

Universal Investment in Infants and Long-Run Health: Evidence from Denmark’s 1937 Home Visiting Program[†]

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This paper examines the long-run health effects of a universal infant health intervention, the 1937 Danish home visiting program, which targeted all infants. Using administrative population data and exploiting variation in the timing of implementation across municipalities, we find that treated individuals enjoy higher age-specific survival rates during middle age (45–64), experience fewer hospital nights, and are less likely to be diagnosed with cardiovascular disease. These results suggest that an improved nutrition and disease environment in infancy “programmed” individuals for lower predisposition to serious adult diseases. (JEL H51, I12, I18, J13, N34)

An existing literature documents dramatic long-run health consequences of exposure to large negative shocks such as epidemics and famines early in life (for an overview, see Currie and Almond 2011). Researchers have more recently begun to investigate the impact of improvements in early life health within the reach of policy and their effect on adult outcomes. Can public health programs targeting infants shift long-run health trajectories? Bhalotra and Venkataramani (2012) show that a reduction in pneumonia among US infants in the 1930s and 1940s, due to the development of antibiotics, reduced disability in adulthood and improved educational and employment outcomes for treated infants. Hoynes, Schanzenbach, and Almond (2016) find that the provision of food stamps for poor families with children in utero and during their early childhood years improved the health outcomes of Americans in adulthood.

This paper is the first to examine the long-run effect on adult health of a public health program aimed at improving the health of all infants: home visits to new mothers. The Danish National Board of Health (DNBH) designed the 1937 home visiting program as a reaction to persistently high infant mortality rates of around 6.5 percent

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in the 1930s. At the time, many infants died from preventable infectious diseases. For example, acute enteritis—a set of infectious diseases causing diarrhea and often resulting from the improper treatment of cows' milk—accounted for around 10 percent of overall infant mortality (Danish Health Authority (DHA) various years).

As the DNBH believed that a lack of postnatal care, health monitoring, and guidance of new mothers contributed to the high infant mortality rate, the board issued uniform guidelines for a new home visiting program: trained nurses were to conduct about ten home visits to all infants during the first year of life. During those visits, nurses were to encourage mothers to breastfeed and keep the home environment clean. Moreover, they should refer ill infants to doctors for treatment (Buus 2001).

Using aggregate historical records, Wüst (2012) shows that the program led to a significant increase in infant survival of about 5–8 lives saved per 1,000 live births. Examining the driving forces behind this effect on infant survival, she shows that especially mortality from diarrhea-related causes decreased. Home visiting accounted for about 17–29 percent of the period's overall decrease in diarrhea-related mortality. This finding highlights mechanisms for potential longer run benefits of this infant health program: the one-year survivors of treated cohorts most likely experienced less severe sickness episodes and better infant nutrition. Both of these factors have been shown to impact later-life outcomes (Hoynes, Schanzenbach, and Almond 2016; Bhalotra and Venkataramani 2012; Bozzoli, Deaton, and Quintana-Domeque 2009; and Maluccio et al. 2009).

Although designed centrally, the home visiting program was implemented locally: around one-third of the 1,345 Danish municipalities initiated the program during the 1937–1949 period we consider.¹ To examine the long-run health effects of the program, we thus follow a difference-in-difference approach and compare changes in adult outcomes across cohorts born in municipalities that initiated the program to changes across the same cohorts born in municipalities with no change in implementation status. Nurses offered visits to all new mothers in municipalities that implemented the program and take-up rates were close to 100 percent (DHA various years). As we detail in Section III, we use individual-level data on health outcomes in middle age (45 to 64)—when a non-negligible share of individuals begin to suffer from serious health conditions and die—for the population of Danish citizens who were born between 1935 and 1949 and are observed in the administrative records 1980–2012.

Historical sources point to at least four factors that introduced variation in the timing of program initiation across municipalities: delays in the central accreditation process, a shortage of qualified nurses, region-wide implementation for all municipalities in some parts of Denmark, and varying support from local health professionals (Buus 2001). Our baseline specification controls for time-invariant differences between municipalities (such as geography) and location-invariant differences between cohorts (e.g., the impact of World War II).² To address

¹From 1974, municipalities were required to implement the home visiting program.

²Especially the potential impact of World War II deserves our attention. If the war, as one important contemporary factor, impacted treated and untreated municipalities differentially, we may falsely attribute this impact to the home visiting program. For further details please see Section II.

concerns about potential differential trends in outcomes across implementing and non-implementing municipalities, we follow four additional approaches. First, we include municipality-specific time trends. Second, we control for pretreatment levels and trends in demographic, political, economic, and health characteristics of municipalities and let the associated coefficients vary by cohort. Third, we restrict the sample to implementing and matched non-implementing municipalities that are comparable on pretreatment characteristics. Fourth, we restrict our analysis to the sample of implementing municipalities, thereby relying only on variation in the exact date of implementation.

We find robust and large long-run health effects of the home visiting program. We start by graphically examining our data: we present a set of event graphs that show no pre-trends in our outcomes (long-run survival, cardiovascular and heart disease, and hospitalization durations) in implementing municipalities. We then continue with our main analysis and show that individuals exposed to home visiting in infancy enjoy long-run survival gains. Examining the timing of this effect, we find that it materializes around age 50 and is strongest during middle ages (ages 50–60). Furthermore, the results for long-run survival are stronger for women than for men. An important pathway appears to be early life “programming” for serious adult diseases: treated individuals spend less time hospitalized and are less likely to suffer from cardiovascular disease in middle age.³ Further examining the robustness of these findings, we allocate random placebo reform years to random municipalities in our sample (applying a permutation test). We show that the estimates for our main health outcomes fall into the tails of the empirical distribution of estimates, i.e., the “true estimates” are among those we are least likely to find.

While we find strong results for long-run health across different specifications and tests, we do not find longer run improvements in treated individuals’ educational or labor market outcomes. Estimates for years of schooling, an indicator for only completing compulsory education, log wages, and occupational status around age 60 are mainly small, imprecise, and unstable across specification. We take this finding as suggestive evidence that the home visiting program impacted individuals predominantly through its direct effect on their health trajectories.⁴

This paper builds its empirical strategy on a set of recent papers, exploiting the rollout of health and social policies across areas and over time (Hoynes, Schanzenbach, and Almond 2016; Bailey and Goodman-Bacon 2015). Our analysis is inspired by theoretical work that lays out mechanisms through which good health in early childhood can unlock lifetime benefits (see e.g., Heckman and Mosso 2014 and Cunha and Heckman 2007), for example, through early programming of

³We also show that treated individuals are less likely to have received at least two of the following diagnoses: cardiovascular diseases, heart disease, and diabetes.

⁴We control in our analysis of education and labor market outcomes for the impact of the 1958 schooling reform that impacted the post-1946 cohorts. The reform gave increased educational possibilities to rural students (Arendt 2008). Thus, we include a $post1946 \times rural$ -indicator in our analyses. Given the presence of both an influential schooling reform and other societal changes, we also acknowledge that any (likely small) effect of the program on labor market outcomes may have faded over the 45 years prior to our measurements. The 1950s and 1960s—the period where individuals from the cohorts that we study enter the labor market—witnessed a rapid expansion of the Danish welfare state with many programs that likely had a large impact on individuals’ labor market outcomes. Any, presumably small, effects of the home visiting program may be hard to detect in outcomes such as wage around age 45–60.

middle-age diseases and dynamic complementarities with parental investment and follow-up policies.

We extend the existing literature in three ways. First, we add to the literature on long-run consequences of *infant health*, which has received less attention than the consequences of in utero health. There is now considerable empirical evidence from various disciplines that adverse conditions in utero can affect long-run health and other outcomes in adulthood (Almond and Mazumder 2011; Almond et al. 2010; Mazumder et al. 2010; Black, Devereux, and Salvanes 2007; Almond 2006; Crimmins and Finch 2006; and Barker 1990). There is broad support in this research for the “Barker hypothesis,” which states that in utero conditions shape individuals’ health trajectories. At the same time, there is a growing interest in the impact of health shocks in infancy and early childhood on longer run outcomes: a number of existing papers on shocks during infancy show long-run effects of famines (Meng and Qian 2009), disease early in life (Almond, Currie, and Herrmann 2012; Cutler et al. 2010; Bleakley 2010; and Lucas 2010), or factors such as weather conditions (Maccini and Yang 2009). Moreover, a growing literature is interested in the effects of policies that target infancy and childhood and that may be able to offset original health disadvantages (Currie and Rossin-Slater 2015; Bütikofer, Løken, and Salvanes 2015; Carneiro, Løken, and Salvanes 2015; Bharadwaj, Løken, and Neilson 2013; and Currie 2009).

We contribute with a study focusing on the effects of a government-run policy focused on infancy. While the potential (biological) mechanisms that lead to longer run health effects of interventions during infancy are less formally pinned down and described, a number of studies indicate that prolonged breastfeeding durations and improved infant nutrition, as well as less severe sickness spells during infancy, may be critical for shaping longer run health trajectories (Fitzsimons and Vera-Hernandez 2013; Kramer et al. 2001; Bozzoli, Deaton, and Quintana-Domeque 2009; and Horta et al. 2007).

