model but insignificant in the sibling model.

We examine a large number of outcomes in addition to obesity. Given our concern with selection bias, we focus on the difference between an estimate derived from the conventional model and the corresponding estimate derived from a sibling model.

Because that difference may vary by outcome, we consider multiple outcomes in order to reach more robust conclusions.

Overview of breastfeeding literature

The overwhelming majority of studies in the infant feeding literature conclude that breastmilk is superior to infant formula in nearly all situations other than cases of maternal drug addiction, maternal HIV infection, and infant metabolic disorders (Lawrence and Lawrence 1998). Studies of infants, young children, adolescents, and adults find adverse outcomes associated with not having been breastfed. This consensus notwithstanding, mass-marketed infant formula has been used widely ever since its introduction in the 1920s (Baumslag and Michels 1995). One-third of American mothers do not breastfeed their newborns. Three-quarters introduce formula before their babies reach six months (Ryan *et al.* 2002), and among low-income mothers, over five-sixths do (Milligan *et al.* 2000).

Compared to breastfed infants, infants who are not breastfed experience 2 to 5 times as many ear infections (Beaudry *et al*. 1995, Dewey *et al.* 1995), 1.5 times as many respiratory illnesses (Beaudry *et al.* 1995), 1.7 to 1.9 times as many gastrointestinal infections (Beaudry *et al.* 1995, PROBIT Study Group 2001, Scariati *et al.* 1997), 1.3 to 1.9 times as many allergy-related problems (van Odijk *et al.* 2003, Kull *et al.*2002, PROBIT Study Group 2001, Oddy *et al.* 1999), twice as many hospitalizations (Chen *et al.* 1988), 3 to 5 times the rate of sudden infant death syndrome (Alm *et al.* 2002, McVea *et al.* 2000, Mitchell *et al.* 1991), and a 25 percent higher mortality rate between the ages of 1 month and 12 months (Chen and Rogan 2004).

The impact of infant feeding choices appears to extend beyond infancy. Children who were not breastfed are 1.3 times as likely as children who were to get childhood cancers (British

Child Cancer Study Investigators 2001, Shu *et al.* 1999, Davis *et al.* 1988), and 2 to 4 times as likely to develop juvenile-onset diabetes (Young *et al.* 2002, Pettitt *et al.* 1997). As young children and as adolescents, they are 1.2 to 1.6 times as likely to be overweight (Armstrong *et al.*2002, Gilman *et al.* 2001, Hediger *et al.* 2001). As adults, they have higher cholesterol levels, corresponding to an 11 percent increase in their risk of heart disease (Owen *et al.* 2002). Premature infants who are not breastfed register an additional 4 mm/Hg of blood pressure as adolescents (Singhal *et al.* 2001) (a 2-mm/Hg increase significantly raises the risk of heart attack and stroke). Breastfeeding's protective effect against meningitis appears to last into adolescence (Silfverdal *et al.* 1997, 1999).

Young children, adolescents, and adults who were breastfed score higher on IQ tests, with the gain varying with a child's weight and maturity at birth. The observed gain is 3.2 points for full-term babies over 6 pounds (Anderson *et al.* 1999, Rao *et al.* 2002), 5 to 6 points for premature infants (Anderson *et al.* 1999, Horwood *et al.* 2001), and 11 points for full-term but underweight babies (Rao *et al.* 2002). Studies using other measures of cognition reach similar conclusions (Quinn *et al.* 2001, for example).

Infant feeding choices may have implications for maternal health, too. For example, mothers who breastfeed have lower odds of developing breast cancer (Zheng *et al.* 2001, Zheng *et al.* 2000, Heinig and Dewey 1997). A recent review of 47 studies from 30 countries suggests that the relative risk of breast cancer declines 4.3 percentage points for every 12 months of breastfeeding, and that the incidence of breast cancer in developed countries would fall by a third if mothers breastfed as long as mothers in developing countries do (Collaborative Group on Hormonal Factors in Breast Cancer, 2002).

There is also evidence of a "dose response" for some outcomes, that is, the more breastmilk a child consumes, the larger the associated positive effects. For example, premature infants given both formula and breastmilk are only half as likely to develop necrotizing enterocolitis as those given only formula, but twice as likely as those given only breastmilk (Lucas *et al.* 1990). Other studies find positive duration effects on cognition (e.g., Rao *et al.* 2000, Mortensen *et al.* 2002, Quinn *et al.* 2001) and on the incidence of infant respiratory infections (e.g., Silfverdal *et al.* 1997), of asthma (e.g., Dell and To 2001), of infant wheeze (e.g., Oddy *et al*. 2003), of childhood cancers (.e.g, Davis *et al.* 1998), and of maternal breast cancer (Zheng *et al.* 2001, Zheng *et al.* 2000).

Data

Using data from the National Longitudinal Study of Adolescent Health (Add Health), we examine the link between breastfeeding and 15 indicators of adolescent well-being that pertain to physical and emotional health, academic performance, and the quality of the mother-child relationship. We choose these indicators for their similarity to outcomes examined in other breastfeeding studies. The fifteen indicators are: (1) body mass index (BMI), converted into percentiles using age-and sex-specific growth charts published by the Centers for Disease Control and Prevention (U.S. National Center for Health Statistics 2000); (2) overweight or at risk of overweight (BMI above the 85^{th} percentile); (3) overweight (BMI above the 95^{th} percentile); (4) whether the child has diabetes; (5) whether the child has asthma; (6) whether the child has allergies; (7) grade point average (GPA) in four subjects (math, science, social studies, and language arts); (8) percentile score on Add Health's abbreviated version of the Peabody Picture Vocabulary Test (PVT), normed for age and sex; (9) whether the child ever has ever repeated a grade; (10) whether the child reports being "highly likely" to go to college; (11) a 19-item index of depression (adapted from the widely used 20-item CES-D scale), normed for age and sex; (12) mother's report of closeness to the child; (13) child's report of closeness to the mother; (14) how strongly the child agrees that the mother is usually warm and loving; and (15) the range of activities in which child and mother participate together each month.

Begun in 1994 and designed to be nationally representative, Add Health's first wave has detailed data on 20,000 adolescents from 80 school districts. Information about the adolescents comes from four sources: the adolescents themselves, their parents, their network of school friends, and school administrators.

