Infant Feeding and Post-Weaning Health: Evidence from Turn-of-the-Century London*

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Abstract

Evidence on the post-weaning benefits of early-life breastfeeding is mixed, and highly context-dependent. Moreover, this evidence is drawn almost exclusively from modern settings, limiting our understanding of the relationship between breastfeeding and subsequent health in the past. We provide novel evidence on the nature and reach of these post-weaning benefits in a historical setting, drawing on a rich new longitudinal dataset covering nearly 1,000 children from the Foundling Hospital, an orphanage in turn-of-the-century London. We find that even after the cessation of breastfeeding, ever-breastfed status reduced mortality risk and raised weight-for-age in infancy, that exclusive breastfeeding conferred additional benefits, and that breastfeeding duration had little impact. We also find a U-shaped pattern in weight-for-age by time since weaning, indicating a deterioration in health shortly after weaning, followed by a recovery. The early post-weaning advantages associated with breastfeeding, however, did not persist into mid-childhood. This indicates that any protective effects of earlier breastfeeding attenuated with age, and suggests a strong role for catch-up growth. This study contributes to the data and empirical settings available to explore the relationship between infant feeding and post-weaning health, and helps shed light on the contribution of changing breastfeeding norms to trends in health in twentieth-century Britain.

Keywords: Early-life health, Breastfeeding, Mortality, Anthropometric growth.

JEL Codes: I14, I15, J13, N33

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1 Introduction

There is a vast literature on the effects of breastfeeding on health. Even so, the centrality of breastfeeding to childhood health—and particularly, the extent of its post-weaning reach—remains contested. While some studies link breastfeeding to a number of positive short- and long-run health outcomes, including successful cognitive and immune development and lower risk of mortality, overweight, and chronic disease (Horta and Victora, 2013a; Howie, 2002; Victora et al., 2016), others suggest that the benefits of breastfeeding cease almost as soon as breast milk is discontinued (Fisk et al., 2011; Howie et al., 1990; Kramer et al., 2003; Quigley et al., 2007). Together, these studies indicate that the post-weaning "shadow" of breastfeeding varies greatly across different health outcomes of interest, and that the underlying mechanisms are largely context-dependent. These studies emphasize that the relationship between breastfeeding and child development—the "what," "why," "how much," and "how long"—depends heavily on the particular empirical setting being studied, with the nuances lying in context-specific details such as levels of baseline health and income, the public health environment, access to food and medicine, and cultural practices.

If context matters, then it is concerning that so little of the quantitative evidence on breastfeeding and health comes from historical settings.[1] This dearth of historical evidence—largely a result of lack of data—is problematic both because it limits our understanding of the complex role of breastfeeding in health generally, and because it limits our understanding of child health in the past. That is, modern evidence alone is likely to paint an incomplete picture of the ways in which breastfeeding influences health, and it may be inappropriate to use modern evidence to understand the nature and relative importance of breastfeeding to historical child health. After all, there are strong reasons to believe that in the past, both the contemporaneous and post-weaning effects of breastfeeding on health might be different from what the modern literature would suggest. For instance, even though historical settings often

[1] Only a handful of studies (e.g., Fildes (1992, 1998); Knodel and Kintner (1977); Reid (2002)), address the relationship between breastfeeding and child health in the past.

share the low incomes, low baseline health status, and poor public health systems seen in modern developing countries, behavioral practices (such as rates of breastfeeding and types of supplementation), medical knowledge,[2] and other health inputs are markedly different (often better) today—even in poor settings—than those that prevailed just a century ago. As a result, we might expect breastfeeding to have mattered a great deal to child health in the past (whether intrinsically or in comparison to alternatives), possibly more so than it does today. But to what extent, and for what outcomes, and for how long after the cessation of breastfeeding? In this paper, we use an unusually large and rich new source of longitudinal microdata to begin to bring historical evidence to these questions surrounding early feeding experiences and subsequent health.

Specifically, we draw on records from the Foundling Hospital, an orphanage in late 19th- and early 20th-century London. The Foundling Hospital accepted first-born, illegitimate infants of "respectable" mothers, who surrendered their children to the care of hospital staff until they graduated from the institution's care around age 15-16.[3] These staff included medical professionals who not only provided care, but also kept meticulous documentation of the children's health at the time of admission and throughout their stay in the institution. The records they produced yield what is to our knowledge one of the oldest, largest, and richest longitudinal datasets available to study early-life health in a historical setting. Crucially, the hospital staff recorded details of the children's pre-admission circumstances, including details of the type and duration of feeding received, providing us a rare window into the association between infant feeding, the weaning process, and post-weaning health in the past.

[2]By medical knowledge, we mean a wide range of public health interventions and medical treatments—from, e.g., knowledge of antibiotics to oral rehydration salts—which are widely available in low-income countries today. Even in richer settings, like poor regions of the modern U.S., for instance, increases in access to and utilization of prenatal care have been credited with narrowing gaps in infant health related to maternal education (Shrestha, 2020).

[3]We establish in Section 3.4 and in Appendices E and F that the children in our Foundling Hospital sample are reasonably representative of the broader population of lower middle- and working-class children in England and Wales at the time. Moreover, we adopt several strategies to address concerns over representativeness and sample selection.

Using these data, we develop a novel historical cohort study linking early-life feeding and weaning experiences to mortality and anthropometric outcomes—both in infancy and over the course of later childhood. We then use this dataset to analyze two key questions, all while controlling for a rich set of child characteristics and potential confounding factors. First, we ask: how relatively important were different aspects of the infant feeding regime—e.g., the exclusivity and duration of breastfeeding, the presence of breast milk, the time since weaning—for post-weaning health? Second, for how long do any "protective effects" remain salient, and what do the patterns in persistence and fadeout reveal about the mechanisms that underlie the relationship between early-life feeding practices and subsequent health?[4]

In practice, breastfeeding is of course not randomly assigned—and without a truly experimental research design, we cannot make strong causal claims. However, in the course of our analysis, we conduct a number of exercises—e.g., tests for selection into feeding regime on observable characteristics, including health at birth; tests following Oster (2019) for selection into feeding regime on unobservables; controls for plausible determinants of the feeding regime; and inverse probability weighting strategies, among others—that suggest it may be reasonable to interpret our results as though the feeding regime in our sample were in fact randomly assigned. Given that very few studies in the breastfeeding literature overcome this random assignment issue—or even address it explicitly—even our somewhat imperfect evidence represents a substantial contribution relative to the prior literature.

[4]Our study's focus on weaning is partly an artifact of our data, which begin at or after the cessation of breastfeeding, and partly motivated by the relative sparsity of evidence on the "reach" of breastfeeding over the life-course. There is a vast literature on the effects of breastfeeding on contemporaneous health, but much less is known about the process of weaning, and what happens to mortality risk and growth subsequently over childhood—that is, well after the cessation of breastfeeding. What evidence we do have on the relationship between feeding regimes in infancy and post-weaning child development comes primarily from high- and upper-middle-income countries (see, e.g., Victora et al. (2016), Kramer et al. (2007), and Del Bono and Rabe (2012)), meaning that we have a relatively limited understanding of the relationship between dietary content, breastfeeding duration, weaning experiences, and post-weaning health in settings—including historical ones—characterized by low incomes, poor baseline health, and weak public health and medical infrastructure. This is an especially important gap in knowledge given the mixed findings of the extant literature, and its emphasis on the highly context-dependent nature of the relationship between early-life feeding, weaning, and later-life health (Foote and Marriott, 2003; Horta and Victora, 2013a,b). Thus, by expanding the range of empirical settings used to study these issues, we can begin to unpack how and why this relationship varies across contexts, and what it might mean for population health as both access to health-promoting technologies and norms in breastfeeding change.

It is worth noting up front that we do not make claims regarding the relative importance of feeding in comparison with other early-life inputs, nor do we make direct comparisons between the importance of breastfeeding in the past versus the present. These limitations are both because our data do not allow it, and because the extreme diversity of study designs, methods, measures, and outcomes used in the wider literature makes like-with-like comparisons along these lines untenable. Instead, our aim with this study is to help build up the existing evidence base: to bring new and rare data to the table that can help expand the empirical contexts and samples from which evidence is drawn, and thereby help enrich and complement our understanding of child health in the past, and of the breastfeeding-health relationship more generally. Indeed, data of the sort presented in this paper are rare enough that there is a contribution to be made even in just establishing some of these basic patterns between breastfeeding and health in the past—patterns we may take for granted in more modern settings.

Our study generates four main findings. First, we find a weight-for-age gradient in infancy by feeding type. Exclusively-breastfed children had the highest weight-for-age, followed by those breastfed with supplementation, and finally, those never breastfed. We see a similar pattern when examining post-weaning mortality risk in infancy. These results suggest that even non-exclusive breastfeeding conferred health advantages, and that for children who never received breast milk, the scarring effects were dominant. Second, we find that breastfeeding duration was insignificantly predictive of weight-for-age. This suggests that the mere presence of breast milk (what we term the "extensive margin" of breastfeeding) may have been more important than the duration of breastfeeding (i.e., the "intensive margin"). Third, we find a U-shaped pattern in weight-for-age as a function of time since weaning. This suggests that the penalties associated with the process of weaning were temporary, and attenuated as children adapted to their new nutritional and disease environment. Finally, we find no significant differences by early feeding practices in mortality risk, weight-for-age, or height-for-age by mid-childhood. Given the early disparities in these outcomes, these results hint at a striking role for catch-up growth among children initially disadvantaged by their feeding regime.

The findings in our study contribute to two closely related literatures. First, they help us better understand how, why, and for how long early-life feeding practices matter in the days and years *after* weaning. Here, as discussed above, the evidence is mixed,[5] conclusions are based almost exclusively on data from modern settings (much of it from wealthy countries), and differences in study design can render it difficult to make like-with-like comparisons. Our study thus augments and diversifies the evidence base on these issues, and helps shed further light on the mechanisms by which breastfeeding might matter to long-run health—in particular, by providing new results from a recently industrialized setting that is both intrinsically important historically, and poorly represented in the extant literature.

Second, they help clarify the relationship between infant feeding, weaning, and health in the past. Much of the historical evidence on early-life feeding and health is impressionistic, and derived from rough comparisons of aggregate trends. For instance, there is some evidence that the incidence and duration of breastfeeding were high in the UK in the late 19th and early 20th centuries,[6] but that these rates fell precipitously across the first half of the twentieth century to an eventual low in the 1960s (Hoddinott et al., 2008; Wharton, 1982). The downward trend, which stood in opposition to contemporaneous trends in cohort health—e.g., rising heights and declining infant mortality rates—led to an assumption that breastfeeding was unlikely to have been an important input to health over this period (Woods, 2000). Although recent work such as Fildes (1992, 1998) and Reid (2002) has interrogated

[5]Where many of these studies do agree, it tends to be on the relative importance of the extensive margin of breastfeeding—that is, the notion that the presence of breast milk matters more than its exclusivity or duration.

[6]Over this period, breastfeeding was common, especially among the working classes: approximately 80% of children in this period were breastfed in the first month of life, with 60-70% still being breastfed at four months (Fildes, 1998). However, with increasing living standards, the development of safer forms of artificial feeding, and the invention of safer feeding devices, rates of breastfeeding declined dramatically in the first half of the twentieth century in much of the developed world. This shift was also encouraged by doctors who no longer saw breastfeeding as critically important for infant health. The nadir of breastfeeding rates was likely reached in the 1960s, but the first reliable modern data for breastfeeding rates in England and Wales appeared in 1975. This showed that only around 50% of mothers initiated breastfeeding at all, and at four months, only 13% of mothers were still breastfeeding their children (Hoddinott et al., 2008; Wharton, 1982).

this assumption more carefully, finding that breastfeeding was associated with lower rates of post-neonatal mortality,[7] there are several limitations to existing quantitative work on historical breastfeeding.

For one, studies like the ones just mentioned are very few, and there are to our knowledge none that explicitly address the possibility of continuing post-weaning effects, or that contain substantial longitudinal information on children. In this way, our study can bring new and unusually rich data to important historical questions on individual and population health. For instance, it can help us better understand 20th-century health transitions by improving our understanding of the importance of breastfeeding to child health, which while an extremely common child health input, is understudied relative to other health inputs such as the early-life disease environment. It can also shed light on the degree of persistence in any feeding-related early-life health differentials in a setting characterized by relatively low living standards and poor medical technology.

In addition, while existing studies provide valuable evidence on the relationship between breastfeeding and mortality in the past, other important indicators of cohort health, such as anthropometric outcomes, have not been studied in the context of historical breastfeeding. Here, our focus on rich longitudinal weight- and height-for-age measures is important not just because these are a new metric in the historical literature on breastfeeding, but also because they are conceptually meaningful. Mortality is a blunt, and thankfully relatively rare, outcome, and although historians are often constrained to such coarse health data, research on historical child health stands to benefit from access to a more sensitive outcome that better reflects morbidity and health scarring—the sort of outcome that can help us understand the impact of *incremental* changes in nutrition, particularly among the large share of the population that survives infancy. In that dimension, our study helps advance knowledge by

[7] Knodel and Van de Walle (1967) offers a similar analysis of three historical German states. Looking to evidence from Roosendaal in the Netherlands, Walhout (2019) likewise corroborates the importance of breastfeeding to mortality in the first couple hundred days. Specifically, she shows that causes of death plausibly sensitive to breast milk are prevalent only for the period shortly after birth, and that the infants whom artificial feeding left most exposed to environmental hazards, such as those born to urban unskilled laborers, were the most vulnerable to such mortality.

presenting results on a measure of net nutritional status and childhood development.

Finally, our study gives us a rare view into the health of illegitimate children—a group that is relatively understudied, often for lack of rich data—and into the potential sources of broader population-level disparities in child health in the early stages of Britain's epidemiologic transition.

2 Related Literature

2.1 Historical Evidence on Breastfeeding and Health

Only a few studies—among them, Fildes (1992, 1998); Knodel and Kintner (1977); Reid (2002)—address the relationship between breastfeeding and child health in the past. Knodel and Kintner (1977) show that the age pattern of infant mortality varies in historical societies with different breastfeeding practices. Fildes (1992, 1998) uses breastfeeding surveys conducted by British Medical Officers of Health in the early twentieth century to show that breastfeeding rates were relatively high in the early twentieth century: approximately 80% of children were breastfed at one month of age and 60-70% were still being breastfed at four months. Fildes (1992) finds that breastfeeding rates were highest in London boroughs with the highest infant mortality rates, and were lower in areas with lower breastfeeding rates. This positive correlation was driven in part by the greater propensity of working-class women to breastfeed their children, and highlights the importance of omitted variables obscuring the association between breastfeeding and health.[8] Fildes (1998) expands her study of infant feeding to all of Britain, but provides a more detailed case study into the relationship between breastfeeding and infant mortality in the county of Derby, where the Medical Officer of Health had reported aggregate infant deaths by infant feeding type. Exclusively-breastfed infants had the lowest mortality, but infants breastfed and then later fed artificially also had

[8] Evidence from the Netherlands also suggests that religion could have played a role in historical breastfeeding propensity (van den Boomen and Ekamper, 2015).

a survival advantage over those who were never breastfed. This evidence was an improvement on the earliest studies, since the "treatment" was clearer, but there is still considerable scope for omitted variables, such as class and illegitimacy, to confound the association.

Reid (2002) provides the most robust historical analysis to date, studying an individual-level, longitudinal dataset from Derby rather than relying solely on regional aggregates, as is the case in the rest of the existing historical literature. She finds that early artificial feeding increased mortality risk, especially from diarrheal diseases, but similar to Fildes (1992), she finds that the majority of mortality was associated with the environment that children lived in, rather than with their feeding type per se. Still, as in the vast majority of studies analyzing the association between breastfeeding and mortality, infant feeding is not randomly or quasi-randomly assigned, so there is still potential for omitted variable bias to confound the association. Our paper builds on this existing historical literature both by contributing a new individual-level longitudinal dataset where we can track a range of outcomes including mortality and anthropometrics, and by applying more sophisticated methods for managing and accounting for the non-random assignment of infant feeding.

2.2 Modern Evidence on Breastfeeding and Post-Weaning Health

Current guidelines for early-childhood nutrition recommend exclusive breastfeeding for the first six months of life, with breastfeeding to continue through to at least age two alongside complementary foods (American Academy of Family Physicians, 2008; Horta and Victora, 2013a; World Health Organization, 2016b). Data drawn from 2007-2014 indicates that roughly 36% of the world's infants were exclusively breastfed from birth to the age of six months; for infants in low-income countries, the figure is 47% (World Health Organization, 2016a). Although there has been a modest increase over the last fifteen years in adherence to these targets (particularly with respect to exclusive breastfeeding in early infancy, and particularly in developing regions), a large share of children either are never breastfed, or are prematurely weaned: to wit, only about three quarters of those aged 12-15 months, and roughly half of those aged 20-23 months, are reported as still breastfeeding (OECD Social Policy Division, 2009; UNICEF, 2016).

Since many of the world's children may be being weaned earlier than recommended, and may be receiving improper supplementation during critical periods of development, it is especially important to gather evidence on how breastfeeding in infancy may influence health even *after* its cessation. While much of the available literature focuses on the contemporaneous effects of breastfeeding on health,[9] the literature investigating the *persistence* of early-life feeding regimes is much smaller. To wit, Feachem and Koblinsky (1984) acknowledge that very few studies have examined whether past (rather than currently ongoing) experiences of breastfeeding have an effect on health. As exceptions, they cite Ferguson et al. (1981), which suggests that the incidence and duration of exclusive breastfeeding are not associated with the period prevalence of diarrhea within the first two years of life; and Cunningham (1981), which suggests that protection from significant illness episodes ceases when breastfeeding stops (Feachem and Koblinsky, 1984). For contrast, Ellestad-Sayed et al. (1979), using data from First Nations people in present-day Manitoba, find that breastfeeding has a protective effect against severe infectious disease even after it is discontinued: although this effect is insignificant in the rate of hospital admissions, it is significant in the duration of hospital stays. Cunningham et al. (1991), too, provides a brief review indicating that some studies have found protective effects of breastfeeding (for instance, against acute respiratory illness) even after the cessation of breastfeeding. A common concern in this literature is that selection into breastfeeding drives many of these associations, and several studies have found minimal long-lasting effects of breastfeeding once controlling for these confounders (Baker and Milligan, 2008; Colen and Ramey, 2014; Fletcher, 2011) although others still

[9]Breastfeeding—which can represent a bundle of maternal investments including breast milk, maternal time, and physical contact—is thought to be important to a range of health and wellbeing outcomes, including metabolism, cognitive function, and immunity (Feachem and Koblinsky, 1984; Horta and Victora, 2013a,b; Victora et al., 2016; World Health Organization, 2000). Few studies in this literature offer causal evidence of the impact of breastfeeding on health (papers such as Kramer et al. (2007), Jayachandran and Kuziemko (2011), and Del Bono and Rabe (2012) are notable exceptions), and most do not distinguish between these bundled investments. Nevertheless, breastfeeding has generally been linked to lower contemporaneous (i.e., as opposed to post-weaning) mortality risk in infancy.

find detectable effects (Belfield and Kelly, 2012).

