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EARLY CHILDHOOD HEALTH SHOCKS AND ADULT WELLBEING: EVIDENCE FROM WARTIME BRITAIN

Jeffrey C. Schiman
Robert Kaestner
Anthony T. Lo Sasso

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ABSTRACT

A growing literature argues that early environments affecting childhood health may influence significantly later-life health and financial wellbeing. We present new evidence on the relationship between child health and later-life outcomes using variation in infant mortality in England and Wales at the onset of World War II. Using data from the British Household Panel Survey, we exploit the variation in infant mortality across birth cohorts and region to estimate the associations between infant mortality and adult outcomes such as disability and employment. Our findings suggest that higher infant mortality is significantly associated with higher likelihood of disability, a lower probability of employment, and less earned income.

Jeffrey C. Schiman
Department of Finance and Economics
Georgia Southern University
Statesboro, GA 30458
jschiman@georgiasouthern.edu

Robert Kaestner
Department of Economics
University of Illinois at Chicago
815 West Van Buren Street, Suite 525
Chicago, IL 60607
and Institute of Government and Public Affairs
and also NBER
kaestner.robert@gmail.com

Anthony T. Lo Sasso
Health Policy and Administration Division
School of Public Health
University of Illinois at Chicago
1603 W. Taylor
Chicago, IL 60612
losasso@uic.edu

Introduction

A growing literature argues that early environments affecting childhood health may influence significantly adult health and socioeconomic status (Almond and Currie 2011). If so, then investments in early childhood health may have particularly large returns and public policy targeted at infant and child health should be encouraged, particularly when private investment is constrained, for example, among low-income families. In addition, if childhood health does have a lasting effect, then associations between adult socioeconomic status and adult health may overstate the importance of socioeconomic status because child health may have influenced both these adult outcomes.

Several studies have assessed the hypothesis of whether childhood health environments have lasting effects. Barker and Osmond (1986) and Ben-Shlomo and Smith (1991) obtained associations between infant mortality and adult cause of death for birth cohorts born in England and Wales in the late 19th and early 20th centuries. While both studies found some evidence that higher rates of infant mortality were associated with higher rates of death, particularly from heart disease, associations were not consistent and adjusting for socioeconomic status eliminated significant associations (Ben-Shlomo and Smith 1991). Similar studies using 19th century birth cohorts from countries other than England and Wales generally found evidence of scarring—birth cohorts that experienced relatively high rates of infant mortality experienced relatively high rates of adult mortality (Bengtsson and Lindstrom 2000; Catalano and Bruckner 2006; Crimmins and Finch 2006; Bengtsson and Broström 2009; Myrskylä 2010; Schellekens and Poppel 2016). Studies of more recent birth cohorts by Bozzoli et al. (2009) and Case and Paxson (2009) reported similar findings. Case and Paxson (2009) reported that infant mortality was positively associated with infectious disease and cognitive decline in adulthood, and Bozzoli et al. (2009)

found that infant (post-natal) mortality was negatively associated with adult height. A limitation of the studies just described is that they do not exploit plausibly exogenous variation in early-life conditions.[1] Therefore, it is unclear whether the associations obtained are causal. In addition, most of these studies examining the lasting impact of child health used samples of people born in the late 19th and early 20th centuries and results may not generalize to birth cohorts alive today.

There are several studies that exploit more plausibly exogenous variation in early life (i.e., in utero or infancy) health conditions, for example, Almond (2006) who studied the 1918 flu pandemic, and Bleakley (2007, 2010) and Lucas (2010) who studied eradicating malaria and hookworm.[2] These studies, and many quasi-experimental studies focused on famines, generally find evidence that early life health shocks are associated with worse adult health and socioeconomic status, which is consistent with a scarring effect of early life health conditions.[3] However, findings from these quasi-experimental studies are not uniform, as a few studies reported no significant association between early health shocks and adult outcomes (e.g., Stanner et al. 1997; and Kannisto et al. 1997; Lumey et al. 2011; Brown and Thomas 2013).

We present new evidence on the relationship between infant health and later-life outcomes using plausibly exogenous variation in infant mortality in England at the onset of World War II. Three contributions of our research are that we focus on relatively recent birth cohorts; we exploit plausibly exogenous variation in infant health; and we examine whether fertility responses mediate (confound) the effect of the infant health shock.[4] In England between

[1] Some of these studies adjust for trends in mortality by birth cohort (Bengtsson and Lindstrom 2000; Myrskylä 2010).
[2] There are several quasi-experimental studies that examine not health shocks, but the effect of early economic environments on adult outcomes (e.g., Van de Berg et al. (2006); Cutler et al. (2007); Banerjee et al. (2010)).
[3] See: Roseboom et al. (2001), Almond and Mazumder (2005), Almond (2006), Almond et al. (2007), Chen and Zhou (2007); Lindboom et al. (2010); Neelson and Stratmann (2011); Kelly (2011), Almond et al. (2012), Bharadwaj et al. (2013), Dinkelman (2017), and Hjort, Sølvsten, and Wüst (Forthcoming).
[4] Our research is somewhat related to studies that have examined the long-term impact of childhood exposure to warfare and armed conflict on adult outcomes (e.g., Akresh et al. (2012); Akbulut-Yuksel (2014); Kesternich et al.

