THE DANISH NATIONAL CENTRE FOR SOCIAL RESEARCH

Jonas Hjort
Mikkel Sølvsten
Miriam Wüst

08:2014 WORKINGPAPER

UNIVERSAL INVESTMENT IN INFANTS AND LONG-RUN HEALTH: EVIDENCE FROM DENMARK'S 1937 HOME VISITING PROGRAM

Columbia University, Columbia Business School, USA
UC Berkeley, USA
SFI – The Danish National Centre for Social Research

UNIVERSAL INVESTMENT IN INFANTS AND LONG-RUN HEALTH: EVIDENCE FROM DENMARK'S 1937 HOME VISITING PROGRAM

Jonas Hjort
Mikkel Sølvsten
Miriam Wüst

Columbia University, Columbia Business School, USA
UC Berkeley, USA
SFI -The Danish National Centre for Social Research,
Copenhagen, Denmark;

Working Paper 08:2014

UNIVERSAL INVESTMENT IN INFANTS AND LONG-RUN HEALTH: EVIDENCE FROM DENMARK'S 1937 HOME VISITING PROGRAM

Jonas Hjort* Mikkel Sølvsten† Miriam Wüst‡

September 16, 2014

Abstract

This paper provides the first estimates of the long-run health effects of a universal infant health intervention. We examine 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 are 5–8 percent less likely to die in middle age (45–57), experience fewer hospital nights and are less likely to be diagnosed with and die from cardiovascular disease. These results suggest that an improved nutrition and disease environment in infancy "programmed" individuals for lower predisposition to serious adult diseases.

JEL: I12, I18; Keywords: early intervention, home visiting program, long-run effects, administrative data, difference in differences

*Columbia University, Columbia Business School, 3022 Broadway, Uris Hall 622, New York, NY 10027, hjort@columbia.edu

†UC Berkeley, 530 Evans Hall, Berkeley, CA 94720, mikkel@econ.berkeley.edu

‡corresponding author, SFI–The Danish National Center for Social Research, and Aarhus University RECEIV, SFI: Herluf Trolles Gade 11, 1052 Copenhagen, Denmark, miw@sfi.dk

We thank Peder Dam and the DigDag project for invaluable help with the data on Denmark's historical administrative structure, and Paul Bingley for access to the administrative data. The Danish Data Archive provided the data from the "Statistical Commune Data Archive." We thank Doug Almond, Paul Bingley, Martin Browning, David Card, Bo Honoré, Hilary Hoynes, Ilyana Kuziemko, Jonah Rockoff, Maya Rossin-Slater, and seminar participants at SFI, the University of Copenhagen, Columbia University, Statistics Norway, Uppsala University, University of Lund, University of Essen, University of Mannheim, and the NBER Cohort Studies meeting 2014 for helpful comments.

Wüst gratefully acknowledges financial support from the Danish Council for Independent Research (grant 11-116669).

1 Introduction

An existing literature documents dramatic long-run health consequences of exposure to large negative shocks such as epidemics and famines early in life (Currie and Almond, 2011). Researchers have recently begun to investigate the possibility that improvement in early life health within the reach of policy may also affect adult health outcomes. Can public health programs targeting infants shift long-run health trajectories? The evidence is scant but promising.[1]

This paper is the first to examine the causal effect on adult health of a public program aimed at improving the health of all infants: the Danish home visiting program, initiated in 1937. The Danish National Board of Health (DNBH) designed the 1937 home visiting program and issued uniform guidelines: Trained nurses conducted about 10 home visits to all infants during the first year of life. During those visits, nurses encouraged mothers to breastfeed and keep the home environment clean. Moreover, they referred ill infants to doctors for treatment (Buus, 2001).

Using aggregate historical records, Wüst (2012) shows that the program let to a significant increase in infant survival of about 5–8 lives saved per 1000 live births and reduced mortality from diarrhea-related causes. That home visiting accounted for about 17–29 percent of the period's overall decrease in diarrhea-related mortality highlights a potential mechanism for longer-run benefits of this infant health program: The one-year survivors of treated cohorts most likely benefited from home visiting through better infant nutrition and less severe sickness periods.

Although designed centrally, the home visiting program was implemented locally: out of 1345 Danish municipalities, 350 initiated the program during the 1937–1949 period we consider.[2] We thus follow a difference-in-differences approach and compare changes in adult outcomes across cohorts born in municipalities that initiated the program to changes

[1] Bhalotra and Venkataramani (2011) show that a reduction in pneumonia among U.S. infants in the 1930s and 1940s, due to the development of antibiotics, reduced disability in adulthood. Hoynes, Schanzenbach and Almond (2012) find that the provision of food stamps for poor families with children in utero and during their early childhood years improves the health outcomes of Americans in adulthood.

[2] From 1974, municipalities were required to implement the home visiting program.

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 (DNBH, various years). As we detail in section 4, we use individual-level data on health outcomes in middle age (45 to 57)—when a non-negligible share of individuals begin to suffer from serious health conditions and die—for the population of Danish citizens born between 1937 and 1949 and observed in the administrative records 1980-2008.

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). To address 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 pre-treatment 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 pre-treatment 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. Individuals exposed to home visiting in infancy are 5–8 percent less likely to die in middle age (between ages 45 and 57). 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 and die from cardiovascular disease in middle age.[3]

The estimated mortality gains are larger for women than for men. We find that the

[3]This result complements the findings in Mazumder et al. (2009), who show that prenatal exposure to influenza increases the risk of cardiovascular disease in middle age.

decrease in cardiovascular disease and cause-specific mortality is greater among those born in municipalities with worse initial levels of infant health. This finding points at the importance of the disease environment in infancy for the long-run effects of the home visiting program.Moreover, those born in urban municipalities experience greater improvements in middle age health due to the home visiting program than those born in rural municipalities. This finding may indicate that follow-up investments specific to urban areas are important for the size of our estimates. In sum, however, we find positive effects on long-run health for all groups of individuals and thus support for universal implementation.

This paper builds on the early influences literature. There is now considerable empirical evidence that adverse conditions early in life can affect health outcomes in adulthood (see reviews by Currie, 2009; Currie and Almond, 2011). Parallel theoretical work lays out mechanisms through which good health in early childhood can unlock lifetime benefits (see e.g. Heckman and Mosso, 2014; Cunha and Heckman, 2007), for example through dynamic complementarities with parental investment or due to early programming of middle age-diseases (e.g., Crimmins and Finch, 2006).

We extend the literature in three ways. First, we provide the first causal evidence on the long-run returns to *universal* investment in early life health. We thus complement two important recent papers on the long-run health effects of policy-relevant, *positive* shocks to early-life health (Bhalotra and Venkataramani, 2011; Hoynes, Schanzenbach and Almond, 2012) by identifying the long-run benefits of investment in the health of all infants. The possibility of heterogeneous treatment effects and general equilibrium effects—such as crowd-out in healthcare—means that estimates from studies of programs targeting at-risk groups cannot be used to assess the desirability of universal implementation, as is the norm in many countries.

Second, we add to the literature on long-run consequences of infant health, which has received less attention than the consequences of in-utero health.[4] We provide some of the

[4] A few existing papers examine the long-run effects of adverse shocks in infancy, typically demonstrating negative impacts on adult health and socioeconomic outcomes of exposure to famine (Meng and Qian, 2009) and disease early in life (Almond, Currie and Herrmann, 2012; Cutler et al., 2010; Bleakley, 2010; Lucas, 2010).

first evidence on long-run effects of positive shocks to infant health.

Third, we present the first evidence on the long-run health effects of one of the most commonly used child health policies. Government-run, universal home visiting programs are in place in many industrialized countries, but not in the U.S.[5] Home visiting programs have received particular attention in research and policy debates because they explicitly encourage early parental investments (such as breastfeeding) that may be instrumental to infants' health and development. A number of studies have shown positive short-run health effects of home visiting in both rich and poor countries (Gogia and Sachdev, 2010; WHO/UNICEF, 2006; Fitzsimons et al., 2012; Moehling and Thomasson, 2014; Wüst, 2012), and two influential randomized trials have also shown positive effects of targeted home visiting in infancy on socioeconomic outcomes in adolescence and early adulthood.[6] The evidence in this paper suggests that studies that ignore the long-run benefits of early life health programs significantly understate the returns to such programs.

The paper is organized as follows. Section 2 presents relevant background on Denmark's 1930s and 1940s medical system and the roll-out of the home visiting program. Section 3 lays out our empirical strategy, and section 4 presents the data. Section 5 presents our results for adult mortality, investigates the underlying health mechanisms, and analyzes heterogeneity in the estimated treatment effects. Section 6 concludes.

[5]The U.S. has a long history of state-run and privately organized, targeted home visiting programs (see e.g. Moehling and Thomasson, 2014), but no universal home visiting programs are operating in the U.S. 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 (2013) argue that the absence of public home visiting programs in the U.S. may be an important reason why infant mortality today is higher in the U.S. than in Europe.