The aforementioned results hint at another feature of the post-weaning health literature: namely, that its findings are highly mixed, with differences that seem to stem from factors such as the particular characteristics of the empirical setting, the specific dimension of health being studied, or the followup period chosen. For instance, using data on a number of developing countries, Palloni and Millman (1986) find that children who were breastfed had a lower probability of mortality that diminished in magnitude over the first 5 years of life, though these were heavily concentrated in the first year of life. Using data from 1990s England, Quigley et al. (2006), for contrast, find that the post-weaning protection breastfeeding offered against diarrhea lasts only two months. At the opposite extreme, Howie (2002) discusses effects on respiratory disease and blood pressure that last even to ages 7-10 and beyond. Even within settings with similar characteristics, however, results can reflect the multidimensionality of health and health inputs. For example, Retherford et al. (1989) find that early weaning greatly increases infant mortality risk, especially in settings like theirs in Nepal where—as likely in our setting as well—pre-weaning nutrition (i.e., breast milk) is generally better than post-weaning nutrition (because of food and water contamination, and poor sanitation and medical services). Meanwhile, other studies suggest that longer durations of breastfeeding (i.e., later weaning) can actually result in *under*nutrition and *excess* mortality, perhaps because if exclusively breastfed, breast milk alone is insufficient to meet the nutritional needs of older infants (Knodel and Kintner, 1977). Yet other studies suggest that *earlier* supplementation (i.e., partial weaning before 3 months) can lead to better outcomes than later supplementation, lowering rates diarrhea and vomiting—indicating perhaps a role for early, "scaffolded" exposure to milk and food in building immunity (Eaton-Evans and Dugdale, 1987). A final class of studies suggest that while growth velocity differs across feeding types, there is ultimately little long-run difference in (level) outcomes—a result which, like our findings in the present study, points to the phenomenon of catch-up (Johnson et al., 2014).

Clearly, these relatively few and mixed results point to the importance of context, and the necessity of increasing the size and diversity of the evidence base exploring these issues. Our study therefore contributes to this literature in a few ways. First, it is one of only a handful of studies investigating the persistence of early-life feeding regimes on health outcomes after weaning—particularly where weaning may be arguably premature. Second, it is one of the rare ones providing evidence from the past, whose points of comparison and contrast relative to settings in the existing literature can help shed light on the broader phenomena of interest. Finally, our study is also one of the few to focus on the relationship between early-life feeding and anthropometric outcomes at multiple points in childhood (De Cao, 2014); on this more sensitive and universal set of metrics, compared to studies on mortality or acute illness following weaning, evidence is even sparser.

3 Data

3.1 Background: The Life of a Child in the Foundling Hospital

To investigate the relationship between infant feeding regimes and post-weaning health in a historical setting, we develop a new cohort study from the records of the London Foundling Hospital. The Foundling Hospital was central to the care of deserted children from 1741 to 1955, helping over 25,000 children during this period (Pugh, 2007). Located in central London, the institution functioned as an orphanage and residential school. It admitted first-born, illegitimate infants who had been abandoned by their father, and whose mother was deemed of "good character," i.e., not a prostitute (Gillis, 1979; Nichols and Wray, 1935).[10] Our study focuses on roughly 1,000 children who entered the Foundling Hospital in the 1892-1914 birth cohorts. Our sample is drawn predominantly from what is today Greater London, although some children came to the Foundling Hospital from further afield.

[10]The hospital paid inspectors to check the mothers' backgrounds, and they rejected petitions of women who provided false information.

Figure 1 provides a schematic diagram of the life-course of a child in our sample. All children were born outside the institution, since mothers could only petition for the child's entry after it was born. This meant that Foundlings spent a short period of time at home with their mothers before being admitted to the hospital,[11] during which time they received from their mothers either exclusive, some, or no breast milk. All children were fully weaned upon entering the Foundling Hospital, as the institution did not provide wet nurses by the late nineteenth century. Thus, it is this pre-admission feeding information that we exploit in our analysis.

Upon admission, the infants were given medical examinations, which recorded their weight and other subjective health outcomes. Almost immediately after intake, the infants were fostered out to married couples in three rural counties neighboring London: most of the children were fostered in Surrey and Kent, with a few sent to Essex. The foster families were carefully selected, and each county had a medical officer that supervised the children's health and ensured that they were living in good conditions. Despite this improvement in living standards, the vast majority of deaths in the sample occurred during this time in the country. The county medical officers recorded the date and cause of death of children who died, and passed the information along to the Foundling Hospital central authorities.[12] The children remained in the country with their foster families until they were 5-6 years old, at which point they were transferred back to the hospital's main site in central London (a milestone we henceforth refer to as re-admission) until the age of 15 or 16. There, the children lived in a typical institutional setting of the time.

[11]The average age at admission to the hospital in our sample was 0.37 years old (134.5 days), but children were admitted as early as 23 days old and as late as 1.45 years old.

[12]Administrative information from the London Metropolitan Archives (LMA), Medical Record of the Foundling Hospital, London 1877-1911 by W. J. Cropley Swift, Medical Officer, A/FH/A/18/10/6; Foundling Hospital dataset.

3.2 The Foundling Hospital Dataset

Importantly for our analysis, the Foundling Hospital kept detailed individual-level records on the children's health at each of the major milestones, and for each of the major sub-periods of childhood, shown in Figure 1. They also recorded a wealth of information on the children's pre-admission circumstances and parental characteristics. Because of the institution's very careful and extensive record-keeping, it is possible to reconstruct a cohort dataset following each foundling over time. This new cohort dataset includes some of the most detailed microdata on children's nutrition, growth, morbidity, and general health of any source for its time, and represents a major contribution to the study of health in poor and historical settings in the era before modern medicine, public health, and social welfare services. For more on these novel data, see Appendix A.[13]

Of the many useful pieces of information reported by Foundling Hospital staff, in this study, we draw primarily on the following: first, the feeding regime, observed prior to admission; second, mortality risk, observed from admission onward; and third, a number of anthropometric measures observed starting at admission. These latter measures include: weight in infancy, and weight and height at ages 5-6. We convert these measures into Z-scores relative to the modern WHO reference in order to standardize comparisons across children of different ages and sexes (de Onis et al., 2007; WHO, 2006).[14] Appendix B presents descriptive statistics for the anthropometric measures and further discussion.

In addition to anthropometric outcomes, we also look at mortality risk in infancy and childhood after the children were admitted to the Foundling Hospital. Appendix C describes

[13] The data used in this study come from records that identify individuals, many of whom are protected by a 110-year rule aimed at safeguarding their personal information. Accordingly, many of the records used to build the dataset are access-restricted, and we thank the Coram Foundation for granting us special permission to view these records.

[14] The Z-scores simply describe a child's height or weight as standard deviations relative to children in the modern reference of the same sex and age. Thus, a child with a weight-for-age Z-score of -1 has a weight-for-age one standard deviation below the modern mean for children at the same age. Schneider (2015) discusses some of the issues related to using the modern WHO growth reference on historical data, and concludes that the modern references may be used if scholars account for differences between the historical and modern growth pattern.

the mortality data in detail and compares mortality in the Foundling Hospital with mortality in London and the counties where the children were fostered.

The measures of infant feeding and weaning are the main focus of our analysis. In the section that follows, we dive deeper into these data, and place the Foundling children's feeding experiences in a broader historical context.

3.3 Infant Feeding and the Foundling Hospital

3.3.1 Overview of Feeding Types

In addition to the range of health outcomes described above, the Foundling Hospital medical records provide rich information about each child's feeding regime in the period before they entered the hospital. First, they recorded whether the children had been given breast milk, milk, food, or any combination of the three prior to admission. For the sake of simplicity and comparison, it is easiest to group children into three broad feeding categories: those who were exclusively breastfed (B), constituting 10% of our sample; those who were breastfed but also received supplementary food or milk (B+; 48% of our sample); and those who were never breastfed, but were instead given milk and/or food (NoB; 42% of our sample). More detail on infant feeding in the Foundling Hospital is provided in Appendix D.

Second, they provide information from which we can reconstruct breastfeeding duration. For exclusively breastfed children, this variable is equal to the age at admission to the hospital, while for never-breastfed children, it is equal to zero. For a subset of the supplemented group, the medical officer recorded the number of months or weeks that each child was breastfed, and we build our duration measures from these.[15] A summary of breastfeeding duration by feeding regime is given in Appendix Figure D.2.

Third, these earlier measures allow us to compute the time since the child last received breast milk. This enables us to analyze the relationship between the weaning circumstances

[15] Because the child had already been accepted to the Foundling Hospital at the time of this inspection, there is little reason to believe that the mothers systematically misreported duration to garner favor with the hospital.

and subsequent health. We model time since weaning non-linearly, since we expect that there may be some initial deterioration and subsequent catch-up in health related to the weaning process. A summary by feeding regime of the time between weaning and admission is given in Appendix Figure D.3. Together, these measures allow us to investigate the relative importance of extensive and intensive margins of breastfeeding as it pertains to post-weaning health.

3.3.2 Dimensions of Feeding and Historical Context

It is useful first to gain an understanding of the incidence, duration, and timing of feeding practices in our data, and to understand—where suitable data are available—how the practices we observe in our sample compare to patterns in the wider population at the time.

We begin by exploring how our feeding-type breakdowns compare to those in other roughly contemporaneous data. Table 1 compares the incidences of exclusive breastfeeding, breastfeeding with supplementation, and never breastfeeding in the Foundling Hospital with those found in several other British surveys from the early twentieth century. Here, we see that in other localities, as in the Foundling Hospital sample, there is a roughly even split between the never-breastfed group and those breastfed with supplementation. However, we can also see that exclusive breastfeeding in the Foundling Hospital sample appears to have been substantially lower than in these contemporary surveys, with roughly equal shares of the "missing" exclusively-breastfed children being distributed across the remaining two categories.

This difference may reflect a lower rate of exclusive breastfeeding among Foundling children relative to the true population average. However, we have reason to believe it also reflects an overestimation of exclusive breastfeeding in the comparator surveys, resulting from peculiarities of the samples and methods these surveys used.[16] To give one example,

[16] This fact yet again underscores the scarcity of high-quality historical data suitable for studying breastfeeding and health, the difficulty of comparisons with extant evidence, and the value of datasets such as ours whose sample characteristics, variable definitions, and data construction are transparently laid out.

the surveys available for use as comparators primarily focused on working-class households. As such, they tended to overestimate breastfeeding incidence, since working-class households were more likely in this period to breastfeed their children than their middle- and upper-class counterparts who had largely switched to cow's milk, condensed milk, or formula (Fildes, 1992; Woods, 2000, pp. 284-5). Our sample, for contrast, incorporates a large number of lower middle-class children as well. To give another example, the surveys to which we compare our Foundling data used many different survey methodologies, some of which oversampled the very young, thereby yielding a higher incidence of breastfeeding than would be drawn from a random sample of infants. For instance, the Salford surveys were conducted as part of a post-natal health visitor service that would have oversampled younger infants. It is therefore unsurprising to see such a high rate of breastfeeding among those surveys, since breastfeeding incidence typically fell as children aged and their mothers weaned them. Results by age from London in Table D.1 (which, incidentally, provide perhaps the closest comparator to our Foundling sample)[17] corroborate this idea. To wit, our numbers look quite a bit similar to those observed among the oldest groups surveyed in the Stepney borough, consistent with the older age distribution of our children. Finally, and notably, our sample pertains exclusively to illegitimate children, who may have been less likely to have been exclusively breastfed than the children in these surveys, for instance, because their mothers may have needed to return to work (Fildes, 1992). Unfortunately, the latter do not differentiate breastfeeding rates by legitimate and illegitimate status. Despite these discrepancies in sampling and methods, however, these surveys nevertheless provide a general perspective on working-class behavior that can be used to interpret the evidence we generate from the Foundling Hospital sample. Clearly, breastfeeding incidence was much lower among infants entering the Foundling Hospital than it was for infants in other urban areas around England, particularly younger ones, lower-class ones, and legitimate ones.[18]

[17] These data were drawn from Medical Officer of Health reports, and were based on health visitor records. Taken together, the reports describe feeding practices in the first month for 222,989 births or 39.2% of registered births in the boroughs covered (Fildes, 1992)

[18] See Section 3.4 for further discussion of selection concerns and how we overcome them.

We can also explore breastfeeding duration in the Foundling Hospital, as well as its representativeness. A few late nineteenth- and early twentieth-century surveys did measure the incidence of each feeding type at different ages, as mentioned above with reference to London borough surveys. Feeding shares from these age-subdivided surveys are provided in Table D in the appendix, alongside Foundling Hospital figures at similar age intervals. As with the general surveys, it is worth mentioning some challenges related to comparability. First, the town surveys reported on a mother's current feeding practice, while the Foundling Hospital reported past practice as well. Second, the London health visitor data may underestimate breastfeeding because health visitors tended to arrange follow-up visits with mothers they thought might run into problems (Fildes, 1992, p. 59). Nevertheless, comparing the Foundling data to these age-subdivided surveys, we can see that the incidence of exclusive breastfeeding was much lower in our sample, even at young ages. Supplementation began in the Foundling Hospital children rather earlier than in the general population, and this group was much larger than in the general population, though this included mothers who breastfed in the past but later weaned their child onto milk and/or food alone. There was a much larger share of children who were never breastfed in the Foundling Hospital as well.

In terms of timing and duration, however, all of the data supports the idea that mothers weaned their children from breast milk to milk/food during the first year of life. We can see this progression quite nicely in the Foundling Hospital by looking at the disaggregated feeding types (Figure D.1 in the appendix). There is a general shift from exclusive breastfeeding to supplementation, which is clear in Panel A, and also from milk to both milk and food as the incidence of the BMF and MF categories increased with the infants' ages. We can observe breastfeeding duration more directly through the notes listed next to children who were breastfed and supplemented in the raw sources. These figures should be taken with caution, however, because there was considerable heaping on one-month units, increasing potential (classical) measurement error. Figure D.2 shows the breastfeeding duration of the exclusively-breastfed and supplemented group. From the supplemented group, we can see that the median infant was weaned at two months though there was some variation around this median. Interestingly, infants who were breastfed and supplemented have slightly longer breastfeeding durations than children who were exclusively breastfed because the supplemented group had a slightly later average admission date, a matter we address in several different ways in our regression analysis (see Section 4.1.1 for more).

Finally, we can explore differences in the time between weaning and admission to the Foundling Hospital. Figure D.3 in the appendix shows this measure as a boxplot for the breastfed-with-supplementation and never-breastfed groups; those exclusively breastfed have a value of zero for this indicator. In general, the figure shows that there were not substantial differences in the time between weaning and admission for the former two groups.

While this section has touched on matters of representativeness and selection as they relate to aspects of infant feeding, the section below provides a more comprehensive treatment of these and related data issues.

3.4 Assessing the Data

Before proceeding with our analysis, it is worth considering two main issues that might affect the interpretation of our results: first, whether there are systematic differences between Foundling Hospital children and their counterparts who were not institutionalized (representativeness); and second, whether there are systematic and potentially confounding differences amongst Foundling Hospital children by feeding regime (nature of and selectivity into "treatment"). The first of these issues will speak to the external validity of our main results, and the ability of our study to comment on broader patterns in historical health. The second will speak to potential confounders that could affect the interpretation of our results on between- and within-feeding-regime differences in subsequent health.

In Appendix E, we present the results of a battery of tests aimed at addressing these concerns. Analyzing potential selectivity into the Foundling Hospital resulting from a range of factors—e.g., parental and child characteristics, a suite of decisions made by mothers at various stages in the petition and admissions process, and a suite of decisions made by the Foundling Hospital—we find no or at most very limited evidence of selectivity into the Foundling Hospital. Instead, the children in our sample seem to be reasonably similar in age-specific health, socioeconomic status, and infant feeding patterns to other lower middle- and working-class children in England and Wales at the time, particularly illegitimate ones. Likewise, we find on the whole little relationship in our sample between infant and parental characteristics and either the feeding regime or the duration of breastfeeding.[19]

In addition to the checks above, which rely in large part on analyzing parental characteristics and behaviors, we also address concerns over confounding factors and representativeness by drawing on information on health at birth. For instance, we might be concerned that pre-admission feeding practices responded to health at birth, that these practices induced selective, feeding-differential culling between birth and admission that would be difficult to detect without knowledge of the underlying distribution of health at birth, or even simply that our children had systematically different distributions of underlying health than their peers outside the Foundling Hospital. To deal with these issues, we generate a second novel set of linked longitudinal data. Specifically, we recover information on our children's *pre-admission* and even *pre-feeding* health status by linking their records to those of a local maternity hospital, London's Queen Charlotte (QC) Lying-In Hospital.[20] Because this hospital served London children of all classes and types, this historical record linkage exercise also allows us visibility into a non-institutionalized comparison group: children born in the Queen Charlotte Hospital who did not go on to the Foundling Hospital.

Our analysis of this linked longitudinal dataset is detailed in Appendix F, and produces three main results. First, the distribution of health at birth among the children in our FH-QC linked sample (proxied alternately by birth weight and birth length) is statistically

[19] One notable exception is the largely mechanical correlation between feeding regime, breastfeeding duration/time-since-weaning, and admission age. We discuss our strategies to address this issue in Section 4.1.1.

[20] This set of FH-QC links is representative of the children in our main Foundling Hospital sample in terms of parental characteristics, anthropometrics, etc.

indistinguishable from that of QC children (Panel A, Figure 2). Appendix Table F.1 makes it clear that this is true of QC children of any parity, whether illegitimate or legitimate. Due to the composition of mothers in the QC data, however (single mothers are overrepresented relative to the London population, and married mothers may be negatively selected on health relative to all married London mothers), we are most confident making claims of external validity with respect to illegitimate children, and particularly, illegitimate firstborns. Accordingly, this result tells us that our Foundling Hospital sample is representative of illegitimate firstborn children in London at the time,[21] lending credence to the ability of our results to speak to broader populations.

Second, the distributions of birth weight by feeding type are statistically indistinguishable (Panel B, Figure 2), particularly after controlling for basic parental characteristics (Table 2). This tells us that the mothers of Foundling Hospital children did not appear to choose feeding types in response to health at birth as they observed it (e.g., as a compensatory/reinforcing investment), and that it therefore may be reasonable in our analysis to treat the feeding regime as as good as randomly assigned[22], especially after controlling for parental characteristics as we do throughout.

Third, the birth weight distributions of QC and matched FH-QC children are similar, despite the fact that the latter is conditioned on both survival to Foundling Hospital admission and the share of children in each feeding type, while the former is not.[23] Moreover, the distribution of birth weight is similar across feeding types. This suggests that any pre-

[21] Insofar as the QC Hospital received a representative set of illegitimate firstborns.

[22] So long as birth weight can be taken as a reasonable proxy for a child's underlying initial health status.