1939 and 1941, infant mortality rose 17 percent, although the increase is even larger than this value implies because infant mortality was declining steadily during this period. The increase in infant mortality was largely driven by a marked increase in post-neonatal deaths mainly from pneumonia and whooping cough. Historical evidence indicates that the rise in infant mortality during this period was due to a combination of a wartime food rationing program and unusually harsh winters. From 1942 onward, infant mortality fell back to its pre-1940 trend because of priority rationing that favored pregnant women, less harsh winters, and improvement in health services in childcare facilities (see Figure 1).

Within England, the extent to which infant mortality increased varied markedly across regions. For example, infant mortality in London changed little between 1939 and 1941 while in Northeast England it rose by nearly 27 percent. Because the negative health shock was short lived, severe, and varied within the country, it provides a natural opportunity to test the relationship between exposure to an adverse, early childhood health environment and later-life outcomes.

Using data from the British Household Panel Survey, we exploit the variation in post-neonatal mortality across birth cohorts and regions to estimate associations between infant mortality and adult outcomes, such as presence of various health conditions, employment, earned income, home ownership, and disability. Our findings suggest that the increase in infant mortality in the early 1940s is associated with worse health as an adult. Specifically, a one

(2014); Lee (2014); and Havari and Peracchi (2017)). Kesternich et al. (2014) and Havari and Peracchi (2017) study the effect of WWII on several European countries, although not England, and they define exposure in terms of ground combat. However, these studies examined exposure to general war conditions including, poor nutrition, mass dislocation, combat and destruction of the physical environment. Our study is different because we do not study the effects of warfare, but the war-induced rise in infant mortality in 1940-41, which is a very specific exposure caused by identifiable factors. Ground combat never occurred in the United Kingdom, and bombing and relocation was focused mainly on London and a few port cities. The German bombing of London began at the end of 1940 subsequent to the initial spike in infant mortality observed in the data (see Figure 1). We describe the nature of the exposure and the quasi-experimental design in more detail below.

standard deviation increase in infant mortality is associated with approximately a 39% increase in reporting a disability; a 17% decrease in the probability of having a job; and a 21% decrease in annual real earned income. Notably, these effects manifest mainly in later life, particularly after age 60. Another important finding of our study is that childhood socioeconomic status, as measured by father's occupation, does not moderate the effect of infant mortality.

Background: England and Wales in the 1940s

Since the 1930s, infant mortality in England followed a marked downward trend except for 1940 and 1941, as shown in Figure 1. Between 1939 and 1942, infant deaths rose from 50 per thousand in 1939 to 59 per thousand in 1941 and the dropped back to 49 per thousand in 1942. The deviation from trend in 1940 and 1941 is arguably the result of the interaction of food rationing policies, World War II, and the unusually harsh winters of 1940 and 1941, each of which we discuss in turn.

The Ministry of Food was created in 1939 and was responsible for food distribution during the war. Various rationing schemes were used including direct distribution of items and allocations based on coupons and points for different foodstuffs. Food items including milk, eggs, cereals, oranges, butter, bacon, sugar, meats, and cheeses were rationed beginning in January of 1940 at the start of major wartime actions and rationing became increasingly stringent as the war went on. The daily rations provided approximately 910 calories, but little calcium and vitamins. To protect the health of women with children and expectant mothers, the National Milk Scheme was started in June of 1940 and provided additional priority allowances, which supplied an additional 540 calories and the bulk of their daily requirement of calcium and vitamins. However, initial take-up was low and the program did not witness significant uptake until 1942.

The priority allowance was later credited as having “done more than any other single factor to promote the health of expectant mothers and young children during the war” (Great Britain Ministry of Health 1946, p. 93).

In addition to rationed food, in the fall of 1940 until the spring of 1941, England was under assault from German bombing campaigns that targeted the more densely populated and better developed areas of England such as London. From these areas, the government evacuated children and expectant mothers to rural areas in England (Great Britain Ministry of Health 1946, 92). The evacuation caused shortages in supplies, staff, and accommodations in destination areas that could adversely affect the quality of infant and child healthcare. By 1940, England had developed day nurseries for displaced children and children of parents engaged in the war effort (Great Britain Ministry of Health 1946, p. 98). Initially, staying in the day nurseries was associated with the transmission of infectious disease given close confinement of children, but by 1942 the quality of the nurseries improved because of better staffing and more abundant supplies and resulted in a decrease in the incidence of disease (Great Britain Ministry of Health 1946, p. 99).