[6]Olds et al. (1997), Olds et al. (1998) and Eckenrode et al. (2010) show that teenagers from at-risk families in upstate New York who were randomly allocated to nurse visits in utero and in infancy were less likely to drink, be arrested, or engage in other risky behavior in adolescence. Gertler et al. (2013) show that Jamaicans who were developmentally delayed as toddlers and randomly chosen to receive home visits focusing on socio-emotional stimulation have significantly higher earnings than control individuals in their early twenties.

2 Background and Roll-out of the Home Visiting Program

In the 1930s, the Danish health care system was organized through 23 medical districts divided into about 1250 rural and 87 urban municipalities.[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. While most women had good access to prenatal care, postnatal care was usually poor and women were not entitled to scheduled contact with health professionals after giving birth (DNBH, various years).[8]

At the time, the DNBH believed that the lack of postnatal care contributed to the comparably high Danish infant mortality rate of around 6.5 percent. 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 (DNBH, various years).

By the early 1930s, home visiting programs had been established in the Netherlands, the U.K., and the U.S. Inspired by these initiatives, the Danish parliament passed the Act on the Home Visiting Program in March 1937,[9] and the DNBH issued detailed guidelines for uniform municipal implementation. During 10 visits in the child's first year of life, nurses were to promote proper infant nutrion (especially breastfeeding) and hygiene, monitor the child's health and development, and refer ill infants to GPs. We limit our analysis to the period between 1937 and 1949, when the program almost exclusively served infants.[10]

[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; Wüst, 2012).

[9] The DNBH had conducted a five-year trial with home visiting in three treatment and control municipalities from 1930 onwards in collaboration with the U.S.-based Rockefeller Foundation. While treatment and control areas were not chosen randomly, the DNBH based its recommendation for expanding the program to the entire country on the positive experiences from the trial. Anecdotal evidence suggests that the treated and control areas were chosen to produce favorable results with respect to the decrease of the infant mortality rate that was the primary outcome considered (Buus, 2001).

[10] Later on nurses also served older children.

Importantly, the DNBH designed the program to provide universal care, and the data shows take up-rates close to 100 percent once a municipality implemented the program (DNBH, various years). Until 1974 each municipality could decide whether to implement the program.[11] 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 depended partly on the workload at the DNBH. 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. Fourth, many local municipal actors had to agree on implementation. While some welcomed the program, others—e.g., some GPs who viewed the program as a threat to their authority—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 earlier than the neighboring town of Roskilde. The medical districts of Holbæk and Bornholm introduced district-wide programs in 1938 and 1945. Importantly, as Figure 2 shows, the timing of treatment in the implementing municipalities varies over the entire period that we consider. 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.

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

3 Empirical Strategy

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

$$y_{jt} = \alpha + \beta post_t \times homevisit_j + \gamma_j + \delta_t + \varepsilon_{jt} \quad (1)$$

where y_{jt} is a health outcome in middle age for individuals born in municipality j in year t,[12] and $post_t \times homevisit_j$ is an indicator for the home visiting program being in place in municipality j in year t.[13] 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.[14] 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),[15] 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,[16] and/or underlying

[12] As treatment varies at the municipal level, we collapse our data into municipality $\times$ birth year-cells for all our analyses.

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

[14] γ_j absorbs the indicator for treated municipalities, and δ_t absorbs the indicator for post-treatment periods.

[15] Denmark is only slightly larger than the state of Maryland.

[16] We are unaware 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. See Wüst (2012) for a discussion.

trends in outcomes that differ in implementing and non-implementing municipalities, would violate this assumption.

To address these concerns, we follow four additional approaches. First, we control for municipality-specific linear time trends.[17] Second, we take advantage of a unique dataset, described in detail in section 4, on the demographic, political, economic and health characteristics of municipalities in the years leading up to 1937. Table 2 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. Thus we flexibly control for pre-treatment characteristics and let the coefficients on municipal pre-treatment characteristics, X_{jpre37}, vary by cohort, θ_t.[18]

$$y_{jt} = \alpha + \beta post_t \times homevisit_j + \gamma_j + \delta_t + \theta_t \times X_{jpre37} + \varepsilon_{jt} \tag{2}$$

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

$$y_{jt} = \alpha + \beta \hat{post}_t \times homevisit_j + \lambda \times \hat{post}_t + \gamma_j + \delta_t + \theta_t \times X_{jpre37} + \varepsilon_{jt} \tag{3}$$

[17] Our results are not sensitive to the inclusion of quadratic trends. Results are available on request.

[18] This strategy is related to Hoynes, Schanzenbach and Almond (2012), who analyze the long-run impact of the U.S. food stamp program. To account for differences in trends across counties that implemented the program at different times, they interact pre-treatment characteristics with linear time trends. We interact pre-1937 characteristics with birth year FE.

where $\hat{post}_t$ is an indicator for post-treatment years for all treated and matched control municipalities.[19]

Our matching procedure results in a sample of 382 municipalities.[20] As columns 3 and 4 in Table 2 show, the matched treatment and control groups are very similar with respect to pre-1937 observables (both levels and trends). We reject the null hypothesis of equality in means only for the 1936 infant mortality rate in the matched sample (at the 10% level). As we also balance pre-1937 trends, we are confident that our analysis on this matched sample (an analysis that still includes fixed effects) credibly tests the robustness of our findings.

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).[21] In our fifth approach we thus exploit only variation in the exact timing of implementation across municipalities. Appendix Figures B.2 and B.3 plot these treated municipalities' year of treatment initiation against their propensity score for treatment initiation (estimated in the matching procedure) and their 1936 infant mortality rate.[22] 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.

[19] As the $post_t$ indicator is defined for both treated and untreated municipalities, it is not collinear with the term $post_t \times homevisit_j$ as in equations 1-2.

[20] We 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*. Given the large differences among municipalities, we impose a rather wide caliper of 0.4 (to ensure a sufficient number of matches) and assign one control without replacement. Appendix Figure B.1 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.

[21] Given that hospital births were more common in towns, we have also tried excluding the 5 major towns—and thus nearly all hospital births—from our sample. Our results are not sensitive to this exclusion.

[22] 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.

4 Data

4.1 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- and district-level data on pre-treatment characteristics. This data on municipal characteristics at various points in time prior to 1937 comes from the Danish Commune Archive (Danish Data Archive, n.d.), 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: municipalities' size and location (distance from Copenhagen), 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.[23] From the Medical Reports for the Kingdom of Denmark (DNBH, various years), we include data on two relevant variables: infant deaths per 1000 live births and infant deaths from acute enteritis in 1936.[24]

Finally, we add individual-level adult outcome data (1980–2008) for the population of individuals born in Denmark between 1935 and 1949. This data contains information on date and place of birth, and on long-run health outcomes, for the population of Danish-born individuals. To construct an indicator for individuals' treatment status, we use date and parish of birth.[25] Three minor restrictions apply: First, to ensure that we capture

[23]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.

[24]These data are aggregated for all urban and rural municipalities in each of the 43 medical districts.

[25]In the administrative data (for historical reasons) the place of birth is recorded at the parish level. In rural areas, a parish was equivalent to a municipality for most cases during the period. In towns, many

the timing of treatment initiation as correctly as possible, we exclude individuals born in the month of treatment initiation. Second, 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. Third, we observe individuals from the year they turn 45.[26] Thus all our analyses are conditional on the indiviudals' having survived and not having permanently left Denmark before age 45.

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, and few individuals die between age 1 and 45. Second, the share of individuals that we do not observe in our outcome data remains stable across the cohorts we consider. This is shown in Appendix A.2 using data on births at the national level.[27] 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. Finally, a crude treatment proxy set to one for a district $\times$ cohort if any municipality in the district had implemented the program before the birth-year of the relevant cohort is uncorrelated with variation in the number of missing individuals across district $\times$ cohorts.[28]

Table 1 shows that for about 94 percent of all Danish-born individuals in the administrative data, we can match the parish of birth to a municipality.[29] Given the exclusion of individuals in month of treatment initiation, we lose 10,914 individuals (1% of observations with a valid parish code) and end with an estimation sample of 938,154 individuals. We collapse our data to 19,673 municipality $\times$ birth cohort $\times$ treatment status-cells.[30]

parishes form one municipality. Given that parishes and municipalities continuously change their size and structure over the period, we use the 1940 parish and municipality structure to uniquely assign parishes to municipalities (the level at which we observe the treatment).

[26]We work with a subset of the register data and one sampling criteria for the outcome data was that individuals had turned 45.

[27]Unfortunately we do not observe the number of births in a cohort at the municipal level. We thus cannot directly test for differential mortality prior to age 45 in treated municipalities.

[28]The same test indicates that the program did not significantly affect fertility.

[29]Of the approximately 12 percent of observations in the administrative data that we cannot merge, 5.7 percent 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 to invalid parish codes.