Second, we provide causal evidence on the long-run returns to universal (rather than means-tested) investments in early life health, which is the dominant model for programs such as home visiting and early childcare in many countries. Most of the existing evidence on the benefits of early interventions comes from targeted programs that focus resources on selected families and children (for a very recent overview, see Currie and Rossin-Slater 2015). The long-run impact of universal programs, which target broader groups in the population, may be substantially different from the impact of these targeted interventions. As an example, we may expect lower marginal social benefits of universal programs because they do not focus resources on the most disadvantaged children, who have been shown to benefit most from early investment programs (see, for example, Bitler, Domina, and Hoynes 2012; Havnes and Mogstad 2011).⁵

Third, we present evidence on the long-run health effects of well-baby home visiting. Parallel to our work, two other recent studies on very similar programs

⁵ Additionally, potential general equilibrium effects—in our application, for example, capacity constraints in hospitals and schools or competition in local labor markets—may make the case for universal implementation of programs more or less appealing.

in Sweden and Norway document important medium-run and long-run benefits to well-baby contacts with health professionals (Bütikofer, Løken, and Salvanes 2015; Bhalotra, Karlsson, and Nilsson 2015). Bütikofer, Løken, and Salvanes (2015) find that access to well-baby center visits in the 1930s and 1940s, which focused on health monitoring and advice on infant care to new mothers, improved educational and labor market outcomes for treated individuals in Norway. Importantly, they also find that treated individuals are less likely to suffer from metabolic syndrome around age 40. Bhalotra, Karlsson, and Nilsson (2015) show that the introduction of health centers and home visits in Sweden in the early 1930s prolonged treated individuals lives in the very long run (probability of survival past age 75). Taken together, three studies from three Scandinavian countries point to important longer run health gains from well-baby contacts for all mothers and infants.

While our study is set in a historical context, the evidence on long-run effects of home visiting is applicable in many contemporary settings as well. Government-run, universal home visiting programs are in place in many countries today and thus an evaluation of long-run effects of these programs is instrumental for policymakers.⁶ Given the content of the program we study (with a focus on nutrition and regular high-frequency visits to families) and the context of its implementation in Denmark during the 1930s and 1940s (a high infant mortality country with infectious disease as one important cause of infant deaths), our results are particularly relevant for research and policy debates in developing countries: the Danish program of the time had the objectives to encourage early parental health investments such as breastfeeding, proper infant care, and hygienic conditions in the home—objectives that are still crucial for infants' health and development, especially (but not exclusively) in these settings (Engle et al. 2007, Horta et al. 2007). Home visiting programs have been shown to impact short-run outcomes such as breastfeeding duration and infant survival in developing countries today (Fitzsimons et al. 2012; Gogia and Sachdev 2010; WHO and UNICEF 2009; Haider et al. 2000; and Bhandari et al. 2003) and in historical settings that are comparable to those in developing countries (Moehling and Thomasson 2014, Wüst 2012). The evidence in this paper suggests that studies that ignore the long-run benefits of early life health programs in these settings significantly understate the potential returns to such programs.

The paper is organized as follows. Section I presents relevant background on Denmark's 1930s and 1940s medical system and the rollout of the home visiting program. Section II lays out our empirical strategy, and Section III presents the data. Section IV presents our results for adult mortality and potential underlying health mechanisms. Section V concludes.

⁶While the United States has a long history of state-run and privately organized, targeted home visiting programs (see e.g., Moehling and Thomasson 2014), no universal home visiting programs are operating in the United States today. The Hawaii Healthy Start Program and Nurse-Family Partnership's programs reach significant numbers of at-risk families in certain regions of the country. Chen, Oster, and Williams (2016) argue that the absence of public home visiting programs in the United States may be an important reason why infant mortality today is higher in the United States than in Europe.

I. Background and Rollout of the Home Visiting Program

In the 1930s and 1940s, the Danish health care system was organized through 23 medical districts divided into about 1,300 rural and 88 urban municipalities (Statistics Denmark 1940).⁷ General Practitioners (GPs) and trained midwives were relatively evenly distributed geographically due to a government refund program. Midwives were responsible for medical services for pregnant women, new mothers, and infants. Apart from births in the five largest towns, home births assisted by midwives were the norm. Postnatal care was usually poor and women were not entitled to scheduled contact with health professionals after giving birth (DHA various years).⁸

The DNBH identified this lack of postnatal care as one potential factor driving the high Danish infant mortality rate, part of which was related to preventable, infectious diseases. Thus, from 1930 onwards, the DNBH conducted a five-year trial with home visiting in three treatment and control municipalities in Denmark. The trial was conducted in collaboration with the US-based Rockefeller Foundation.⁹

Buus (2001) illustrates that the design of the home visiting program was a project taking place in an expert arena in the central administration and was not emerging as a “bottom up” initiative in local communities.¹⁰ By the early 1930s, home visiting programs had been established in the Netherlands, the United Kingdom, and the United States. Inspired by these initiatives, Danish health authorities aimed at increasing “the public health nursing in Denmark [that] has been inadequately developed” (DNBH in a letter to the Rockefeller Foundation in Buus 2001, 150). The DNBH further detailed: “The nurse must visit each newborn in her district as soon as possible after birth. She establishes friendly relations with the mother, wins her confidence and proceeds with instructions regarding the hygienic care of the infant. The routine is to impress on the mother the need for breastfeeding at regular intervals, to provide the infant with its own bed; to demonstrate how it should be bathed and clothed; convince the mother of the need for fresh air; inquires regarding when infant is older and finally gives instructions concerning the mother’s care in the next pregnancy” (Buus 2001, 152).

In March 1937, the Danish parliament passed the Act on the Home Visiting Program, and the DNBH issued detailed guidelines for uniform municipal implementation. During ten visits in the child’s first year of life, nurses were to register and to report on their work on the promotion of proper infant nutrition (especially breastfeeding) and hygiene; they were to monitor the child’s health and development, and to refer ill infants to GPs. We limit our analysis to the period between 1937 and

⁷ Some rural municipalities merged during the period we consider.

⁸ As an exception, infant care wards in the major cities provided well-baby visits to a targeted group of mothers (for details, see Buus 2001, Løkke 1998, and Wiist 2012).

⁹ The treatment and control groups for this trial with home visiting were not chosen randomly and thus the results from the trial could not credibly claim that they identified causal effects of the program. However, the DNBH based its recommendation for expanding the program to the entire country on the positive experiences from the trial (Buus 2001).

¹⁰ As opposed to the United States—where the development of health and child policies was strongly related to women’s suffrage (Miller 2008)—Danish women gained suffrage in 1915 and their political movement was not the driving force behind the introduction of the home visiting program.

1949, when the program exclusively served infants, and its main focus was on what was perceived the most important principles of infant care at the time: “Calmness, cleanliness, and orderliness” (Buus 2001).¹¹

Earlier work on the short-run effects of the 1937 program demonstrates that home visiting contributed significantly to a decrease in infant mortality and morbidity—especially from relevant causes such as diarrhea-related diseases (Wüst 2012)—and thus indicate that the DNBH succeeded in its immediate goals.¹² Unfortunately, due to lack of data, neither studies based on historical records nor the present study are able to disentangle the relative importance of different mechanisms underlying this effect. The most important candidate mechanisms (which well may be important in combination with each other) comprise improved infant nutrition, faster referral of ill infants to doctors, and an improved (hygienic) home environment.

Importantly, while the DNBH designed the program to provide universal care, and the data show take-up rates close to 100 percent once a municipality implemented the program (DHA various years), until 1974 each municipality could decide whether to implement the program.¹³ Historical sources point to at least four factors that introduced variation in the timing of treatment initiation across municipalities. First, to qualify for a refund of 50 percent of program expenses, municipalities had to obtain central accreditation from the DNBH. The time-consuming accreditation process, which involved local policymakers, local health professionals, other interest groups, and the DNBH experts, depended partly on the workload at the DNBH. Thus, accreditation of municipal programs could easily delay the exact timing of local implementation. Second, to work in the home visiting program, nurses had to complete two years of training at a newly established school in Aarhus. The resulting shortage of accredited nurses led to delays in implementation in some municipalities, especially in the early years. Third, some medical districts implemented the program district-wide so that neighboring municipalities that were located in different medical districts could face very different costs of implementation. Moreover, in the case of district-wide implementation, it was not necessary for every single municipality to file a request for accreditation. Fourth, many local municipal actors had to agree on implementation. While some welcomed the program, others—e.g., many GPs, who viewed the program as a threat to their authority and income source—opposed it. Such opposition led to implementation delays in some areas.

Figure 1 shows a map of Denmark in 1940. The darker a municipality, the earlier it implemented the home visiting program. Although towns on average implemented the program earlier than rural areas, the figure shows considerable variation in the timing of implementation among both towns and rural municipalities. For example, the town of Køge, outside of Copenhagen, implemented the program much

¹¹ Later on nurses also served older children. Also, the content of the program has changed significantly since the 1950s with a rising focus on factors such as social interaction and child development in a broader perspective.

¹² This finding is in line with the contemporary literature on home visiting in developing countries that indicates similar gains for programs that are structured around visits by trained health professionals (see e.g., Gogia and Sachdev 2010).

¹³ Decentralized implementation was important to the liberal party platform (in the Danish parliament), which emphasized the importance of municipal autonomy.