Furthermore, the 1939-40 and 1940-41 winters produced extremely low temperatures, extensive frosts, and large snows. The meteorological record from the period described January 1940 as “exceptionally cold; intense frost; considerable snow in the latter half of the month”; January 1941 was described as “cold, with frequent snow” (“Monthly Weather Reports 1940s” 2016). The inclement weather of the period was not uniform across regions, however, as

England, despite being a relatively small country, has surprisingly diverse microclimates that exacerbate or buffer general weather patterns.[5]

Table 1 displays infant mortality rates by cause between 1939 and 1942. The noteworthy contributions to the increase in mortality in 1940 and 1941 are bronchitis, pneumonia, and whooping cough. These illnesses account for 60% of the total increase in infant mortality between 1939 and 1941, and are conditions that are plausibly affected by inadequate nutrition, inclement weather, and wartime dislocation. For example, harsh winters, poor nutrition combined with a lack of fuel products to heat homes (from the ration), and the crowding of children into day nurseries may have facilitated the transmission of pneumonia and whooping cough because of their infectious nature (Griffiths and Brock 2003). Moreover, several researchers have linked pneumonia incidence to vitamin D deficiency (Muhe et al. 1997; Wayse et al. 2004; McNally et al. 2009; Wonodi et al. 2012). Eggs are rich in vitamin D, and were among the rationed food items that were particularly scarce in the early war years.

Figure 2 displays the overall trend in neonatal and post-neonatal mortality in England and Wales from the period. Note that while overall neonatal mortality increases somewhat, the significant deviation from trend occurs for post-neonatal mortality. To more closely focus on the post-neonatal period, Appendix Figure 1 displays child mortality for children aged 1-2 and those aged 3-5. Both groups experienced significant percentage increases in mortality in 1940 and 1941.

Notably, we also observe substantial variation in the magnitude of infant deaths within England across regions. Generally, areas farthest from London experienced the greatest rise in

[5] See appendix figure 2 and http://www.metoffice.gov.uk/binaries/content/assets/mohippo/pdf/n/9/fact_sheet_no._14.pdf (last accessed 7 August 2017)

infant mortality. In Figure 3 we present the difference in infant mortality between observed value and a predicted linear trend by region. A value of zero indicates that infant mortality in a region is perfectly on trend. In 1940-41, there is a clear upward divergence from trend with regions farthest from London experiencing the largest increases.

To explore further the relationship between weather and infant mortality, we compiled historic weather data from the UK meteorological office. [6] The data contain monthly measures of average minimum temperatures which we use to illustrate the relationship between infant mortality and weather in Figure 4. The solid line represents deviations from a linear trend in infant mortality from 1931 to 1960. The dashed line represents deviations from a linear trend in the average minimum daily temperature in January of each year. The two measures appear inversely correlated, with mortality rising above trend when minimum temperatures fall below. Particularly salient is the sharp decline in minimum temperatures during 1940 and 1941, which coincide with sharp increases in mortality. Note too, that the increase in infant mortality in response to the colder winters in 1940-41 is significantly larger than the increase in infant mortality corresponding to similarly a cold winter in 1945, which is consistent with the interaction between poor nutrition, poor conditions in day nurseries, and cold weather.

The evidence from rationing and harsh winters is consistent with areas farther from London experiencing a greater increase in infant mortality. More remote areas likely had fewer resources and fewer rationed goods. Historical weather information suggests that the winters in the north of England were somewhat harsher than in the south, which also partly explains the within-country variation. Appendix Figure 2 presents the difference in average January temperatures between the observed value and a region-specific linear trend. The figure illustrates

[6] http://www.metoffice.gov.uk/public/weather/climate-historic/#?tab=climateHistoric (last accessed 7 August 2017). Only six of our ten regions are represented during the entire 1930 to 1960 timeframe.

that the 1940/41 winters were particularly harsh across the UK, with the regions experiencing January temperatures around three to six degrees below trend. Moreover, the predominately rural areas where shelters were built to house expectant mothers were also away from London, which would additionally explain part of the pattern in infant mortality.

To summarize, Figures 1-4 and Table 1 document the circumstances surrounding the significant spike in post-neonatal and early childhood mortality. The combination of food shortages, harsh weather, and disruption in children's healthcare resulted in greater incidence of infectious, respiratory disease for young children in 1940 and 1941. Additionally, the incidence differed substantially by region within England. The variation in child health across both time and region, as measured by infant mortality, represents our measure of the exposure to adverse health shocks. These infant health shocks mainly reflect infectious disease, and, as Crimmins and Finch (2006) and others have articulated, there are plausible biological mechanisms linking infectious disease during infancy/childhood to adult disease.

Data

To test whether exposure to a health shock in infancy affects adult outcomes, we use data from the British Household Panel Survey (BHPS) and the Great Britain Historical Database. The BHPS is a longitudinal survey of approximately 5,500 households in Britain from 1991 to 2009.[7] The survey is well suited for our study because respondents are asked their date and place of birth, which we match with the Great Britain Historical Database to determine exposure to the health shock as an infant. The Great Britain Historical Database contains information on infant deaths and births, as well as population by location and year.

[7] See (University of Essex. Institute for Social and Economic Research. 2010)

The sample we use is limited to those born between 1935 and 1950, which is a period that spans the sharp rise in infant mortality, but is a relatively narrow window of time to reduce potential confounding from economic and technological changes occurring during the period. There are 52,348 person-by-wave observations for this sample. From the initial 52,348 person-waves selected, we drop 5,001 observations that are missing information for place of birth and another 11,741 born in Scotland because there is no data available on infant mortality in Scotland. We drop two additional observations due to missing month of birth. Our final sample consists of 35,604 person-by-wave observations for 3,312 persons.