[23] That is, FH-QC children can only appear in our data if they survived long enough to be admitted to the Foundling Hospital. Non-linked QC children, for contrast, are observed only at birth, and could have died anytime thereafter. We might, as a result, expect to see survivorship bias in the FH-QC sample—either in general or differentially by feeding regime—especially since mortality rates among illegitimate children were roughly twice as high as for legitimate children at the time. Note as well that insofar as differences in pre-admission mortality risk by feeding type affect the distribution of health at birth in the FH-QC sample, such compositional issues do not operate in the QC-only sample, since presence in the QC data arises only from birth: a pre-admission, pre-feeding period. The fact, then, that the distribution of health at birth is similar in the FH-QC sample (where post-birth feeding and survival to Foundling Hospital admission both potentially determine one's presence in the sample) and in the QC sample (where one's presence in the sample has nothing to do with either subsequent survival or feeding), is strong evidence of both representativeness and the absence of feeding-differential culling on the basis of latent health.

admission mortality among our FH children was not culling per se (in that mortality was not systematically related to a health threshold), either in a general or in a feeding-differential sense.[24] This tells us that children in our sample were not of higher underlying "quality" than the average child of their birth cohort, simply by dint of having survived to admission. Perhaps more importantly to this study's main questions, it tells us that we can reasonably assume a similar underlying (i.e., latent) health distribution of children *by feeding type* at the time of admission, such that differences in health at admission by feeding type can be interpreted as *scarring* related to the feeding type.

Taken as a whole, then, these checks indicate that the selectivity we see in our sample is limited. Moreover, due to the nature of the between-foundling comparisons we make, it is of the sort unlikely to have a practical impact on the central question of this paper: the differential relationship between infant feeding regimes and post-weaning health outcomes in a historical setting.

4 Feeding, Weaning, and Health in Infancy

4.1 Weight-for-Age around Infancy

4.1.1 Empirical Strategy

We begin by examining how pre-admission feeding practices and weaning experiences influenced the children's short-run health, as given by weight-for-age Z-scores at the time of admission. We estimate a series of OLS regressions as follows:

$$WAZ_i = \alpha + \beta_1 B_i + \beta_2 Bplus_i + {x_i}' \gamma + \epsilon_i \tag{1}$$

[24] This is true even if mortality risk itself may have been correlated with the feeding regime. Put another way, even if never-breastfed children may have died disproportionately to other types prior to entering the Foundling Hospital, these children were not disproportionately drawn from the lower tail of either the overall birth weight distribution or the never-breastfed birth weight distribution. Instead, these deaths appear to have been drawn roughly randomly from across the distribution of never-breastfed birth weights.

where WAZ_i is the WHO weight-for-age Z-score for each child i at admission to the Foundling Hospital. B_i is an indicator that takes the value of 1 if child i was exclusively breastfed before admission to the Foundling Hospital, and was therefore weaned upon entry to the Foundling Hospital. $Bplus_i$ is an indicator that takes the value of 1 if the child received breast milk alongside supplementary foods such as milk or other foods, and as such had been at least partially weaned prior to admission. The reference category is those who were never breastfed. $x_i{'}\gamma$ are a set of individual-level controls including dummies for admission age in months, male children, birth location types, birth season, mother's age, father's class, birth year, and birth district.

In alternate specifications, we include measures of breastfeeding duration and time since weaning as defined in Section 3.2, either by themselves, or alongside the feeding-type dummies in Equation 1. For time since weaning, we include a set of dummy variables capturing the time between a child being weaned and their admission to the Foundling Hospital. Because we have no variation in breastfeeding duration for the never-breastfed group, and no variation in time since weaning for the exclusively-breastfed group, we prefer to exclude the never-breastfed group in regressions that include breastfeeding duration measures, and to likewise exclude the exclusively-breastfed group in regressions that include the time-since-weaning dummies. Table 3 presents the results.

4.1.2 Results

We begin by examining the simple association between feeding regime and weight-for-age at admission. Specifications 1 and 2 show that exclusively-breastfed children (B_i) were substantially and statistically significantly heavier than their counterparts, weighing approximately 0.9 standard deviations more than children who were never breastfed, and 0.44 to 0.53 standard deviations more than children who were breastfed with supplementation ($Bplus_i$). Children who were breastfed with supplementation were also approximately 0.40 standard deviations heavier than children who were never breastfed, suggesting that even non-exclusive breast milk provided weight advantages to infants at the time. The results are relatively similar whether controlling for admission age dummies or not. Put another way, there is a clear extensive-margin effect: having ever received breast milk was associated with better net nutritional status in and around infancy, and having been exclusively breastfed was best of all. These differences in weight are not just statistically significant, but they also reflect meaningful differences in health: per WHO growth standards, exclusively-breastfed children would have a much lower risk of being categorized as underweight than their counterparts.[25]

One might wonder, however, the extent to which breastfeeding duration and the health effects of weaning may be driving these results. Specifications 3 and 4 in Table 3 show that breastfeeding duration is positively associated with weight in infancy (an intensive-margin result), but the coefficients are not statistically significant.[26]

In Specifications 5 of Table 3, we examine the association between time since weaning and weight-for-age in infancy. Here we exclude the admission age dummies because they are highly collinear with the time since weaning categories. There is a clear, non-linear, U-shaped pattern by days since weaning in weight-for-age in infancy. Children admitted within either 30 days of weaning, or more than 180 days since weaning, weighed approximately 0.5 standard deviations more, respectively, than children admitted to the Foundling Hospital within 60 and 89 days of weaning. Accordingly, it seems that the health costs of weaning—such as exposure to new pathogens and inferior nutrition—took around a month to have an impact on infant weight, and that children were generally able to recover from this shock within six months of weaning. This pattern holds whether excluding (not reported) or including the $Bplus_i$ indicator, although the U-shape is flattened somewhat when including it (as in Specification 5). This U-shaped pattern is intriguing, but we should note that it is

[25] Underweight children are those that have weight-for-age Z-scores below -2 standard deviations using the WHO 2006 growth standard.

[26] We tried an interaction between B_i and $BDur_i$, but this was insignificant. We also tested non-linear functional forms for breastfeeding duration. These did not improve the fit of the model, and the coefficients were never statistically significant (not reported).

predicted from pooled cross-sectional data comparing different children observed in different time-since-weaning bins. Ideally, this result would be replicated with true longitudinal data, but we do not know of any historical data that would allow for this type of calculation.

4.1.3 Robustness

As discussed earlier, the Foundling Hospital appears not to have considered a child's health or pre-admission feeding regime in its admission decisions, and we find no substantial evidence of selectivity into feeding regimes on the basis of parental or child characteristics, including the child's health at birth. Nevertheless, feeding regimes are not randomized at birth, and as such, we might worry that the choice of feeding type and duration might be systematically related to factors which are unobservable here—for instance, when the mother decided to apply for admission to the Foundling Hospital, or her broader beliefs about breastfeeding.

To rule out omitted confounders that may bias our results, we implement a test proposed by Oster (2019), which uses coefficient and R-square movements arising from the inclusion and exclusion of control variables to produce bounds for how much unobserved factors would alter the main coefficient of interest. This procedure is discussed at length in Appendix G. Note that we can only test for one treated variable at the time, so the separate checks for the exclusively breastfed and supplemented groups in specifications 1 and 2 are tested separately. Using this approach, we find little difference in the omitted variable bias-adjusted coefficients and the estimated coefficients presented in Table 3. Indeed, selection on unobservables would need to be over sixteen times greater than selection on observables for our coefficients to be driven to zero (i.e., for results to be spuriously driven by unobserved factors). In general, then, these results suggest that the differences that we observe between the feeding types, and especially for the exclusively breastfed, are not substantially influenced by omitted variable bias.

A second concern is the possibility of pre-admission culling that is systematically related to feeding regimes, simply because admission age is mechanically correlated with the feeding regime. For instance, a child admitted at nine months of age might have been drawn from a higher point in the underlying (i.e., health-at-birth) distribution than its counterpart admitted to the hospital at a much earlier age, simply because the former child had survived for much longer outside the hospital. Insofar as admission age is related to feeding, then, we may therefore misinterpret age-at-admission effects as feeding regime effects. Because children receiving breast milk and supplementation were particularly more likely to be admitted at later ages than exclusively breastfed children, it would be useful to understand whether differences between the two groups remained when balancing on admission age.

To account for this concern, we implement an inverse probability weighting approach that balances characteristics between the exclusively-breastfed and supplemented groups. The results are presented in Appendix G in Table G.1. The average treatment effects on the treated in these specifications generally confirm the magnitude and significance of our preferred OLS regressions in Table 3, suggesting that differences in admission age and breastfeeding duration were not driving weight differences between the groups. This result also comports with the evidence from our FH-QC linked sample (Figure 2; Table 2).

4.1.4 Conclusions

The analysis of weight-for-age Z-scores in infancy highlights four key findings about infant feeding and short-run child health in our setting. First, exclusively-breastfed children had a large weight-for-age advantage over their counterparts who either were supplemented or never received breast milk, a result that survives even when considering the role of omitted variables, and when balancing on admission age and breastfeeding duration. This confirms that exclusive breastfeeding helped to keep children healthy in this setting. Second, we do not find any significant association between breastfeeding duration and weight-for-age. Although the coefficient is generally positive, it is never statistically significant. This null result indicates that the presence and exclusivity of breastfeeding may have been more important to health in infancy than breastfeeding duration. Third, we show that the time since weaning had an important influence on weight-for-age in our sample, but that this relationship was temporary. Infants observed shortly after weaning had yet to suffer the consequences of weaning fully, and were relatively better off than the most vulnerable infants who were observed between 30 and 180 days of weaning. However, infants observed more than 180 days after weaning had by then adapted to their new disease and nutritional environment, and had similar weights-for-age as infants observed within 30 days of weaning. This U-shaped pattern is derived from pooled cross-sectional data, but is nevertheless intriguing. Finally, we show that breastfeeding had a protective effect during the weaning process, with infants who were breastfed with supplementation higher weight-for-age Z-scores than infants who were never breastfed.

4.2 Mortality in Infancy

4.2.1 Empirical Strategy

Even at or after the point of weaning, these early experiences of feeding mattered for weight-for-age in infancy—but did they also affect survival over this period? Because we observe only those children who survive to admission, we cannot calculate mortality risk for children before they entered the Foundling Hospital. However, we can estimate mortality risk for children in the hospital between admission and age one. Since all of the children were weaned upon entering the Foundling Hospital, if they had not been already—and since pre-admission mortality appears not to have been selective on health at birth in ways that would confound the interpretation of the role of feeding types—this method gives us another opportunity to test whether breastfeeding provided protection to children even after weaning had occurred.

We estimate mortality risk—the probability that an individual's death occurs at a given point in time—using a Cox proportional hazard model. We again estimate a set of specifications represented by the following equations, wherein the baseline hazard function $M_{0g}(t)$ is unspecified with strata g, and the time-invariant independent variables have a proportional effect on the hazard rate:

$$M_g(t) = M_{0g}(t) \times exp(\beta_1 B_i + \beta_2 Bplus_i + \sum_{j=1}^{4} \lambda_j AgeExposed(t) + {x_i}^{'}\gamma + \epsilon_i), g = 1, 2, \ldots, k \tag{2}$$

$$M_g(t) = M_{0g}(t) \times exp(\beta_1 B_i + \beta_2 Bplus_i + \sum_{n=1}^{5} \phi_n WeaningTime(t) + \sum_{j=1}^{4} \lambda_j AgeExposed(t) + {x_i}^{'}\gamma + \epsilon_i), g = 1, 2, \ldots, k \tag{3}$$

We allow for left-truncation, since children did not become at risk of death in our data until entering the Foundling Hospital, and censoring occurs at the exact age of 366 days. Again, B_i and $Bplus_i$ are indicators that take a value of 1 when a child was exclusively breastfed or breastfed with supplementation, respectively, before entering the Foundling Hospital. ${x_i}^{'}\gamma$ are a set of individual-level, time-invariant controls that include a dummy for whether the child was born in London, and dummies for their season of birth. These variables capture differences in the disease environment before entry to the Foundling Hospital that might affect the children's latent health. They were included because they had p-values less than 0.2 in a univariate Cox model.

In addition to these time-invariant controls, we also introduce time-varying controls that capture shifts in the baseline hazard rate related to the process of weaning and aging. We use discrete time-varying controls to capture non-linearities in each variable with respect to time. We now calculate $WeaningTime(t)$ for all individuals with a known breastfeeding duration including the exclusively breastfed group. We use the same five categories as were used above in our analysis of weight-for-age; i.e., if a child is in the first 29 days since weaning, they get a value of 1 for the indicator variable. Thus, this set of variables captures changing risk as a child ages, moving farther away from its date of weaning. Importantly, because days since weaning will be correlated with admission age, especially for the never-breastfed group, we include the time-varying $AgeExposed(t)$ as a control in all specifications. It divides each child's first year of life into three-month intervals with each dummy indicating whether individual i was present in the Foundling Hospital during that age range.[27] This helps to capture the expected greater mortality risk for younger infants (Woods, 2000, p. 257), and also captures the change in environment as children moved from urban London to being fostered in the countryside.

Finally, in some specifications, we stratify the analysis by admission weight-for-age Z-score as indicated by g in Equations 2-3.[28] The strata are calculated by rounding the admission weight-for-age down to the nearest integer and using indicator variables for the nine integer groups. We include these strata to test whether our covariates of interest are still significant and meaningful when controlling for the children's health status at the beginning of their period at risk (i.e., admission).

The goal of these regressions is twofold. First, they provide an estimate of how mortality risk varied with distance from weaning, controlling for age of current exposure. Second, they help us understand whether breastfeeding provided a protective effect even after weaning had occurred.[29]

4.2.2 Results

Table 4 presents the results. Specification 1 shows that both exclusively-breastfed and supplemented children had lower risks of mortality than the never-breastfed group. The

[27]We explored constructing this as monthly or two-month time-interval dummies, and it did not substantially alter the main results.

[28]Admission weight-for-age Z-scores could not be included linearly as a covariate in the model because the variable failed proportional hazards tests.

[29]The exclusively-breastfed children in theory had two advantages. They had access to the protective effects of breast milk up until they were admitted to the hospital, and they were weaned in a much better environment than their counterparts of other feeding types. For the exclusively breastfed, nearly all of their weaning exposure would have occurred while fostered in the countryside, where the disease environment was less virulent, and there was greater access to unadulterated milk. Supplemented children, for contrast, would have some of the protective effects of breastfeeding, but they were weaned in a much harsher environment.

exclusively-breastfed coefficient may not be statistically significant, but this may be because of the smaller sample size and smaller number of deaths for this group. The coefficient on $Bplus_i$ in specification 1 suggests that supplemented infants had a 71.4% lower risk of death than never-breastfed children. These coefficients are attenuated somewhat when stratifying by weight-for-age at admission, suggesting that some of the protective effects from breastfeeding are related to the better health status of children who were ever-breastfed upon entering the Foundling Hospital, a result we have seen in the previous admission weight-for-age analysis. Specifications 3 and 4 show that breastfeeding duration prior to weaning was not associated with the mortality risk of infants after weaning. This finding bolsters our earlier results that showed that the intensive margin of breastfeeding (breastfeeding duration) seems less important than the extensive margin (having been breastfed or not).

Finally, specifications 5 and 6 add the time-varying controls for time since weaning. There is a clear decline in mortality risk associated with greater time since weaning, with children within 30 days of weaning experiencing a 10-fold greater risk of death than children more than 180 days since weaning. Interestingly, the magnitude and significance of the feeding type dummies also increases dramatically, with both exclusively-breastfed and supplemented children experiencing substantial mortality advantages over their never-breastfed peers. The increase in protection for the exclusively-breastfed is driven by their greater exposure to the highest-risk periods immediately following weaning; i.e., once we account for the (mechanically) high prevalence of exclusively-breastfed children in the early time-since-weaning categories, the overall mortality risk of exclusively-breastfed children falls dramatically. Interestingly, the feeding type coefficients do not change much when we stratify on admission weight-for-age, although the mortality risk following weaning increases substantially.

4.2.3 Conclusions

Taken together, these results show substantial protective effects on post-weaning mortality risk for children who were ever breastfed, and the fact that these coefficients remain of similar magnitude and significance when stratifying on admission weight-for-age suggests that these protective effects are unrelated to health status at admission, the point at which the children became at risk of death in our sample.[30] Again, those exclusively breastfed were weaned in a better health environment than the other children (being that their weaning took place at the Foundling Hospital rather than outside of it), so the greater protective effects for this group are likely a combination of their established immunity at weaning, and their having been weaned under better conditions. However, the substantial, though only marginally statistically significant, protective effect for the group who were breastfed with supplementation suggests that breastfeeding did ameliorate subsequent mortality risk in infancy even after the practice itself had been discontinued.

5 Do Earlier Feeding and Weaning Experiences Continue to Matter into Childhood?

Our results so far have shown that infant feeding and weaning were important factors shaping weight-for-age and mortality risk in infancy[31], and that the protective effects of breastfeeding could extend for months after children were weaned. We now test the reach of these infant feeding effects, and specifically, whether they persisted even later into childhood. To do this, we estimate the relationship between feeding regimes and mortality risk from exact ages 1 to 5 following the same empirical strategy employed in Equations 2-3. Second, we test for associations between anthropometric measures of health in mid childhood (given by weight-for-age and height-for-age Z-scores around ages 5 to 6) and measures of infant feeding, breastfeeding duration, and days outside the hospital between weaning and admission. These results suggest that there is little persistence of the association between infant feeding

[30] This is of course insofar as admission weight-for-age captures or proxies the relevant aspects of health.

[31] For weight, this means prior to entry to the Foundling Hospital and any positive intervention that implied.

characteristics and health beyond infancy.[32]

Beginning with mortality risk, we estimate variations of the following regression equation:

$$M_g(t) = M_0 g(t) \times exp(\beta_1 B_i + \beta_2 Bplus_i + \beta_3 BDur_i + \beta_4 WeaningTime(t) + \epsilon_i),$$
$$g = 1, 2, \ldots, k \qquad (4)$$

where $M_g(t)$ is the hazard rate modeling the risk of death for each individual i at time t. We left-truncate the data at exact age 1, and censoring occurs at exact age 5. We look at the typical variables described above. The main difference is that rather than including time since weaning as a set of time-varying categorical windows, we simply include time since weaning as a linear time-varying variable. This is because the original windows specified in Table 4 above are fairly meaningless in this instance, since most children were weaned long before the age of 1, making the inclusion of this information in linear form ultimately more sensible. We also include slightly different controls because by this point in childhood, birth season no longer has a p-value less than 0.2 in a univariate test. We stratify on whether a child was born in London or not because this variable failed a proportional hazards test. We also stratify in some specifications on weight-for-age at admission in a similar way described above. The results presented in Table 5 show that there is no association between mortality risk between ages 1 and 5 and either the feeding type dummies, breastfeeding duration, or time since weaning. Thus, it seems that the protective effects of breastfeeding, and the mortality risk associated with weaning, both attenuated relatively quickly, such that after infancy, they no longer significantly contributed to or ameliorated mortality risk.