[30]As we observe the exact date of treatment initiation, there are two cells for a given municipality in

4.2 Administrative Data on Outcomes

Data on individuals' health in middle age (45-57) comes from the Danish Inpatient Register and the Danish Death Register for the years 1980-2008. First, we construct an indicator for the individual's death in the given age range. Second, we create a variable equal to the number of nights an individual spent in a hospital in the given age range. 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; Hoynes, Schanzenbach and Almond, 2012), including heart disease, cardiovascular disease, and diabetes. We combine heart and cardiovascular disease in one indicator. Exploiting the death records' information on cause of death, we also create an indicator for death from cardiovascular or heart disease. As the cause of death is missing for around 26 percent of all deaths in our data, we are cautious in interpreting this variable. Information on the coding of diagnoses and causes of death appears in Appendix A.1.

We have also explored 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.[31]

Appendix Table B.1 presents summary statistics for our health outcomes for the full sample, for urban areas only, and by sex. Compared to our full sample, urban municipalities have similar prevalence rates of deaths and serious diagnoses. Men are more likely to experience both death and sickness. These differences may point to potential heterogeneous

the year of treatment initiation.

[31]Results for these analyses are in Appendix Table B.3. We analyse indicators for acquiring more than the seven years of compulsory schooling and for completing "exam school," i.e., academic middle schools (grades 8 and 9) that prepared students for higher education. Before a 1958 school reform that affected birth cohorts from 1946 onwards, a tracking system at grade 5 applied (i.e., children entered either the academic exam school track or a 7-year track preparing them for vocational training). Before this reform, only towns had exam schools. As the 1958 reform impacted rural students' probability of attending exam schools (Arendt, 2008), we control for the potential impact of this reform by adding a post-1946 $\times$ rural-indicator. Arendt (2008) also shows that while women in treated cohorts experienced improvements in health as measured as hospitalizations, men's health benefits were limited. Furthermore, we study a measure for being in a blue-collar occupation. For this measure we consider all individuals who ever held a job between ages 45 and 57 and their Danish ISCO code. We examine the log wage income for employed individuals, we measure the number of weeks of unemployment an individual experienced, and finally, we construct an indicator that is one for all individuals who were ever employed.

effects for men and women, differences we examine in the results section.

5 Long-run Health Effects of the Home Visiting Program

5.1 Main Results

To graphically analyze our data, Figure 3 presents an event graph based on the specification that accounts for municipal pre-1937 controls (equation (3)) for our main outcome, adult mortality. The graph plots estimates and confidence intervals for a set of indicators for half-years to treatment initiation for a balanced sample of ever-treated municipalities. To minimize the impact of compositional changes in the event study sample, we focus on a balanced sample of implementing municipalities that we observe for at least three half-years before and after treatment initiation. We "bin up" the endpoints of the event time axis and include an indicator for up to 10 half-years both before and after treatment initiation.[32]

Figure 3 shows that, before treatment implementation, estimates for the event time indicators are insignificant, with no clear pre-trend. After the implementation at $t = 0$, a program effect on adult mortality appears. While confidence intervals initially do not exclude 0, the effects are negative and significant after one year. The increasing size of the estimated effects for consecutive half-years after treatment initiation may reflect the program's increasing effectiveness, e.g., due to nurses experience with conducting home visits.

Table 3 presents our regression results for the effects of the home visiting program on mortality in middle age. Each row presents estimates for the treatment indicator from our five approaches. All columns 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 pre-treatment municipal characteristics with coefficients allowed

[32] Results for unbalanced samples are similar and available on request. Results for a model that included linear time trend are very similar and available on request.

to vary by year. Columns 4 and 5 present results based on two subset 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 3 are based on the sample of eventually implementing municipalities (from 1937–1949). This specification also flexibly controls for municipal characteristics. For convenience we present coefficients that are pre-multiplied by 100 and thus interpretable as percentage point changes (except for the number of hospital nights).

Table 3 shows that treated individuals are significantly less likely to die in middle age, the period in our outcome data that we observe for all individuals in our sample. At the mean of the dependent variable of around 6 percent, our estimates imply that individuals are 5–8 percent less likely to die in that age range. The coefficients are relatively stable across specifications but somewhat bigger once we include controls for pre-treatment 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. The lower panels of Table 3 analyse our main mortality outcome separately for men and women. As expected, female mortality rates are lower than male ones in our sample. At the same time, women enjoy—at the relevant mean—larger effects of the home visiting program on overall mortality rates. This finding may point to heterogeneity in the mechanisms that drive the mortality results.

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 conservatively assume that treated individuals die at age 58, but would, in the absence of the program, have died at age 57, our estimates translate into 1041 to 1992 saved life-years in the cohorts we study (only through its direct effect on mortality).[33] 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

[33] (0.03*size of treatment group in our cohorts) to (0.08*size of treatment group in our cohorts)

hospitalizations.

5.2 Mechanisms

In Table 4, we turn to the analysis of rarely available measures of health that reveal more about the driving factors of the mortality 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 57) and are less likely to be diagnosed with lifestyle-related diseases. In line with studies suggesting long-run impacts of early-life health for later-life disease prevalence (Barker, 1992; Forsdahl, 1979), we find that those exposed to the home visiting program in infancy are less likely to be diagnosed with a cardiovascular disease in middle age. While 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 (Hoynes, Schanzenbach and Almond, 2012).[34] Thus our finding suggest that treated infants (a) enjoy more healthy life years as adults and (b) consequently are likely to cost the medical system significantly less.[35]

Appendix Table B.3 shows estimates for the effects of the home visiting program on a set of educational and labor market outcomes of treated individuals. While we may expect education and occupational choices to be important pathways that explain some of the health effects we find, our empirical results are somewhat inconclusive. Our estimates are on average small, less precise than the health results, and not stable across specifications. Thus we conclude that the home visiting program likely impacted adult mortality predominantly through its impact on adult morbidity.

[34] While Hoynes, Schanzenbach and Almond (2012) emphasize the importance of overweight in adult life in this context, we cannot study individuals' weight and height with our data.

[35] Parallel to our analyses for adult mortality, we also split our analyses of diagnoses and hospitalizations for males and females. Appendix Table B.2 shows that while both men and women see rather similar effects of the program on the health outcomes in question, the results for cardiovascular and heart disease mortality are stronger among women. These results may explain the stronger mortality results that we see among women.

5.3 Heterogeneity of Treatment Effects

Unlike the Danish home visiting program, many infant health programs target at-risk families (e.g., Olds et al., 1998; Eckenrode et al., 2010; Gertler et al., 2013). The appeal of universal implementation depends on variation in costs and benefits of the program across families and/or areas, and on possible general equilibrium effects. We find no evidence that the magnitude of the treatment effect in a given municipality depends on the implementation status of neighboring municipalities.[36]

While we cannot examine the heterogeneity of effects across different types of families (because we lack historical data on family-level characteristics), an analysis of heterogeneous effects across municipalities may help assess whether targeted implementation of home visiting programs towards municipalities with certain characteristics is more cost effective than universal implementation in all types of municipalities. In Table 5 we investigate how the impact on long-run health varies across individuals born in different types of municipalities, using specification (1). As municipalities with higher exposure of infants to disease or poor nutrition may have had larger scope for positive effects of the home visiting program, we divide our sample at the median of the 1936 infant mortality rate (IMR). This rate is a proxy for prevailing levels of infant health. On average, we may expect larger health effects in municipalities with high infant mortality rates, because they were most likely exposed to higher level of infectious disease and potentially worse nutrition than infants in municipalities with lower mortality levels.

Additionally, improved health for infants and children may have translated into different impacts for adult health as a function of local conditions, which likely had an important impact on individual choices. Thus we classify municipalities according to the urban-rural division as a proxy for local educational and labor market chances. Moreover, urban and

[36]The benefits of home visiting nurses referring ill infants to hospitals could depend on the treatment status of neighboring municipalities if (shared) hospitals faced capacity constraints, for example. To test for the importance of such spillover effects, we interact the treatment indicator with an indicator that equals one for municipalities with a neighboring municipality (the municipality with the centroid closest to the centroid of the municipality in question) that has already implemented the program. We find no significant interaction effects (results are available upon request).

rural municipalities have most likely differed in the types of follow-up interventions they offered. As we observe only the “reduced form” effects of the program inclusive of effects for all potential follow up intervention (e.g., expansion in public housing, daycare and health monitoring), we may expect that health returns to the program vary along this dimension.[37]

Each panel in Table 5 shows coefficients from regressions based on subsamples of municipalities. The top panels show that individuals from municipalities with a high 1936 IMR benefited more in terms of adult disease prevalence and related cause-specific mortality, while low IMR municipalities did not benefit with respect to these outcomes. These findings—which use the IMR as a proxy for infants’ disease environment—suggest that improvements of the disease environment in infancy are more important at higher baseline levels of disease prevalence. At the same time, for overall mortality and hospitalization outcomes, the estimates are larger (compared to the relevant mean) and more precisely estimated in the sample of low IMR municipalities. Thus infants born in low IMR municipalities also benefited in the long run. Finally, across health outcomes, the lower panels of Table 5 show that effects are larger in towns than in rural municipalities. This finding may indicate that factors related to educational and labor market chances or follow-up interventions play a role for the long-run health effects of home visiting.