The BHPS are longitudinal data covering an 18-year period. In our sample, the average individual is observed in approximately 10.75 waves of the survey. Our primary regression approach uses all person-wave observations thereby treating each person-wave observation as a separate observation. This is a reasonable approach because the outcomes we measure are changing over time as the person ages.

We calculate infant mortality (IM) and birth rates by region and year.[8] Infant mortality (IM_{irc}) is measured as a weighted average of infant mortality in the year of birth and in the following year, for example, because those born in January experience a different environment than those born in December of the same year. Specifically:

$$IM_{irc} = \alpha IM_{irc} + (1 - \alpha) IM_{ir,c+1}$$

where birth-year mortality (IM_{irc}) and the following-year mortality ($IM_{ir,c+1}$) are weighted (α) by birth month. An individual born in January receives a weight of 1. The weight reduces by 1/12 for each additional month so that an individual born in February receives a weight of $\alpha =$

[8] Specifically, we use table "mort_lgd" which records "Birth & Death Statistics for Local Government Districts from 1921-1974." Although county-level information is available in both data sets, a significant number of the counties of birth in the BHPS are not listed in the historical database, and therefore, we aggregate infant mortality to the region level; there are ten regions in total—nine in England plus Wales.

0.916; March $\alpha = 0.833$; and so on. The approach places more weight on actual birth-year infant mortality for those born earlier in the year; for those born later in the year more weight is placed on the subsequent year's mortality. IM_{irc} is our measure of treatment, or exposure, to adverse early health conditions. We standardize infant mortality to have an overall mean of 0 and standard deviation of 1. The standard deviation of infant mortality is 11.8, roughly the size of the mortality shock in 1940/41.

Survey respondents in the BHPS answer questions about their health including self-reported health status, presence of health problems, and disability status in addition to information about employment, income, and home ownership. For self-reported health status, individuals are asked, "Please think back over the last 12 months about how your health has been. Compared to people of your own age, would you say that your health has on the whole been" and they may check excellent, good, fair, poor, or very poor from which we define two indicators: an indicator for good or excellent health and an indicator for poor or very poor health. For the presence of health problems, individuals are asked "Do you have any of the health problems or disabilities listed on this card?" which include thirteen conditions including problems with arms or legs, problems with chest or breathing, problems with heart or blood pressure, difficulty seeing, difficulty hearing, skin conditions, stomach/liver/kidney, diabetes, nerves/anxiety/depression, alcohol/drugs, epilepsy, migraine/chronic headache, or other. Respondents could check any conditions that apply and our measure of any self-reported health problems is an indicator that equals one for those who suffer from at least one of the aforementioned conditions. We also examine the top three most prevalent conditions individually.[9]

Our measure of disability status comes from the question “Can I check, are you registered as a disabled person, either with Social Services or with a green card?” Those who check yes are defined as disabled. Home ownership status is derived from a question that asks individuals if they own a home or rent a home. We define an indicator for home ownership equal to one if the individual responds that they own their home outright or with a mortgage and zero otherwise. We measure employment using a dichotomous measure that equals one if the respondent did any paid work in the previous week and zero otherwise. Finally, respondents report annual income from labor (i.e., earned income).

Respondents are also asked “Thinking back to when you were 14, what job was your father doing at that time?” If working and not deceased, the father’s job title, nature of work, and industry were recorded and used to create several variables that measures the social class of the father’s job. Occupations are grouped into the following categories: professional, managerial/technical, skilled (manual/non-manual), partly-skilled, and unskilled. From this we categorize respondents as growing up in a high (professional or managerial/technical) or low (remaining categories plus non-working/deceased) socioeconomic class.

Table 2 reports descriptive statistics. On average, the sample is 56 years of age, although there is significant variation in age indicated by a standard deviation of 6.8 years. Slightly more of the sample is female (54%), which is consistent with longer life expectancy of females vis-à-vis males. Figures related to our outcomes indicate that approximately 69% report a health

[9] Approximately 69% of our sample report suffering from one or more of the thirteen conditions. The top three reported conditions include arm/leg problems (38%), heart/BP problems (23%), and chest or breathing problems (13%).

problem, although only 12% report being in poor or very poor health. The apparent discrepancy between the proportion being in poor health and the proportion with a health problem likely reflects the self-reported nature of the questions and the fact that many respondents report a health problem that is relatively minor.

Measurement of Exposure

Our measure of exposure, or treatment, is the infant mortality in the region and year, which is derived from administrative data. Given the wartime relocations, there is a question as to whether births and deaths were recorded consistently with respect to place of residence or place of occurrence. The administrative data are quite clear that deaths were recorded based on place of occurrence. However, it is possible that births might have been recorded at place of permanent residence if relocation occurred soon after birth, but there were great efforts to relocate pregnant women which would minimize this possibility. For example, in 1939, 13,900 pregnant women were relocated out of greater London (Johnson 1985). Consistent with this, births in London fell by roughly 20,000 between 1939 and 1941 (Appendix Figure 3). Therefore, it is likely that most births were recorded in the place of occurrence and thus the extent of any misreporting of births would appear to be small.