[32]It is worth noting that in the analysis on childhood outcomes that follows, it is not possible to disentangle the underlying dynamics from the potential role of the Foundling Hospital as a positive intervention in health—one that could have attenuated the health disparities associated with earlier feeding experiences by offering differential returns by feeding type, or diminished the post-weaning reach of breastfeeding by improving health conditions across the board. We therefore see our childhood-era evidence as showing that early-life experiences are not deterministic of subsequent health, whether because timely intervention can overcome these disadvantages, or because these advantages would have waned even in the absence of intervention.

Our results are very similar when looking at anthropometric outcomes when the children returned to the Foundling Hospital's main London site around the age of 5 or 6 (readmission). If we substitute weight-for-age and height-for-age[33] Z-scores at readmission as the dependent variable in the analysis originally conducted for infant weight-for-age, we find no statistically significant associations between these anthropometric outcomes and either the infant feeding types or breastfeeding duration (see Tables 6 and 7).[34] For the time-since-weaning variables, rather than recalculating these to update them at the time when the children were readmitted, we keep them the same. However, the interpretation is now slightly different. They now capture the intensity of the health cost of being weaned in a bad environment (i.e., one outside the Foundling Hospital). In any case, all of these variables are statistically insignificant.

Given the substantial differences in infant weight-for-age by feeding type, it is somewhat puzzling that there are no differences between the groups by mid-childhood. One might wonder whether feeding-differential culling on the basis of underlying health sometime between admission and readmission might produce this kind of outcome, but Figure 3 shows that deaths were drawn across the distribution of weight-for-age at admission for all feeding types. Thus, culling on the basis of admission weight is unlikely to be important. If such culling was unimportant, necessarily there must have been catch-up growth between admission and re-admission to account for these differences. To show this mechanism at play, we add weight-for-age at admission interacted with feeding type to the regressions as in the equation below:

$$WAZ_{i,t} = \alpha + \beta_1 B_i + \beta_2 Bplus_i + \beta_3 BDur_i + \sum_{n=1}^{5} \phi_n WeaningTime$$
$$+\beta_4 WAZ_{i,t-1} + \beta_5 WAZ_{i,t-1} \times B_i + \beta_6 WAZ_{i,t-1} \times Bplus_i + x_i{}' \gamma + \epsilon_{i,t} \qquad (5)$$

[33]Height measures are only available only at readmission.

[34]We run similar treatment effects models as above and find few significant differences between the feeding types (not reported).

where time t is readmission and time $t-1$ is admission to the Foundling Hospital. If all children stayed on track and maintained their same weight-for-age Z-score as they aged between admission and re-admission, we would expect β_4 to equal 1. Likewise, if there was no difference in growth between feeding types, we would expect β_5 and β_6 to equal zero. However, if there were catch-up growth, then β_4 should be substantially lower than 1 and β_5 and β_6 would capture differences in catch-up growth by feeding type. There would of course be some noise in using weight-for-age at admission in the height-for-age at readmission regressions, but the same principles apply. The results are presented in specifications 5 and 6 of Tables 6 and 7. Our estimates of β_4 are far below 1, suggesting that there was substantial potential for children to experience catch-up growth in childhood. Interestingly, and although it is not always statistically significant, β_5, the interaction between admission weight-for-age and the exclusively breastfed dummy, is positive. This suggests that catch-up growth was not as strong for the exclusively-breastfed group, eliminating any advantage they once had in infancy. Figure 4 presents these results graphically, and shows a clear gap in catch-up growth between the exclusively breastfed and the other groups. In fact, the never-breastfed group outperformed the exclusively breastfed in terms of weight-for-age at readmission at all levels of admission weight-for-age.

The lack of any associations between either infant feeding or weaning, and health in childhood, concurs with our findings that the health costs of weaning were already weakening in infancy. Although mortality risk was markedly higher shortly after weaning, it declined rapidly as children aged and moved farther away in time from weaning (Table 4), with no statistically significant difference in mortality risk after two months. We also find that children had begun to recover from the health costs of weaning within 180 days post-weaning, a point at which their weight-for-age was no different than children admitted within 30 days of weaning. Although there were no indications that the protective effects of breastfeeding were declining across infancy, they were clearly no longer important by childhood. This may be because children had adapted to the pathogens introduced with supplementary foods, or that the antibodies passed from the mother to the child were no longer active.

6 Conclusion

This study analyzes the health consequences of infant feeding and weaning in industrializing Britain using a novel, historical cohort dataset based on the records of the London Foundling Hospital (1892-1919). We find that children receiving breast milk in infancy had higher weight-for-age and lower mortality risk in infancy, even after the cessation of breastfeeding—with exclusively-breastfed children faring best of all. Breastfeeding duration, however, appears to have mattered very little. Weight-for-age had a U-shaped relationship with time since weaning, suggesting that the experience of weaning led to short-term declines in weight-for-age, followed by almost complete recovery. However, by mid-childhood, the anthropometric advantage of breastfed children had attenuated, and they no longer experienced lower mortality risk. Together, our findings suggest that in this setting, while exclusivity in breastfeeding was intrinsically valuable to post-weaning health, some breast milk was still better than none. Put another way, the extensive margin of breastfeeding mattered, even if there was little role for its intensive one. Nevertheless, our results also show that substantial catch-up was possible among the never-breastfed, such that the early post-weaning advantages of ever-breastfed children were not persistent over the long run.

These findings speak to two issues. First, they add to the evidence that the health benefits of having been breastfed can extend beyond the cessation of breastfeeding. The fact that these benefits appear to be largely independent of net nutritional status in infancy suggests that maternal antibodies may be an important mechanism—at least in a low-income, pre-antibiotic, and relatively unsanitary environment like ours. In such a setting, which shares many of these features with modern developing countries, the benefits of having received maternal antibodies may well be greater than in more affluent contexts. In order to continue to unpack the mechanisms behind the relationship between early-life feeding practices and post-weaning health, more research is needed from a diverse set of empirical settings.

In addition, our results provide insight into historical demography. Specifically, they shed light on how infant feeding may have influenced long-run health across the epidemiologic transition, with a focus on anthropometric measures which, due to a previous lack of rich individual-level data, have not been studied in relation to historical breastfeeding. Our result that breastfeeding provided a protective effect even after weaning yet again highlights the importance of infant feeding for infant mortality in the early twentieth century (Reid, 2002). Given that mothers of illegitimate children were much less likely to breastfeed them (Fildes, 1992), our results also help explain why illegitimate children at the time might have been at such high risk of infant death. Woods (2000, pp.283-89) has argued that changes in infant feeding practices are unlikely to explain the decline in infant mortality in Britain at the turn of the twentieth century. While our results do not dispute Woods's conclusion, they do suggest that the decline in breastfeeding rates across the twentieth century likely slowed the decline in infant mortality rates. This would be especially true in the first half of the twentieth century, when individual and household hygiene practices were changing, food safety guidelines were still being established, and antibiotics were not widely available (Atkins, 1992). With respect to historical trends in stature, however, the interpretation is even clearer: our results suggest that changes in breastfeeding rates were unlikely to have influenced the secular increase in adult height over the twentieth century, since we find that anthropometric differences based on weight in infancy had disappeared by mid-childhood. Thus, when it comes to changes in child growth over time, changes in other health inputs, such as the disease environment and later-childhood nutrition (see, e.g., Bailey et al. (2016); Gao and Schneider (2020); Hatton (2011)), were likely more important than those in infant feeding.

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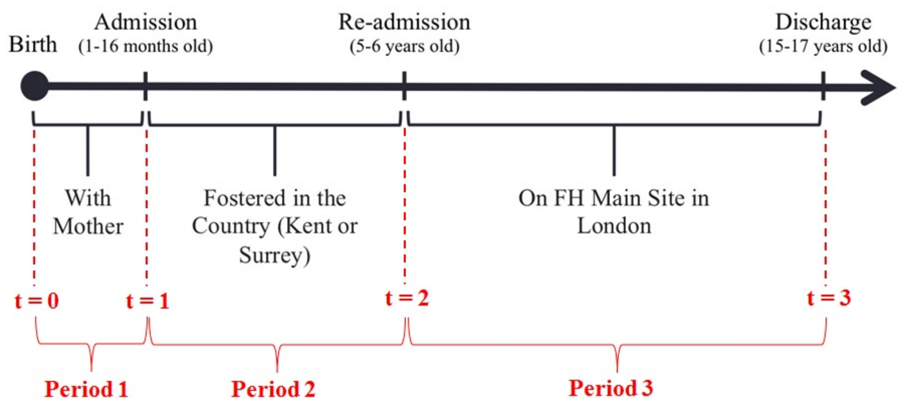
Figure 1: Schematic of children's lives in the Foundling Hospital



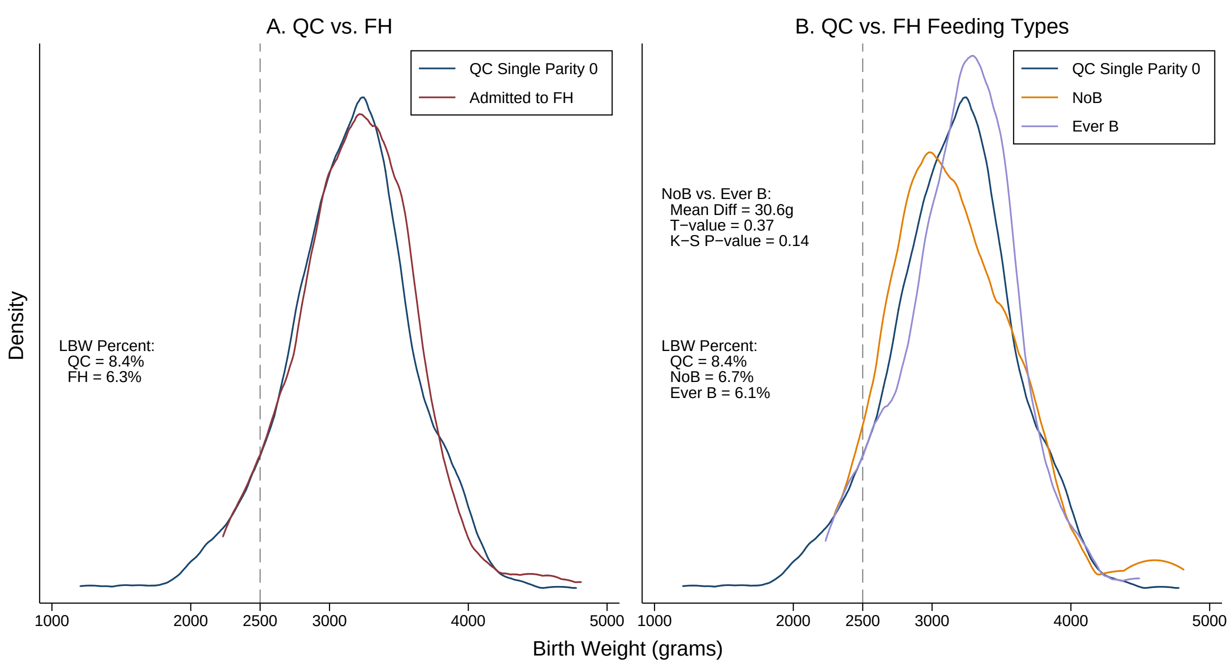
Notes: The schematic diagram indicates the key milestones (i.e., admission, re-admission, discharge) and periods (i.e., birth to admission, admission to re-admission, and re-admission to discharge) over which health data were recorded by hospital staff. This paper focuses on health up to the end of Period 2.

Table 1: Breastfeeding incidence among infants in the Foundling Hospital compared with British towns in the early twentieth century

		Feeding Type		
	Years	B	B+	NoB
Foundling Hospital	1892-1914	10.1	47.8	42
Salford (Greengates)	1902-07	82.3	7.9	9.9
Salford	1907-10	80.9	11.3	8
Stockport	1903-11	72.2	8.4	19.4
Derby	1900-07	67.7	14.5	17.8
Brighton	1903-05	62.7	15.3	22
Birmingham	1908-09	55.7	21.8	22.5

Notes: B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed. The surveys conducted in the various towns used different sampling methodologies targeting working class households and classified infant feeding in slightly different ways. They refer to the incidence of the three feeding types among children under one year, but they do not account for breastfeeding duration to approximate prevalence. The rates in Salford are especially high because the data were collected by health visitors offering post-natal care, leading them to oversample early infancy. Breastfeeding data for the historical UK is very difficult to find, so this is the best available evidence to date (Woods, 2000, p. 285). Note that Atkins (2003) discusses a new sample, which draws on Medical Officer of Health data from 130 Local Authorities, that may offer an even richer picture of historical breastfeeding in the UK than has hitherto been available. However, this analysis is still in progress, and to date has focused on city-specific case studies.
Sources: Foundling Hospital Dataset - see Appendix A; Woods (2000, p. 287).

Figure 2: Comparison of birth weight distributions of children born in the Queen Charlotte Hospital to single, primiparous mothers: infants later admitted to Foundling Hospital versus all other QC infants



Notes: See Appendix F for full details for the Queen Charlotte Hospital sample. Feeding types were only recorded for children who were admitted to the Foundling Hospital.
Sources: Foundling Hospital Dataset - see Appendix A.

Table 2: Queen Charlotte Sample: Birth weight regressions comparing children of different feeding types born in the Queen Charlotte Hospital and admitted to the Foundling Hospital, relative to non-foundling children born in the Queen Charlotte Hospital

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	Full	Single Parity 0	Single Parity 0	Single	Parity 0	Full	Full
Ever B	69.25*	64.34	54.03	52.28	55.99	54.54	53.51
	(41.03)	(42.77)	(50.91)	(50.93)	(49.39)	(49.78)	(50.03)
NoB	38.64	28.06	33.58	31.57	48.72	50.20	48.91
	(74.47)	(76.42)	(68.91)	(69.05)	(70.58)	(73.08)	(73.46)
Queen Charlotte-Only (non-FH)	(ref)	(ref)	(ref)	(ref)	(ref)	(ref)	(ref)
Additional Controls:							
Male	No	No	Yes	Yes	Yes	Yes	Yes
Mother's Age Dummies	No	No	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes
Birth Season Dummies	No	No	Yes	Yes	Yes	Yes	Yes
Parity of Child	No	No	No	Yes	No	Yes	Yes
Mother's Marital Status	No	No	No	No	Yes	Yes	Yes
Marital Status - Parity Interactions	No	No	No	No	No	No	Yes
N	1329	727	724	731	911	1308	1308
R-square	0.002	0.003	0.116	0.115	0.123	0.105	0.108

Notes: Coefficients with robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Sources: Foundling Hospital Dataset - see Appendix A.

Table 3: Weight-for-Age Z-Scores in Infancy

	(1) All	(2) All	(3) Ever Breastfed	(4) Ever Breastfed	(5) Ever Non-Breastfed
B	0.933***	0.847***			
	(0.185)	(0.183)			
B+	0.400***	0.407***			0.234
	(0.117)	(0.116)			(0.168)
NoB	(ref)	(ref)			(ref)
Breastfeeding Duration (days)			0.001	0.002	
			(0.002)	(0.003)	
Less than 30 days since weaning					0.533*
					(0.298)
30-59 days since weaning					0.135
					(0.239)
60-89 days since weaning					(ref)
90-179 days since weaning					0.087
					(0.235)
180 or greater days since weaning					0.472**
					(0.239)
Constant	-2.663***	-2.173***	-2.636**	-1.997*	-2.897***
	(0.516)	(0.541)	(1.124)	(1.178)	(0.653)
Admission Age Dummies	No	Yes	No	Yes	No
Individual-Level Controls	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Birth District Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	996	996	330	330	652
R-square	0.265	0.314	0.417	0.514	0.348
Treated Variable for OVB Check	B	B	B Dur	B Dur	B+
Bias-adjusted β when $\delta = 1$ and $R_{max} = 1.3\tilde{R}$	1.112	0.979	0.001	0.004	0.168
δ which produces $\beta = 0$ with $R_{max} = 1.3\tilde{R}$	-16.12	90.10	2.54	2.29	2.23
Treated Variable for OVB Check	B+	B+			
Bias-adjusted β when $\delta = 1$ and $R_{max} = 1.3\tilde{R}$	0.502	0.516			
δ which produces $\beta = 0$ with $R_{max} = 1.3\tilde{R}$	-16.07	-20.04			

Notes: Coefficients with robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed. Specifications 3, 4 and 5 restrict the sample to individuals with known values of breastfeeding duration or days since weaning. Individual-level controls include dummies for male, birth location type, birth season, mother's age and father's class. We do not include monthly admission age dummies in Specification 5 because these are collinear with time since weaning categories. However, there were not substantial differences in admission age between the supplemented and never breastfed groups, so this should not be problematic.
Sources: Foundling Hospital Dataset - see Appendix A.

Table 4: Cox Models, Mortality in Infancy (Ages Admission-1)

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	All	Ever Breastfed	Ever breastfed	All	All
B	-0.618	-0.390	-0.451	-0.466	-1.352**	-1.319**
	(0.407)	(0.411)	(0.546)	(0.589)	(0.578)	(0.622)
B+	-0.539**	-0.425*	(ref)	(ref)	-0.712*	-0.733*
	(0.260)	(0.256)			(0.410)	(0.411)
NoB	(ref)	(ref)			(ref)	(ref)
Breastfeeding Duration (days)			-0.000	0.000		
			(0.007)	(0.007)		
Days Since Weaning (time-varying):						
Less than 30 days					2.346**	2.812**
					(1.108)	(1.193)
30-59 days					1.298	1.629*
					(0.900)	(0.956)
60-89 days					1.105	1.016
					(0.794)	(0.829)
90-179 days					0.963	0.990
					(0.602)	(0.620)
180 or greater days					(ref)	(ref)
Strata:						
Admission WAZ	No	Yes	No	Yes	No	Yes
Additional Controls:						
Age Exposed (time-varying)	Yes	Yes	Yes	Yes	Yes	Yes
London Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Birth Season Dummies	Yes	Yes	Yes	Yes	Yes	Yes
N Subjects	953	953	309	309	717	717
N Deaths	75	75	18	18	62	62
Model Chi-square	19.80	20.08	7.38	9.81	22.53	28.35
Prop. Hazard - Global Test Chi-square	6.71	7.12	3.91	3.33	5.70	6.18
Prop. Hazard - Global Test p-value	0.667	0.624	0.917	0.950	0.956	0.939

Notes: Coefficients with robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed. Age exposed and days since weaning are time-varying categorical variables. Age exposed indicates whether a child was present in the Foundling Hospital at roughly 3 month intervals. Admission WAZ is the weight-for-age Z-score for each child at admission. The strata are constructed by rounding admission WAZ down to the nearest integer. This produces nine strata on admission WAZ. We test whether the scaled Schoenfeld residuals have a nonzero slope versus time (proportional hazards test) for all specifications, and we canot reject the null hypothesis of proportional hazards on global as well as variable-specific tests.
Sources: Foundling Hospital Dataset - see Appendix A.