6 Conclusion

This paper presents the first evidence on the causal effect on adult health of a policy aimed at improving the health of all infants. We study the long-run effects of the Danish home visiting program, which was rolled out across municipalities from 1937 onwards. In munici-

[37]While a comparison of municipalities of different levels of per capita income or wealth among their inhabitants would be very informative, we lack good proxies for municipalities’ wealth. The tax measures in our data are not well suited for capturing this dimension. In the period under consideration the tax system was still emerging, with relatively low percentages of the population paying (income or property) taxes. These percentages do not easily translate into measures of municipal wealth. We have used measures of tax base to classify municipalities but find across the board insignificant estimates that do not reveal anything informative about eventual heterogeneity.

palities that implemented the program, nurses visited all infants and their mothers at home ten times during the first year of the child's life to encourage proper care, breastfeeding, and 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, 5–8 lower mortality rate between age 45 and 57 (the range of our outcome data). This finding indicates that improved nutrition and infant care—and as a consequence better health in infancy—programs individuals for lower predisposition to serious adult diseases. We find that treated individuals are less likely to be diagnosed with cardiovascular disease, spend fewer nights in hospital, and are less likely to die from cardiovascular or heart disease in middle age. Recent research highlighting the importance of parental knowledge and skills for child development and the informational nature of the Danish program suggests that the observed benefits may arise through greater parental investment in child health (Cunha, Elo and Culhane, 2013).

The magnitude of the impact on adult mortality we find may be specific to disease- and poor nutrition-prone infant environments, such as Denmark of the 1930s. As such our findings support the argument for home visiting in developing countries (see also WHO/UNICEF, 2006). The health mechanism we document, however—early programming of middle age diseases—suggests that our findings are relevant also for rich countries today. If so, the public home visiting programs in place in e.g. most European countries may lower middle age mortality rates relative to those found int he U.S. Overall our findings indicate that estimates that ignore the long-run benefits of early life health programs may significantly understate the returns to such programs.

References

Almond, D., J. Currie, and M. Herrmann. 2012. "From Infant to Mother: Early Disease Environment and Future Maternal Health." *Labour Economics*, 19(4): 475–483.

Arendt, Jacob Nielsen. 2008. "In sickness and in health—Till education do us part: Education effects on hospitalization." *Economics of Education Review*, 27(2): 161–172.

Barker, David J. 1992. *The Fetal and Infant Origins of Adult Disease.* London: British Medical Journal.

Bhalotra, Sonia, and Atheendar Venkataramani. 2011. "The Captain of the Men of Death and his Shadow: Long-run Impacts of Early Life Pneumonia Exposure." IZA Working Paper.

Bleakley, H. 2010. "Malaria Eradication in the Americas: A Retrospective Analysis of Childhood Exposure." *Am. Econ. J.:Appl. Econ*, 2: 1–45.

Buus, Henriette. 2001. *Sundhedsplejerskeinstitutionens Dannelse [The Introduction of the Danish Home Visiting Program].* (Copenhagen: Museum Tusculanum Press).

Chen, Alice, Emily Oster, and Heidi Williams. 2013. "Why is infant mortality higher in the U.S. than in Europe?" unpublished working paper.

Crimmins, Eileen M., and Caleb E. Finch. 2006. "Infection, Inflammation, Height and Longevity." *Proceedings of the National Academy of Sciences*, 103: 498–503.

Cunha, Flavio, and James Heckman. 2007. "The Technology of Skill Formation." *American Economic Review*, 97(2): 31–47.

Cunha, Flávio, Irma Elo, and Jennifer Culhane. 2013. "Eliciting Maternal Expectations about the Technology of Cognitive Skill Formation." National Bureau of Economic Research Working Paper 19144.

Currie, J. 2009. "Healthy, wealthy, and wise: Socioeconomic status, poor health in childhood, and human capital development." *Journal of Economic Literature*, 87–122.

Currie, J., and D. Almond. 2011. "Chapter 15: Human Capital Development before Age Five." In *Handbook of Labor Economics.* Vol. 4, Part 2, , ed. Orley Ashenfelter and David Card, 1315–1486. Elsevier.

Cutler, D.M., W. Fung, M. Kremer, M. Singhal, and T. Vogl. 2010. "Early Life Malaria Exposure and Adult Outcomes: Evidence from Malaria Eradication in India." *Am. Econ. J.:Appl. Econ*, 2: 196–202.

Danish Data Archive. "Statistical Danish Commune Archive."

DNBH. various years. *Medical Report for the Kingdom of Denmark.* The Danish National Board of Health.

Eckenrode, J., M. Campa, D. W. Luckey, et al. 2010. "Long-term Effects of Prenatal and Infancy Nurse Home Visitation on the Life Course of Youths. 19-Year Follow-up of a Randomized Controlled Trial." *Archives of Pediatrics and Adolescent Medicine*, 164(1): 9–15.

Fitzsimons, Emla, Bansi Malde, Alic Mesnard, and Marcos Vera-Hernandez. 2012. "Household Responses to Information on Child Nutrition: Experimental Evidence from Malawi." IFS working paper 12/07.

Forsdahl, Anders. 1979. "Are poor living conditions in childhood and adolescence an important risk factor for arteriosclerotic heart disease?" *British Journal of Preventive and Social Medicine*, 31(2): 91–95.

Gertler, Paul, James Heckman, Rodrigo Pinto, Arianna Zanolini, Christel Vermeersch, Susan Walker, Susan M Chang, and Sally Grantham-McGregor. 2013. "Labor market returns to early childhood stimulation: a 20-year followup to an experimental intervention in Jamaica." NBER Working Paper No. 19185.

Gluckman, P. D., M. A. Hanson, A.S. Beedle, and D. Raubenheimer. 2008. "Fetal and neonatal pathways to obesity." *Frontiers of Hormone Research*, 36: 61–72.

Gogia, S., and H. S. Sachdev. 2010. "Home visits by community health workers to prevent neonatal deaths in developing countries: a systematic review." *Bulletin of the World Health Organtion.*

Heckman, James J., and Stefano Mosso. 2014. "The Economics of Human Development and Social Mobility." National Bureau of Economic Research Working Paper 19925.

Hoynes, Hillary W., Diane W. Schanzenbach, and Douglas Almond. 2012. "Long Run Impacts of Childhood Access to the Safety Net." National Bureau of Economic Research Working Paper 18535.

Løkke, Anne. 1998. *Døden i barndommen: Spædebørnsdødelighed og moderniseringsprocesser i Danmark 1800 til 1920.* Copenhagen: Gyldendal.

Lucas, A. 2010. "Malaria Eradication and Educational Attainment: Evidence from Paraguay and Sri Lanka." *Am. Econ. J.:Appl. Econ*, 2: 46–71.

Lynch, James, and George Davey Smith. 2005. "A life course approach to chronic disease epidemiology." *Annual Review of Public Health*, 25: 1–35.

Mazumder, Bhashkar, Douglas Almond, E.M. Crimmins, and C.E. Finch. 2009. "Lingering Prenatal Effects of the 1918 Influenza Pandemic on Cardiovascular Disease." *Journal of Developmental Origins of Health and Disease*, October.

Meng, Xin, and Nancy Qian. 2009. "The Long Term Consequences of Famine on Survivors: Evidence from China's Great Famine." NBER Working Paper 14917.

Moehling, Carolyn M., and Melissa A. Thomasson. 2014. "Saving Babies: The Impact of Public Education Programs on Infant Mortality." *Demography*, 1–20.

Olds, D.L., J. Eckenrode, Henderson C.R., et al. 1997. "Long-term Effects of Home Visitation on Maternal Life Course and Child Abuse and Neglect: Fifteen-Year Follow-up of a Randomized Trial." *Journal of the American Medical Association*, 278(8): 637–643.

Olds, D.L., J. Eckenrode, Henderson C.R., et al. 1998. "Long-term Effects of Nurse Home Visitation on Children's Criminal and Antisocial Behavior. 15-Year Follow-up of a Randomized Controlled Trial." *Journal of the American Medical Association*, 280(14): 1238–1244.

WHO/UNICEF. 2006. "Home visits for the newborn child: a strategy to improve survival." *Joint Statement*.

Wüst, Miriam. 2012. "Early interventions and infant health: Evidence from the Danish home visiting program." *Labour Economics*, 19: 484–495.

7 Tables and Figures

Table 1: Parish-municipality match for individuals of the cohorts 1935–1949.

Place of birth	*No. of obs*	*Percent*	*Percent, cum.*
Other country	59,876	5.5	5.5
Greenland	1,569	0.1	5.7
Unknown, Denmark	2,366	0.2	5.9
Post-1970 municipal code	65,229	6.0	11.9
Medical district code	2,116	0.2	12.1
Other religious groups	1,996	0.2	12.3
Undocumented code	21	0.0	12.3
Parish in Denmark	949,068	87.7	100.0
Total	1,082,241	100.0	

Source: Authors' calculations from administrative data.