A second concern is measuring exposure for individuals who were relocated. Note that the adverse health shocks were manifest within the first year of life, as shown in Figure 2. Thus, infant mortality in the place of birth accurately reflects exposure for all those who stayed in the relocation region for several months, which is likely to be the vast majority of persons that were relocated.[10] Exposure would be incorrectly assigned if survey respondents who had been relocated reported their birth region as their permanent residence rather than the actual region of

[10] However, for those who return to their permanent residence, some of the effect of exposure could be confounded with their moving during childhood.

birth. To address this concern, given that most of the relocation is associated with London, we re-estimate our regressions omitting survey respondents born in London. Our findings are largely unchanged when we omit London births.

Empirical Approach

The hypothesis we are interested in assessing is whether exposure to a health shock in infancy affects adult outcomes. This hypothesis is motivated by two broad causal mechanisms. The first is often referred to as "scarring" (Elo and Preston 1992). The scarring channel suggests that exposure to a negative health shock may adversely affect developing organs and immune system (e.g., inflammation) and cause worse later-life health. For example, the observed increase in infectious disease during 1940-41 because of food rationing and harsh winters may lead to worse later-life outcomes for those born during this period. A second mechanism linking early and late heath is often referred to as "culling." The culling channel suggests that during exposure to an adverse health event the weakest children die leaving a healthier group of children. In this case, early health shocks result in better adult health.

Of course, these broad causal pathways are not mutually exclusive and which channel dominates is an empirical question. There may also be behavioral responses to the health shock in childhood. Parents may respond by investing in affected children and, as we discuss below, fertility may be affected by the health shock. Parental responses and their influence are additional considerations relevant to the interpretation of estimates of the association between child health and adult outcomes. Such behavioral responses may reinforce or offset the scarring and culling effects.

Regression Model Specification

To estimate the effect of infant mortality on later-life outcomes, we use regression methods and the following regression model:

(1) $$Y_{ircma} = \alpha + \beta_1 IM_{irc} + \gamma X_{ia} + \theta_r + \phi_c + \delta_m + \lambda_a + \varepsilon_{ircma}$$

i = (individuals)
$r = 1,2,\ldots,10$ (Regions)
$c = 1935,\ldots.,1950$ (Birth year cohort)
$m = 1,\ldots,12$ (Birth month)
$a = 40,\ldots,73$ (Survey Age)

where adult outcome Y_{ircma} for individual i born in region r in birth year c and month m and surveyed at age a is a function of the infant mortality rate (per one thousand live births) in their region and year of birth, IM_{irc}. Other covariates include controls for observable individual characteristics at the time of survey X_{ia} including current place of residence and sex, a region of birth fixed effect θ_r, a birth cohort fixed effect ϕ_c, a birth month effect δ_m, a survey age fixed effect λ_a, and an error term ε_{ircma}. The adult outcomes we consider are self-reported health status, incidence of health problems, disability status, labor force participation, real annual labor income, and home ownership.[11]

The region fixed effect (θ_r) accounts for unobserved differences between the regions including resources available, local development, and any other fixed differences across regions related to both infant mortality and later-life outcomes for the cohorts born between 1935 and 1950. As Figure 3 showed, each region experienced different exposure to infant mortality, and by including the fixed effect, we compare outcomes within region. The birth cohort fixed effect (ϕ_c) accounts for differences by year of birth common to all individuals in England and Wales,

[11] We adjust annual labor income for inflation using the CPI index available here https://www.ons.gov.uk/economy/inflationandpriceindices/timeseries/d7bt/mm23 (accessed 21 December 2016). We use 2008 as the base year.

such as changing medical technology and environmental conditions that account, partly, for the downward trend in infant mortality experienced during this time. The survey age fixed effect (λ_a) accounts for any differences in age at the time of the survey (and any survey year effects because birth year plus age equals survey year).

The identifying assumption of our approach is that, conditional on region, age, and birth year fixed effects, variation in infant mortality is largely driven by exogenous factors associated with the war such as food rationing and adverse weather conditions. In some specifications, we include region-specific, linear birth-year trends and the results remain unchanged.[12]

To estimate equation (1), we use a linear probability model for dichotomous outcomes, but estimated effects are similar when using a logit model. In the case of earned income, we use a generalized linear model (GLM) specification with a Gaussian distribution assumption. The effect of infant mortality as embodied in the coefficient β_1 represents the consequences of an adverse health shock early in life.

As noted earlier in the description of the data, the sample includes multiple observations for each person, which raises three potential concerns: one related to the construction of standard errors; another related to the timing of any effect (or age that an effect becomes manifest); and a third related to possible bias from including different numbers of observations for individuals if, for example, survival is correlated with infant mortality.

With respect to standard errors, we used two methods: a cluster-robust method that assumes the errors are correlated (non-independent) within region-by-year of birth cells (i.e., the level of treatment), and randomization inference. Randomization inference relies on virtually no

[12] In a separate specification (available upon request) we also included a survey year linear trend, which had virtually no effect on our estimates. Following Almond and Mazumder (2005), we also interacted age and survey wave which also had virtually no effect on our estimates (available upon request).

statistical assumptions (Imbens and Rubin 2015). While we believe these methods are valid even in the context of multiple observations per person, particularly randomization inference, we also used an alternative. Specifically, we used cluster-robust methods that assume non-independence of errors within a person over time. Estimates from this alternative were very similar to those reported (available on request).