Table 5: Cox Models, Mortality in Early to Mid-Childhood (Ages 1-5)

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	All	Ever Breastfed	Ever Breastfed	All	All
B	-0.510	-0.605	-0.771	-0.843	-0.330	-0.420
	(0.769)	(0.760)	(0.816)	(0.795)	(0.786)	(0.802)
B+	0.196	0.127	(ref)	(ref)	0.409	0.337
	(0.429)	(0.430)			(0.547)	(0.573)
NoB	(ref)	(ref)			(ref)	(ref)
Breastfeeding Duration (days)			-0.002	-0.002		
			(0.004)	(0.005)		
Days Since Weaning (time-varying)					0.002	0.002
					(0.004)	(0.004)
Strata:						
Admission WAZ	No	Yes	No	Yes	No	Yes
London	Yes	Yes	Yes	Yes	Yes	Yes
N Subjects	952	952	333	333	721	721
N Deaths	26	26	13	13	26	26
Model Chi-square	0.82	0.84	1.09	1.25	1.09	1.06
Prop. Hazard - Global Test Chi-square	1.58	1.66	2.71	2.88	3.02	3.13
Prop. Hazard - Global Test p-value	0.454	0.435	0.258	0.236	0.388	0.372

Notes: Coefficients with robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed. Days since weaning is a time-varying linear variable. Admission WAZ is the weight-for-age Z-score for each child at admission. The strata are constructed by rounding admission WAZ down to the nearest integer. This produces nine strata on admission WAZ. We test whether the scaled Schoenfeld residuals have a nonzero slope versus time (proportional hazards test) for all specifications, and we canot reject the null hypothesis of proportional hazards on global as well as variable-specific tests.
Sources: Foundling Hospital Dataset - see Appendix A.

Table 6: Mid-Childhood Weight-for-Age Z-Scores

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	Ever Breastfed	Ever Non-Breastfed	All	All	All
B	-0.093	-0.098		-0.039	-0.134	-0.001
	(0.113)	(0.154)		(0.220)	(0.143)	(0.232)
B+	0.042	(ref)	0.095	-0.016	-0.118	-0.184
	(0.075)		(0.108)	(0.139)	(0.129)	(0.211)
NoB	(ref)		(ref)	(ref)	(ref)	(ref)
Breastfeeding Duration (days)		0.001		0.001		0.001
		(0.001)		(0.001)		(0.001)
Days btw Weaning & Admission:						
Less than 30 days			-0.204	-0.159		-0.191
			(0.198)	(0.195)		(0.190)
30-59 days			0.057	0.060		0.033
			(0.163)	(0.152)		(0.150)
60-89 days			(ref)	(ref)		(ref)
90-179 days			-0.221	-0.192		-0.193
			(0.147)	(0.141)		(0.138)
180 or greater days			-0.104	-0.071		-0.135
			(0.156)	(0.152)		(0.145)
Admission WAZ					0.195***	0.182***
					(0.035)	(0.037)
Admission WAZ × B					0.098	0.121*
					(0.061)	(0.066)
Admission WAZ × B+					-0.040	-0.073
					(0.050)	(0.069)
Constant	-1.120***	-1.401*	-0.789**	-0.942***	-0.600**	-0.420
	(0.307)	(0.761)	(0.381)	(0.355)	(0.290)	(0.352)
Additional controls:						
Individual-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	850	290	548	639	849	638
R-square	0.296	0.579	0.350	0.336	0.371	0.404

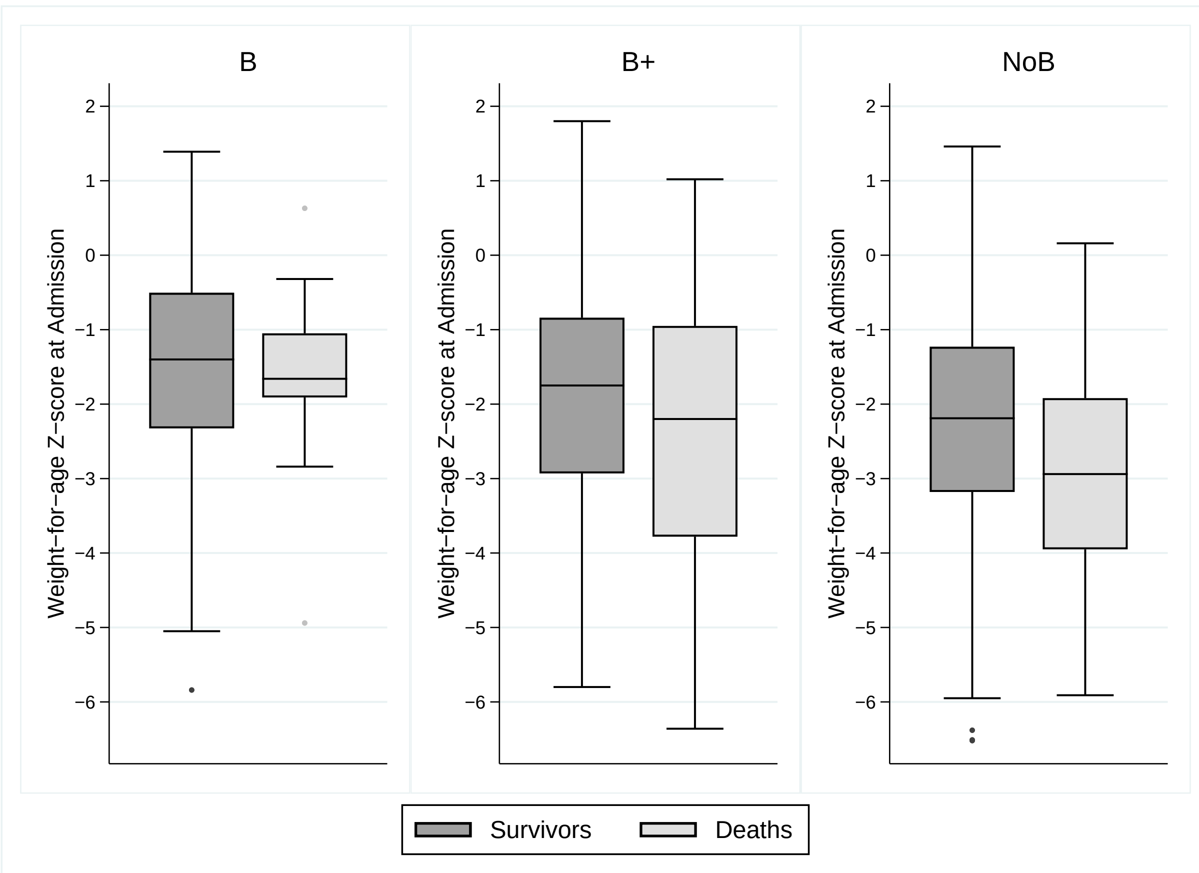
Notes: Coefficients with robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed. Individual-level controls include dummies for male, re-admission age, birth location type, birth season, mother's age and father's class. We do not include admission age by month dummies in these specifications, but there is no meaningful difference in the results if we include them.
Sources: Foundling Hospital Dataset - see Appendix A.

Table 7: Mid-Childhood Height-for-Age Z-Scores

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	Ever Breastfed	Ever Non-Breastfed	All	All	All
B	-0.029	0.026		-0.105	0.012	-0.017
	(0.152)	(0.206)		(0.280)	(0.181)	(0.284)
B+	-0.047	(ref)	-0.077	-0.194	-0.159	-0.236
	(0.093)		(0.135)	(0.194)	(0.146)	(0.226)
NoB	(ref)		(ref)	(ref)	(ref)	(ref)
Breastfeeding Duration (days)		0.001		0.001		0.001
		(0.001)		(0.001)		(0.001)
Days btw Weaning & Admission:						
Less than 30 days			0.022	0.094		0.058
			(0.268)	(0.266)		(0.259)
30-59 days			0.219	0.247		0.223
			(0.227)	(0.220)		(0.218)
60-89 days			(ref)	(ref)		(ref)
90-179 days			0.007	0.056		0.037
			(0.199)	(0.198)		(0.190)
180 or greater days			-0.156	-0.091		-0.174
			(0.196)	(0.197)		(0.190)
Admission WAZ					0.186***	0.184***
					(0.044)	(0.048)
Admission WAZ × B					0.146	0.150
					(0.092)	(0.097)
Admission WAZ × B+					-0.016	-0.017
					(0.063)	(0.088)
Constant	-1.522***	-2.235**	-1.341***	-1.469***	-1.010***	-0.890**
	(0.385)	(0.960)	(0.500)	(0.453)	(0.372)	(0.449)
Additional controls:						
Individual-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	852	290	550	641	851	640
R-square	0.276	0.521	0.334	0.339	0.330	0.392

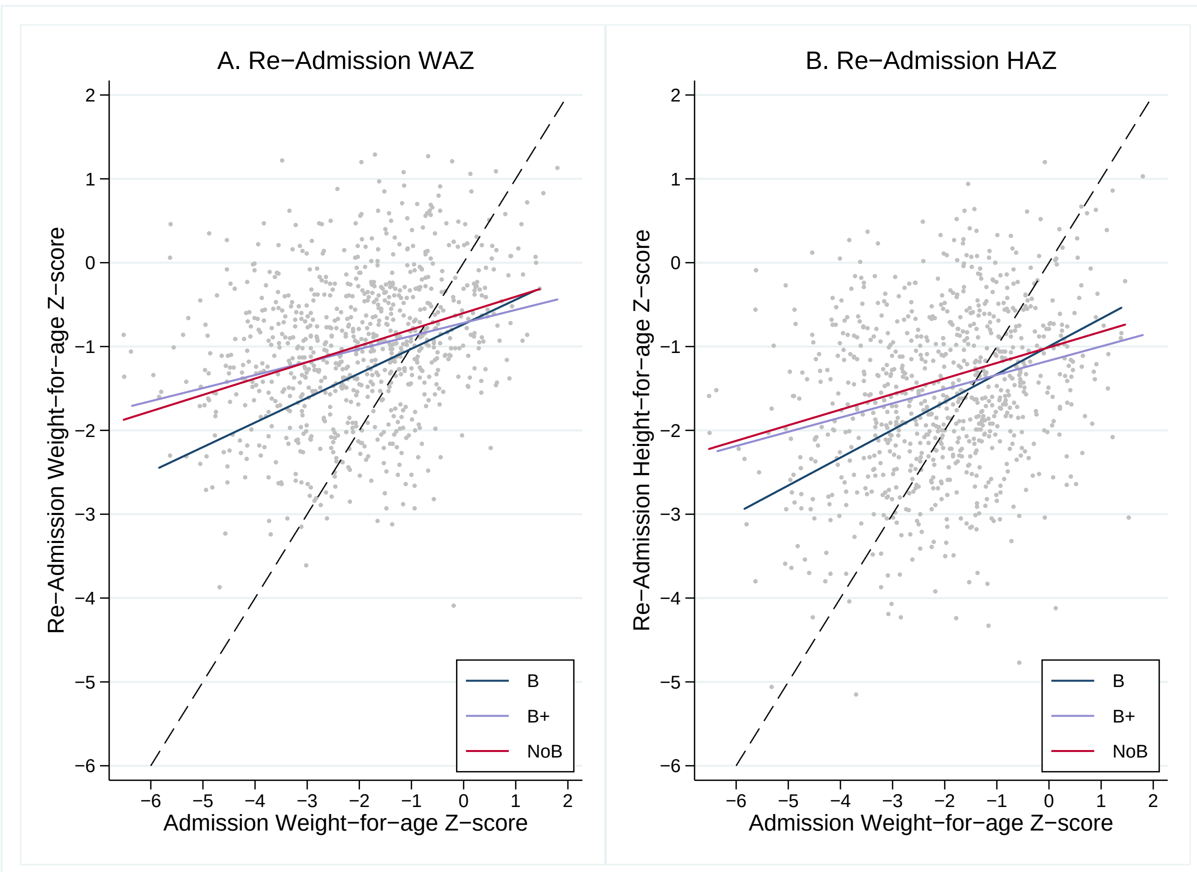
Notes: Coefficients with robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed. Individual-level controls include dummies for male, re-admission age, birth location type, birth season, mother's age and father's class. We do not include admission age by month dummies in these specifications, but there is no meaningful difference in the results if we include them.
Sources: Foundling Hospital Dataset - see Appendix A.

Figure 3: Admission weight-for-age Z-score distributions of children who survived to re-admission versus those who died before re-admission



Notes: B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed.
Sources: Foundling Hospital Dataset - see Appendix A.

Figure 4: Predicted association between weight- and height-for-age Z-scores at re-admission and admission weight-for-age Z-scores across the three feeding types



Notes: The regression lines depicted in the graph are predicted from Specification 5 in Tables 6 and 7. Thus, this is simply the graphical representation of the regression results. The underlying data are provided as a reference. The black dashed line (at $y = x$) shows where the regression lines would have been if children had simply stayed at their admission Z-score. B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed.
Sources: Foundling Hospital Dataset - see Appendix A.

Online Appendices

A Data Sources and Structure of the Dataset

As mentioned in the main text, the Foundling Hospital Cohort Dataset is to our knowledge the oldest cohort study providing evidence of child and adolescent health. We have carefully reconstructed the dataset from the administrative records collected by administrators at the Foundling Hospital. In order to gather a complete set of information about each child, we have linked four separate sets of records (see Table A.1 for the full details). The backbone of the dataset is the medical record, which was initiated by the medical officer, William J. Cropley Swift, after he was promoted from assistant medical officer. Beginning with children being admitted to the Foundling Hospital in 1893, he measured variables related to the children's health at three life stages (infancy, mid-childhood and adolescence) and recorded information about their health for the periods in between as well.[35] Medical information including extensive anthropometric measurements was recorded about the children at three points in their lives mirroring the life stages of a foundling child presented in Section 3.1 of the main text and described in Figure 1: when they were admitted to the hospital, when they returned from the country around the age of five, and when they were discharged from the hospital. Swift also recorded diseases that the children were treated for while fostered in the countryside and in the hospital and importantly for this study information about the feeding regime in infancy. When children died, the dates, locations and causes of death were also recorded in the medical record.

We supplement this medical data with information about the children's circumstances at birth drawn from two sources: the petitions of mothers to the hospital and the register of applications, which recorded similar information after 1909. From these sources, we can learn the mother's age and address, the father's occupation, where the child was born (i.e.

[35] Several of the children admitted in early 1893 were born in 1892, which is why our data cover birth cohorts from 1892.

in a hospital, at home or at a relative's house), when the mother last saw the father and what happened to the father. Thus, from this information we are able to calculate a rich set of socioeconomic characteristics for each child. We converted the father's occupations into HISCO codes and used the simplified version of the HISCLASS system to control for socioeconomic background in the regressions (Maas and van Leeuwen, 2005; van Leeuwen and Maas, 2011; van Leeuwen et al., 2002). Table A.2 reports the number and percentage of children in each class, highlighting that the children were not solely drawn from working-class fathers. Mother's occupations were not systematically recorded on the petitions, but in some cases the mother's occupation was given in the notes taken by the Foundling Hospital administrators when confirming the veracity of the petition. Scholars studying these occupations have found that mothers were mostly working in domestic service or other service sector employment (Campbell-Johnston, 2016; Sheetz-Nguyen, 2012). We also geo-located the mothers' places of residence and children's places of birth so that we could link these to registration districts. It was not always possible to find precise addresses for the addresses because of changes in London addresses over the past century, but if the recorded street no longer existed, we assigned the residence at the centroid of the sub-borough neighbourhood listed. We then include registration district fixed effects in some regression specifications to account for differences in environmental circumstances that varied with geography and also link to infant mortality rate in the registration district of birth for those children living in London before admission from the Registrar General Quarterly Reports.[36]

In addition to the children's details at birth, we have also collected information on the diseases that children were treated for on the Foundling Hospital main site in London from mid-childhood to adolescence. This information was drawn from weekly infirmary reports, but we do not use this data in the current paper.

Taken together, these four sources provide a wealth of socioeconomic and health data for the foundling children across their childhood and adolescence. All of the records cited

[36]We are grateful to Graham Mooney for providing us with this London IMR data.

in Table A.1 are held at the London Metropolitan Archives, but (particularly because many birth cohorts are commingled within the same physical document) substantial portions of these records (including the entire medical record) are currently closed to general access under a 110-year rule to protect the private, sensitive information of foundling children who may still be alive. We received special permission from Coram, the Foundling Hospital's successor, to view and analyze these records, but in line with our agreement with Coram, we are not able to publish or share the data.

Having described the information available in the Foundling Hospital records and where it came from, we can now turn to the structure of the dataset. As mentioned above, the medical officer William J. Cropley Swift started keeping the medical register in 1893, so children admitted in this year mark the beginning of our sample. However, it is less clear how the record keeping ended. The final information for children at discharge was recorded on 16 April 1919, whereas the last information on children at re-admission was recorded on 5 May 1919. However, because discharge occurred roughly ten years after re-admission, we have substantially more data for children up to re-admission than we do for children up to discharge. Panel A of Figure A.1 displays this point graphically by producing a lexis diagram for the complete sample, with each line representing a different child. The lines begin when a child enters observation upon entry to the hospital and end when the child leaves observation either by being discharged from the hospital in adolescence, being restored to a family member, or dying while under the hospital's care. Panel A clearly shows that the abrupt end to record keeping in 1919 censors our data and limits the information we have for children at all three life stages to a smaller group in our sample.

To understand the differences in information available, it is helpful to split the sample into two cohorts: a full-age cohort and an early-age cohort. The full-age cohort, displayed in Panel B of Figure A.1, consists of all children who were admitted to the hospital after the first child with complete information at all three age milestones and before the final child with information at all three age milestones. This allows us to precisely identify attrition in our sample and discuss how it might influence our results. The early-age cohort, displayed in Panel C, includes all children who were admitted to the hospital after the first child with complete information at admission and re-admission and before the final child with information at both ages. Thus, the early-age cohort contains more children and covers a wider range of birth years than the the full-age cohort. In this paper, all of the analysis is conducted on the early-age cohort.

There are three sources of attrition in our sample. These are reported in detail in Table A.3. The most prevalent is child deaths responsible for removing around 11-13% of the sample. A small number of children were also restored to their mother or another relative who came to claim them. The third source of attrition is equivalent to loss to follow up in modern cohort studies and occurs around the cutoff dates for each cohort. Because re-admissions were staggered across various ages, among the last children in the cohort, there were a few children who were admitted prior to the final child that we observe at re-admission who had not been re-admitted when the final child was re-admitted. Thus, we do not observe these children's re-admission information despite the fact that they are within the censoring dates of the cohort. We have tested whether attrition in general or specific to any of the three types is related to any individual characteristics. We find significant effects for deaths, which we expect and are one of our main outcomes of interest, but there are no significant relationships between individual characteristics and the other forms of attrition (not reported). The attrition in the full-age and early-age cohorts are represented graphically in the lexis diagrams in Panels D and E of Figure A.1.