Table 2: Municipality characteristics, full sample and matched sample of municipalities; means and p-values for difference in means in the matched sample

	(1) *Control*	(2) *Treated*	(3) *Matched control*	(4) *Matched treated*	(5) *p-value ((3) vs (4))*
Municipal controls, levels					
Area, km^2	33.90	25.45	26.08	26.52	0.80
Ln Population, 1935	7.10	7.66	7.24	7.23	0.91
Distance to Cph., km	187.90	107.01	113.68	115.03	0.84
Distance to implementing munic., km	58.47	58.20	54.05	57.91	0.23
Degree of urbanization (0-100), 1935	14.25	35.23	19.21	18.12	0.71
Infant mortality rate (per 1000 live births), 1936	70.53	72.45	69.80	71.64	0.08
Mortality rate, acute enteritis (per 1000 population), 193	0.14	0.16	0.16	0.15	0.86
Votes for Liberals 1935, perc.	38.75	23.76	30.37	30.23	0.92
Votes for Social Democrats 1935, perc.	24.80	39.62	35.06	33.76	0.28
Votes for Conservatives 1935, perc.	10.25	14.07	11.20	11.35	0.83
Votes for Social Liberals 1935, perc.	11.51	13.88	13.52	14.74	0.25
Population share female, 1930	48.48	49.27	48.62	48.60	0.93
Population share in agriculture, 1930	62.50	44.95	54.58	55.43	0.62
Population share in industry, 1930	14.76	23.70	18.87	18.45	0.65
Ln assessed property for prop. tax, 1936	7.29	8.30	7.80	7.81	0.91
Share paying property tax, 1936	4.37	5.36	5.42	5.52	0.62
Ln taxable income, 1936	6.01	6.99	6.39	6.38	0.91
Share paying income tax, 1936	20.06	24.92	23.04	23.15	0.84
Share population on public aid, 1936	3.49	5.12	4.24	4.21	0.87
Municipal controls, changes					
Ln population, 1930-35	-0.17	0.26	-0.16	-0.25	0.53
Urbanization, 1930-35	0.17	0.29	0.16	0.16	0.99
Votes for Liberals, 1929-35	-2.39	-1.70	-1.78	-1.83	0.65
Votes for Social Democrats, 1929-35	0.61	0.64	0.52	0.62	0.17
Votes for Conservatives, 1929-35	0.30	0.39	0.46	0.41	0.45
Votes for Social Liberals, 1929-35	-0.45	-0.36	-0.42	-0.45	0.62
Populaton share females, 1930-40	-0.03	-0.06	-0.09	-0.06	0.56
Population share in agriculture, 1930-40	-0.20	-0.11	-0.15	-0.13	0.69
Population share in industry, 1930-40	0.37	0.31	0.33	0.33	0.89
Ln assessed property for prop. tax, 1927-36	-23.50	-16.85	-21.20	-21.56	0.75
Share paying property tax, 1927-36	-1.29	-1.56	-1.55	-1.53	0.58
Ln taxable income, 1927-36	-5.25	-3.93	-4.95	-5.04	0.62
Share paying income tax, 1927-36	-0.70	-0.78	-0.85	-0.84	0.82
Share population on public aid, 1935-36	0.15	-0.01	0.08	0.14	0.59
Number of munics.	*968*	*348*	*191*	*191*	

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

Table 3: Effect of the home visiting program on mortality from age 45 to 57

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (Matched)	(5) (Ever impl.)
Deaths	-0.269**	-0.355***	-0.421**	-0.479	-0.501**
	(0.106)	(0.127)	(0.165)	(0.322)	(0.209)
Mean of dep. var. × 100	5.982	5.982	5.982	5.962	6.374
No. of obs.	19673	19673	19673	5829	5493
Deaths, male	-0.389**	-0.472**	-0.454*	-0.344	-0.499*
	(0.153)	(0.200)	(0.239)	(0.477)	(0.291)
Mean of dep. var. × 100	7.056	7.056	7.056	7.094	7.565
No. of obs.	19435	19435	19435	5766	5414
Deaths, female	-0.133	-0.264	-0.369*	-0.639*	-0.485*
	(0.134)	(0.170)	(0.209)	(0.385)	(0.260)
Mean of dep. var. × 100	4.878	4.878	4.878	4.812	5.146
No. of obs.	19419	19419	19419	5765	5399
Cohort FE	Yes	Yes	Yes	Yes	Yes
Muncipal:					
FE	Yes	Yes	Yes	Yes	Yes
X (level) × year interactions	No	No	Yes	No	Yes
X (trend) × 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. *Deaths* is an indicator for individuals dying between age 45 and 57. All coefficients are pre-multiplied by 100 and interpretable as percentage point changes. The units of observation are municipality×year of birth×treatment status-cells. We weight regressions with the number of observations in each cell. Column (4) presents the estimate of the treated×post indicator in the matched sample that assigns a treatment date to the matched control municipalities (see section 3 for details). We cluster all standard errors at the municipal level (1344 clusters). ***significant at the 1 pct level, **significant at the 5 pct level *significant at the 10 pct level

Table 4: Effect of the home visiting program on pathway health outcomes from age 45 to 57

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (Matched)	(5) (Ever impl.)
Deaths, cardio/heart	-0.122**	-0.078	-0.257***	-0.106	-0.336***
	(0.062)	(0.073)	(0.095)	(0.166)	(0.109)
Mean of dep. var. × 100	1.903	1.903	1.903	1.968	1.991
No. of obs.	19673	19673	19673	5829	5493
Hospital Nights, cont.	-0.434**	-0.497**	-0.563***	-0.762**	-0.527**
	(0.170)	(0.224)	(0.200)	(0.342)	(0.232)
Mean of dep. var.	11.007	11.007	11.007	10.834	11.445
No. of obs.	19673	19673	19673	5829	5493
Cardiovascular Disease	-0.232	-0.432*	-0.518*	0.193	-0.800**
	(0.172)	(0.252)	(0.272)	(0.437)	(0.315)
Mean of dep. var. × 100	18.527	18.527	18.527	18.628	18.429
No. of obs.	19673	19673	19673	5829	5493
Diabetes	0.061	-0.140	-0.153	-0.345*	-0.117
	(0.080)	(0.104)	(0.102)	(0.207)	(0.125)
Mean of dep. var. × 100	2.809	2.809	2.809	2.735	3.059
No. of obs.	19673	19673	19673	5829	5493
Cohort FE	Yes	Yes	Yes	Yes	Yes
Muncipal:					
FE	Yes	Yes	Yes	Yes	Yes
X (level) × year interactions	No	No	Yes	No	Yes
X (trend) × year interactions	No	No	Yes	No	Yes
Linear time trends	No	Yes	No	No	No

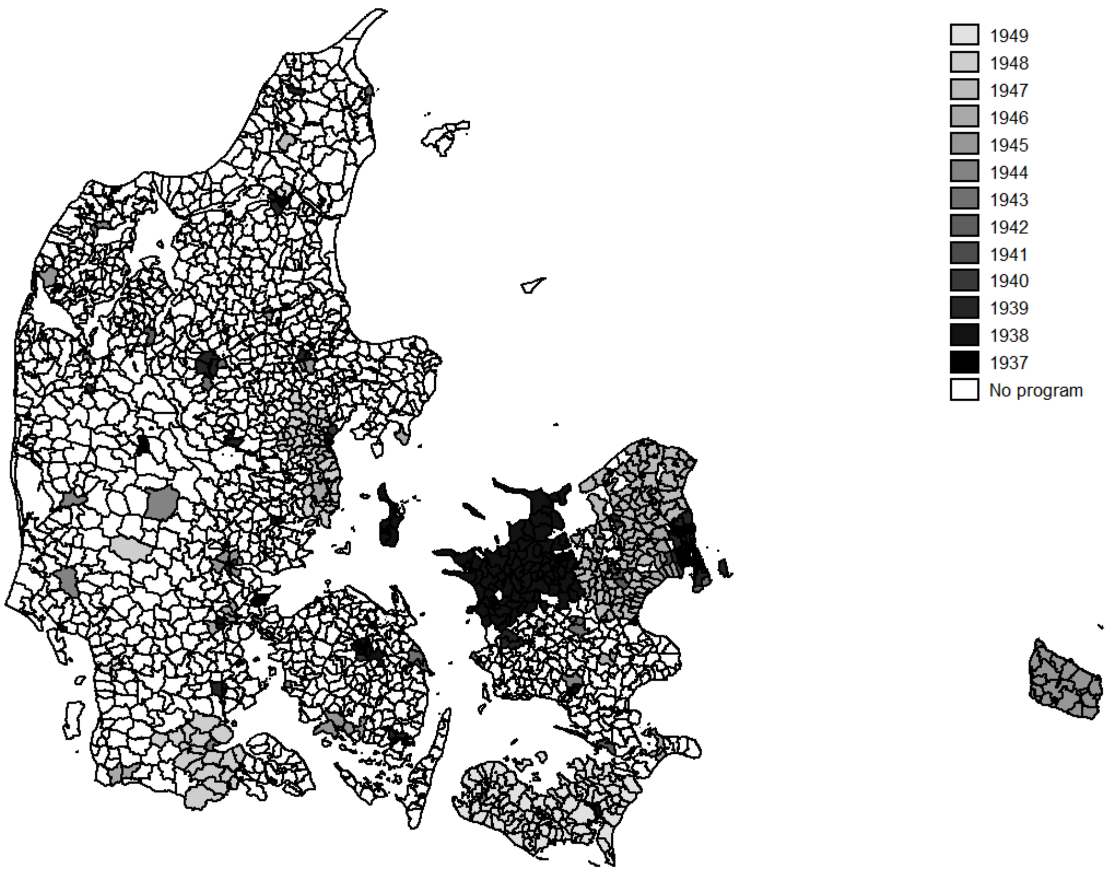
Notes: Each cell presents the coefficient for the treatment indicator for a different regression. *Deaths from cardio/heart disease* is an indicator for individuals dying between age 45 and 57 (dying from from cardio/heart disease). *Hospital nights* is the number of nights at hospital between the ages 45–57.*Cardiovascular disease* is an indicator for individuals diagnosed with or dying of cardiovascular disease. *Diabetes* is an indicator for individuals diagnosed with diabetes. All coefficients are pre-multiplied by 100 and interpretable as percentage point changes (with the exception of the estimate for the number of hospital nights). The units of observation are municipality×year of birth×treatment status-cells. We weight regressions with the number of observations in each cell. Column (4) presents the estimate of the treated×post indicator in the matched sample that assigns a treatment date to the matched control municipalities (see section 3 for details). We cluster all standard errors at the municipal level (1344 clusters). ***significant at the 1 pct level, **significant at the 5 pct level *significant at the 10 pct level