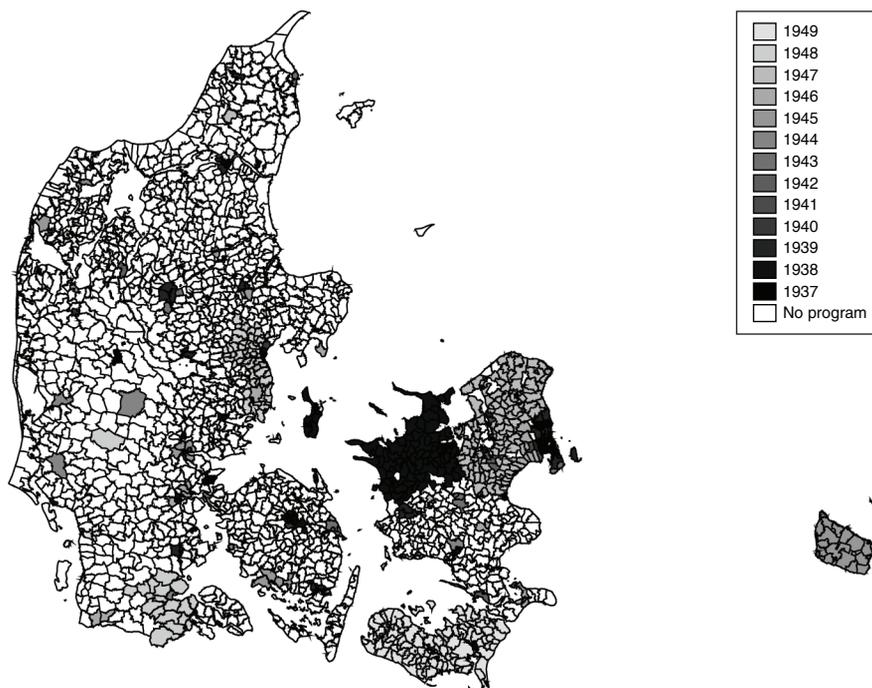


FIGURE 1. MUNICIPALITIES AND THEIR DATE OF ENTRY INTO TREATMENT, 1937–1949

Notes: The figure shows 1940 parishes. Rural municipalities consisted of one single parish; towns consisted of several parishes. All parishes of a given municipality were either treated or untreated.

Source: DigDag (Digital Atlas of the Danish Historical, Administrative Geography, www.DigDag.dk) and data on municipal treatment initiation from the Danish National Archives (for details, see Wüst 2012)

earlier than the neighboring town of Roskilde. The medical districts of Holbæk and Bornholm introduced district-wide programs in 1938 and 1945.¹⁴ Importantly, as Figure 2 illustrates, the timing of treatment in the implementing municipalities varies over the entire period that we consider, including the years before and after World War II, as well as the period of occupation during World War II (1940–1945).¹⁵ Our empirical strategy, discussed in the next section, relies on this variation in the timing of treatment initiation to identify the long-run effects of the program.

II. Empirical Strategy

To estimate the effect of the home visiting program on long-run health, we follow a difference-in-difference approach (DiD), beginning with the following baseline specification:

$$(1) \quad y_{jt} = \alpha + \beta \text{homevisit}_{jt} + \gamma_j + \delta_t + \epsilon_{jt},$$

¹⁴In total, 7 out of the 23 medical districts implemented district-wide programs in the period that we consider.

¹⁵The high number of municipalities in selected years is partly due to district-wide implementation in those years.

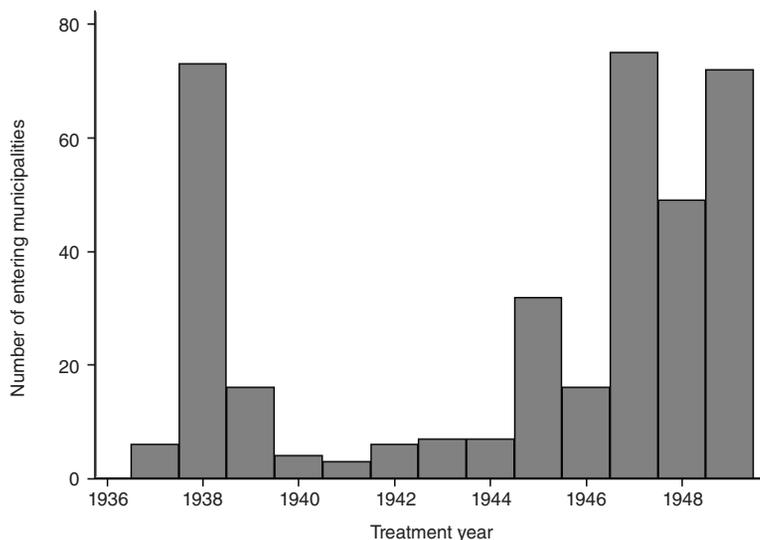


FIGURE 2. NUMBER OF MUNICIPALITIES BY THEIR YEAR OF ENTRY INTO TREATMENT, YEARLY BINS, 1937–1949

Note: The figure shows only municipalities entering the program in the period.

where y_{jt} is a health outcome in middle age for individuals born in municipality j in year t ,¹⁶ and $homevisit_{jt}$ is an indicator for the home visiting program being in place in municipality j in year t .¹⁷ To control for time-invariant differences between municipalities, such as geography, and location-invariant differences between birth cohorts, such as the impact of World War II, we include municipality fixed effects γ_j and year-of-birth fixed effects δ_t .¹⁸ We thus estimate β by comparing the difference in outcomes between individuals born before and after implementation in implementing municipalities to the difference in outcomes between individuals of the same cohorts born in municipalities with no change in treatment status in between those cohorts' years of birth. As treatment initiation varies over a 13-year period and the size of the average Danish municipality at the time was 32 square kilometers (12 square miles),¹⁹ we compare outcomes at many points in time and within small geographical areas.

Arriving at a consistent estimate of β through specification (1) hinges on the assumption that the timing of treatment implementation is orthogonal to the variation in counterfactual development of adult health. Time-varying shocks that are specific to implementing (or non-implementing) municipalities and correlated with outcomes, and/or underlying trends in outcomes that differ in implementing and non-implementing municipalities, would violate this assumption. We are not aware

¹⁶We collapse our data into municipality \times treatment status-cells for all our analyses and weight all analyses with cell size. Thus, the analyses are equivalent to regressions performed at the individual level.

¹⁷We assume that once a municipality implemented the program it remained in place, as happened in almost all cases (see Buus 2001).

¹⁸ γ_j absorbs the indicator for treated municipalities, and δ_t absorbs the indicator for posttreatment periods.

¹⁹Denmark is only slightly larger than the state of Maryland.

of other programs implemented during the same period with the same timing and geographic variation as the home visiting program. Although factors such as vaccines, new drugs, and better prenatal care were expanded during the period, these were rolled out for the entire country at the same time (for a discussion, see Wüst 2012).

As one important contemporaneous factor, the impact of World War II deserves mentioning. In some European countries such as the Netherlands, World War II had adverse and regionally diverse effects on living conditions of infants (importantly on nutrition) and, as a consequence, on war cohorts' long-run outcomes (see, e.g., Lumey et al. 2007; Heijmans et al. 2008; and Scholte, van den Berg, and Lindeboom 2012). In Denmark, the war did not lead to critical supply shortages or the suspension of implementation of the home visiting program. Moreover, the impact of the war years (e.g., in the form of food rationing) was comparably mild (Pedersen 2009). As stated in historical research, "Denmark made it through the war years without material damage. Neither the occupation of Denmark nor its liberation were accompanied by hard combat [...]. At the same time, living conditions in Denmark were the best when compared to all other European countries. Although there was a shortage of goods like coffee, tea, and tobacco, Denmark remained the country where milk and potatoes were not rationed [...]" (Poulsen 2002, 52, authors' own translation).

Unfortunately, we lack power to perform our main analysis on data only for the preoccupation or post-occupation period (i.e., the period 1937–1939 or 1946–1949). To further examine one potential impact of the occupation years on our findings, we look at fertility patterns in the years 1930–1949. If the occupation years 1940–1945 led to a dip in births in some areas but not others (e.g., in areas with greater hardship), we may be worried about the impact of the composition of births on the conclusions of our main analyses. Online Appendix Figure B.1 shows the average number of live births in the Danish counties in the period 1930–1949. Rather than a dip in the number of births, we see that the number of births had steadily increased since the 1930s and also increased during the occupation years. This pattern is in line with descriptive evidence from a number of other European countries and the United States, which experienced increases in birth rates in the 1940s (see Grove and Hetzel 1968, 60).²⁰ Online Appendix Table B.1 regresses the logged number of live births in the urban and rural areas of all Danish counties on area fixed effects and an indicator for the occupational years (column 1), as well as its interaction with an urban indicator (column 2).²¹ As we show here, the number of births increased during the occupation period and especially urban areas witnessed increases.²² In the light of these results, we find it unlikely that selective fertility during the war years (among "well-off" parents in selected areas of the country) drive our results. However, as other studies set in similar contexts and given our data constraints, we should keep the potential confounding impact of WWII in mind when interpreting our results.

²⁰ While in general there is greater focus on the fertility rate changes during the "baby boom" following the war years, many countries saw increases in birth rates in the early 1940s.