Table A.1: Structure and sources of the Foundling Hospital Dataset

Life Stage	Years	Source (LMA Reference)	Socioeconomic/Administrative Information	Health Information
Birth	1892-1908	FH Petitions (A/FH/A/8/1/2/102-117)	Mother's approximate address Mother's age Where child was born Father's occupation When mother last saw father What became of father	Child's birthday Child's sex
	1909-1914	Register of applications (A/FH/A/8/5/1)	Mother's approximate address Mother's age Father's occupation	Child's birthday Child's sex
	1892-1908	Registers of In-Patients Queen Charlotte Hospital (H27/QC/B/1/8-13)	Mother's marital status Mother's age	Child's birthday Child's sex Child's parity Birth weight Birth length
Pre-admission to FH (0-1 year old)	1892-1914	Medical Record (A/FH/A/18/15/1)		Infant feeding practice (breast, milk or food) Duration of breastfeeding
Admission to FH (around 0.37 years old)	1893-1914	Medical Record (A/FH/A/18/15/1)	Admission date Hospital number Admission age	Child's birthday Child's sex Weight Subjective nutritional assesment Vaccinated Diseases present at entry
Time Fostered in Country (1-6 years old)	1893-1919	Medical Record (A/FH/A/18/15/1)	County child was fostered in	Diseases child was treated for in country
Return from Country to FH (around 6 years old)	1897-1919	Medical Record (A/FH/A/18/15/1)	Re-admission date Re-admission age	Weight Height Subjective nutritional assesment Eye exam Ear exam
Time Resident in FH (6-17 years old)	1897-1919	Medical Record (A/FH/A/18/15/1)	School standard	Diseases child was treated for in hospital Re-vaccinated
	1897-1915	Weekly Infirmary Reports (A/FH/A/18/5/30-35)		All diseases child was treated for in infirmary Complications from diseases Dates of entry to and exit from the infirmary Duration of each sickness event
Discharge from FH (around 15-17 years old)	1907-1919	Medical Record (A/FH/A/18/15/1)	Discharge date Discharge age Employment after discharge	Weight Height Subjective state of health
		Other Life Events		
Restored to Parents (any age)	1892-1919	Medical Record (A/FH/A/18/15/1)	Date of restoration Who child was restored to	
Deaths (any age)	1892-1919	Medical Record (A/FH/A/18/15/1)	Date of death Place of death	Cause of death

Table A.2: Father's occupations of children in the Foundling Hospital Dataset

Condensed HISCLASS Class	Father's Occupations	Number of Admissions	% of Admissions
1	Higher managers and professionals	56	5.3
2	Lower professionals, clerical and sales personnel	256	24.1
3	Foremen and skilled workers	209	19.7
4	Farmers and fishermen	11	1.0
5	Lower-skilled workers	328	30.9
6	Lower-skilled and unskilled farm workers	41	3.9
7	Unskilled workers	105	9.9
Unknown	Occupation not listed or unclassified	56	5.3
All		1062	100.0

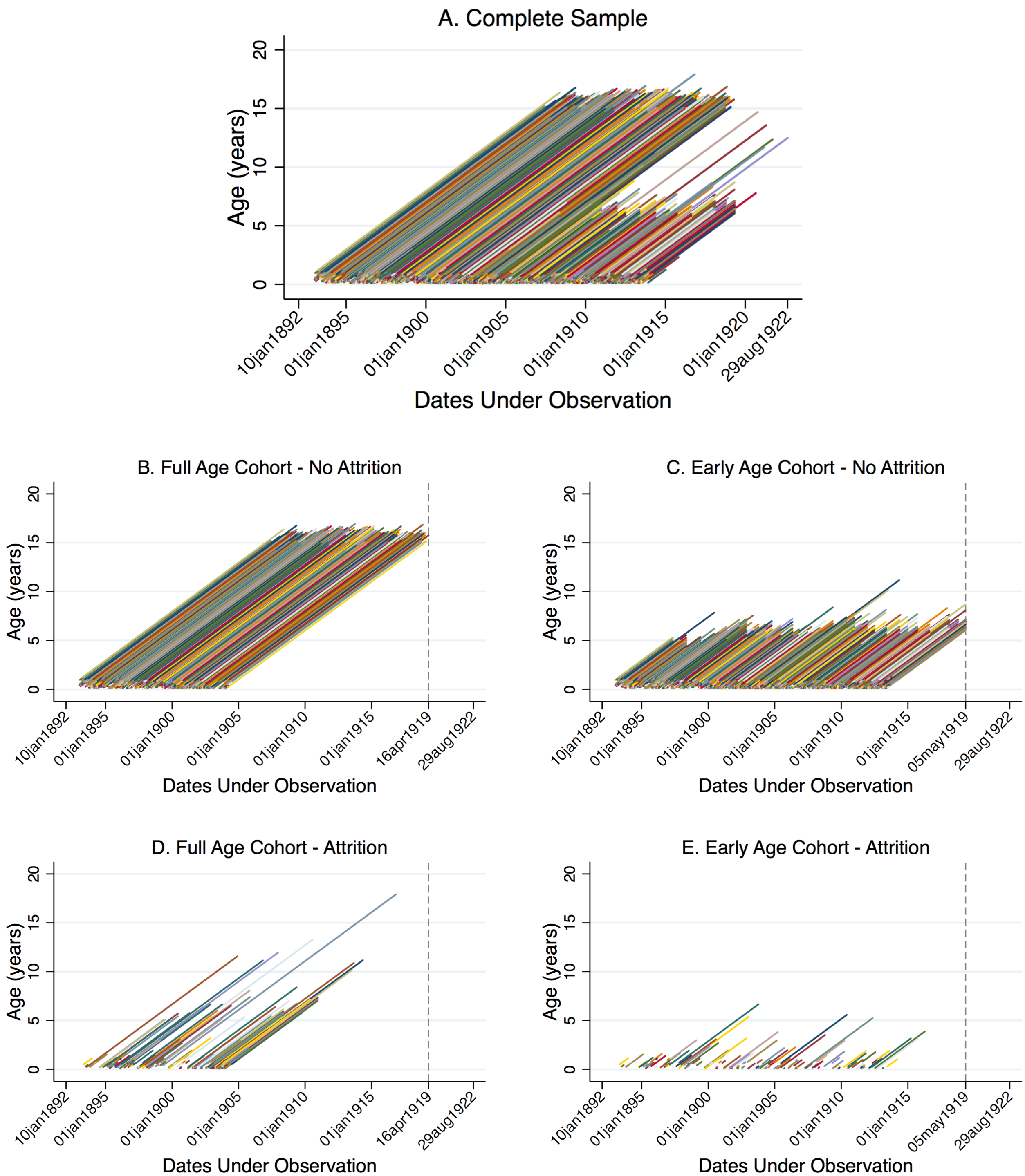
Notes: We use a condensed version of the HISCLASS occupational class system recommended by Maas and van Leeuwen (2005).
Sources: Foundling Hospital Dataset - See Appendix A; van Leeuwen and Maas (2011).

Table A.3: Attrition tables for the Foundling Hospital Dataset

Life Stage	Children Observed	Deaths	Restored	Lost to Follow-up	Total Observations
Admission	1032	0	0	0	1032
	100.0%	0.0%	0.0%	0.0%	100.0%
Re-admission	890	111	14	17	1032
	86.2%	10.8%	1.4%	1.6%	100.0%

Notes: Restored children were returned to their parents. The lost at follow up category relates to children who did not have information recorded at a certain cut-off date but were admitted to the Foundling Hospital during the period between the first admission and final admission with information recorded at re-admission.
Sources: Foundling Hospital Dataset - See Appendix A.

Figure A.1: Lexis diagrams illustrating the full sample, each of the cohorts and attrition in each cohort



Notes: See text in Appendix A for definitions of the cohorts.
Sources: Foundling Hospital Dataset - See Appendix A.

B The Founding Hospital in Context: Child Growth

As mentioned in the main text and in Appendix A, the medical officer reported heights and weights of children in the Foundling Hospital at each of the three major stages in the Foundling Hospital, reported in Figure 1. In addition, we were able to link 160 children to their medical records in the Queen Charlotte Hospital in order to gain information about their birth weights and lengths. The Queen Charlotte Hospital data is described in detail in Appendix F. Table B.1 reports descriptive statistics for the anthropometric measures: see Schneider (2015) for a discussion about using the WHO growth standards and reference with historical data. Child weight-for-age Z-scores fall between birth and admission, but then children experience catch-up growth in weight between admission and re-admission. Figure B.1 shows the distributions of the anthropometric measures from admission onwards by feeding type.

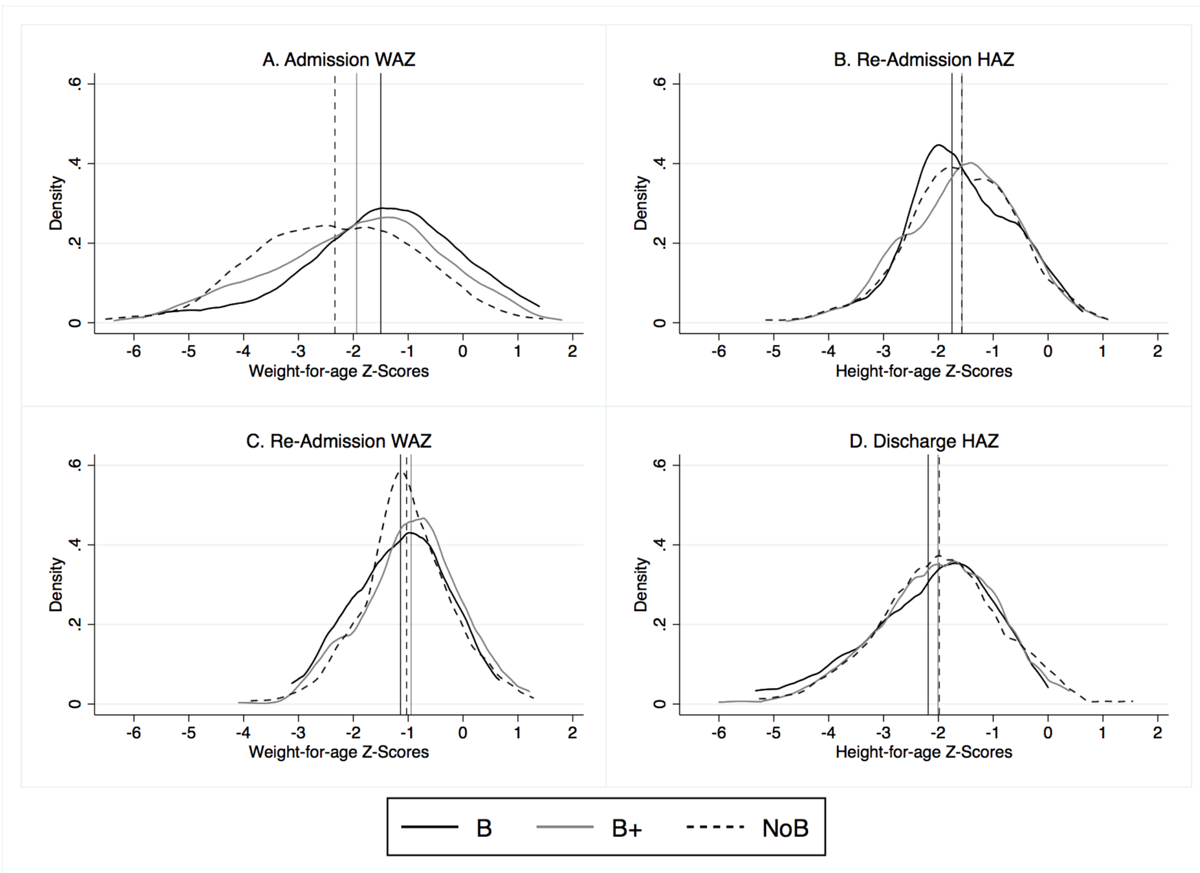
The birth weights are roughly similar to other mean birth weights in Northwest European countries (Schneider, 2017) and compared to other children born at the Queen Charlotte Hospital who were not admitted to the Foundling Hospital (see Appendix F for details). However, by mid-childhood, the Foundling children were a centimeter or two taller than the average London child in the early twentieth century (Cameron, 1979). They were also substantially taller than children admitted into the West London School District Poor Law School, which admitted children whose parents were in the workhouse (Schneider, 2016). This suggests that the Foundling Hospital children were relatively representative of London children at birth, but perhaps had a more salubrious upbringing than the typical child in London since they were fostered in the countryside and had much greater access to medical care.

Table B.1: Descriptive statistics of anthropometric measures of Foundling Hospital children at various ages

	Age (years)	Obs	Mean	Std. Dev.	Min	Max
Birth Weight (g)	0	160	3,205	434	2,233	4,812
Birth WAZ	0	160	-0.24	0.91	-2.44	3.03
Birth Length (cm)	0	160	53.2	2.6	48.3	62.2
Admission WAZ	0.25-1	1,031	-2.06	1.50	-6.52	1.80
Re-Admission WAZ	4-5	883	-1.00	0.87	-4.09	1.29
Re-Admission HAZ	4-5	886	-1.59	1.07	-7.80	3.21
Discharge HAZ	15-17	387	-2.01	1.11	-6.00	1.55

Sources: Foundling Hospital Dataset - See Appendix A.

Figure B.1: Distributions of weight- and height-for-age Z-scores of Foundling Hospital children at various ages



Notes: Vertical lines show the means of each distribution. B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed.
Sources: Foundling Hospital Dataset - see Appendix A.

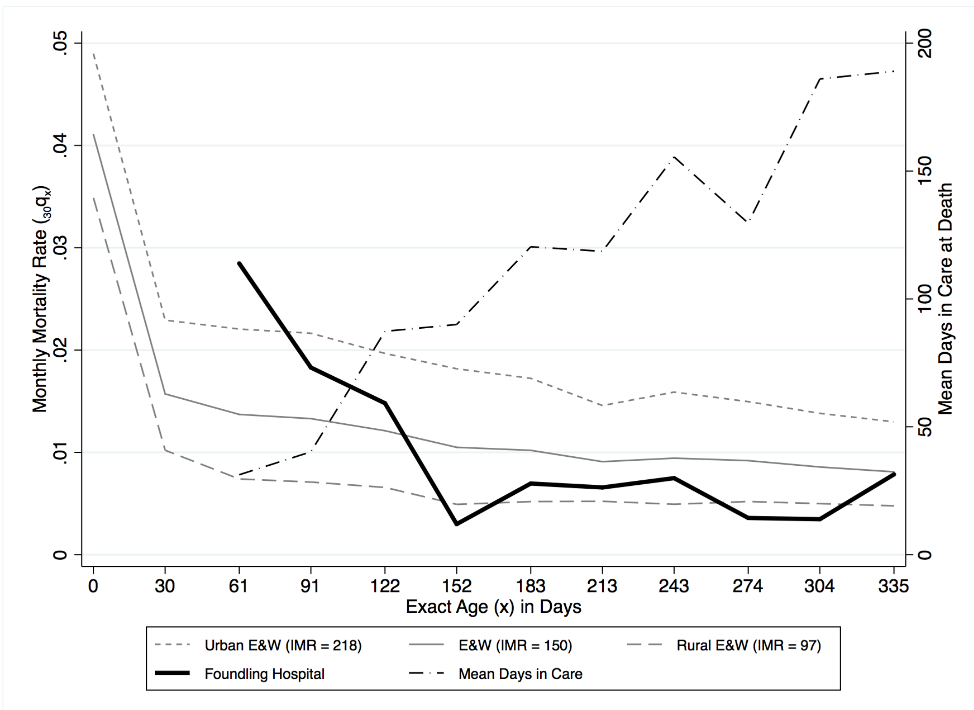
C The Founding Hospital in Context: Mortality

Calculating mortality for foundling hospitals around Europe has always been a complex undertaking since the children were not technically observed from birth. Levene and others have established slightly different techniques to deal with this by estimating the number of children who were lost before coming into observation (Levene, 2007). This kind of estimation is more difficult for this period in the London Foundling Hospital because only 15 children (1.5 per cent of the sample) entered the hospital in their first month of life. Thus, it is very difficult to use the trajectory of mortality in the first month to estimate mortality rates for the first week as Levene did. Rather than trying to estimate earlier mortality, we calculate the probability of death in each month of the first year of life and compare this with Galley and Woods's infant life tables for Victorian England and Wales. This procedure solves the question of who is under observation because the only children who are counted are those who entered the hospital prior to the month being analyzed and survived to the first day of that month. We then simply calculate the percentage of these children who died in the next month to get an ${}_nq_x$ rate.

Figure C.1 shows these mortality rates compared with Galley and Woods's infant life tables for England and Wales, 1889-91 (Woods, 2000, p. 260). Mortality in the Foundling Hospital was initially higher than urban areas in England and Wales, but over the months mortality rates fell below the average and approximated the rate in rural England and Wales. This pattern follows the average time that the children had been in the care of the Foundling Hospital when they died, suggesting that as they were fostered in the country, the children were subjected to rural levels of mortality conditions. Splitting the Foundling Hospital sample into an early group of children born in or before 1903 and a later group of children born after 1903, there was some improvement in mortality over time, especially in the third and fourth months of life. Clearly, although mortality was high, especially in the earlier months, the picture of mortality in the Foundling Hospital was much different in this later period than it was in the eighteenth century when Levene estimated infant mortality rates as high as 833 per thousand during the general reception and 357 per thousand from 1761-1800 after the general reception (Levene, 2007, p. 57). These mortality rates are also far below mortality rates in other foundling hospitals around Europe at the beginning of the twentieth century (Revuelta-Eugercios, 2013, p. 54-5).

We can also compare early childhood mortality in the Foundling Hospital with mortality in London and the two counties where the children were fostered to see whether our foundling children did better or worse than the surrounding population (see Table C.1). For the most part, the children had lower levels of mortality than both London and the surrounding counties, suggesting that the treatment and care the children received from the county medical officers and their foster mothers was fairly good for the most part. Clearly, unlike many foundling hospitals in Europe and the earlier history of the London Foundling Hospital itself, by the turn of the twentieth century, children in the London Foundling Hospital did not face a higher mortality risk than the children surrounding them in the population. This is quite remarkable considering that the children were admitted at relatively late ages to the hospital and may have suffered poor treatment before entering the hospital.

Figure C.1: Mortality rates ($_{30}q_x$) in the first year of life in the Foundling Hospital compared with mortality rates from Galley and Woods's infant life table for England and Wales, 1889-91



Notes: Urban refers to deaths in three towns (Blackburn, Leicester and Preston) with over 100,000 inhabitants and rural refers to deaths in three rural counties (Dorset, Hertfordshire and Wiltshire). See Appendix C text for comments on how mortality rates were calculated for the Foundling Hospital.
Sources: Foundling Hospital Dataset - see Appendix A; (Woods, 2000, p. 260).

Table C.1: Child mortality risk in the Foundling Hospital compared with London, Surrey and Kent, the counties where the foundling children lived and were fostered

		Registration Counties 1901		
	Foundling Hospital	London	Surrey	Kent
$_1q_1$	0.0280	0.0540	0.0318	0.0330
$_1q_2$	0.0063	0.0209	0.0099	0.0105
$_1q_3$	0.0042	0.0134	0.0066	0.0083
$_1q_4$	0	0.0090	0.0065	0.0071
$_4q_1$	0.0388	0.0944	0.0539	0.0579
Total Deaths 1-5	18	9447	788	1164

Notes: Mortality rates for the registration counties are actually age-specific mortality rates rather than probabilities of death (nqx). However, the 4q1 rate for the counties was imputed from the year-by-year mortality rates presented in the table.
Sources: Foundling Hospital Dataset - see Appendix A; 'Sixty-fourth Annual Report of the Registrar- General', pp. 124-26; 'Census of England and Wales 1901: County of London', p. 66; 'Census of England and Wales 1901: Count of Surrey', p. 41; 'Census of England and Wales 1901: Count of Kent', p. 69.