Although Add Health was designed for studying adolescents' health-related behaviors, it is well suited to the purposes of this study. First, it allows more sibling comparisons than other large U.S. surveys. Not only are there more siblings (2,734 pairs), but the siblings can also be compared along more dimensions. Add Health respondents are all adolescents, and the same information is gathered about every child. In the National Longitudinal Survey of Youth, the only other U.S. survey with a comparable number of sibling pairs, the information gathered about a child varies with the child's age, limiting the points of comparison between siblings.

Second, Add Health contains a broad range of information, with data on children's physical and mental health, cognitive abilities, and academic achievement. It reports, for example, whether a child suffers from allergies, asthma, diabetes, or obesity, conditions that have been associated with formula-feeding. It also reports whether a child's biological father or mother suffers from those conditions, helping to separate genetic factors from the effects of infant feeding choices.

Third, Add Health oversamples low-income, African-American, and Hispanic children. These subgroups are important because of their heavier reliance on formula, and their increased exposure to the federal government's Supplemental Nutrition Program for Women, Infants, and Children (WIC). WIC buys about half of all infant formula sold in the United States (GAO 2003). The provision of free or subsidized formula may thwart the program aim of encouraging breastfeeding (e.g., Oliveira et al. 2002, Raisler 2000, Rossi 1998, and Schwartz et al. 1995). These subgroups are also important because of their higher rates of asthma, diabetes, obesity, and academic failure.

Fourth, with a sample consisting entirely of adolescents, Add Health permits us to focus on the long-term benefits of breastfeeding, which are much less studied than the benefits to infants and very young children.

Finally, Add Health offers many important control variables. Anderson *et al.*'s (1999) meta-analysis identifies 15 controls important for studying the link between infant feeding and cognitive development, and identifies only 11 published studies that include five or more; Add Health contains 12 of the 15.[1] In addition, Add Health contains potential controls for parental investment (e.g., the number of activities shared by parent and child, the child's extracurricular activities, the quality of the child's school, how often the parent is home when the child goes to bed, the child's bedtime, the fraction of evening meals that are eaten together, the degree of parental involvement in the child's schoolwork and with the child's school, and the hours the child spends watching television or playing video games).

Because Add Health does not ask about infant formula consumption, we cannot distinguish exclusive breastfeeding from breastfeeding supplemented by formula or solid food. Add Health reports only whether a child was breastfed, and for how long. As in many retrospective surveys of breastfeeding, duration is reported as a bracketed variable (0-3 months, 3-6 months, 6-9 months, 9-12 months, 12-24 months, and over 24 months). Table 1 reports the distribution of duration among Add Health children. The figures are comparable to other estimates of U.S. breastfeeding rates in the late 1970s and early 1980s, the era in which Add Health children were born (U.S. National Center for Health Statistics 2004). In the full sample, 81.7 percent of children have a known breastfeeding history; the remaining 18.3 percent consists almost entirely of cases in which the surveyed adult is not the child's mother. Of children whose breastfeeding history is known, 43.9 percent were breastfed, for an average of 5.4 months. The proportions are similar in the sibling subsample.

A sibling study is only feasible if there is sufficient within-family variation in breastfeeding history. A closer look at the duration data suggest that there is. Table 2 presents the full distribution, for the sibling sample, of the between-sibling differences in breastfeeding duration. In 79.1 percent of cases, the two siblings have identical breastfeeding histories. Thus, the identification of breastfeeding effects hinges on the remaining 20.9 percent. Focusing on those 523 pairs, we see that the average duration difference between the siblings (6.1 months) is slightly larger than the average duration of breastfeeding for all breastfed children in Add Health (5.4 months).[2] In 288 cases, one sibling was not breastfed at all; in those cases, the other sibling was breastfed for an average of 5.8 months. In the other 235 cases, both siblings were breastfed but for different durations, with an average duration difference of 6.5 months. These 523 cases divide almost equally into cases in which the elder child was breastfed longer and cases in which the younger sibling was breastfed longer.

Estimation method

We estimate two reduced-form models of child well-being. The first contains no family fixed effect; it is a between-family model typical of the existing literature:

(1) $$\boldsymbol{W}_i = \beta_0 + \beta_1 \boldsymbol{B}_i + \beta_2 \boldsymbol{H}_i + \beta_3 \boldsymbol{C}_i + \beta_4 \boldsymbol{E}_i + \epsilon_i$$

where *i* indexes the child, ***W*** is a measure of child well-being, ***B*** is a measure of consumption of breastmilk, ***H*** and ***C*** are vectors of characteristics of the household and the child, ***E*** is a vector of environmental characteristics (such as neighborhood crime rates), and ϵ_i is the error term.

Estimating this model for each child outcome provides a benchmark for the size and significance of the effect of breastfeeding, β_1, in the absence of a family fixed effect.

The error term is assumed to consist of a household-specific error, ω_i, a child-specific error, γ_i, an environment-specific error, η_i, and a random error, ν_i:

(2) $$\epsilon_{ih} = \omega_i + \gamma_i + \eta_i + \nu_i$$

In estimating equation (1), selection bias can arise if any of the first three components of the error term is correlated with infant feeding choice.

To reduce selection bias (negative as well as positive), we then estimate a family fixed effect (or within-family) model. First-differencing between two siblings eliminates any bias due to time-invariant family or environmental characteristics that affect both siblings equally. This second model is given by:

(3) $$\Delta_{ij} \boldsymbol{W}_h = \beta_1 \Delta_{ij} \boldsymbol{B}_{ijh} + \beta_2 \Delta_{ij} \boldsymbol{H}_{ijh} + \beta_3 \Delta_{ij} \boldsymbol{C}_{ijh} + \beta_4 \Delta_{ij} \boldsymbol{E}_{ijh} + \Delta_{ij} \epsilon_{ijh}$$

where h indexes the household, the subscript ij denotes a comparison between siblings i and j, and $\Delta_{ij} \boldsymbol{W}_h$ is the difference between two siblings in an indicator of well-being. The coefficient of particular interest, β_1, is on the difference in breastfeeding history. In theory, comparing this coefficient in the between-family model (Equation 1) to that in the within-family model (Equation 3) gives an idea of the direction and magnitude of selection bias present in the former.