With respect to the timing of any observed effects, we estimate a separate specification that allows the effect of exposure to vary by age. With respect to the possibility that using more observations for some individuals and fewer for others may introduce bias, we obtained estimates using a sample consisting of one randomly selected observation for each person. In this case, each individual contributes only one observation and the sample of unique persons has the same distribution of characteristics as the full sample that includes multiple observations per person. Estimates from this sample address concerns over the standard errors and concerns related to potential bias from using more observations for some people than others.

Moderation by Socioeconomic Status

It is possible that family socioeconomic status during childhood could moderate effects of early-life health shocks. For instance, even with a food ration in place, wealthier families may be able to obtain more food through alternative channels, which would tend to lessen any adverse effect. To explore this possibility, we estimate a model that allows the effect of infant mortality to differ by socioeconomic status, as measured by indicators for whether your father worked in a lower (unskilled, partly-skilled, or skilled manual/non-manual) or higher (managerial/technical or professional) social class occupation when you were 14. Specifically, we estimate:

(2) $$Y_{ircma} = \alpha + \beta_1(IM_{irc} * HIGH_{irc}) + \beta_2(IM_{irc} * LOW_{irc}) + \beta_3(HIGH_{irc}) + \gamma X_{ia} + \theta_r + \phi_c + \delta_m + \lambda_a + \varepsilon_{ircma}$$

Equation (2) is identical to equation (1) except now we include an interaction between infant mortality (IM_{irc}) and higher ($HIGH_{irc}$) and lower (LOW_{irc}) early-life socioeconomic status. We include both interactions between infant mortality and socioeconomic status and exclude the infant mortality main effect.

If differences by early-life socioeconomic status exist, they would be reflected in differences between β_1 and β_2 in equation (2).

Age when Effects Become Manifest

To investigate the timing (age of occurrence) of observed effects, we estimate a model that allows the effect of infant mortality to differ by age. Specifically, we estimate:

$$(3) \qquad Y_{ircma} = \alpha + \beta_1(IM_{irc} * AGE_1) + \beta_2(IM_{irc} * AGE_2) + \beta_3(IM_{irc} * AGE_3) + \gamma X_{ia} + \theta_r + \phi_c + \delta_m + \lambda_a + \varepsilon_{ircma}$$

Equation (3) is also identical to equation (1) except now we allow the effect of infant mortality to differ by age classified by terciles [the 1st (lowest) tercile ranges from 40 to 53; the 2nd tercile ranges from 54 to 59; the 3rd (highest) tercile ranges from 60 to 73]. We include all three interactions between infant mortality and age groups and exclude the infant mortality main effect.

Considering Birth Rates

One issue that merits consideration is the potential response of births to infant mortality.[13] For example, parents may try to replace children lost to illness (Ben-Porath 1976). Wartime in the UK brought considerable changes in birth rates. Total births in London fell along with the population, but rebounded beginning in 1942 (available in Appendix Figure 3). A similar pattern is evident from the regions outside London (Appendix Figure 4). Also, note that London makes

[13] There may be other (parental) responses, but we are unable to measure them because of data limitations.

up a relatively small fraction (15%) of total births. We also document birth rates for the entire country (Appendix Figure 5), which indicates that the birth rate started increasing beginning in 1942, eventually peaking in the immediate post-war years. Birth rates from 1935-41 were stable at around 15 per 1,000 total population, but in 1942 birth rates increased to 17 per 1,000 total population. From 1942 until well after the end of the war, birth rates stay at or above this elevated level with some noticeable decline after 1947.

The increase in birth rates in 1942 is suggestive of a parental response to the negative health shock that may vary at the region-by-time level and would not be accounted for by any of the fixed effects in the regression specification.[14] In short, the fertility response may have altered later life outcomes because of differences in family size (spacing) by birth cohort and region. Therefore, we estimate some models with birth rates to assess whether estimates of the association between infant mortality and later life outcomes is sensitive to the inclusion of birth rates.

Results

Table 3 presents estimates of the effect of infant mortality in the respondent's region and year of birth on adult outcomes.[15] In all regressions, infant mortality is standardized to have an overall mean of 0 and standard deviation of 1 and standard errors are clustered by region of birth-by-year of birth cells. The first column of Table 3 shows the baseline specification without additional person-level covariates or controls for birth rates. The second column adds person-level controls including age, sex, and current region of residence. The third column adds the birth

[14] As noted earlier, there may be other parental responses, but we cannot measure them. We are able to measure births,

[15] We also estimated a non-linear specification where infant mortality entered as a quadratic form (available upon request), but the results do not differ qualitatively from the linear specification.

rate in the year and region the respondent was born. The fourth column adds region-specific, linear birth-year trends.

Estimates for each outcome are similar across the four columns. Estimates that are statistically significant suggest a scarring effect and an adverse effect of poor infant health on adult health and socioeconomic status. For instance, the effect of infant mortality on self-reported disability in columns 1 through 4 suggest that a one standard deviation increase in infant mortality (roughly the size of the increase between 1939 and 1941) is associated with approximately a 3.9 percentage point increase in disability, which is a 39% increase relative to the mean. The estimate remains stable across all specifications. A summary of the other significant effects of a one standard deviation increase in infant mortality is as follows:[16]

- approximately a 9.3 percentage point (17%) decrease in the probability of having a job;
- and an approximately a £2180 reduction (21%) in annual real labor income.