²¹ The unit of observation are all urban and rural areas of the 23 Danish counties, i.e., 46 areas and Copenhagen (N = 47).

²² As we do not observe the number of births for all municipalities, we cannot examine the same pattern across implementing and non-implementing municipalities.

To further address concerns about the validity of our analyses, we follow four additional approaches. First, we control for municipality-specific linear time trends.²³ Second, we take advantage of a unique dataset, described in detail in Section III, on the demographic, political, economic, and health characteristics of municipalities in the years leading up to 1937. Table 1 compares the means of pre-1937 characteristics for municipalities that implemented the program between 1937 and 1949, and those that did not. We examine both levels and trends in the observable characteristics. Columns 1 and 2 reveal considerable differences in means between eventual-implementers and never-implementers. If these differences are not captured in municipality and cohort fixed effects or linear trends, and if the differences impact both treatment initiation and adult outcomes of treated individuals, we may falsely attribute their effect to the home visiting program. Inspired by the strategy of Hoynes, Schanzenbach, and Almond (2016), we therefore flexibly control for pretreatment characteristics and let the coefficients on municipal pretreatment characteristics, X_j^{pre37} , vary by cohort, θ_t :²⁴

$$(2) \quad y_{jt} = \alpha + \beta \text{homevisit}_{jt} + \gamma_j + \delta_t + \theta_t \times X_j^{pre37} + \varepsilon_{jt}.$$

Third, we combine our DiD approach with propensity score matching. We restrict our sample to implementing municipalities and matched non-implementing control municipalities of similar pre-1937 characteristics. Given that we match each eventually treated municipality with a unique control, we can assign treatment dates to untreated municipalities and estimate a model of the form

$$(3) \quad y_{jt} = \alpha + \beta \hat{p}ost_t \times \text{homevisit}_j + \lambda \times \hat{p}ost_t + \gamma_j + \delta_t + \varepsilon_{jt},$$

where $\hat{p}ost_t$ is an indicator for posttreatment years for all treated and matched control municipalities.²⁵

Our matching procedure results in a sample of 404 municipalities.²⁶ As columns 3 and 4 in Table 1 show, the matched treatment and control groups are very similar with respect to pre-1937 observables (both levels and trends). We cannot reject the null hypothesis of equality in means for any municipality-level observable in the matched sample. As we also balance pre-1937 trends, we are confident that our analysis on this matched sample credibly tests the robustness of our findings.

²³Our main results are not sensitive to the inclusion of quadratic trends, as detailed in Section IV.

²⁴Hoynes, Schanzenbach, and Almond (2016) analyze the long-run impact of the US food stamp program. To account for differences in trends across counties that implemented the program at different times, they interact pretreatment characteristics with linear time trends. We interact pre-1937 characteristics with birth year fixed effects.

²⁵Given that the $\hat{p}ost_t$ indicator is defined for both treated and untreated municipalities, it is not collinear with the term homevisit_j , as is the case in equations (1) and (2).

²⁶We use the information on the pre-1937 municipal (and for health data medical district-wide) characteristics for the matching. We use a nearest neighbor matching. To perform the matching, we use *psmatch2*. We impose a caliper of 0.05 and assign one control without replacement. Online Appendix Figure B.2 plots the sample restriction that results from our matching procedure: the figure plots the estimated propensity score for all municipalities and shows that a number of treated municipalities are off the common support, i.e., there are not enough unique matches for high-probability implementers among the untreated municipalities in our sample.

TABLE 1—MUNICIPALITY CHARACTERISTICS, FULL SAMPLE, AND MATCHED SAMPLE OF MUNICIPALITIES; MEANS AND *p*-VALUES FOR DIFFERENCE IN MEANS IN THE MATCHED SAMPLE

	Control (1)	Treated (2)	Matched control (3)	Matched treated (4)	<i>p</i> -value ((3) versus (4)) (5)
<i>Municipal controls, levels</i>					
Area, km ²	33.56	25.67	26.54	27.68	0.49
In population, 1935	7.09	7.63	7.21	7.23	0.69
Distance to Cph., km	187.02	110.62	127.35	127.64	0.97
Degree of urbanization (0–100), 1935	13.98	34.82	21.76	18.50	0.26
Infant mortality rate (per 1,000 live births), 1936	70.47	76.48	74.03	74.85	0.52
Mortality rate, acute enteritis (per 1,000 pop.), 1936	0.14	0.13	0.13	0.13	0.61
Votes for Agrarian Liberals 1935, pct.	38.99	23.51	27.68	29.25	0.22
Votes for Social Liberals 1935, pct.	11.63	13.55	12.97	14.06	0.29
Votes for Social Democrats 1935, pct.	24.81	39.32	34.93	33.94	0.41
Votes for Conservatives 1935, pct.	10.20	14.09	12.64	11.45	0.13
Population share female, 1930	48.46	49.27	48.86	48.79	0.79
Population share in agriculture, 1930	62.67	45.34	53.80	55.73	0.25
Population share in industry, 1930	14.65	23.46	18.38	18.31	0.94
In assessed property for property tax, 1936	7.29	8.24	7.74	7.74	0.98
Share paying property tax, 1936	4.40	5.24	5.26	5.25	0.95
In taxable income, 1936	6.01	6.93	6.33	6.34	0.92
Share paying income tax, 1936	20.15	24.60	22.58	22.71	0.80
Share population on public aid, 1936	3.47	5.08	4.36	4.27	0.68
<i>Municipal controls, changes</i>					
In population, 1930–1935	−0.18	0.26	−0.36	−0.26	0.43
Urbanization, 1930–1935	0.16	0.30	0.22	0.18	0.56
Votes for Agrarian Liberals, 1929–1935	−2.37	−1.73	−1.91	−1.94	0.76
Votes for Social Liberals, 1929–1935	−0.45	−0.36	−0.38	−0.45	0.29
Votes for Social Democrats, 1929–1935	0.62	0.61	0.53	0.63	0.15
Votes for Conservatives, 1929–1935	0.29	0.40	0.43	0.42	0.89
Share female voters, 1929–1933	−0.02	−0.05	−0.03	−0.08	0.31
Population share in agriculture, 1930–1940	−0.20	−0.12	−0.11	−0.12	0.74
Population share in industry, 1930–1940	0.36	0.31	0.29	0.30	0.79
In assessed property for property tax, 1927–1936	−23.48	−17.21	−21.69	−21.78	0.94
Share paying property tax, 1927–1936	−1.30	−1.54	−1.51	−1.56	0.27
In taxable income, 1927–1936	−5.23	−4.00	−4.93	−5.11	0.30
Share paying income tax, 1927–1936	−0.68	−0.80	−0.83	−0.85	0.73
Share population on public aid, 1935–1936	0.15	0.00	0.04	0.13	0.42
Number of municipalities	957	364	202	202	

Source: The data for this table comes from the “Statistical Commune Archive” (Danish Data Archive 2017), that consists of data collected from different censuses and elections. Changes in the levels are computed as differences between 2 years or as slope coefficients from 3–5 data points.

Finally, we perform our analysis on a sample that only includes municipalities that implement the program during the period in question, maintaining the specification in (2).²⁷ In our final approach, we thus exploit only variation in the exact timing of implementation across municipalities. Online Appendix Figures B.3 and B.4 plot these treated municipalities’ year of treatment initiation against their propensity score for treatment initiation (estimated in the matching procedure) and their 1936

²⁷ Given that hospital births were more common in towns, we have also tried excluding the five major towns—and thus nearly all hospital births—from our sample. Our results are not sensitive to this exclusion.

infant mortality rate.²⁸ The figures show considerable variation in ex ante levels of child health and ex ante probability of implementing among the implementing municipalities and no indication that healthier municipalities implemented earlier than less healthy municipalities (or vice versa). This evidence points to considerable arbitrary variation in the exact timing of implementation.

III. Data

A. Merge of Historical Data Sources and Creation of Estimation Sample

Our analysis combines unique data from three sources. We have collected data on the exact date of implementation of the home visiting program for all implementing municipalities during 1937–1949 in the Danish National Archives (see Wüst 2012).

We combine this data with municipal-level and district-level data on pretreatment characteristics. This data on municipal characteristics at various points in time comes from the Danish Commune Archive (Danish Data Archive 2017), which combines information from several censuses and elections. The Commune Archive contains data on the following municipal characteristics (pre-1937) that we use in our matching analysis and as control variables: percentages of votes for a set of Danish parties in a sequence of three elections, population in 1935, urbanization percentage in 1935, percentage of female population in 1930, percentage of workers in agriculture or industry in 1930, percentage of population on public aid in 1936, percentage of income and property tax payers in 1936, and aggregated taxable income and assessed property value in 1936.²⁹ From the Medical Reports for the Kingdom of Denmark (DHA various years), we include data on two relevant variables: infant deaths per 1,000 live births and infant deaths from acute enteritis in 1936.³⁰ We compute measures of municipalities' size and location (distance from Copenhagen) from data on the historical administrative structure of Denmark.³¹

Finally, we add individual-level adult outcome data (1980–2012) for the population of individuals born in Denmark between 1935 and 1949. These data contain information on date and place of birth and on long-run health outcomes, for the population of Danish-born individuals. Unfortunately, we cannot link families in the Danish register data for the given cohorts. Thus, we cannot add estimates that compare between siblings to test for the robustness of our results. However, in the likely presence of parental responses to program exposure that may systematically differ across treated and untreated siblings, it is not straightforward to interpret results from a within-family estimate.³² Furthermore, while one mechanism for a short-run

²⁸ Given that the infant mortality rate data is at a higher level of aggregation (all towns or rural areas in a medical district), the figure has fewer (due to overlapping) data points.