Many of the observed determinants of the initiation and duration of breastfeeding are factors that can vary between siblings. We control for birth weight, for example, because children born prematurely have poorer outcomes on average and lower odds of having been breastfed. Similarly, we control for birth order and gender, in case either characteristic is correlated with adolescent well-being as well as with infant feeding decisions. Because we compute sibling differences by subtracting values for the younger child from those of the older child, the birth order effect is represented by the regression constant.

As a control for parental investment of time or money in a child, we also include the number of the child's extracurricular activities. This is to help distinguish the effects of infant feeding mode from the effects of a more general pattern of unequal investment in two siblings. Breastfeeding is sometimes viewed as signaling a family's willingness to invest time, money, and effort in a child (see Michael 2002, for example). If breastfeeding is one of the many ways in which a family might systematically invest more in one child than another, the effects of breastfeeding may otherwise be confounded with the positive effects of being a favored recipient of parental investment.

We present two sets of estimates, based on two different measures of breastfeeding history. The first estimates use the duration of breastfeeding, measured in months. For parsimony, we convert Add Health's categorical duration variable into a quasi-continuous measure, treating the midpoint of each interval as the duration in months. (We experimented with several values for the open-ended "Over 24 months" category, and the results were not sensitive to the chosen value.) The second set of estimates use a Yes/No measure ("Was the child ever breastfed?").

We use two measures of breastfeeding because each has advantages. The Yes/No measure minimizes recall error, because whether a child was breastfed at all is easier to remember than the precise duration of breastfeeding. However, the Yes/No measure may create a worse measurement problem than it solves.[3] If the benefits of breastfeeding are duration-dependent, then the Yes/No measure introduces another type of error by equating, say, two days of breastfeeding with two years' worth.

Besides controlling for duration differences, there are two more reasons for using the duration measure. One is that it makes maximum use of the information in our data. To ignore duration differences between two breastfed siblings would be to ignore fully half of the within-family variation in our data (all cells in Table 2 not in the first column or row), variation that is vital for identifying statistically significant effects if duration effects are important. Second, in a sibling study, the potential vulnerability of duration measures to recall bias is less problematic than in a conventional non-experimental study. Even if duration were recalled with bias, it need not follow that our within-family estimates must be biased. Sibling differencing eliminates recall bias from the estimates if the bias is a characteristic of the mother rather than of her child, that is, if recall error can be captured by a mother fixed-effect.

Results

As a precursor to regression analysis, we confirm that the relationships between breastfeeding and child outcomes in our data resemble those observed in other data. For each outcome measure, Table 3 reports the average difference between children who were breastfed and children who were not, with the difference broken out by duration. (Note that each number is not an estimated difference, but merely the difference between the unadjusted averages of two groups.) The patterns in Table 3 are largely consistent with the existing literature. In the full sample, the difference between breastfed children and others is significant for 12 of the 15 outcomes, and for seven outcomes it is significant at every duration. The breastfed children appear to be brighter and lighter, for example, scoring 4.9 percentiles higher on the Add Health PVT and having a BMI that is 0.77 lower. The sibling subsample shows similar patterns, suggesting that, for the purposes of this study, it is representative of the full sample.

Table 3 also reveals an unexpected relationship between duration and outcomes, one that underlines the value of sibling comparisons. For the majority of the indicators, the mean difference between breastfed children and other children increases with duration through the 9-12 month category, consistent with the belief that longer breastfeeding improves child outcomes. However, for 11 of the 15 indicators, the mean difference between breastfed children and others drops as duration increases beyond a year, as if it were harmful to be breastfed longer than a year. This conflicts with the generally held prior that breastfeeding is rarely harmful. One could imagine a causal factor to explain harm from prolonging breastfeeding beyond 12 months, such as increased exposure to environmental toxins in breastmilk. Indeed, a recent study of breastmilk contaminants in the Northwestern United States found, in every sample, levels of polybrominated diphenyl ethers (PBDEs) at levels approaching those associated with learning, memory, and behavior problems in mice (Northwest Environment Watch 2004). However, sample selection is a simpler and more plausible explanation, and is consistent with the pattern of demographic characteristics shown in Table 4.

In Table 4, we list the control variables and their means for non-breastfed children, for breastfed children, and for each duration subgroup. We see, for instance, that breastfeeding rates rise with income and education, and are lowest among African-American mothers and highest among white mothers. We also see that low-birthweight babies are only about half as likely as other babies to be breastfed. These are the familiar patterns behind the generally acknowledged possibility of positive selection bias, that is, of bias that leads to overestimates of the benefits of being breastfed. However, Table 4 also raises the rarely discussed possibility of negative selection bias in estimates of the duration effects of breastfeeding. In the table we see a shift in demographic composition for the longest durations. Compare, for example, mothers who breastfed longer than 12 months to those who breastfed 9-12 months. They have lower incomes, are less educated, are less likely to be white, and are more likely to be Hispanic, all factors correlated with worse child outcomes. Unless socioeconomic and demographic controls capture fully the disadvantages faced by these households, conventional estimates of breastfeeding duration effects are likely to be biased downward.

The results of regressions using "Months breastfed" as the infant feeding measure are summarized in Table 5. For each outcome, the table reports only the coefficient on "Months breastfed." (More detailed regression results are available from the authors upon request.) Controlling for family and child characteristics, we first estimate the between-family model (Equation 1) and then the sibling-difference, or within-family, model (Equation 3). The estimates in the first two columns are from the between-family model, for the full and sibling samples. The estimates in the third column are from the within-family model. For comparability, all estimates are unweighted. (The within-family estimates are unweighted by necessity, as Add Health has not yet released weights for the sibling pairs.) However, the similarity between weighted and unweighted between-family estimates (the former not reported here) suggests that the lack of weights is not a serious concern.

With the addition of controls to the between-family model, we find that breastfeeding is significantly correlated with ten outcomes in the full sample (Table 5, first column) and with nine in the sibling subsample (Table 5, second column). However, after taking sibling differences and estimating the within-family model (Table 5, last column), PVT score is the only outcome that remains significantly correlated with the duration of breastfeeding. The within-family estimate of the effect of breastfeeding on PVT score (0.16 percentiles per month of breastfeeding) is about three-quarters as large as the between-family estimate (0.21 percentiles per month).