Notably, estimates in column 4, which come from regression models with region-specific, birth-year linear trends are largely similar to estimates in other columns. The specification of the model in column 4 controls for potentially unmeasured confounding from factors that vary by region and birth year. Therefore, the similarity of estimates across columns suggest that the variation in infant mortality arises largely exogenously due to the war conditions described earlier.

In Table 4, we explore whether the effects of infant mortality differ by father's occupational (socioeconomic) status. In particular, we estimate equation (2) which includes interactions between infant mortality and indicators for whether your father worked in a lower

[16] If we apply the Holm-Bonferroni correction for multiple hypothesis testing, only the estimates associated with having a job remains significant. Note, however, that the Holm-Bonferroni correction is overly conservative because it assumes that estimates are independent, which is not the case.

(unskilled, partly-skilled, or skilled manual/non-manual) or higher (managerial/technical or professional) social class occupation when you were 14. In the regression, we include interactions between mortality and both SES categories and exclude the main infant mortality effect. Estimates in Table 4 indicate that for most outcomes the effect of infant mortality shock on adult outcomes is statistically the same for those from lower and higher SES families. The one exception pertains to the estimates of whether a person has a job. In this case, the estimate for the high socioeconomic group is noticeably smaller and statistically different from the estimate for the low socioeconomic group. P-values reported in column (3) test the equality of the estimates from columns (1) and (2). In all but two cases, we fail to reject the null hypothesis that the estimates are statistically the same. For one of these outcomes, owns home, neither estimate is statistically different from zero. These results suggest that, in general, either exposure to the shock was no worse for those in lower and higher SES households or that family SES did not moderate the relationship, with the exception related to whether a person works.

In Table 5, we explore whether the infant mortality effects differ by age of onset. To do so, we estimate equation (3), allowing the effect of infant mortality to differ by age, which is measured as indicators denoting age groups. 40 to 53; 54 to 59; and 60 to 73. In the regression, we include all interactions and exclude the main infant mortality effect.[17] The results from Table 5 are consistent with those in Table 3, but show that the adverse effects of mortality are concentrated later in life. For instance, the effect of mortality on the probability of reporting a disability are smaller for those in the first two age groups (i.e. those 59 and under) than for those in the oldest group and not statistically significant. For those in the oldest age group, the estimate

[17] We also estimated a specification interacting infant mortality and a quadratic in age, and calculated the partial effects of mortality on the outcomes at the midpoints of the age terciles. The partial effects at these ages line up closely with the effects by age terciles (available upon request).

suggests a one standard deviation in mortality is associated with a 4.6 percentage point increase in the probability of reporting a disability, which is a roughly 46% increase relative to the mean. A similar pattern emerges for the probability of having and job and real annual labor income with associations becoming larger at later ages.

Randomization Inference

In order to assess the validity of the analytical approach to statistical inference, we conducted two randomization inference analyses in which we expect to find null effects. First, we reassign the value of infant mortality using the value of infant mortality in a randomly selected birth region. For example, the randomization assigned all those born in East Midlands (in any year) the infant mortality rate in the South East (in the same birth year). We did this randomization 1000 times and re-estimated the model using the specification in column (3) of Table 3.[18] The second randomization analysis reassigned the value of infant mortality using the value of infant mortality in a randomly selected birth year. For example, we reassigned all persons born in 1948 the infant mortality in their region in 1942. Again, we did this randomization 1000 times and re-estimated the model.

Table 6 displays the results of these two randomization analyses. Column (1) of Table 6 shows the estimates from column 3 of Table 3. Columns (2) and (3) report p-values from the two randomization analyses. The p-values are calculated as the proportion of estimates (out of 1000) that are larger in absolute value than the estimate in column (1). So, the p-value of 0.566 in row (1) and column (2) of Table 5 indicates that 566 out of the 1000 estimates from the randomization exercise that reassigned regions were larger than the actual estimate. This finding suggests that the actual estimate is not "unusual" and is unlikely to be a true effect. Consistent

[18] There are 362,880 possible permutations.

with this result is the fact that the estimate in row (1) and column (1) of the effect of infant mortality on excellent health is small (<3%) and not statistically significant. For every outcome, the results of the two randomization-inference analyses are consistent with the actual results reported in column (1) of Table 6. When the actual estimate is relatively large and statistically significant, the corresponding p-value from the randomization inference tests are small. These results provide strong evidence that the inference approach used in Table 3 are reliable.

Sensitivity Analyses

In Table 7, we report a variety of sensitivity analyses that address possible concerns related to sample selection and the measurement of infant mortality.

Altering the Birth Cohorts Examined

As noted earlier, our sample includes individuals born between 1935 and 1950. We hypothesize that including cohorts born before the shock may attenuate estimates if older children (e.g. those born in 1935 who would be 6 or 7 in 1940/41) were themselves harmed by the 1940-41 health environment. We assess this hypothesis by re-estimating the regression model with a sample that excludes these earlier cohorts. We also explore whether our estimates are sensitive to using different ending birth cohorts instead of 1950, specifically, 1945 and 1955.For comparison, column 1 of Table 7 replicates column 3 of Table 3.