Table 5: Effect of the home visiting program on health outcomes from age 45 to 57 in different types of municipalities

Outcome	(Deaths)	(Deaths, Cardio)	(Hosp. Nights)	(Cardio. Dis.)	(Diab.)
Treated, low IMR sample	-0.308**	-0.070	-0.558**	0.028	-0.019
	(0.131)	(0.073)	(0.227)	(0.231)	(0.100)
Mean of dep. var. $\times$ 100	6.178	1.940	11.221	18.219	2.952
No. of obs.	9789	9789	9789	9789	9789
Treated, high IMR sample	-0.153	-0.257**	-0.228	-0.470	0.103
	(0.176)	(0.105)	(0.244)	(0.366)	(0.134)
Mean of dep. var. $\times$ 100	5.703	1.850	10.701	18.967	2.605
No. of obs.	9884	9884	9884	9884	9884
Treated, urban sample	-0.450**	-0.054	-0.703***	-0.372	-0.106
	(0.178)	(0.107)	(0.247)	(0.315)	(0.137)
Mean of dep. var. $\times$ 100	6.449	2.006	11.609	18.633	3.156
No. of obs.	1002	1002	1002	1002	1002
Treated, rural sample	-0.059	-0.061	-0.238	-0.235	-0.085
	(0.204)	(0.116)	(0.253)	(0.291)	(0.117)
Mean of dep. var. $\times$ 100	5.579	1.814	10.509	18.435	2.509
No. of obs.	18671	18671	18671	18671	18671
Cohort FE	Yes	Yes	Yes	Yes	Yes
Muncipality FE	Yes	Yes	Yes	Yes	Yes

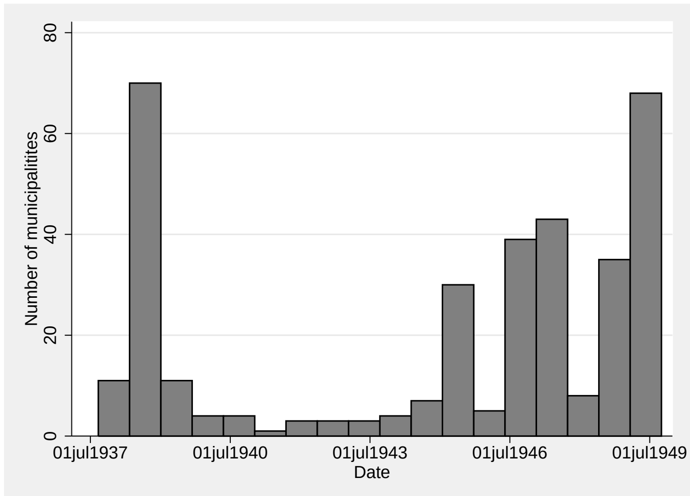
The estimates in each row are from regressions based on equation (1) on a different subsample of municipalities. IMR is the 1936 infant mortality rate and is measured at the medical district level. Urban is an indicator for all urban municipalities. Clustered standard errors in parenthesis. ***significant at the 1 pct level, **significant at the 5 pct level *significant at the 10 pct level

Figure 1: Municipalities and their date of entry into treatment, 1937–1949



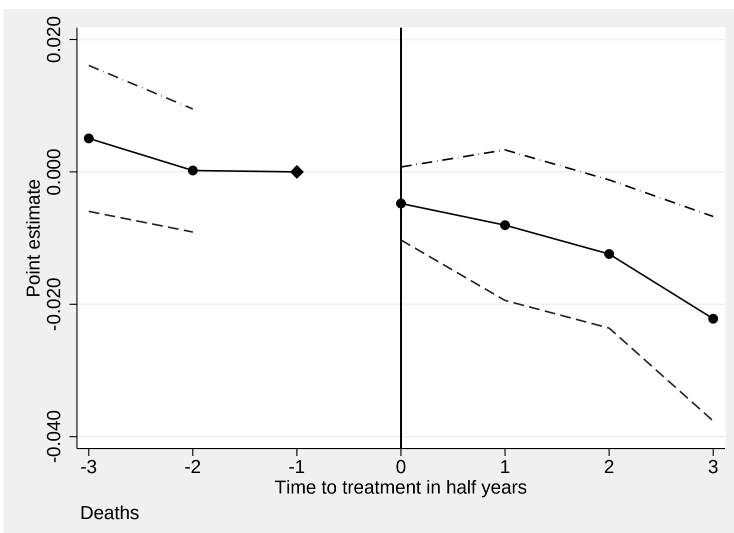
Notes: The figure shows 1940 parishes. Rural municipalities consisted of one single parish; towns consisted of several parishes. All parishes of a given municipality were either treated or untreated. *Source:* Digdag (Digital Atlas of the Danish Historical, Administrative Geography, www.digdag.dk) and data on municipal treatment initiation from the Danish National Archives (for details, see Wüst, 2012).

Figure 2: Number of municipalities by their date of entry into treatment, 1937–1949



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

Figure 3: Event study for effect of the home visiting program on mortality from age 45 to 57



Notes: Time to treatment in half-years. Treated municipalities and balanced sample (eventually-treated municipalities observed for at least three half-years before and after treatment initiation). Models include indicators for three half-years for both before and after treatment initiation, as well as indicators for more than three half-years before and after treatment initiation, year fixed effects, municipality fixed, and an interaction of year effects with pre-1937 municipal controls. The omitted indicator for event time is t=-1. The figure displays coefficients and a 95 percent confidence interval.

A Appendix: Data Sources and Data Structure Diagnoses for Medical Conditions Data

A.1 Diagnoses for Medical Conditions Data

The health data comes from the Danish Inpatient Register and the Danish Death Register for 1980-2006. The Inpatient Register uses ICD 8 coding until 1993 and ICD 10 coding from 1994 onwards.

If an individual uses the Danish hospital system, we observe diagnoses and hospitalizations: Individuals have to be hospitalized (1980–1993) or have at least one outpatient visit (1994–2006) to appear in the diagnosis data. While we thus may not capture minor health conditions, the hospitalization data most likely contains clinically relevant diagnoses. As health care is publicly funded and universally accessible, our health measures are well suited for capturing the underlying prevalence of health conditions in the population.

ICD 8 codes for diagnoses groups:

- Diabetes: 249, 250
- Cardiovascular Disease: 390–458
- Heart disease: 410–414

ICD 10 codes for diagnoses groups:

- Diabetes: DE10–DE14
- Cardiovascular Disease: DI00–DI99
- Heart disease: DI20–DI25

The causes of death are grouped according to the 23 groups used by the Danish National Board of Health. We merge groups 20-23 and 12-13 (the ICD 8 system does not distinguish between these last two).