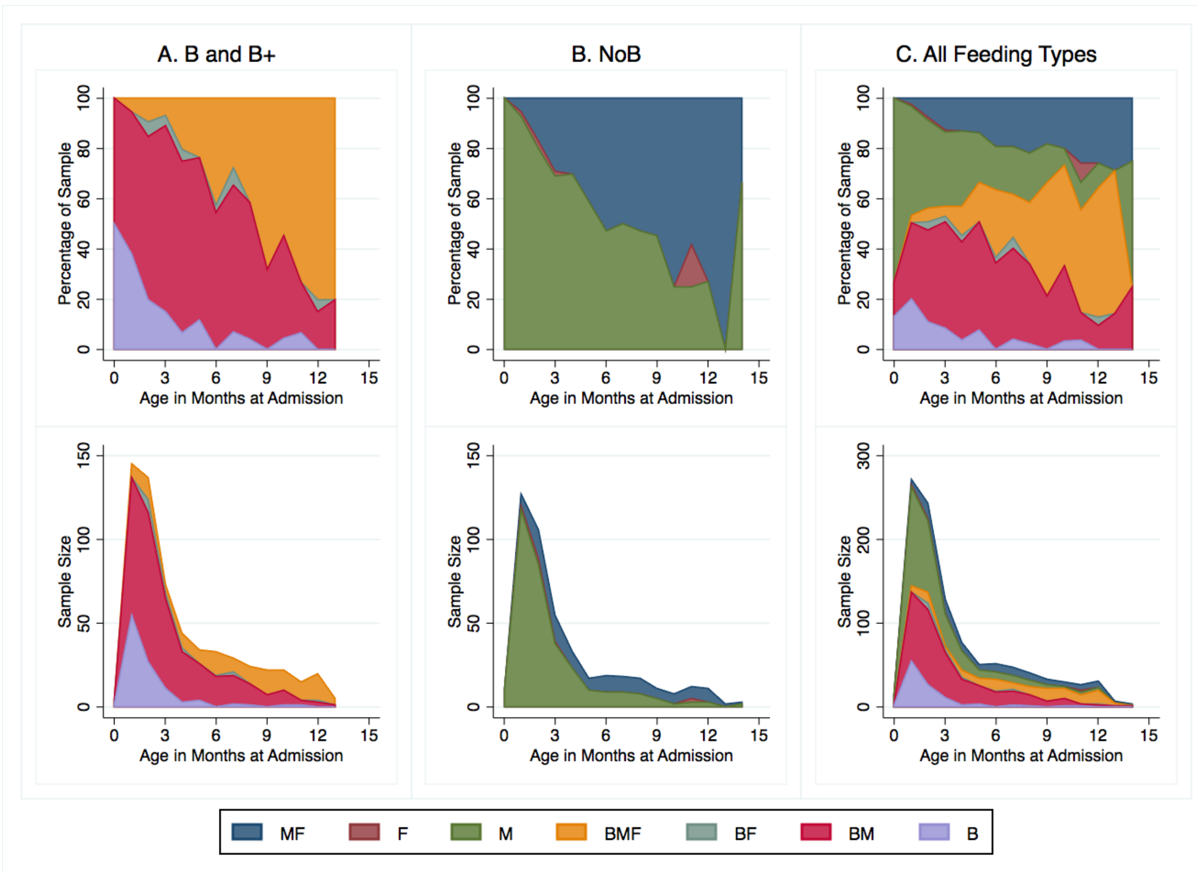
D The Foundling Hospital in Context: Infant Feeding

Table D.1: Breastfeeding incidence at various ages in the Foundling Hospital and other urban centers in the late nineeteenth and early twentieth centuries

	B	B+	NoB
Foundling Hospital (1893-1914)			
0-2 months	15.8	38.1	46.0
3-5 months	7.0	52.0	41.0
6-11 months	2.2	60.9	37.0
London - Fildes (1992)			
London - 23 boroughs (1905-19)			
0-1 months	85.9	6.5	7.6
St Pancras (1905-13)			
0-2 months	84.1	7.9	8.0
3-4 months	65.7	15.2	19.2
Paddington (1907-11)			
0-2 months	80.7	7.3	12.0
3-5 months	67.3	11.9	20.8
6-8 months	59.6	14.9	25.5
Finsbury (1908-9)			
0-2 months	81.2	7.9	10.8
3 months	72.1	6.8	21.1
Stepney (1912-13)			
0-2 months	82.5	9.8	7.8
3-5 months	72.1	6.8	21.1
6-11 months	6.4	57.4	36.3
Other Cities - Woods (2000)			
Brighton (1903-5)			
0-2 months	84.4	6.9	8.7
3-5 months	66.8	11.6	21.6
6-11 months	48.2	22.4	29.4
Liverpool (1894)			
0-2 months	48.7	20.7	30.6
3-5 months	37.3	24.5	38.2
6-11 months	35.8	24.5	39.7

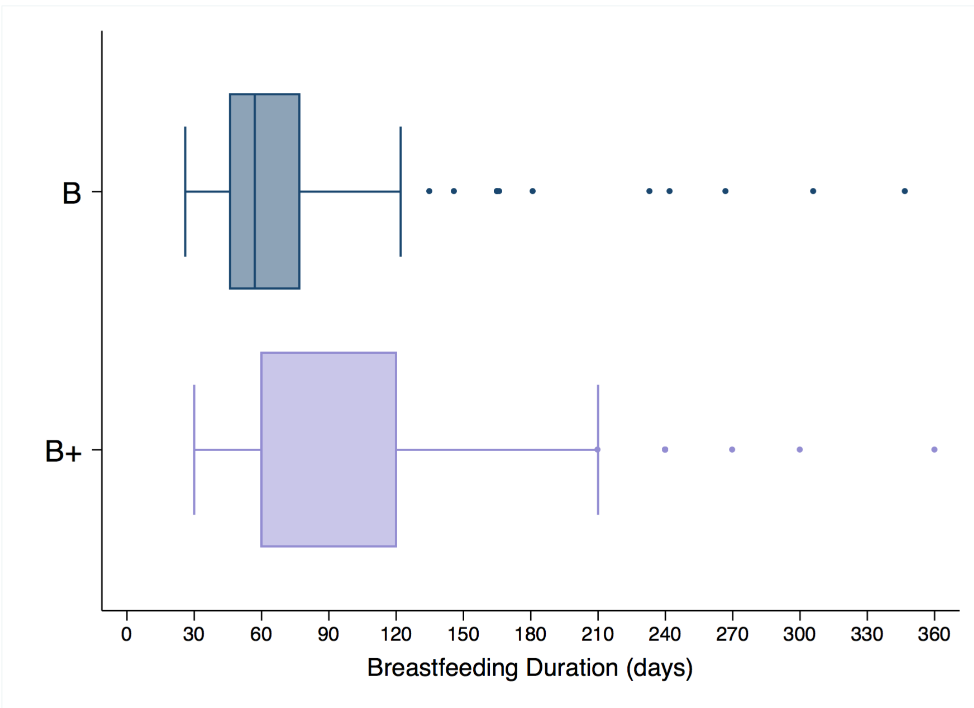
Notes: B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed. Again, the data between the Foundling Hospital and the town surveys are not entirely comparable. While the town surveys reflect the current feeding practice, the Foundling Hospital values reflect past practice as well. Thus, there is a large share of foundling children in the breastfed and supplemented group at age 6-11 months because this group also includes those who were breastfed exclusively at earlier ages and then weaned on to other substances. This is confirmed by the breastfeeding duration listed in the medical record. The duration figures for London may underestimate breastfeeding rates since health visitors in some boroughs (e.g. Stepney) tended only to re-visit problem cases.
Sources: Foundling Hospital Dataset - see Appendix A; Fildes (1992, p. 60); Woods (2000, p. 288).

Figure D.1: Feeding type incidence in the Foundling Hospital as observed across admission ages



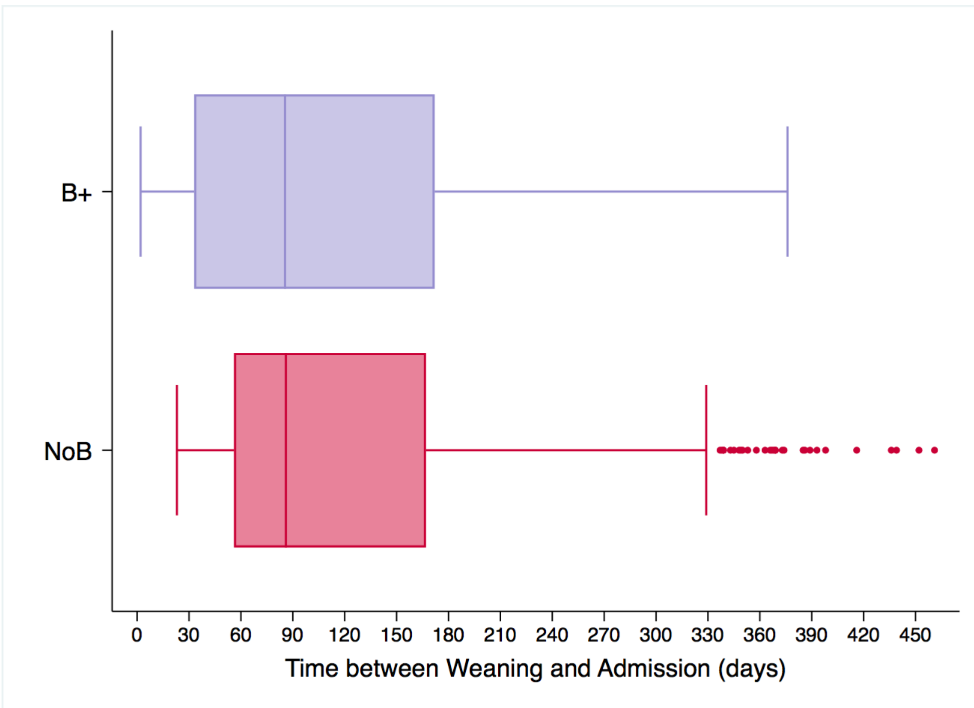
Notes: The various feeding categories correspond to combination of breast milk (B), milk (M) and food (F) presented by the month of admission to the Foundling Hospital.
Sources: Foundling Hospital Dataset - see Appendix A.

Figure D.2: Boxplot showing the duration of breastfeeding in days of the exclusively-breastfed (B) and breastfed-with-supplementation (B+) groups



Notes: For the exclusively-breastfed group (B), the breastfeeding duration was equal to the child's age at admission to the Foundling Hospital assuming that their mother continued to breastfeed them to that point. For the breastfed-with-supplementation group (B+), we have used the breastfeeding duration recorded in the medical record. There was considerable error in this measurement though since the duration was mostly recorded in months with a few in weeks. Thus, the 25th percentile and median for the B+ group are both equal to 60 days.
Sources: Foundling Hospital Dataset - see Appendix A.

Figure D.3: Boxplot showing the time between weaning and admission to the Foundling Hospital for the breastfed-with-supplementation (B+) and never-breastfed (NoB) groups



Notes: For the never-breastfed group (NoB), the time between weaning and admission was equal to the child's age at admission to the Foundling Hospital. For the breastfed-with-supplementation group (B+), we have subtracted age at admission from the breastfeeding duration recorded in the medical record.
Sources: Foundling Hospital Dataset - see Appendix A.

E Representativeness and Selection in the Full Foundling Hospital Sample

As mentioned in Section 3.4, we are concerned primarily with two issues related to selection and representativeness of the Foundling Hospital sample. First, were children admitted to the Foundling Hospital representative of other children in London; and second, was there differential selection into feeding regimes, whether ex post or ex ante? This appendix presents evidence to alleviate concerns drawing on what we know from the complete Foundling Hospital sample. The next appendix, Appendix F, extends these results by linking a sub-sample of Foundling Hospital children to their birth records in the Queen Charlotte Lying-in Hospital. Thus, both appendices act in concert to assuage concerns that unobserved confounding factors could be responsible for our results.

E.1 Testing selectivity into the Foundling Hospital

We begin by addressing questions of representativeness that will allow us to comment on broader patterns in cohort health. First, we look at selection on initial health and socioeconomic status. Despite the explicit admission criteria imposed by the Foundling Hospital, the children in our data were reasonably similar to the other illegitimate children in England and Wales at the time. The fathers of Foundlings were drawn from across the socioeconomic distribution and were not overly representative of poor and working classes (see Table A.2). We also compare mortality following admission to the Foundling Hospital with that in London and the counties in which the children were fostered (see Appendix C). Here, we find that mortality in the Foundling Hospital was lower than for children in similar non-institutional locations. Accordingly, there is little evidence that these institutionalized children were negatively selected relative to, or suffered poorer health than, their counterparts outside the hospital.

There is also little evidence of sample selection related to the hospital's admissions decisions. Using information from successful and unsuccessful petitions for a subset of our period (1909-1914), we find a slight acceptance advantage for boys and for the children of young mothers and of farmers. Meanwhile, we find that a petition's chances of success are unrelated to the season of birth or of petition, proximity to London, or other occupational categories (not reported). Indeed, these quantitative findings concur with the explicit policies of the Foundling Hospital: the medical officer's notes expressly state that children were not to be rejected on the basis of health status.[37]

We also assess how the early-life feeding practices in our sample compare to prevailing feeding patterns in the population. Appendix Tables 1 and D.1 show that rates of exclusive breastfeeding are considerably lower, and rates of both supplemented breastfeeding and of never-breastfeeding are much higher, in our sample than in those derived from surveys of working-class mothers in London and and other large towns in England during the same period (Fildes, 1992, 1998; Woods, 2000). These differences in feeding type likely reflect differences in feeding practices between legitimate and illegitimate children in London during the same time period. For instance, Fildes (1992) cites figures from the London borough of Wandsworth (1907-13) that show illegitimate breastfeeding rates to be approximately half those of legitimate children. Only 44.1% and 27.1% of illegitimate infants were breastfed in the first month and at four months, respectively, compared to 82.3% and 66.6% of legitimate infants. Fildes argues that this difference existed because illegitimate infants tended to be artificially fed by someone else while their mother was at work. Compared to these figures, women who placed their children in the Foundling Hospital were actually slightly more likely to have breastfed their children than other unwed mothers (see Appendix D).

Although lower breastfeeding rates among our foundling children may not be strictly representative of the general British population at the time, it should be noted that any differences in the composition of our sample relative to wider British society are unlikely to influence our results, since the comparisons we make in this study are entirely between and

[37] London Metropolitan Archives (LMA), Medical Record of the Foundling Hospital, London 1877-1911 by W. J. Cropley Swift, Medical Officer, A/FH/A/18/10/6, p. 4.

within groups of admitted children who experience different pre-admission feeding regimes. Thus, the interpretation of results on between- and within-feeding-regime differences in subsequent health should be unaffected, so long as the differences between feeding regimes do not vary along the same margin by which our sample may be un-representative of the general population—and indeed, as discussed above, there is little evidence of the latter sort of sample selection.[38]

E.2 Testing selectivity into the feeding regimes

Having established that selection into the Foundling Hospital is unlikely to have a material impact on our study's central question—namely, the differential relationship between infant feeding regimes and post-weaning health outcomes in a historical setting—we next interrogate differential selection by feeding regime. Unlike broader sample selection, selectivity into feeding regime is potentially important since it could confound the interpretation of our results on the relationship between pre-admission feeding and subsequent health.

To look into this, we examine whether the feeding regime chosen by mothers prior to admission was correlated with parental characteristics, the duration of breastfeeding, or the child's age at petition or admission. On the whole, we find little relationship between parental characteristics and either the feeding regime or duration of breastfeeding (not reported). Here, we find that feeding type is uncorrelated with infant characteristics such as maternal age, season of birth, and the majority of father's occupations (not reported). However, never-breastfed status is strongly positively associated with high-SES paternal occupations (consistent with the secondary literature), and negatively associated with institutional birth, which is instead positively associated with supplementation.

[38]Beyond the external validity checks discussed here, we conduct additional in-depth analysis of potential selectivity into the hospital resulting from parental and child characteristics, a suite of decisions made by mothers at various stages in the petition and admissions process, and a suite of decisions made by the hospital. In this analysis, as above, we find no or at most very limited evidence of selectivity into the Foundling Hospital. Since such sample selection will not affect the interpretation of our results, we do not report this analysis here, although the results are available upon request.

F Selection on Health at Birth: The Queen Charlotte Hospital Sub-Sample

F.1 Overview

Two important concerns for our study that we cannot resolve simply by analyzing the Foundling Hospital data are: 1) whether children admitted to the Foundling Hospital were selected on initial health status relative to other children in London at the time, and 2) whether there was differential selection on health at birth by feeding type, either because the choice of feeding type resulted in selective mortality (i.e., mortality correlated with health status as proxied by health at birth), or because a mother's choice of feeding type was a maternal investment made in response to the observed health status (e.g., birth weight). If there were selection on either of these dimensions, it would limit the external validity of our study and potentially confound the relationships that we find between feeding types and subsequent health. In order to overcome these issues, we use historical linkage to generate a novel longitudinal sample drawing on records from London's Queen Charlotte Lying-In (Maternity) Hospital.

Our procedure is as follows. Ideally, we would like to have visibility into the health of foundlings before they entered the Foundling Hospital, and to be able to compare their health status and characteristics to similar children not institutionalized in the Foundling Hospital. The records of the Queen Charlotte's Lying-In Hospital, a maternity hospital serving both foundlings and other London children at the time, allow us to look into precisely these issues. In this set of robustness checks, we gather and digitize records from the Queen Charlotte Hospital for a period coinciding with our main Foundling analysis. We then take a sample of the foundling children who we know from Foundling Hospital records were born in the Queen Charlotte Hospital, and we find them in the records of the Queen Charlotte Hospital. We then use these linked longitudinal data to comment on their pre-admission health status and its correlation with feeding regime, as well as to compare their health at birth to their non-foundling peers who were found in the Queen Charlotte Hospital around the same time.

We show that children admitted to the Foundling Hospital had similar levels of initial health (birth weight and birth length) to other illegitimate, first-born children born at the Queen Charlotte Hospital, but who did not go on to be foundlings. We also find that there are no significant differences in initial health between foundling children of different feeding types. Therefore, these concerns are unlikely to threaten the interpretation of our results.