²⁹ We have compiled the municipal data from several files delivered by the Danish Data Archive. The material documenting our merging of data sources is available upon request.

³⁰ These data are aggregated at a higher level, namely for all urban and rural municipalities in each of the 23 medical districts and Copenhagen.

³¹ We use data from the DigDag-project (Digital Atlas of the Danish Historical, Administrative Geography); for details, see www.DigDag.dk.

³² Our reduced-form estimate includes the impact of parental responses to the program when we estimate its treatment effect. We believe that this approach results in a meaningful estimate given the informational nature of the program that aimed at changing maternal behaviors.

and long-run effect of the home visiting program may be its impact on the timing of subsequent births (because prolonged breastfeeding duration may lead to longer time periods between births, as shown in Jayachandran and Kuziemko 2011), we cannot examine the impact of the home visiting program on spacing or family size.³³

To construct an indicator for individuals' treatment status, we use date and parish of birth.³⁴ Two minor restrictions apply: first, as most Danish register data dates back to 1980, individuals in our outcome data have to reside in Denmark at least one year after that date. Second, to secure that all cohorts that we study enter the risk period at the same time (i.e., are left-censored at the same age), we perform our main analyses on a sample comprising all individuals who survive until they turn 45. Thus, all our main analyses are conditional on the individuals having survived and not having permanently left Denmark before age 45.³⁵

We are confident, however, that differential mortality among treated and untreated individuals prior to age 45 is unlikely to significantly affect our estimates. First, Wüst (2012) estimates that only 325–520 additional infants per cohort survived infancy as a result of the program. We show that omitting 1 percent of our individuals in the sample does not impact our main results.³⁶

Second, only few individuals die between ages 1 and 45. In online Appendix A.2, we analyze this issue further. First, using aggregate data, we show that the number of missing observations per cohort (i.e., the difference between one-year survivors and the individuals observed in our register data) remains relatively small and stable. If the program had affected mortality before age 45, we should see a decreasing (or increasing) share of missing observations as the program spread to more municipalities.³⁷ Second, we use data on the youngest cohorts in our sample to examine the program's effect on mortality prior to age 45 (age 40–45). Online Appendix Figure A.2 shows that there are no survival gains of the program at these earlier ages.

Online Appendix Table B.2 shows that for the vast majority of all relevant individuals in the administrative data, we can match the parish of birth to a municipality. Half of the approximately 12 percent of observations in the administrative data that we cannot merge are foreign-born or born in Greenland and thus not relevant for our study. Thus, we omit only about 6 percent of relevant Danish-born residents due

³³Bütikofer, Løken, and Salvanes (2015) exploit sibling data and show that their main results are robust to the inclusion of a sibling fixed effect. Bütikofer, Løken, and Salvanes (2015) do not present results for the impact of well-baby visits on children's spacing or family size.

³⁴In the administrative data (for historical reasons), the place of birth is recorded at the parish level. In rural areas, one or two parishes were equivalent to a municipality for most cases during the period. In towns, many parishes form one municipality. Given that during the period, new parishes are formed once older parishes become too large in terms of population, we use the 1930, 1940, and 1950 parish and municipality structure defined in data from the DigDag-project (Digital Atlas of the Danish Historical, Administrative Geography) to assign parishes to municipalities (the level at which we observe the treatment).

³⁵We condition on having survived until age 45 because our data otherwise would (at any given age) contain survivors and deaths for younger cohorts but only survivors for older cohorts (because we only observe deaths after 1980). See online Appendix A.2 for more information on the left censoring and our sample creation. Here, we also present results for mortality at younger ages for the youngest cohorts in our sample.

³⁶Five hundred individuals constitute less than 1 percent of a given birth cohort of the time. To be conservative in our test, we omit 1 percent of individuals at the bottom percentile of the income distribution. We omit individuals from the bottom of the empirical distribution because we may assume that the weakest infants of each cohort survived due to the program. Our main results are not impacted by this test and are available from the authors.

³⁷Unfortunately, we do not observe the number of births in a cohort at the municipal level. We thus cannot directly look at fertility responses in treated municipalities.

to invalid parish codes. Conditioning on non-missing information on municipality observables and omitting hospital births,³⁸ we end with an estimation sample of 876,912 individuals. We collapse our data to 20,078 municipality \times birth cohort \times treatment status-cells.

B. Administrative Data on Outcomes

Data on individuals' health in middle age (45–64) comes from the Danish Inpatient Register (1980–2012) and the Danish Death Register (1980–2012). In our main analyses, we only consider individuals at ages that allow our entire sample to have aged through the period of interest (45–64 years). First, we construct an indicator for the individual's survival beyond a given age (age-specific survival rates for the ages 46–64). Second, we create a variable equal to the total number of nights an individual spent in a hospital between ages 45–64.³⁹ Third, we consider medical diagnoses that the epidemiological and economics literature suggests that early life conditions may affect (Gluckman et al. 2008; Lynch and Davey Smith 2005; and Hoynes, Schanzenbach, and Almond 2016). We focus on heart disease, cardiovascular disease, and diabetes. We also include individuals who die from these conditions (i.e., are diagnosed at death) into our measures of disease morbidity. Additionally, we create an indicator for being diagnosed with more than one of the above conditions. Finally, because cancer accounts for the largest share of deaths in our sample, we also study the probability of being diagnosed with cancer.

Exploiting the death records' information on cause of death, we create indicators for death from several causes. Online Appendix Table B.3 shows the distribution of causes of death among all the observed deaths in our data (deaths between ages 45 and 64).⁴⁰ We separately consider the four biggest groups of causes of death: cancer (41 percent), heart disease and other cardiovascular disease (together 28 percent), deaths related to the digestive system (10 percent), and respiratory disease (9 percent). Furthermore, we study the probability of not having a cause of death. As the cause of death is missing for around 7 percent of deaths in our data, we are cautious in interpreting our results of these analyses. More information on the coding of diagnoses and causes of death appears in online Appendix A.1.

IV. Long-Run Health Effects of the Home Visiting Program

A. Event Study

To graphically analyze our data and assess the validity of our design, Figure 3 presents an event graph based on our baseline specification for our main outcome,

³⁸ We cannot merge hospital births to a parish of birth, and thereby we cannot merge individuals to their municipality of birth, where the treatment is defined. However, hospital births were not yet common at the time (Vallgård 1996). By omitting hospital births, we omit 5 percent of our sample.

³⁹ Given that this outcome measure is skewed, we have also considered a winsorized version of it. Here, we set hospital nights to a maximum of the ninety-ninth percentile at around 190 nights. We find similar results that are available on request.

⁴⁰ The table both considers first and second cause of death on the death certificate. There can be up to three causes of death in the administrative data.

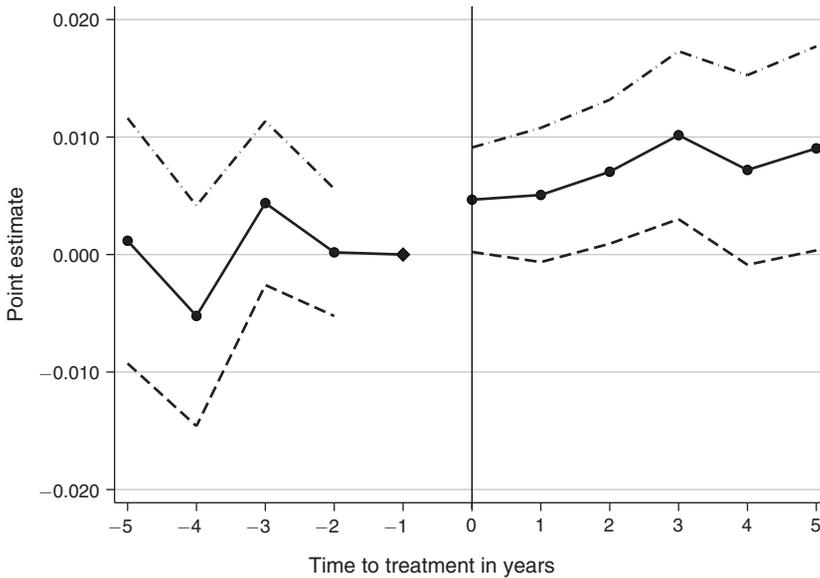


FIGURE 3. EVENT STUDY FOR EFFECT OF THE HOME VISITING PROGRAM ON SURVIVAL BEYOND AGE 64

Notes: Time to treatment is in years for the unbalanced sample of all eventually treated municipalities (with non-missing data on controls, i.e., the sample used in the main estimations). Models include indicators for five years for both before and after treatment initiation, as well as indicators for more than five years before and after treatment initiation, year fixed effects, and municipality fixed effects. Figures that also include municipality-specific trends or controls interacted with year fixed effects are very similar. The omitted indicator for event time is $t = -1$. The figure displays coefficients and a 95 percent confidence interval.

adult survival beyond age 64. The graph plots estimates and confidence intervals for a set of indicators for years to treatment initiation for a sample of eventually treated municipalities. To be able to observe a total of up to ten years around treatment initiation, we do not balance our sample of implementing municipalities.⁴¹ We “bin up” the endpoints of the event time axis and include an indicator for up to ten years both before and after treatment initiation.