Measuring breastfeeding simply as "Yes/No" rather than in months yields mostly similar results. As Table 6 shows, in the between-family model, having been breastfed is significantly correlated with nine outcomes in the full sample (first column), and six in the sibling subsample (middle column). However, after we take sibling differences and estimate the within-family model (last column), it remains significantly correlated with only two outcomes. One is PVT score; as before, the within-family effect is about three-quarters as large as the between-family effect (1.68 versus 2.41 percentiles). The other is "overweight or at risk of overweight." Unexpectedly, the between-family and within-family estimates have opposite signs, with the latter implying that the breastfed sibling is more likely to be overweight. This anomaly merits further investigation. The other two sibling studies of obesity and breastfeeding (Anderson *et al.* 2003, Nelson *et al.* 2003) report no, rather than a reversed, correlation in their within-family models.

Discussion

This study uses sibling comparisons to reduce the selection bias that bedevils most efforts to measure the benefits of breastfeeding. While an enormous literature associates breastfeeding with better health and cognitive outcomes, most of the studies are non-experimental and therefore vulnerable to sample selection bias. In this study, we examine fifteen adolescent outcomes, using data from the National Longitudinal Survey of Adolescent Health (Add Health). After estimating the effects of breastfeeding in a typical between-family model, we estimate a within-family model to see whether differences in outcomes between two adolescent siblings are correlated with differences in their breastfeeding histories. We find that, for all but one measure, the correlations that are statistically significant in the between-family model become insignificantly different from zero in within-family model. The notable exception is the persistent positive correlation between breastfeeding and our measure of cognitive ability (PVT score).

The significant correlation between breastfeeding and PVT score in our within-family model provides more credible evidence of a causal link between breastfeeding and cognitive ability than do existing non-experimental studies. The effect is large enough to matter, and it is lasting, persisting into adolescence. Stronger evidence of causality may argue for intensifying breastfeeding promotion, particularly among groups that suffer from high rates of academic failure and other problems that some researchers have correlated with lower IQ (e.g., incarceration, poverty, or welfare recipiency). Some of the same social problems that justify additional expenditures on education and Head Start, for example, may also warrant additional efforts to raise breastfeeding rates.

Our results also suggest, however, that many of the other long-term effects of breastfeeding have been overstated. The implication for breastfeeding researchers is that selection bias remains a serious problem even with controls for household income, family size, parental education, race, ethnicity, and other sociodemographic characteristics of the family. A productive direction for breastfeeding research lies in seeking data and methods to attack the selection problem. An implication for researchers interested in child outcomes unrelated to breastfeeding is that a child's breastfeeding history may nevertheless be a good proxy for unobservable family characteristics that are correlated with child outcomes.

The applicability of our results should not be overstated. They must not be extrapolated to infants or to poor countries, as we examine only a specific set of long-term effects in a sample of American adolescents.

Some caveats about the validity of our estimates are also in order. One is that sample size limits the robustness of any individual estimate. In the case of a relatively rare outcome like diabetes, the sample is too small (only 78 cases in the full sample, and 19 in the sibling subsample) to permit meaningful conclusions. More generally, our effective sample size depends on the number of cases in which two siblings have different breastfeeding histories. The smaller the true effects of breastfeeding, the more cases needed to identify them. Thus, our sample may be too small to let us distinguish between small effects and zero. The consistency of our results across the different outcomes, however, suggests that the sample is large enough to let us conclude that the within-family estimates are significantly different from the between-family estimates.

A second caveat is that families may try to equalize outcomes across siblings, by allocating family resources in ways that compensate for, rather than reinforce, each child's perceived deficits. Such compensating parental investments might blunt any inter-sibling differences owed to differences in breastfeeding history, making it harder to detect the benefits of breastfeeding. For example, if parents get extra tutoring for the less able sibling, and that sibling is less able because he was weaned earlier, the benefit of the tutoring could mask the effect of early weaning. However, there is no consensus that American families commonly allocate resources in this way. In the area of education, for example, Griliches (1979) and Behrman et al. (1982) find evidence of compensatory behavior, while Behrman et al. (1994) find evidence of reinforcing behavior.

A third caveat is that sibling differencing amplifies any errors-in-variables bias (Griliches 1979, Card 1999). Mismeasuring a variable biases estimates downward, and the bias is greater in within-family estimates than in between-family estimates. If measurement errors are large enough, errors-in-variables bias could completely mask the true relationships between breastfeeding and adolescent outcomes. The smaller the true effects of breastfeeding, the stronger this possibility. We believe, however, that our errors-in-variables bias is relatively small. Our findings are similar whether breastfeeding is measured as Yes/No or in terms of duration. (As mentioned earlier, duration is subject to rounding error as well as greater recall error.) That this is true for multiple outcome measures further suggests that measurement errors do not fully account for the differences between our between-family and within-family estimates.

A final caveat is that sibling comparisons are not a panacea for selection bias. They cannot eliminate bias due to selection into the study sample, or bias due to unobserved factors that lead a mother to feed two infants differently and that also drive children's later outcomes. In a school-based sample like Add Health, for example, children who have dropped out of school or been institutionalized are underrepresented, and those who die in infancy are missing altogether. To the extent that these outcomes are associated with not having been breastfed, attrition bias leads to an understatement of the long-term benefits of breastfeeding that sibling differencing cannot correct. Likewise, omitting child-specific characteristics that drive both breastfeeding and later outcomes can lead to bias, despite differencing. For example, if low gestational age makes it difficult to breastfeed and also independently impairs later cognitive ability, failing to control for gestational age would lead to an overstatement of the cognitive benefits of breastfeeding. It is important to remember that bias from omitting child-specific characteristics is a problem that dogs virtually all breastfeeding studies, and is in no way a by-product of sibling differencing.

Caveats notwithstanding, this study provides the strongest non-experimental evidence to date that having been breastfed improves cognitive ability. Furthermore, our results suggest that non-experimental studies overstate some of the other long-term effects of being breastfed. Finally, given the obstacles to experimental studies, the problem of selection bias in breastfeeding studies calls for sibling studies with larger samples and for better data on infant feeding and its determinants.