F.2 Creating a Linked Foundling-Queen Charlotte Sample

Alongside other information about the foundling children's parental characteristics reported in the petitions, the Foundling Hospital administrators also noted the location of the child's birth. Of the children where the location of birth was recorded in our data, 58.5% were born in institutions such as workhouses, workhouse infirmaries, and maternity or lying-in hospitals. The largest sending institution by far, however, was the Queen Charlotte's Lying-In Hospital located in Marylebone, London. Between January 1893 and October 1908, when birth locations were reported in the records we transcribed, the Foundling Hospital admitted 169 children who were born in the Queen Charlotte Hospital, 22.1% of admissions in the same period.[39]

Before discussing the data in detail, it is helpful to present a short description of the hospital and its patients. The Queen Charlotte Lying-In Hospital was a maternity hospital founded in the eighteenth century. Its charter stated its purpose as follows: "For the reception as in-patients of lying-in women from all parts of the kingdom, including deserving single women with their first child, and for the delivery of married women and women giving birth to posthumous children at their own habitations in the metropolis." The hospital had an in-patient wing where women gave birth in the hospital, but it also sent midwives and doctors to assist with births in women's homes. Women could obtain help from the hospital

[39]The 169 Foundling Hospital children born in the Queen Charlotte Hospital were representative of all children admitted to the Foundling Hospital in our sample. There were no significant differences in admission weight or in any parental characteristics between the QC sub-sample and the complete sample (not reported).

either by applying to a committee or being recommended by a subscriber, someone who gave regular donations to the hospital. In practice, subscribers gave their recommendations letters to local clergy and district visitors, who gave them to women who could not afford to pay for maternity care. Single mothers were required to be "deserving," which meant that they had to be "respectable" (typically implying not a prostitute) and that the child had to be their first. Single mothers were only cared for in the in-patient department. The hospital did not charge patients any fee, but it is clear that through their selection procedures, they mainly catered to the lower working classes: the modal mother was a domestic servant and the modal father was a labourer according to hospital officials. Difficult cases were transferred from the hospital's out-patient to its in-patient department (Select Committee of the House of Lords on Metropolitan Hospitals, 1891, pp. 519-25). However, despite this potential for negative selection, the stillbirth rate in the hospital was 45 during our period (Queen Charlotte's Maternity Hospital, 1931, p. 40), which is very similar to the estimated stillbirth rate for England and Wales during the same period (Woods, 2005). Between 0.8 and 1.5% of births in London in the 1890s and 1900s occurred in the in-patient ward of the Queen Charlotte Hospital (Queen Charlotte's Maternity Hospital, 1931, p. 39). Thus, taken together, while the women giving birth in the Queen Charlotte Hospital were not representative of all women in London, there is no reason to believe that the single women giving birth there were substantially different from the women giving their children to the Foundling Hospital.

Fortunately, rich medical records from the Queen Charlotte Hospital have also survived, containing information about the health of mothers and children born in the hospital, including the following variables: mother's age, mother's marital status, parity of the child, sex of child, live birth or stillbirth, birth weight and birth length. We were able to link 160 children between the Foundling Hospital and and the Queen Charlotte Hospital (94.7% linkage rate) to obtain this extra information at birth. Separately, we also collected a random sample of 1,210 live births in the Queen Charlotte Hospital from 1893 to 1905. The sample includes both married and single women, and children of all parities, so that we can test the extent to which children in the Foundling Hospital were positively or negatively selected on initial health conditions, relative to the wider London population, and relative to similarly firstborn and illegitimate children who were not institutionalized in the Foundling Hospital.[40]

F.3 How Did Foundlings Compare to Non-Institutionalized Children?

Figure 2, Panel A compares the birth weight distribution of children born in the Queen Charlotte Hospital to single, primiparous mothers (i.e., the closest non-institutionalized comparators to our Foundlings), to the birth weights of children born in the Queen Charlotte Hospital who would later be admitted to the Foundling Hospital. This shows that the distributions look very similar. Children admitted to the Foundling Hospital had somewhat lower percentages of low birth weight, but a Kolmogorov-Smirnov test of equality of distributions indicates that any raw differences are actually statistically insignificant. Table F.1 tests whether children admitted to the Foundling Hospital had higher or lower birth weights than non-foundling children born in the Queen Charlotte Hospital. The various specifications restrict the comparison to several relevant sub-samples, such as children of single, primiparous mothers. They also add individual-level controls to control for factors that might confound weight differences between the foundling children and control children from the Queen Charlotte Hospital. In these specifications, the indicator for children subsequently admitted to the Foundling Hospital is never statistically significant, although foundling children were approximately 50 grams heavier at birth than the random sample from the Queen Charlotte Hospital. We also performed these same tests on birth length, and found broadly consistent results.

Because our Foundling children necessarily must have survived the pre-admission period in order to appear in our sample, all our Foundling analysis in the main paper is conditioned

[40]This implicitly assumes that the patients of the Queen Charlotte Hospital were roughly representative of middle- and working-class Londoners at the time.

on survival to admission, along with whatever (unobservable) pre-admission culling or scarring that might imply. This lack of visibility into prior health distributions may therefore be a cause for concern. However, the linked analysis discussed here, which allows us to recover birth information for Foundlings, and to compare their distribution of health at birth to that of children who we observe *unconditional* on their later survival (i.e., non-foundlings in the Queen Charlotte Hospital), allays that concern. Instead, there is strong evidence that Foundlings were similar in terms of characteristics and health at birth to their non-Foundling counterparts, and that any mortality prospective Foundlings faced prior to admission was not selective—that is, it was not systematically related to health status as proxied by birth weight in a way that would lead to a dominant culling effect.

Taken together, these results illustrate two important points about selection into the Foundling Hospital. First, it seems that foundling children were representative of illegitimate, first-born children in London, at least with regard to initial health measured by birth weight and length. There is no evidence that mothers differentially chose to give up or institutionalize unhealthy children, or that the Foundling Hospital itself selected healthier children. Second, one might be concerned that because foundling children were admitted to the hospital only after surviving for a few months, there might be selective culling of weak infants so that foundling children would be positively selected relative to the initial health distribution, whether generally or on a feeding regime-differential basis. There is at best very weak evidence that this may be the case: foundling children did weigh approximately 50 grams more, though the difference was statistically insignificant, and, at less than 2% of mean birth weight, very small in magnitude.[41] Therefore, there is no reason to believe that

[41] This difference is almost entirely driven by missing children at the lower tail of the birth weight distribution of foundling children. If we remove children in the Queen Charlotte reference group below the minimum birth weight of children admitted to the Foundling Hospital (2,233 grams), the mean birth weight difference between the two groups shifts to less than 10 grams. Thus, there is some evidence of selective mortality on latent health at the very low end of the birth weight distribution, but this was not feeding-differential, and given the medical knowledge and technology of the time, low birth weight children had a very high risk of dying early in infancy anyway. Thus, if we were to observe our reference group conditional on surviving to the same age as Foundling Hospital children, it is very likely that the birth weight distributions would be even more similar.

selective culling—at least related to birth weight—is confounding our results.

F.4 Is There Evidence That Foundlings Were Selected Into or Within Feeding Types?

Using this novel data on the distribution of health at birth (i.e., prior to any feeding decisions of feeding-related health outcomes), it is also important to consider whether there was differential selection related to feeding types. This selection could take three forms. First, mothers might compensate for their child's initial health status by investing more in children with low health status, e.g., they might preferentially breastfeed children at the lower end of the birth weight distribution. On the other hand, the opposite could also be true. Mothers may reinforce the initial health status of children by preferentially investing in (e.g., breastfeeding) children at the higher end of the birth weight distribution. Finally, beyond these maternal choices, feeding practices on their own could potentially influence the distribution of health we observe at admission (the first point at which we have a measure of health status for the full or main sample of Foundling Hospital children) through culling: for instance, since we know from our main analysis that breastfeeding has a strong protective effect on infant survival, this could mean that exclusively-breastfed children had a lower latent health distribution at admission than, say, never-breastfed kids, the weakest of whom may have died off before admission. Unfortunately, we are not able to disentangle these three effects, but we can still show differences in initial health by feeding type, and speculate about the extent to which such selection is a problem in our main analysis.

Figure 2, Panel B shows the birth weight distributions by feeding type, and Table 2 runs similar OLS regressions to Table F.1, except showing the difference between infant feeding types among children admitted to the Foundling Hospital. To be clear, we do not observe feeding types for non-foundling children born in the Queen Charlotte Hospital, only for those admitted to the Foundling Hospital. We have combined the exclusively-breastfed (B) and breastfed-with-supplementation (B+) groups because the sample size of exclusively-breastfed children in our linked Foundling-Queen Charlotte data is very small, at only 17 observations. The results suggest that children ever breastfed had higher birth weights than children never breastfed, although the differences between the two are not statistically significant. This rules out compensating investments, since mothers seem if anything to reinforce initial health endowments by preferentially breastfeeding who were heavier children at birth, though it is worth noting that the coefficient is small in magnitude (equivalent to about 2% of mean birth weight) and only marginally statistically significant.

The pattern is also inconsistent with there being strongly selective mortality by feeding type between birth and admission into the Foundling Hospital. If we assume that the birth weight distribution of the ever-breastfed and never-breastfed groups was initially the same, which seems reasonable given the distributions seen in Figure 2 Panel B, higher culling in the left tail of the NoB group would mean that the mean birth weight of survivors in the never-breastfed group would increase above the mean birth weight of of ever-breastfed survivors. However, the evidence here is the opposite: ever-breastfed children have if anything higher birth weights, although the difference is not statistically significant. This would tend to suggest that if anything, never-breastfed children bore scarring by the time of admission that outweighed any culling they may have experienced. More likely yet, there was no feeding-differential culling: insofar as children died before admission in a way that was correlated with feeding type (as they almost certainly did, if we believe our main results about the protective effect of breastfeeding in infancy), this was not selective within feeding types on the basis of birth weight.[42] It should be noted, though, that we cannot rule out selective culling completely. This is because if the reinforcing investment decisions were strong enough, it is possible that they could overcome the selective culling effect. However, none of these differences is strong in our data, which allows us to proceed with the assumptions that there was no important or statistically significant differences in initial health between children

[42]Note that our assumption here is that birth weight is a reasonable proxy for underlying health status at birth. Insofar as there are unobservable dimensions of health along which there was feeding-differential selective mortality, we would not be able to capture such culling.

in the three feeding type groups (indicating that we can proceed almost as if feeding type were randomly assigned), and that there was likewise no differential culling by feeding type (indicating that we can assume that all children we observe at admission, irrespective of feeding type, are drawn from the same underlying health distribution).

Table F.1: Queen Charlotte Sample: Birth weight regressions comparing children born in the Queen Charlotte Hospital and admitted to the Foundling Hospital to other children born in the Queen Charlotte Hospital

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	Full	Single Parity 0	Single Parity 0	Single	Parity 0	Full	Full
In Foundling Hospital	60.64	54.17	48.12	46.29	53.86	53.27	52.16
	(37.42)	(39.45)	(45.67)	(45.72)	(44.87)	(45.67)	(45.89)
Additional Controls:							
Male	No	No	Yes	Yes	Yes	Yes	Yes
Mother's Age Dummies	No	No	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes
Birth Season Dummies	No	No	Yes	Yes	Yes	Yes	Yes
Parity of Child	No	No	No	Yes	No	Yes	Yes
Mother's Marital Status	No	No	No	No	Yes	Yes	Yes
Marital Status - Parity Interactions	No	No	No	No	No	No	Yes
N	1329	727	724	731	911	1308	1308
R-square	0.002	0.002	0.116	0.115	0.123	0.105	0.108

Notes: Coefficients with robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Sources: Foundling Hospital Dataset - see Appendix A.

G Short-Run Health Robustness Checks

Again, a skeptical reader might worry that the non-random assignment of feeding types, breastfeeding duration, and time since weaning could bias our coefficients when estimating the association between feeding types and short-run health. This appendix discusses in detail the tests and additional robustness checks we conduct to ensure that this is not the case.

The first concern is that omitted confounders may lead to bias in our coefficients of interest. The most important omitted confounders would be the latent health of children at birth, the maternal support network, and non-feeding health investments, all of which could influence both weight-for-age in infancy and infant feeding decisions. However, it is not clear how important these omitted variables would be in biasing our results since there are not any historical studies showing the extent to which compensating or reinforcing investments were dominant in Britain during this period. Moreover, the variety and extent of non-feeding health investments was much smaller in the past since many of the health investments available today, such as pediatric care, vaccinations, nutritional supplementation (vitamins), etc., were either less available, unavailable, or unavailable in their present form.

To get some bounds on the potential bias in our regressions, we implement a test proposed by Oster (2019), which uses coefficient and R-square movements when including and excluding control variables to produce bounds for how much unobserved controls would alter the main coefficient of interest. The key assumption of the test is that selection on observable characteristics is informative about selection on unobservable characteristics. We also follow Oster in setting the maximum R-square (R_{max}) in the test at 1.3 times the R-square in the fully controlled regression, which accounts for the fact that even with a full set of controls, measurement error and individual genetic potential would prevent us from explaining 100% of the variation in weight-for-age. Thus, we compute two figures that explain the potential for omitted variable bias to affect our regressions. The first is a bias-adjusted coefficient (β) for our coefficient of interest assuming that selection on unobservables is equal to the selection on observables. The second reports the proportional degree of selection on unobservables relative to observables (δ) that is necessary to make the coefficient of interest equal to zero.

These figures are reported for each specification at the bottom of Table 3. We can see that when allowing proportionally equal selection on observables and unobservables in Specifications 1 and 2, the bias-adjusted coefficients comparing exclusively breastfed and supplemented children to the never breastfed become larger. In addition, selection on unobservables would need to be positively correlated with the treatment (i.e., feeding) and 90.1 times greater than selection on observables or negatively correlated with the treatment and 16.1 times greater than the selection on observables in order to reduce the exclusively-breastfed coefficients from their large effects to zero. In general, then, these results suggest that the differences that we observe between the feeding types, and especially for the exclusively breastfed, are unlikely to be substantially influenced by omitted variable bias.

A second concern might be that differences in the age at admission to the Foundling Hospital or in breastfeeding duration or time since weaning between groups might bias our estimate of the benefits of exclusive breastfeeding or having ever been breastfed. This might be the case if these variables were associated with greater degrees of scarring and culling, i.e., a never-breastfed child admitted at nine months of age might have been healthier on average because it had survived longer than an exclusively-breastfed child admitted to the hospital at a much earlier age. In addition, because children breastfed with supplementation were admitted on average at much later ages than exclusively breastfed children, it would be useful to understand whether differences between the two groups remained when balancing on admission age.

To account for these biases, we consider exclusive breastfeeding and breastfeeding with supplementary food to be treatment groups, and estimate regression-adjusted, inverse probability weighted treatment effects for the two groups.[43] We model binary treatment as-

[43] We use both inverse probability weights and regression adjustment because this "doubly robust" method produces unbiased estimates of the treatment effect if the functional form is correct in either the treatment or outcome model.

signment using logistic regressions with admission age, breastfeeding duration, time since weaning, birth location type and father's occupation as covariates, though not all included in the same specification.[44] The inverse probability weights are predicted from these regressions and ensure that there is balance between the treatment and control groups on these covariates. We then predict the treatment effects with OLS regressions, executed for each treatment level separately, using nearly the same covariates presented in Equation 1 and Table 3.[45] If there is still a significant difference between the treated and untreated after this adjustment, then it suggests that the differences between the groups are not spurious remnants of differential selection related to the covariates.

The results are presented in Table G.1. Specifications 1 and 2 use multi-level treatments to compare the exclusively breastfed and breastfed with supplementation to the never breastfed when ensuring balance between the groups on admission age. We also recreate these treatments in pairs in Specifications 3-5 so that we can conduct covariate balance tests to ensure that the inverse probability weights balance the groups on the covariates in the treatment model: the balance tests are always insignificant, meaning that we cannot reject the null hypothesis that the covariates are balanced. The average treatment effects on the treated in these specifications generally confirm the magnitude and significance of our OLS regressions in Table 3, suggesting that differences in admission age were not driving weight differences between the feeding groups.

Turning to breastfeeding duration and time since weaning, it is necessary to restrict the analysis when balancing on these variables to satisfy the overlap assumption, which requires that any individual could get any treatment level. Thus, we exclude the never breastfed when balancing on breastfeeding duration since never-breastfed children have no variation in breastfeeding duration, and similarly we exclude the exclusively-breastfed children when balancing on time since weaning. Specification 6 presents the results for balancing on breast-

[44]We cannot include admission age, breastfeeding duration or time since weaning in the regressions because this violates the overlap assumption necessary to estimate the inverse probability weights.

[45]We have to exclude birth district fixed effects because the maximum likelihood estimator requires multiple observations in each bin to converge.

feeding duration. It shows an average treatment effect on the treated of 0.55 standard deviation between the exclusively breastfed and breastfed with supplementation, suggesting that there were benefits to exclusive breastfeeding above and beyond any differences in breastfeeding duration that might have been different across groups. This accords with our main results.

Specification 7 limits the analysis to those breastfed with supplementation and never breastfed, and balances on time since weaning as a series of five dummy variables, the same as those presented in Table 3 above. When balancing on time since weaning, we see that having ever been breastfed has a relatively small but important and statistically significant positive impact on weight-for-age of 0.28 standard deviations. This effect is slightly larger than the coefficient in Specification 7 of Table 3, and suggests that there are some benefits to breastfeeding during the aging and weaning processes.

Table G.1: Average treatment effects on the treated for infant feeding types and weight-for-age Z-scores in infancy

Sample	(1) All	(2) All	(3) B & B+	(4) B & NoB	(5) B+ & NoB	(6) B & B+	(7) B+ & NoB
Average Treatment Effect on the Treated (ATET):							
B vs. B+			0.320*			0.552***	
			(0.176)			(0.207)	
B vs. NoB	0.931***	0.833***		0.934***			
	(0.176)	(0.168)		(0.176)			
B+ vs. NoB	0.603***	0.431***			0.430***		0.277**
	(0.144)	(0.110)			(0.110)		(0.132)
Potential Outcome Means:							
B+			-1.819***			-2.052***	
			(0.105)			(0.143)	
NoB	-2.431***	-2.355***		-2.433***	-2.355***		-2.296***
	(0.105)	(0.088)		(0.104)	(0.088)		(0.092)
Outcome Model Covariates:							
Standard Covariates	X	X	X	X	X	X	X
Treatment Model Covariates:							
Admission Age	X	X	X	X	X		
Age at Weaning						X	
Days since Weaning (binned)							X
Birth Location Type	X	X	X	X	X	X	X
Father's Occupation	X	X	X	X	X	X	X
N	1016	1016	585	535	912	341	668
Covariate Balance Test - Chi2			11.66	5.92	11.44	8.68	20.28
Covariate Balance Test - p-value			0.390	0.878	0.407	0.651	0.122
Converged	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Coefficients with robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. B is exclusively breastfed. B+ is breastfed with other supplemental food. NoB is never breastfed. Standard outcome model covariates include fixed effects for male, birth location type, mother's age, birth season, father's HISCLASS and birth year. Admission age and age at weaning enter the models linearly whereas days since weaning has been binned in order to capture the non-linear effects shown above. We use the same time bins as presented in Table 3. The treatment model was estimated as a multinomial logistic or logistic regression. The outcome model was estimated linearly. Specification 1 uses B as the treated group in calculating ATET whereas specification 2 uses B+ as the treated group. Covariate balance tests were not possible for specifications 1 and 2 because they used multi-level treatments.

Sources: Foundling Hospital Dataset - see Appendix A.