Figure 3 shows that, before treatment implementation, estimates for the event time indicators are insignificant, around zero, and display no clear pre-trend. After the implementation at $t = 0$, a program effect appears. While confidence intervals initially do not exclude 0, the effects are positive from year $t = 0$ onwards. The initially increasing size of the estimated effects for consecutive years after treatment initiation may reflect the program’s increasing effectiveness, e.g., due to nurses experience with conducting home visits. Moreover, earlier work has shown that prolonged breastfeeding can impact the spacing of births (Jayachandran and Kuziemko

⁴¹ We face a trade-off between balancing the sample of municipalities that contribute with data for a rather short number of years around treatment initiation or including longer time-to-treatment spans but then allowing for the sample of municipalities to be unbalanced (as municipalities that implement in later years/earlier years only have few posttreatment/pretreatment years). To show more pretreatment and posttreatment initiation years, we accept a compositional change in the group of implementing municipalities in the graphs that we present. Graphs that condition on a more balanced sample and only look at shorter periods around treatment initiation are very similar and available upon request. Graphs that are based on a specification with baseline municipality controls or trends are very similar and available upon request.

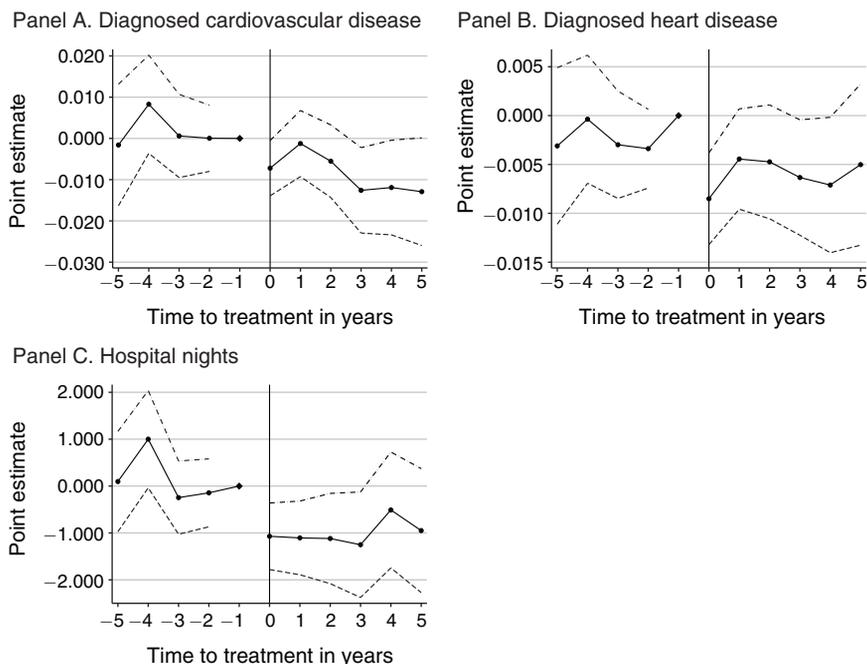


FIGURE 4. EVENT STUDY FOR EFFECT OF THE HOME VISITING PROGRAM ON OTHER HEALTH OUTCOMES

Notes: Time to treatment is in years for the unbalanced sample of all eventually treated municipalities (with non-missing data on controls, i.e., the sample used in the main estimations). Models include indicators for five years for both before and after treatment initiation, as well as indicators for more than five years before and after treatment initiation, year fixed effects, and municipality fixed effects. Figures that also include municipality-specific trends or controls interacted with year fixed effects are very similar. The omitted indicator for event time is $t = -1$. The figure displays coefficients and a 95 percent confidence interval.

2011). If longer spacing impacts infant health (and in turn longer run health trajectories) positively, we may expect that the effects of the home visiting program become stronger over time (for cohorts born at later event times). Finally, nurses likely also gave advice on health behaviors during (following) pregnancies, and thus we may expect an increased effect for consecutive children.

Figure 4 presents equivalent event graphs for two diagnoses outcomes, the probability of being diagnosed with cardiovascular disease and heart disease, and the number of nights hospitalized between ages 45–64. For both diagnoses outcomes, the event graphs suggest no clear pre-trends and a drop in the probability of diagnosis, although estimates are not always precisely estimated. The graph for hospital nights indicates a clear drop in the number of hospital nights for individuals born after treatment initiation.

B. The Effect of Home Visiting on Long-Run Survival

Table 2 presents our main regression results for the effects of the home visiting program on the probability of surviving to ages 50, 55, 60, and 64. Each row presents estimates for the treatment indicator from our five approaches. All columns

TABLE 2—EFFECT OF THE HOME VISITING PROGRAM ON SURVIVAL BEYOND VARIOUS AGES, COHORTS 1935–1949

Outcome	All (1)	All (2)	All (3)	Matched (4)	Ever impl. (5)
Survival until age 50	0.064 (0.063)	0.087 (0.078)	0.165 (0.081)	0.063 (0.171)	0.198 (0.096)
Mean of dependent variable \times 100	98.342	98.342	98.342	98.377	98.218
Observations	20,078	20,078	20,078	6,204	5,769
Survival until age 55	0.228 (0.091)	0.264 (0.119)	0.312 (0.120)	0.317 (0.273)	0.335 (0.151)
Mean of dependent variable \times 100	95.880	95.880	95.880	95.925	95.610
Observations	20,078	20,078	20,078	6,204	5,769
Survival until age 60	0.337 (0.123)	0.260 (0.140)	0.469 (0.167)	0.587 (0.372)	0.469 (0.207)
Mean of dependent variable \times 100	92.432	92.432	92.432	92.535	91.967
Observations	20,078	20,078	20,078	6,204	5,769
Survival until age 64	0.382 (0.155)	0.257 (0.178)	0.454 (0.195)	0.592 (0.435)	0.387 (0.235)
Mean of dependent variable \times 100	88.756	88.756	88.756	88.791	88.121
Observations	20,078	20,078	20,078	6,204	5,769
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes
<i>Municipal</i>					
Fixed effects	Yes	Yes	Yes	Yes	Yes
X (level) \times year interactions	No	No	Yes	No	Yes
X (trend) \times year interactions	No	No	Yes	No	Yes
Linear time trends	No	Yes	No	No	No

Notes: Each cell presents the coefficient for the treatment indicator for a different regression. Survival indicates that the individual has survived at least to the given age. All means and coefficients are pre-multiplied by 100 and interpretable as percentage point changes. The units of observation are municipality \times year of birth \times treatment status-cells. We weight regressions with the number of observations in each cell. Column 4 presents the estimate of the treated \times post indicator in the matched sample that assigns a treatment date to the matched control municipalities (see Section II for details). We cluster all standard errors at the municipal level (1,322 clusters).

include municipality and cohort fixed effects. In column 2, we additionally control for municipality-specific time trends. In column 3, we add controls for levels and trends in pretreatment municipal characteristics with coefficients allowed to vary by year. Columns 4 and 5 present results based on two subsets of municipalities: the first is a sample of matched treatment and control municipalities. In this specification, we assign control municipalities the treatment year of the matched treated municipalities. Finally, the results in the last column in Table 2 are based on the sample of eventually implementing municipalities (from 1937–1949). This specification also flexibly controls for municipal characteristics. For convenience, in all tables, we present coefficients that are pre-multiplied by 100, and thus interpretable as percentage point changes (except for the number of hospital nights).

Table 2 shows that treated individuals have significantly higher survival probabilities in the age range that we consider, especially at and after age 55. As an example, at the mean of the dependent variable of around 92 percent, our estimates imply that individuals are 0.2–0.6 percent more likely to survive past age 60. The coefficients are relatively stable across specifications but somewhat bigger once we include

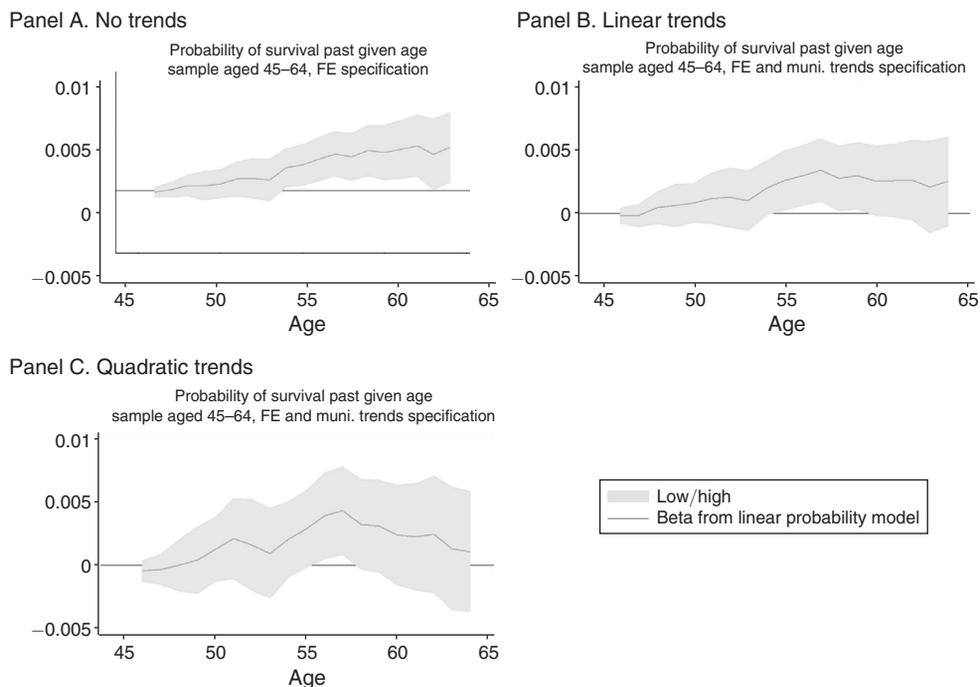


FIGURE 5. THE EFFECT OF THE HOME VISITING PROGRAM ON YEARLY SURVIVAL PROBABILITIES, COHORTS 1935–1949

Notes: The figure plots estimates and 95 percent confidence intervals from separate regressions of probability of survival beyond the given age on treatment status and year and municipality fixed effects, fixed effects and linear municipality-specific trends, and fixed effects and quadratic municipality-specific trends. Standard errors are clustered at the municipality level.