NOTES

[1] The 15 variables are: "duration of breastfeeding, gender, maternal smoking history, maternal age, maternal intelligence, maternal education, maternal training, paternal education, race or ethnicity, socioeconomic status, family size, birth order, birth weight, gestational age, and childhood experiences." Add Health lacks measures of maternal intelligence, the child's gestational age, and whether the mother smoked during pregnancy. (In sibling comparisons, a measure of maternal intelligence matters little, as two siblings have the same mother.)

[2] The accuracy of these averages is limited by the fact that Add Health records duration as a categorical variable. To compute duration differences, we used the midpoint of each duration interval, and 30 months as the average for the "Over 24 months" category.

[3] Recall error in breastfeeding data has been little studied. We have found only one study on the topic, a study of 1,000 Brazilian babies born in 1982 (Huttly *et al.* 1990). That study suggests that mothers recalled duration with significant error, with mothers of higher socioeconomic status (SES) tending to overstate duration. However, the higher SES mothers also tended to breastfeed for fewer months, making it unclear whether the main characteristic associated with recall bias was high SES or short duration.

References

Alm, B., G. Wennergren, S. G. Norvenius, R. Skjaerven, H. Lagercrantz, K. Helweg-Larsen and L. M. Irgens. 2002. Breast feeding and sudden infant death syndrome in Scandinavia, 1992-95. *Archives of Disease in Childhood* 86(6):400-2

Anderson, James W., Bryan M. Johnstone and Daniel T. Remley. 1999. Breastfeeding and cognitive development: A meta-analysis. *The American Journal of Clinical Nutrition* 70:525-35

Anderson, Patricia M., Kristin F. Butcher, and Phillip B. Levine. 2003. Maternal employment and overweight children. *Journal of Health Economic*s 22:3 (May):477–504

American Academy of Pediatrics. 1997. Breastfeeding and the use of human milk. *Pediatrics* 100:1035-9

Armstrong, J., J. J. Reilly and Child Health Information Team. 2002. Breastfeeding and lowering the risk of childhood obesity. *Lancet* 359 (9322):1249-50

Baumslag, Naomi, and Dia L. Michels.1995. *Milk, Money & Madness: The Culture and Politics of Breastfeeding*. Bergin & Garvey.

Beaudry, M., R. Dufour and S. Marcoux.. 1995. Relationship between infant feeding and infections during the first six months of life. *Journal of Pediatrics* 126:191-97

Behrman, Jere R., Robert Pollack, and Paul Taubman. 1982. Parental preferences and the provision of progeny. *Journal of Political Economy* 90: 52-73.

Behrman, Jere R., Mark R. Rosenzweig, and Paul Taubman. 1994. Endowments and the allocation of schooling in the family and the marriage market: The twins experiment. *Journal of Political Economy* 102: 1131-1174.

British Childhood Cancer Study Investigators. 2001. Breastfeeding and childhood cancer. *British Journal of Cancer* 85(11):1685-94

Card, David. 1999. The causal effect of education on earnings. In Orley Ashenfelter and David Card, eds., *Handbook of Labor Economics*, Volume 3A, North-Holland

Centers for Disease Control and Prevention, National Center for Health Statistics. 2000. CDC growth charts: United States. http://www.cdc.gov/growthcharts/ (May 30).

Chen, A., and W. Rogan. 2004. Breastfeeding and the risk of postneonatal death in the United States. *Pediatrics* 113(5) (May):e435-e439

Chen, Y., S. Z. Yu, W. X. Li. 1988. Artificial feeding and hospitalization in the first 18 months of life. *Pediatrics* 81(1):58-62

Davis, M. K. 1998. Review of the evidence for an association between infant feeding and childhood cancer. *International Journal of Cancer Supplement* 11:29-33

Davis, M. K., D. A. Savitz and B. I. Graubard. 1988. Infant feeding and childhood cancer. *Lancet* 2 (8607):365-8

Dell, S. and T. To. 2001. Breastfeeding and asthma in young children: findings from a population-based study. Archives of Pediatric and Adolescent Medicine 155 :11 (November): 1261-5.

Dewey, K.G., M. J. Heinig and L. A. Nommsen-Rivers. 1995. Differences in morbidity between breast-fed and formula-fed infants. *Journal of Pediatrics* 126:696-702

Gilman, M., S. L. Rifas-Shiman, C. A. Camargo, Jr., C. S. Berkey, A. L. Frazier, H. R. Rockett, A. E. Field and G. A. Colditz. 2001. Risk of overweight among adolescents who were breastfed as infants. *Journal of the American Medical Association* 285(19):2461-7

Griliches, Zvi. 1979. Sibling models and data in economics: Beginnings of a survey. *The Journal of Political Economy* 87:5 (Part 2: Education and Income Distribution):S37-S64

Hediger, Mary, M. D. Overpeck, R. J. Kuczmarski and W. J. Ruan. 2001. Association between infant breastfeeding and overweight in young children. *Journal of the American Medical Association* 284:2453-60

Heinig, M. J., and K. G. Dewey. 1997. Health effects of breastfeeding for mothers: A critical review. *Nutrition Research Reviews* 10:35-56

Horwood, L. J., B. A. Darlow and N. Mogridge. 2001. Breast milk feeding and cognitive ability at 7-8 years. *Archives of Disease in Childhood, Fetal Neonatal Edition* 84(1):F23-7

Huttly, S. R., F. C. Barros, C. G. Victora, J. U. Beria, and J. P. Vaughan. 1990. Do mothers overestimate breast feeding duration? An example of recall bias from a study in southern Brazil. *American Journal of Epidemiology* 132(3):572-5

Kull, I., M. Wickman, G. Lilja, S. L. Nordvall and G. Pershagen. 2002. Breast feeding and allergic diseases in infants-a prospective birth cohort study. *Archives of Disease in Childhood* 87(6):478-81

Lawrence, R. A. and R. M. Lawrence. 1998. *Breastfeeding: A Guide For the Medical Profession*, 5th ed. Mosby-Year Book, Inc.