Causes of death:

- 1 Infection: A00–B99, 000–136
- 2 Cancer: C00–C97, 140–209
- 3 Other cancer: D00–D48, 210–239
- 4 Blood and bloodforming organs: D50-D89, 280-289
- 5 Endocrine, metabolic disease: E00-E90, 240-246, 250-279
- 6 Mental disorders: F03-F99, 290-315
- 7 Nervous system: G00-G31, 320-389
- 8 Heart disease: G35-H95, I00-I25, I27, I30-I51, 390-398, 400-404, 410-414, 420-429
- 9 Other cardiovascular disease: I26, I28, I60-I99, 430-438, 440-448, 450-458
- 10 Respiratory system: J00-J99, 460-474, 480-486, 490-493, 500-519
- 11 Digestive system: K00-K92, 520-577
- 12, 13 Skin, musculosceletal system, connecting tissue: L00-L99, M00-M99, 680-738
- 14 Genitourinary system: N00-N98, 580-629
- 15 Pregnancy and childbirth: O00-O99, 630-678
- 16 Perinatal period: P00-P96, 760-779
- 17 Congential disease: Q00-Q99, 746-759
- 18 Symtoms not elsewhere classified: R00-R98, R99, 780-793, 795-796
- 19 Accidents: V01-X59, Y40-Y86, Y88, E80-E94
- 20 Suicide, murder, legal interventions: X60-X99, Y00-Y36, Y89, R99, E95-E99

A.2 Missing Observations

While we have a uniquely high match of cohort members with available outcome data to their municipality of birth and its treatment status, we do not observe individuals who die or leave Denmark before age 45. If treated and untreated individuals selectively die or emigrate before this age, this selection could confound our analysis. As we cannot observe the number of births per municipality, we use aggregated statistics on live births and infant deaths from the Medical Reports of Denmark to examine this issue.

To investigate whether treatment initiation predicts the number of missing observations, we analyze the relationship of the relative number of missing observations in a medical subdistrict (i.e., all pooled urban or rural municipalities in the 23 districts) and an indicator for *at least one* municipality implementing the treatment in the area. We regress the missing observations on this indicator, and year and area fixed effects. We find that this very crude treatment proxy is unrelated to the variation in the number of missing observations across subdistricts. Results are available upon request.

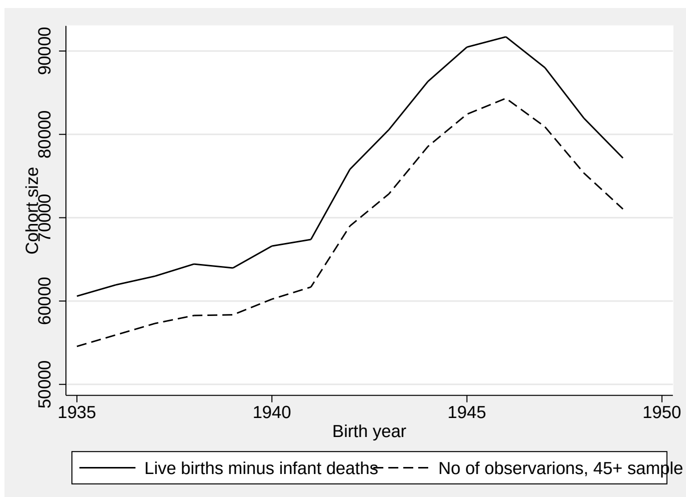
If untreated individuals died at an increased rate between age 1–45, we would underestimate the program's effects on adult mortality, and we should see a decreasing percentage of missing observations for subsequent birth cohorts (because subsequent cohorts contain increasing shares of treated municipalities/individuals). We show that the number of missing observations is relatively stable across birth cohorts, with a very small tendency towards fewer missing observations in later cohorts. Figure A.1 plots the number of observations we would expect in the absence of mortality or emigration between ages 1–45 and the number of observations observed in our data (DNBH, various years). The figure shows that we lack around 8–9 percent of Danish-born first-year survivors in each cohort. Appendix Table A.1 contains the national figures.[38]

As weaker infants most likely survived in treated municipalities, we may also expect a compositional change of the population of treated survivors that may attenuate our findings. Wüst (2012) has estimated that at the mean infant survival rate of the time,

[38]Here we include individuals with non-valid parish information.

5–8 additional infants per 1000 live births survived in treated municipalities. We argue that at the average cohort size of around 65,000, this number of around 325-520 additional survivors for each cohort should not drive our results. If these additional infants were drawn from the lower end of the infant health distribution, the population of treated individuals that we later observe may be negatively selected (relative to the population from untreated municipalities). Consequently, we may underestimate the long-run benefits of the program, and we should interpret our findings as lower bounds.

Figure A.1: 1-year survivors and cohort size for the survivors until age 45 including individuals with no valid parish information but born in DK (1935-1949).



Notes: Figure based on data from table A.1.
Source: Authors' calculations from administrative data.

Table A.1: Missing observations, 1935-1949.

Birth cohort	*Danish born sample* (1)	*Valid parish sample* (2)	*Live births* (3)	*Infant deaths* (4)	*1-yr survivors* (5)	*Percent miss.: (1) and (5)* (6)
1935	54562	50736	65223	4634	60589	9.947351
1936	55936	52165	66418	4473	61945	9.700541
1937	57311	53406	67440	4455	62985	9.008494
1938	58268	54370	68462	4022	64440	9.577902
1939	58349	54448	67914	3945	63969	8.785505
1940	60228	56280	70121	3517	66604	9.572999
1941	61682	57767	71306	3919	67387	8.466024
1942	69006	64537	79545	3737	75808	8.972668
1943	72860	67941	84346	3780	80566	9.564829
1944	78547	73395	90669	4322	86347	9.033319
1945	82420	76970	95062	4590	90472	8.899991
1946	84326	75732	96111	4408	91703	8.044448
1947	80909	72353	91714	3710	88004	8.062134
1948	75352	67627	84938	2999	81939	8.038907
1949	71040	63274	79919	2758	77161	7.932764

Source: Authors' calculations from administrative data and the Medical Reports for the Kingdom of Denmark (DNBH, various years).
Notes: Columns (1) and (2) are no. of observations in our full and valid-parish samples. Columns (3), (4) and (5) are national aggregate statistics for the number of life births, infant deaths, and number of one-year survivors of the given cohorts. Column (6) shows the percentage of observations that are missing when we compare columns (1) and (5).

B Additional results

Table B.1: Descriptive statistics, means and standard deviations.

Outcome	(All)	(Urban)	(Female)	(Male)
Deaths, pct	5.982	6.449	4.878	7.056
Deaths, cardio/hearth, pct	1.903	2.006	1.176	2.608
Hospital Nights, no.	11.007	11.609	11.294	10.735
	(0.032)	(0.069)	(0.044)	(0.043)
Cardiovascular Disease, pct	18.527	18.633	16.212	20.781
Diabetes, pct	2.809	3.156	2.072	3.525
No. of obs.	19673	1002	19419	19435

Notes:The unit of observation is the municipality×year cell.

Table B.2: Scaled coefficients for the effect of the home visiting program on adulthood health outcomes, women and men of the cohorts 1935–1949.

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (Matched)	(5) (Ever impl.)
Deaths, cardio/heart, male	-0.100	-0.047	-0.182	0.102	-0.233
	(0.096)	(0.127)	(0.159)	(0.282)	(0.190)
Mean of dep. var. × 100	2.608	2.608	2.608	2.689	2.745
No. of obs.	19435	19435	19435	5766	5414
Hospital Nights, cont., male	-0.624**	-0.589*	-0.752**	-0.386	-0.693*
	(0.255)	(0.318)	(0.327)	(0.506)	(0.393)
Mean of dep. var. × 100	10.735	10.735	10.735	10.599	11.302
No. of obs.	19435	19435	19435	5766	5414
Cardiovascular Disease, male	-0.317	-0.672**	-0.609	0.662	-0.862*
	(0.239)	(0.308)	(0.385)	(0.600)	(0.441)
Mean of dep. var. × 100	20.781	20.781	20.781	20.826	21.108
No. of obs.	19435	19435	19435	5766	5414
Diabetes, male	0.051	-0.146	-0.173	-0.476	-0.032
	(0.113)	(0.157)	(0.164)	(0.336)	(0.215)
Mean of dep. var. × 100	3.525	3.525	3.525	3.391	3.898
No. of obs.	19435	19435	19435	5766	5414
Deaths, cardio/heart, female	-0.141*	-0.126	-0.346***	-0.357*	-0.481***
	(0.080)	(0.096)	(0.105)	(0.199)	(0.127)
Mean of dep. var. × 100	1.176	1.176	1.176	1.237	1.210
No. of obs.	19419	19419	19419	5765	5399
Hospital Nights, cont., female	-0.177	-0.382	-0.240	-1.242***	-0.241
	(0.187)	(0.250)	(0.275)	(0.429)	(0.326)
Mean of dep. var. × 100	11.294	11.294	11.294	11.098	11.600
No. of obs.	19419	19419	19419	5765	5399
Cardiovascular Disease, female	-0.146	-0.187	-0.420	-0.409	-0.721*
	(0.228)	(0.338)	(0.325)	(0.658)	(0.378)
Mean of dep. var. × 100	16.212	16.212	16.212	16.395	15.669
No. of obs.	19419	19419	19419	5765	5399
Diabetes, female	0.083	-0.124	-0.101	-0.260	-0.176
	(0.096)	(0.124)	(0.131)	(0.255)	(0.150)
Mean of dep. var. × 100	2.072	2.072	2.072	2.068	2.192
No. of obs.	19419	19419	19419	5765	5399
Cohort FE	Yes	Yes	Yes	Yes	Yes
Muncipal:					
FE	Yes	Yes	Yes	Yes	Yes
X (level) × year interactions	No	No	Yes	No	Yes
X (trend) × year interactions	No	No	Yes	No	Yes
Linear time trends	No	Yes	No	No	No