controls for pretreatment municipal characteristics. As the informational components of the program may have spilled over to parents in control municipalities, we interpret our estimates as lower bounds of the true effect.

To study survival probabilities in more detail over the period that we observe for the given cohorts, we display results for survival past all ages in the range 45–64 in Figure 5. As Figure 5 shows, survival gains of the home visiting program are apparent for individuals who have survived to age 55. The estimates are largest around this age and—for the specifications with trends—decrease in size after that age. One reason may be that untreated individuals begin to suffer from serious diseases that affect mortality earlier in middle age than treated, healthier individuals do.

Figure 5 also illustrates the impact of the inclusion of linear (and quadratic) trends on our conclusions. When accounting for trends, we lose precision for survival beyond age 60. However, point estimates are similar across specifications.⁴² Another way of examining the potential influence of pretreatment trends is to examine the impact of placebo reforms. We should not be likely to find estimates that are similar to our “true estimates” for the effect of home visiting when assigning random

⁴²Consult online Appendix Table B.4 for point estimates for selected survival and diagnoses outcomes and different trend specifications.

treatment dates to municipalities. Thus, we apply a permutation test inspired by Chetty, Looney, and Kroft (2009): first, we randomly pick a treatment initiation year between 1937–1948.⁴³ Second, we assign this treatment year to a random municipality in our sample (if this municipality is not yet taken, i.e., has not been assigned another random treatment year). We repeat these 2 steps 400 times and end up with a sample that has around one-third of all municipalities being in the “eventually treated” group. Next, we estimate our treatment effect on the full sample with placebo treatment indicators as markers for treatment. We estimate both our baseline specification (equation (1)) and our specification with pretreatment control variables (equation (3)).⁴⁴ We repeat this entire procedure 500 times and plot the distribution of placebo estimates for long-run survival in the top panel of Figure 6, along with a vertical line that marks our true estimate. If there is a significant effect of home visiting on survival outcomes, we expect the true estimate to fall into the tails of the distribution of placebo estimates, i.e., among the 5–10 percent we are least likely to find. As Figure 6 illustrates, our estimates fall into the tails of the distribution of estimates that we create (among the 5 percent most extreme estimates), and thus gives further credibility to our results.

Online Appendix Table B.5 shows results for our survival measures separately for men and women. As expected, female mortality rates are lower than male ones in our sample. However, we find stronger results for our survival measures for women. While point estimates are generally similar in size and not statistically different from each other, our main estimates are driven by more precise estimates for females.

Our results suggest that the program saved a significant number of life years for treated individuals. One way of cautiously quantifying the long-run health benefits of the program is to calculate the number of saved life years. If we very conservatively assume that treated individuals die at age 61, but would, in the absence of the program, have died at age 60, our estimates translate into 690 to 2,070 saved life years in the cohorts we study (only through its direct effect on survival).⁴⁵ Likely even more important for an assessment of the cost-effectiveness of the program, in the next section, we analyse the mechanisms that drive these mortality results, namely a set of medical diagnoses and a measure for hospitalizations.

C. The Effect of Home Visiting on Pathway Health Outcomes and Educational and Labor Market Outcomes

In Table 3, we turn to the analysis of rarely available measures of adult health that reveal more about the driving forces behind the survival effect that we find. Our estimates for hospitalizations and diagnoses given at the hospital show that treated individuals enjoy better health during adulthood: they are hospitalized fewer nights (about half a night less between ages 45 and 64) and are less likely to be diagnosed with lifestyle-related diseases. In line with studies suggesting long-run impacts of

⁴³We find it reasonable to constrain the placebo years to this period as we always want to allow for at least two treated years.

⁴⁴Estimating our trends specification is computationally very demanding, and thus we opt for these two specifications.

⁴⁵ $(0.002 \times \text{size of treatment group in our cohorts})$ to $(0.006 \times \text{size of treatment group in our cohorts})$

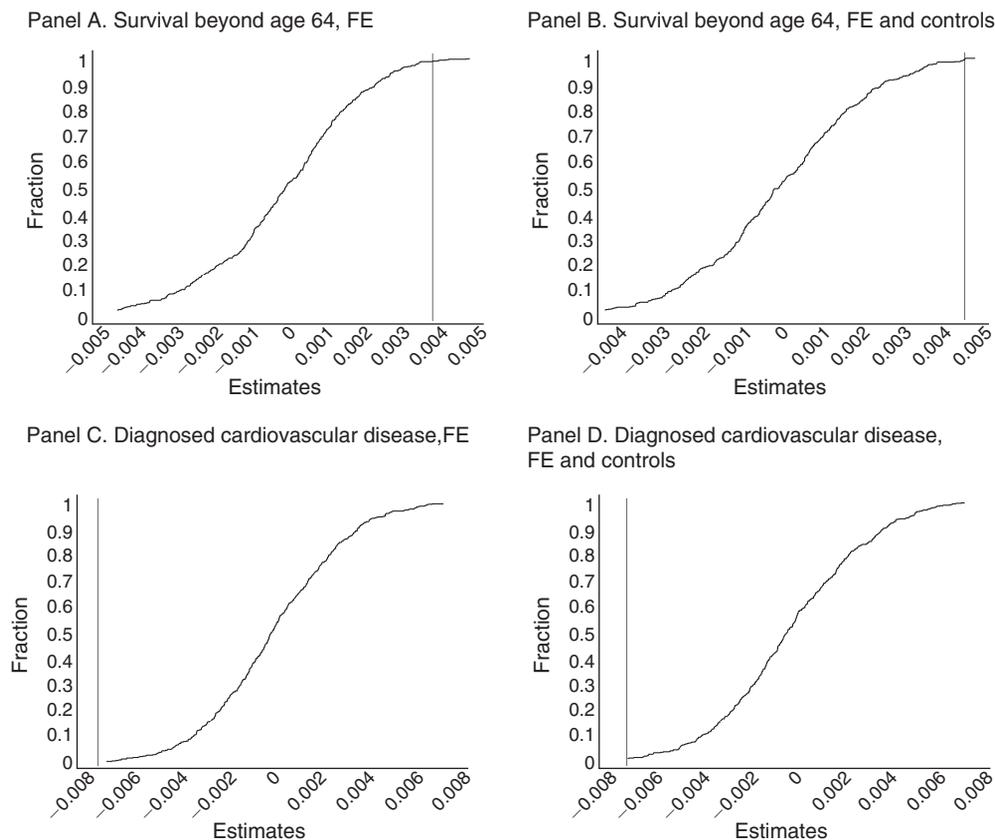


FIGURE 6. PERMUTATION TEST FOR SURVIVAL BEYOND AGE 64 AND DIAGNOSED WITH CARDIOVASCULAR DISEASE

Notes: The figure shows the CDF of 500 placebo estimates for the effect of the home visiting program on survival past age 64 and probability of a diagnosis with cardiovascular disease. We use the specification of equation (1) (with fixed effects) and (2) (with interacted pretreatment controls and fixed effects). To estimate placebo effects, we allocate random treatment years to up to 350 random municipalities in our sample. The vertical line marks our true estimate. For details, see Section IV.

early life health for later-life disease prevalence (Barker 1990, Forsdahl 1977) and with similar studies on the impact of well-baby visits on measures of cardiovascular risks at younger ages (Bütikofer, Løken and Salvanes 2015), we find that those exposed to the home visiting program in infancy are less likely to be diagnosed with a cardiovascular disease by age 64. Although estimates for diabetes diagnoses are not precise, together with the results for cardiovascular and heart disease, they suggest an impact of the program on diseases earlier shown to be related to proper infant nutrition.