Lucas, A., and T. J. Cole. 1990. Breast milk and neonatal necrotising enterocolitis. *Lancet* 336(8730):1519-23

McVea, K. L., P. D. Turner and D. K. Peppler. 2000. The role of breastfeeding in sudden infant death syndrome. *Journal of Human Lactation* 16(1):13-20

Michael, Robert T. 2002. Family influences on a child's verbal ability. Unpublished paper, University of Chicago (September)

Mitchell, E. A., R. Scragg, A. W. Stewart, D. M. Becroft, B. J. Taylor, R. P. Ford, I. B. Hassall, D. M. Barry, E. M. Allen and A. P. Roberts. 1991. Results from the first year of the New Zealand cot death study. *New Zealand Medical Journal* 104:71-6

Mortensen, E. L., K. F. Michaelsen, S. A. Sanders and J. M. Reinisch. 2002. The association between duration of breastfeeding and adult intelligence. *Journal of the American Medical Association* 287(18):2365-71

Nelson, M., P. Gordon-Larsen, and L. Adair. 2003. The protective effect of breastfeeding on adolescent overweight disappears with use of sibling controls. Presentation at the 2003 Add Health Users Workshop, Bethesda, MD

Norris, J. M. and F. W. Scott. 1996. A meta-analysis of infant diet and insulin-dependent diabetes mellitus: do biases play a role? *Epidemiology* 7(1): 87-92

Northwest Environment Watch. 2004. Flame retardants in the bodies of Pacific Northwest residents: A study on toxic body burdens. (September 29). Seattle, WA

Oddy, W. H, M. Halonen, F. D. Martinez, I. C. Lohman, D. A. Stern, M. Kurzius-Spencer, S. Guerra, and A. L. Wright. 2003. *Journal of Allergy Clinical Immunology* 112(4):723-8

Oliveira, V., E. Racine, J. Olmstead, and L.M. Ghelfi. 2002. The WIC Program: Background, Trends, and Issues. Washington, DC: Food Assistance and Nutrition Research Report #5.

Owen, C. G., P. H. Whincup, K. Odoki, J. A. Gilg and D. G. Cook. 2002. Infant feeding and blood cholesterol: a study in adolescents and a systematic review. *Pediatrics* 110(3): 597-

608

PROBIT Study Group (Kramer, Michael S., B. Chalmers, E. D. Hodnett, Z. Sevkovskaya, I. Dzikovich, S. Shapiro, J. P. Collet, I. Vanilovich, I. Mezen, T. Ducruet, G. Shishko, V. Zubovich, D. Mknuik, E. Gluchanina, V. Dombrovskiy, A. Ustinovitch, T. Kot, N. Bogdanovich, L. Ovchinikova, E. Helsing). Promotion of breastfeeding intervention trial (PROBIT): A randomized trial in the Republic of Belarus. 2001. *Journal of the American Medical Association* 285:413-20

Raisler, J. 2000. Against the odds: Breastfeeding experiences of low income mothers. *Journal of Midwifery and Women's Health* 45(3):253-63

Rao, M., M. L. Hediger, R. J. Levine, A. B. Naficy and T. Vik. 2002. Effect of breastfeeding on cognitive development of infants born small for gestational age. *Acta Paediatrica* 91(3):267-74

Rossi, P. H. 1998. Feeding the poor: Assessing federal food aid. Washington, DC: The American Enterprise Institute

Scariati, P. D., L. M. Grummer-Strawn and S. B. Fein. 1997. A longitudinal analysis of infant morbidity and the extent of breastfeeding in the United States. *Pediatrics* 99(6):E5

Schwartz, J. B., B. M. Popkin, J. Tognetti, and N. Zohoori. 1995. Does WIC participation improve breast-feeding practices? *American Journal of Public Health* 85:729-731.

Shu, X. O., M. S. Linet, M. Steinbuch, W. Q. Wen, J. D. Buckley, J. P. Neglia, J. D. Potter, G. H. Reaman and L. L. Robison. 1999. Breast-feeding and risk of childhood acute leukemia. *Journal of the National Cancer Institute* 91(20):1765-72

Silfverdal, S.A. , L. Bodin, and P. Olcen. 1999. Protective effect of breastfeeding: an ecologic study of *Haemophilus influenzae* meningitis and breastfeeding in a Swedish population. *International Journal of Epidemiology* 28:152-6

Silfverdal, S. A., L. Bodin, S. Hugosson, Ö. Garpenholt, B. Werner, E. Esbjörner, B. Lindquist, and P. Olcén. 1997. Protective effect of breastfeeding on invasive *Haemophilus influenzae* infection: A case-control study in Swedish preschool children**.** *International Journal of Epidemiology* 26(2):443-50

Singhal, A., T. J. Cole and A. Lucas. 2001. Early nutrition in preterm infants and later blood pressure: two cohorts after randomised trials. *Lancet* 357(9254):413-9.

Quinn, P.J., M. O'Callaghan, G. M. Williams, J. M. Najman, M. J. Andersen and W. Bor. 2001. The effect of breastfeeding on child development at 5 years: a cohort study. *Journal of Pediatric and Child Health* 37(5):465-9

United States General Accounting Office. 2003. *Food Assistance: Potential to Serve More WIC Infants by Reducing Formula Cost.* GA-03-331.

U.S. National Center for Health Statistics. Health, United States, 2003: Chartbook on Trends in the Health of Americans. http://www.cdc.gov/nchs/data/hus/tables/2003/03hus018.pdf (accessed May 30, 2004).

van Odijk, J., I. Kull, M. P. Borres, P. Brandtzaeg, U. Edberg, L. Å. Hanson, A. Høst, M. Kuitunen, S. F. Olsen, S. Skerfving, J. Sundell, and S. Wille. 2003. Breastfeeding and allergic disease: a multidisciplinary review of the literature (1966-2001) on the mode of early feeding in infancy and its impact on later atopic manifestations. *Allergy* 58(9) (September):833-843.

Zheng, T., L. Duan, Y. Liu, B. Zhang, Y. Wang, Y. Chen, Y. Zhang and P. H. Owens. 2000. Lactation reduces breast cancer risk in Shandong Province, China. *American Journal of Epidemiology* 152:1129-1135.

Zheng, T., T. R. Holford, S. T. Mayne, P. H. Owens, Y. Zhang, B. Zhang, P. Boyle and S. H. Zahm. 2001. Lactation and breast cancer risk: a case-control study in Connecticut. *British Journal of Cancer* 84(11):1472-6.