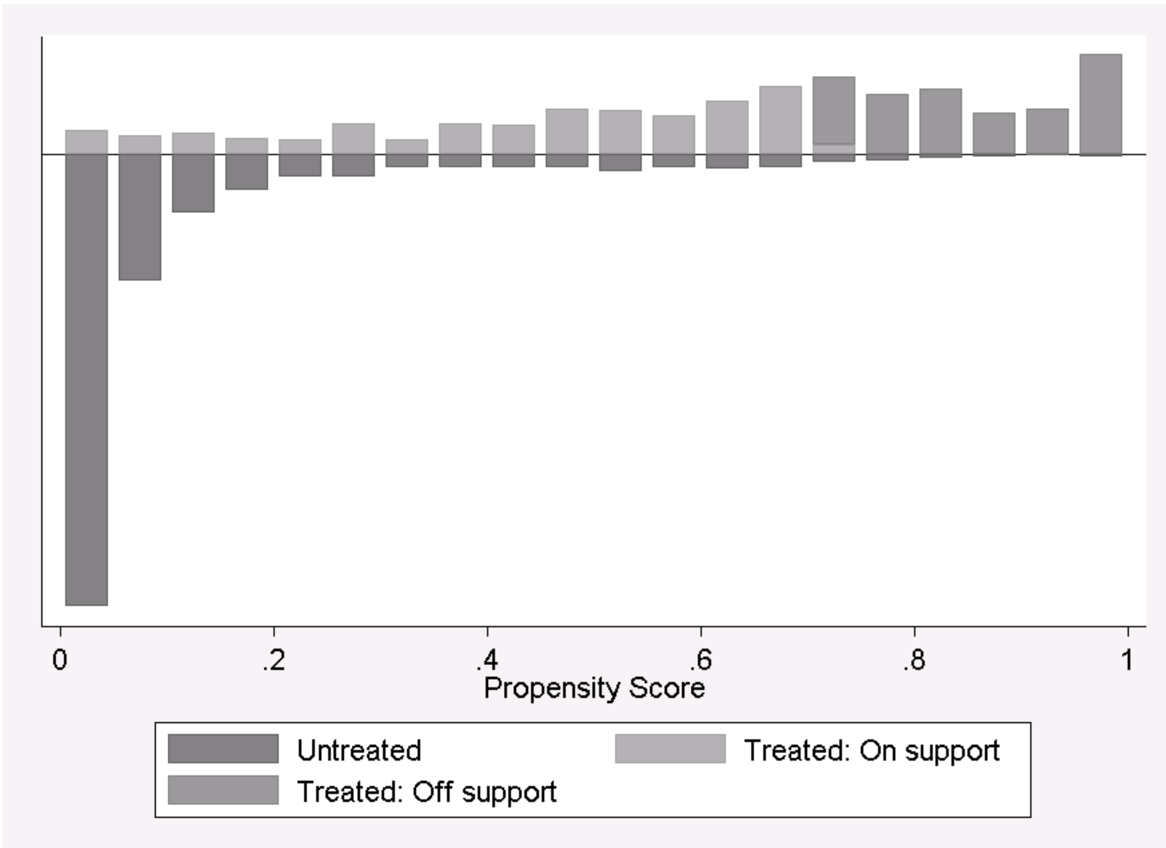
See Notes for Table 4.

Table B.3: Scaled coefficients for the effect of the home visiting program on completed education and adulthood labor market outcomes, cohorts 1935–1949.

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (Matched)	(5) (Ever impl.)
Exam school	-0.729*	-0.269	-0.204	1.210	-0.409
	(0.373)	(0.529)	(0.410)	(0.928)	(0.485)
Mean of dep. var. × 100	40.657	40.657	40.657	33.387	49.017
No. of obs.	19673	19673	19673	5829	5493
7 plus yrs	-3.935***	0.134	-0.611*	-1.234	-0.652
	(0.335)	(0.327)	(0.332)	(0.800)	(0.397)
Mean of dep. var. × 100	65.675	65.675	65.675	60.265	72.910
No. of obs.	19673	19673	19673	5829	5493
Blue collar	1.890***	0.453	0.456	0.558	1.017***
	(0.319)	(0.391)	(0.364)	(0.717)	(0.387)
Mean of dep. var. × 100	38.332	38.332	38.332	43.275	32.915
No. of obs.	19655	19655	19655	5827	5491
Ever in lb force	-1.629***	0.784***	0.270	-0.233	0.211
	(0.190)	(0.211)	(0.189)	(0.460)	(0.228)
Mean of dep. var. × 100	87.168	87.168	87.168	86.872	87.836
No. of obs.	19673	19673	19673	5829	5493
Log earnings, total	-0.307	0.487	1.020*	0.885	0.856
	(0.491)	(0.745)	(0.570)	(0.816)	(0.686)
Mean of dep. var. × 100	1474.187	1474.187	1474.187	1470.123	1478.205
No. of obs.	19496	19496	19496	5766	5436
Total unemployment, weeks	1.561***	0.778*	0.203	-0.113	0.145
	(0.241)	(0.457)	(0.353)	(0.694)	(0.390)
Mean of dep. var. × 100	12.829	12.829	12.829	13.556	11.571
No. of obs.	19497	19497	19497	5766	5437
Cohort FE	Yes	Yes	Yes	Yes	Yes
Muncipal:					
FE	Yes	Yes	Yes	Yes	Yes
X (level) × year interactions	No	No	Yes	No	Yes
X (trend) × year interactions	No	No	Yes	No	Yes
Quadratic time trends	No	Yes	No	No	No

Notes: See Notes for Table 3. *Exam school* is an indicator for individuals having completed the academic track middle school. > 7 *years* is an indicator for individuals having completed any education beyond compulsory schooling. *Blue-collar* is an indicator for individuals employed in any year of our outcome data in a blue-collar occupation. *Ever in lb force* is an indicator for individuals being employed at least in one year in the range of our outcome data. *Log earnings* is the logged wage income for individuals who are employed throughout the entire period age 45–57. *Total weeks of unemployment* is the number of weeks individuals are listed as unemployed age 45–57. ***significant at the 1 pct level, **significant at the 5 pct level *significant at the 10 pct level

Figure B.1: Density of municipalities over the propensity score



Notes: The figure displays the density of municipalities across the propensity score estimated with *psmatch2*. Nearest neighbour matching without replacement and a caliper of 0.4 results in 191 matched treated municipalities. Untreated corresponds to never-implementing municipalities.

Figure B.2: Propensity score for ever-implementing municipalities and their year of treatment initiation

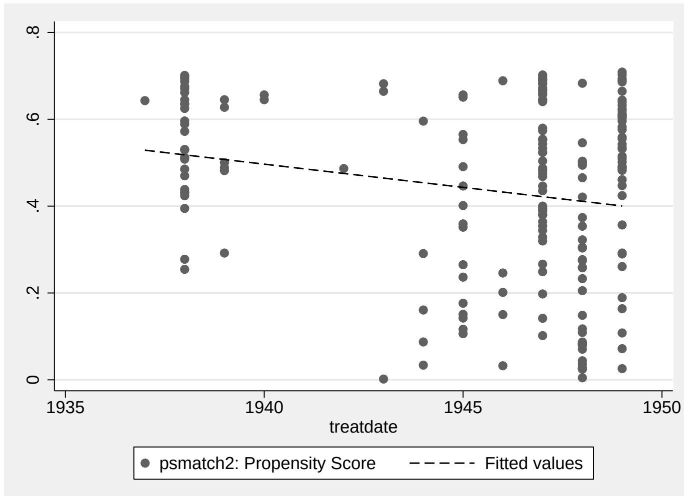
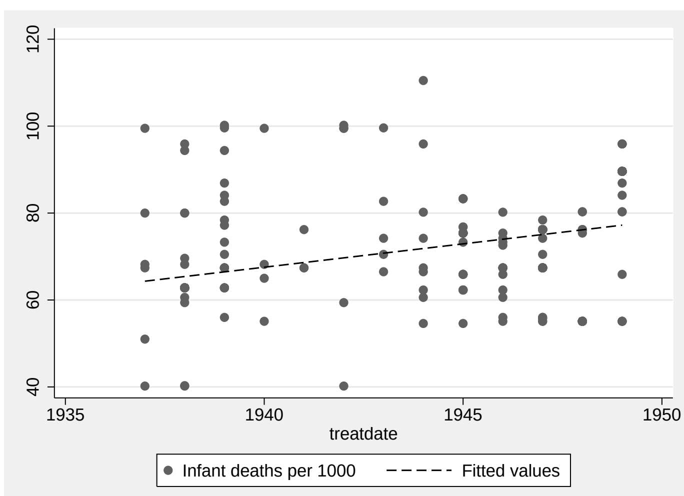


Figure B.3: 1936 IMR for ever-implementing municipalities and their year of treatment initiation, 1937-1949



Notes: The IMR is measured at a more aggregated level (medical districts), resulting in overlapping data points for all municipalities in the same district.