While Hoynes, Schanzenbach, and Almond (2016) and Bütikofer, Løken, and Salvanes (2015) emphasize the importance of overweight and other risk factors in adult life in this context, we cannot study individuals' weight and height with our data. However, by studying whether treated individuals have more than one diagnosis (among heart disease, cardiovascular disease, and diabetes), we attempt to study an indicator that is more closely related to their measures of "cardiac risks"

TABLE 3—EFFECT OF THE HOME VISITING PROGRAM ON PATHWAY HEALTH OUTCOMES: PROBABILITY OF DIAGNOSIS BY AGE 64 AND HOSPITAL NIGHTS IN THE AGE RANGE 45–64, COHORTS 1935–1949

Outcome	All (1)	All (2)	All (3)	Matched (4)	Ever impl. (5)
Hospital nights, age 45–64	–0.569 (0.225)	–0.547 (0.275)	–0.491 (0.278)	–1.194 (0.503)	–0.271 (0.328)
Mean of dependent variable	17.018	17.018	17.018	16.797	17.657
Observations	20,078	20,078	20,078	6,204	5,769
Diagnosed cardio	–0.737 (0.207)	–0.529 (0.253)	–0.707 (0.310)	–0.359 (0.597)	–0.729 (0.392)
Mean of dependent variable \times 100	26.653	26.653	26.653	26.815	26.146
Observations	20,078	20,078	20,078	6,204	5,769
Diagnosed heart	–0.199 (0.140)	–0.275 (0.161)	–0.399 (0.169)	0.072 (0.382)	–0.435 (0.197)
Mean of dependent variable \times 100	8.293	8.293	8.293	8.487	8.320
Observations	20,078	20,078	20,078	6,204	5,769
Diagnosed diabetes	–0.018 (0.095)	–0.171 (0.129)	0.020 (0.127)	–0.304 (0.304)	0.066 (0.156)
Mean of dependent variable \times 100	5.052	5.052	5.052	4.950	5.425
Observations	20,078	20,078	20,078	6,204	5,769
Multiple diagnoses by age 64	–0.663 (0.215)	–0.575 (0.268)	–0.588 (0.313)	–0.562 (0.589)	–0.601 (0.400)
Mean of dependent variable \times 100	29.714	29.714	29.714	29.811	29.444
Observations	20,078	20,078	20,078	6,204	5,769
Diagnosed cancer	0.019 (0.168)	0.069 (0.197)	0.059 (0.216)	–0.368 (0.448)	0.118 (0.251)
Mean of dependent variable \times 100	11.756	11.756	11.756	11.553	12.308
Observations	20,078	20,078	20,078	6,204	5,769
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes
<i>Municipal</i>					
Fixed effects	Yes	Yes	Yes	Yes	Yes
X (level) \times year interactions	No	No	Yes	No	Yes
X (trend) \times year interactions	No	No	Yes	No	Yes
Linear time trends	No	Yes	No	No	No

Notes: Each cell presents the coefficient for the treatment indicator for a different regression. Outcomes denote probability of being diagnosed between ages 45 and 64 and the number of hospital nights in this age range. All means and coefficients (except for hospital nights) are pre-multiplied by 100 and interpretable as percentage point changes. The units of observation are municipality \times year of birth \times treatment status-cells. We weight regressions with the number of observations in each cell. Column 4 presents the estimate of the treated \times post indicator in the matched sample that assigns a treatment date to the matched control municipalities (see Section II for details). We cluster all standard errors at the municipal level (1,322 clusters).

and “metabolic disorder” and combines the individual diagnoses that we observe, i.e., we study whether treated individuals have a lower probability of suffering from closely-related comorbidities. In line with previous findings, our analysis shows that treated individuals have a lower probability of being diagnosed with at least two of the diagnoses groups that we also study individually. Thus, our findings suggest that

treated infants enjoy more healthy life years as adults and consequently are likely to cost the medical system significantly less.⁴⁶

Moreover, as a significant share of individuals in our sample is diagnosed with cancer, we examine this diagnosis group in the bottom of Table 3. We find mostly very small and always insignificant estimates for the effect of the home visiting program on this diagnosis group. While this result is not a formal falsification test, we note that we only find precise and negative results for diagnoses that elsewhere have been shown to be impacted by early life circumstances. Finally, following the same intuition as for our survival results and shown in the lower panel of Figure 6, our estimate for the effect of the home visiting program on cardiovascular disease falls in the lower tail of placebo estimates from our permutation test. This finding—together with the event graphs—documents the robustness of our morbidity results.

Important for policies in developing countries today, our results show that the home visiting program improved adult life health in those dimensions that have been shown to relate to early life nutrition and disease exposure, factors that are still concerns in many contemporary settings. Furthermore, the sizable long-run effects that we find stem from a relatively cheap intervention aiming at monitoring health and informing parents on proper infant care through contacts with trained health professionals.⁴⁷ While recent evidence from developing country contexts confirms that these types of interventions have important short-run impacts (for an example, see Bhandari et al. 2003), our findings suggest that we should factor in important long-run gains when assessing their effectiveness.

Does the “diagnoses advantage,” which we have established, also show in cause-specific mortality? Appendix Table B.7 relates our treatment indicator to mortality from several causes, some of which we do not—given existing studies on the impact of early life nutrition and disease—expect to be modified. Unfortunately, all our estimates are imprecise and do not allow us to make strong conclusions based on an analysis of causes of death.⁴⁸

Finally, we explore the impact of the home visiting program on a set of educational and labor market outcomes, which may constitute important pathways for an impact of the program on adult mortality. Results for these analyses are available in Appendix Table B.8. All of these analyses control for a schooling reform that affected mostly rural students of the post-1946 cohorts (for details, see Arendt 2008).⁴⁹ We have explored potential effects of the program on individuals’ years of schooling, the probability of acquiring more than compulsory schooling, log wages, and occupation. While we may expect education and occupational choices to be important pathways that explain some of the health effects we find, our empirical results do

⁴⁶ Parallel to our analyses for adult survival, we also split our analyses of diagnoses and hospitalizations for males and females. However, our results in online Appendix Table B.6 are less precise. They may cautiously suggest stronger effects on hospitalizations for women. These results may explain larger survival gains that we see among women.

⁴⁷ Wüst (2012) calculates that the program cost \$159 USD per person year saved (in 2010 USD).

⁴⁸ We have also run these regressions for causes of death as outcomes based only on the sample of deaths. Focusing on the sample of deaths does not change our conclusions.

⁴⁹ To account for the impact of the reform on schooling outcomes, we include an indicator for rural \times post 1946 cohort. The motivation of this approach is that the reform increased the educational chances especially of rural students of impacted cohorts.

not support this hypothesis. Our estimates are mostly very small in absolute size (evaluated at the relevant sample mean), less precise than the health results, and not stable across specifications. Also, event study graphs (online Appendix Figure B.5) do not suggest a clear effect of the home visiting program.⁵⁰ Thus, we conclude that the home visiting program impacted adult mortality predominantly through its direct impact on adult morbidity.

V. Discussion and Conclusion

This paper presents the first evidence on the causal effect on adult health of the 1937 Danish home visiting program, a policy aimed at improving the health of all infants. In municipalities that implemented the program, nurses visited all infants and their mothers at home around ten times during the first year of the child's life to encourage breastfeeding and proper infant care and to monitor the child's health.

Exploiting variation in the timing of implementation across municipalities, we show that those visited by nurses in infancy experience a robustly estimated increase in adult survival rates in their 50s and 60s. Our findings further show that the intervention "programs" individuals for lower predisposition to serious adult diseases. We find that treated individuals are less likely to be diagnosed with cardiovascular and heart disease and spend fewer nights in the hospital during middle ages.

While we, like many existing studies on long-run effects of early life health, cannot disentangle the relative importance of different program components for the observed program effects, the content of the Danish home visiting program suggests that its effects were driven by improved infant nutrition, faster referral of ill infants to doctors, and an improved (hygienic) home environment. Our findings are in line with other research highlighting the effectiveness of interventions that focus on information on proper infant nutrition and care in high infant mortality contexts. Moreover, recent research highlights the importance of parental knowledge and skills for child development (Cunha, Elo, and Culhane 2013). Our study suggests that interventions with an informational nature may successfully change parental investment in child health and this change may have very long-run consequences.

Moreover, while we cannot examine the heterogeneity of program effects across different types of families (because we lack historical data on family-level characteristics) or across relevant municipal characteristics,⁵¹ we may hypothesize that families with higher exposure of infants to disease or poor nutrition have had a larger scope for positive effects of the home visiting program. Also follow-up investments may have varied systematically across families, and the impact of follow-up policies like childcare or improved housing may vary according to whether individuals had access to the nurse program. We leave this type of analysis of heterogeneous effects and potential complementarities for future research.

⁵⁰ Finally, a permutation test analogous to the one we perform for adult survival and diagnoses does not suggest a similar uniqueness of our estimate for the effect of home visiting on years of education. Results are available upon request.

⁵¹ We have attempted an analysis of effects across urban versus rural municipalities and districts with a high versus low infant mortality. Results of these analyses are inconclusive, likely due to the level of aggregation of our background data (which is not at the individual level).

The magnitude of the impact on adult mortality we find may be specific to disease and poor nutrition environments, such as Denmark of the 1930s. As such, our findings support the argument for home visiting in developing countries (see, also, World Health Organization and UNICEF 2009). Furthermore, our study is among the few studies that focus on the effectiveness of universal programs, i.e., programs that grant access to all infants and their mothers. Our findings—together with similar evidence on the long-run effects of universal programs in Norway and Sweden—indicate that estimates that ignore the long-run benefits of these early life health programs may significantly understate the returns to such programs.

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