Table 1. Duration of breastfeeding

Duration	Whole sample		Sibling sample	
	Number	Percent	Number	Percent
Child was not breastfed	9,486	45.8	2,166	48.9
Breastfed under three months	2,475	12.0	574	13.0
Breastfed between three and six months	1,746	8.4	378	8.5
Breastfed between six and nine months	1,176	5.7	233	5.3
Breastfed between nine and 12 months	882	4.3	200	4.5
Breastfed between 12 months and 24 months	918	4.4	191	4.3
Breastfed 24 months or more	220	1.1	31	0.7
Breastfeeding history unknown	3,794	18.3	652	14.7
Total	20,697	100.0	4,425	100.0

Source: National Longitudinal Survey of Adolescent Health, Wave 1 (1994).

Table 2. Sibling differences in breastfeeding duration

	Older sibling breastfed for...						
	0 months	0 to 3 months	3to 6 months	6 to 9 months	9 to 12 months	12 to 24 months	Over 24 months
Younger sibling breastfed for...							
0 months	**1427**	72	28	10	6	6	1
0 to 3 months	54	**210**	30	13	3	2	0
3 to 6 months	36	16	**116**	28	10	5	0
6 to 9 months	26	9	16	**60**	22	2	0
9 to 12 months	25	4	10	7	**53**	11	1
12 to 24 months	21	11	7	6	11	**62**	1
Over 24 months	3	1	1	1	2	5	**9**

Source: National Longitudinal Survey of Adolescent Health, Wave 1.

Table 3. Unadjusted mean differences in outcomes by breastfeeding history

(a) Full sample

Outcome	Ever breastfed vs Never	0-3 months vs never	3-6 months vs never	6-9 months vs never	9-12 months vs never	12-24 months vs never	Over 24 months vs never
Body mass index	**-0.77**	**-0.45**	**-0.83**	**-0.88**	**-1.13**	**-0.99**	**-0.88**
Overweight or at risk of overweight	**-0.054**	**-0.024**	**-0.064**	**-0.065**	**-0.087**	**-0.068**	**-0.049**
Overweight	**-0.030**	**-0.016**	**-0.033**	**-0.041**	**-0.049**	**-0.025**	-0.032
Diabetes	-0.002	-0.001	**-0.004**	-0.001	-0.003	-0.001	**-0.005**
Asthma	0.003	-0.011	**0.021**	0.011	-0.001	-0.006	0.013
Allergies	**0.029**	**0.025**	**0.044**	**0.029**	**0.031**	0.018	0.011
GPA (0-4 scale)	**0.19**	**0.10**	**0.18**	**0.26**	**0.32**	**0.23**	**0.28**
PVT score (percentile)	**4.9**	**3.7**	**4.3**	**6.4**	**7.0**	**5.8**	**4.4**
Held back a grade	**-0.10**	**-0.07**	**-.09**	**-0.13**	**-0.13**	**-0.12**	**-0.11**
Likely to go to college (per the child)	**0.09**	**0.06**	**0.08**	**0.13**	**0.10**	**0.11**	0.05
Depression index (percentile)	**-4.0**	**-2.9**	**-3.3**	**-4.7**	**-6.3**	**-5.0**	**-3.7**
Mother reports feeling close to child	**0.02**	0.00	0.02	0.02	0.03	**0.05**	0.04
Child reports feeling close to mother	**-0.05**	**-0.03**	**-0.04**	**-0.08**	**-0.07**	**-0.08**	**-0.09**
Child says mother warm and loving	0.02	0.01	0.00	**0.03**	**0.04**	0.01	-0.00
High number of activities w/ mother	**0.016**	**0.010**	**0.015**	**0.018**	**0.024**	**0.021**	0.023

(b) Sibling sample

Outcome	Ever breastfed vs Never	0-3 months vs never	3-6 months vs never	6-9 months vs never	9-12 months vs never	12-24 months vs never	Over 24 months vs never
Body mass index	**-0.63**	**-0.39**	**-0.93**	-0.40	-0.46	**-1.16**	-0.91
Overweight or at risk of overweight	**-0.031**	-0.007	**-0.066**	0.001	-0.040	**-0.054**	**-0.113**
Overweight	-0.015	-0.008	**-0.028**	-0.007	0.002	**-0.039**	-0.007
Diabetes	**-0.004**	-0.004	-0.004	-0.002	**-0.007**	**-0.007**	**-0.007**
Asthma	-0.012	-0.001	0.015	0.040	0.003	0.036	-0.054
Allergies	**0.045**	**0.046**	**0.055**	0.055	-0.016	**0.105**	**-0.150**
GPA (0-4 scale)	**0.24**	**0.12**	**0.21**	**0.32**	**0.36**	**0.36**	**0.53**
PVT score (percentile)	**5.7**	**3.6**	**5.8**	**7.4**	**6.7**	**8.0**	**8.6**
Held back a grade	**-0.12**	**-0.09**	**-0.12**	**-0.17**	**-0.11**	**-0.16**	**-0.19**
Likely to go to college (per the child)	**0.11**	**0.08**	**0.11**	**0.13**	0.06	**0.18**	**0.31**
Depression index (percentile)	**-4.7**	**-3.5**	**-4.7**	**-7.7**	-3.1	**-5.2**	**-13.2**
Mother reports feeling close to child	0.02	-0.03	0.000	0.08	0.03	**0.08**	0.17
Child reports feeling close to mother	**-0.05**	0.00	**-0.05**	**-0.08**	**-0.10**	**-0.12**	-0.06
Child says mother warm and loving	0.01	0.03	-0.03	-0.01	0.01	0.00	0.04
High number of activities w/ mother	**0.024**	**0.025**	**0.021**	**0.037**	0.012	0.026	0.031

Notes: Boldface indicates significance level >0.10. "Overweight" defined as BMI>95th percentile; "at risk of overweight" defined as BMI between 85th and 95th percentiles.

Table 4. Means of regression controls, by child's breastfeeding history

Full sample

	Never breastfed	Ever breastfed	0-3 months	3-6 months	6-9 months	9-12 months	12 to 24 months	Over 24 months
Child's age (years)	15.7	15.4	15.4	15.4	15.4	15.3	15.4	15.4
Child is male	0.49	0.49	0.51	0.49	0.49	0.48	0.49	0.44
Birthweight<5 lbs	0.08	0.04	0.05	0.03	0.03	0.02	0.04	0.03
White	0.60	0.71	0.70	0.71	0.72	0.76	0.75	0.66
Black	0.29	0.14	0.15	0.15	0.13	0.11	0.12	0.10
Hispanic	0.15	0.18	0.20	0.20	0.16	0.15	0.16	0.24
Asian	0.04	0.08	0.07	0.09	0.09	0.09	0.09	0.12
Native American	0.04	0.04	0.04	0.03	0.03	0.03	0.04	0.05
Asthmatic parent	0.14	0.14	0.14	0.14	0.12	0.14	0.14	0.13
Diabetic parent	0.09	0.07	0.07	0.06	0.06	0.07	0.06	0.06
Allergic parent	0.51	0.56	0.57	0.58	0.54	0.55	0.55	0.56
Father is overweight	0.10	0.10	0.11	0.10	0.09	0.10	0.11	0.07
Mother is overweight	0.19	0.18	0.19	0.18	0.17	0.15	0.16	0.15
Income-to-needs ratio	2.63	3.37	3.16	3.33	3.49	3.70	3.61	3.23
Child's extracurricular activities (0-10)	1.55	1.81	1.72	1.79	1.90	1.85	1.98	1.82
Parent's education:								
Dropout	0.20	0.13	0.15	0.13	0.10	0.10	0.13	0.16
High school	0.35	0.22	0.27	0.22	0.20	0.21	0.17	0.19
Some college	0.27	0.31	0.31	0.32	0.31	0.31	0.30	0.30
Bachelor degree	0.10	0.18	0.17	0.18	0.21	0.18	0.20	0.20
Grad/prof degree	0.05	0.13	0.08	0.13	0.15	0.17	0.19	0.13
N	8,901	6,938	2,308	1,636	1,101	829	859	205

Notes: Data from the National Longitudinal Survey of Adolescent Health, Wave 1 (1994). Standard deviations in parentheses. Parent refers to surveyed parent, usually the mother.

Table 5. Comparing between-family and within-family estimates of the effects of "Months breastfed"

Outcomes	*Between-family estimates*				*Within-family estimate*	
	Full sample		*Sibling sample*			
Body mass index (BMI)‡‡	**-0.03**	(0.006)	**-0.03**	(0.01)	0.01	(0.03)
Overweight or at risk of overweight†	**0.98**	(0.00)	**0.98**	(0.01)	1.01	(0.01)
Overweight†	**0.98**	(0.01)	0.99	(0.01)	1.00	(0.02)
Diabetes†	0.99	(0.02)	0.90	(0.08)	0.98	(0.05)
Asthma†	**1.007**	(0.004)	1.01	(0.01)	0.98	(0.01)
Allergies†	1.00	(0.00)	1.00	(0.01)	1.02	(0.01)
GPA (0-4 scale)‡	**0.007**	(0.001)	**0.013**	(0.003)	0.005	(0.006)
PVT score (percentile)‡	**0.12**	(0.02)	**0.21**	(0.04)	**0.16**	(0.08)
Held back a grade†	**0.97**	(0.00)	**0.97**	(0.01)	0.99	(0.01)
Likely to go to college (per the child)†	1.00	(0.00)	**1.02**	(0.01)	1.01	(0.01)
Depression scale (percentile)‡	**-0.13**	(0.04)	**-0.17**	(0.10)	-0.03	(0.21)
Mother reports feeling close to child†	**1.01**	(0.00)	**1.02**	(0.01)	1.01	(0.01)
Child reports feeling close to mother†	**0.98**	(0.00)	**0.98**	(0.01)	1.01	(0.01)
Child says mother warm and loving†	1.00	(0.00)	1.00	(0.01)	1.01	(0.01)
High number of activities with mother†	0.00	(0.00)	0.00	(0.00)	0.002	(0.002)

Notes: Table only reports coefficient on length of breastfeeding, measured in months. "Overweight" defined as BMI>95th percentile; "at risk of overweight" defined as BMI between 85th and 95th percentiles. †Odds ratio from logit regression (standard error in parentheses). ‡OLS regression coefficient (standard error in parentheses). Boldface denotes significance at the 10-percent level. Standard errors adjusted for within-family correlation.

Table 6. Comparing between-family and within-family estimates of the effects of "Ever breastfed"

Outcomes	*Between-family estimates*				*Within-family estimate*	
	Full sample		*Sibling sample*			
Body mass index (BMI)‡‡	**-0.41**	(0.07)	**-0.34**	(0.16)	0.40	(0.33)
Overweight or at risk of overweight†	**0.79**	(0.03)	0.87	(0.08)	**1.32**	(0.21)
Overweight†	**0.77**	(0.04)	0.88	(0.11)	1.17	(0.25)
Diabetes†	0.87	(0.22)	0.69	(0.48)	0.40	(0.24)
Asthma†	1.08	(0.06)	1.21	(0.15)	1.20	(0.22)
Allergies†	1.02	(0.04)	1.07	(0.13)	1.15	(0.17)
GPA (0-4 scale)‡	**0.09**	(0.01)	**0.12**	(0.03)	-0.01	(0.06)
PVT score (percentile)‡	**1.95**	(0.22)	**2.41**	(0.45)	**1.68**	(0.94)
Held back a grade†	**0.80**	(0.04)	**0.74**	(0.08)	1.07	(0.17)
Likely to go to college (per the child)†	**1.14**	(0.04)	**1.29**	(0.11)	0.83	(0.12)
Depression scale (percentile)‡	**-1.86**	(0.47)	**-2.42**	(1.08)	-1.87	(2.41)
Mother reports feeling close to child†	1.01	(0.04)	1.05	(0.09)	1.21	(0.19)
Child reports feeling close to mother†	**0.83**	(0.03)	0.88	(0.08)	1.14	(0.18)
Child says mother warm and loving†	0.97	(0.04)	0.99	(0.08)	0.97	(0.15)
High number of activities with mother†	0.004	(0.003)	0.01	(0.01)	0.03	(0.02)

Notes: Table only reports coefficient on "Ever breastfed" indicator. "Overweight" defined as BMI>95th percentile; "at risk of overweight" defined as BMI between 85th and 95th percentiles. †Odds ratio from logit regression (standard error in parentheses). ‡OLS regression coefficient (standard error in parentheses). Boldface denotes significance at the 10-percent level. Standard errors adjusted for within-family correlation.