In column 2 of Table 7, we present estimates from a model that includes cohorts born between 1940 and 1950. Comparing estimates in columns (1) and (2) indicates that there is little difference, particularly for estimates that are statistically significant. The similarity of estimates and lack of attenuation when earlier birth cohorts are included suggests that the effect of health shocks in infancy are more important than health shocks at later childhood ages.

Estimates in columns 3 and 4, which alter the ending year of the birth cohorts included, are also consistent with those in Table 3. These estimates show that the adverse effects of infant mortality are not sensitive to the sample period. Overall, changing the beginning or ending period of birth cohorts has relatively little effect on the estimates.[19]

Using Only One Observation per Individual

As noted earlier, the average individual is observed in approximately 11 waves of the survey, with a standard deviation of 6. To address any concern that estimates and standard errors may be biased by using multiple observations per person, we re-estimate models using a sample consisting of a randomly selected observation for each person. So, the sample for this analysis consists of the 3,312 unique individuals with characteristics that have the same distribution as the full sample. The results from this analysis are presented in column 5 of Table 7.

Qualitatively, estimates in column 5 are similar to other estimates and indicate that infant health shocks are associated with adverse adult health. For example, a one standard deviation increase in infant mortality is associated with approximately a 6.8 percentage point increase in disability. For this sample, however, we observe evidence of scarring for more outcomes. Estimates suggest that infant mortality is negatively associated with self-reported health (decrease in good health and increase in poor health) and positively associated with presence of health problems. However, while magnitudes of estimates are suggestive, estimates are not significant. Finally, we note that standard errors using the randomly selected sample are similar to estimates using all person-year observations.

[19] The one exception to this conclusion may be estimates for whether a person has a chest/breathing problem.

Omitting London

Because residents of London experienced the greatest amount of relocation during the war, there is a concern about potential measurement error in infant mortality. To address this, we re-estimating our primary specification omitting people who reported being born in London (Column 6 of Table 7). A comparison between estimates in Column 6 and Column 1 (original estimates) indicates that they are very similar suggesting that any measurement error introduced by dislocation does not affect estimates.

Conclusion

The marked increase in infant mortality in England and Wales during WWII represents a severe infant health shock. As other research has suggested, such adverse health shocks early in life may have long lasting effects. In this paper, we add to that literature by examining how the spike in infant mortality in Great Britain at the start of WWII affected adult outcomes. Historical evidence suggests that the sharp rise in infant mortality in England in 1940 and 1941 was largely driven by a combination of a wartime food rationing program, unusually harsh winters, and dislocation of families and health services due to the war. Moreover, the extent of the adverse health shocks varied considerably across regions within England. It is this plausibly exogenous variation in infant health, arising largely due to greater rates of infectious disease, which we exploit to obtain estimates of the effect of early life health on adult outcomes.

We find that the wartime spike in infant mortality had a negative effect on later-life outcomes. The results are consequential in magnitude. We find that a one standard deviation increase in the region-specific infant mortality rate (roughly the size of the increase between 1939 and 1941) was associated with a 39% increase in reporting a disability; a 17% decrease in the probability of having a job; and a 21% decrease in annual real earned income. The estimates suggest plausibly that a disability causes a person to drop out of employment.

One contribution of our study was focusing on recent birth cohorts, as relatively few studies have done so. Bozzolli et al. (2009) reported that, for birth cohorts from a comparable period as those in our sample, higher rates of post-neonatal mortality from pneumonia, which is similar to our context, was the most consistent predictor of lower adult height in the US and European countries. The effect size for the US implied by these estimates is small—the change in post-neonatal mortality rate in the US from 1950 to 1980 is predicted to have increased adult height by 0.2 centimeters. Case and Paxson (2009), reported that higher rates of infant mortality among US cohorts born between 1910 and 1950 was associated with reduced cognitive function. Here too, the effect size was quite small; a 50% increase in infant mortality was associated with one-tenth of a standard deviation decrease in cognitive test (word recall) score. Our estimates also show scarring effects of infant health shocks like these studies, but are effect sizes are larger.

Three other findings merit note. While descriptive evidence suggested that there was a fertility response to WWII and that average family size differed by birth cohort, this variation in fertility was not a confounding factor of the effect of infant mortality. Second, we found some evidence that higher socioeconomic status moderated the effect of the infant health shock. Finally, the evidence was clear that effects of the early life health shock grew with age and were more present after age 60.

In summary, the findings of our study add to the literature documenting the adverse effects on adult health and socioeconomic status of early life health shocks. Our study examined the effects of a specific health shock during a narrowly defined age (post-neonatal) on adult outcomes for a contemporary population of adults in a developed country setting—England and Wales. Whether these findings are relevant to today is unknown, but poor infant health remains a problem among disadvantaged populations in developed countries. It is possible that the effects of such poor health on future adult outcomes may be diminished by current medical technology, and social and economic institutions, but if so, then this beneficial effect underscores the importance of these mediating forces. If not, then our results highlight the potential for significant benefits from interventions targeted at improving infant health.

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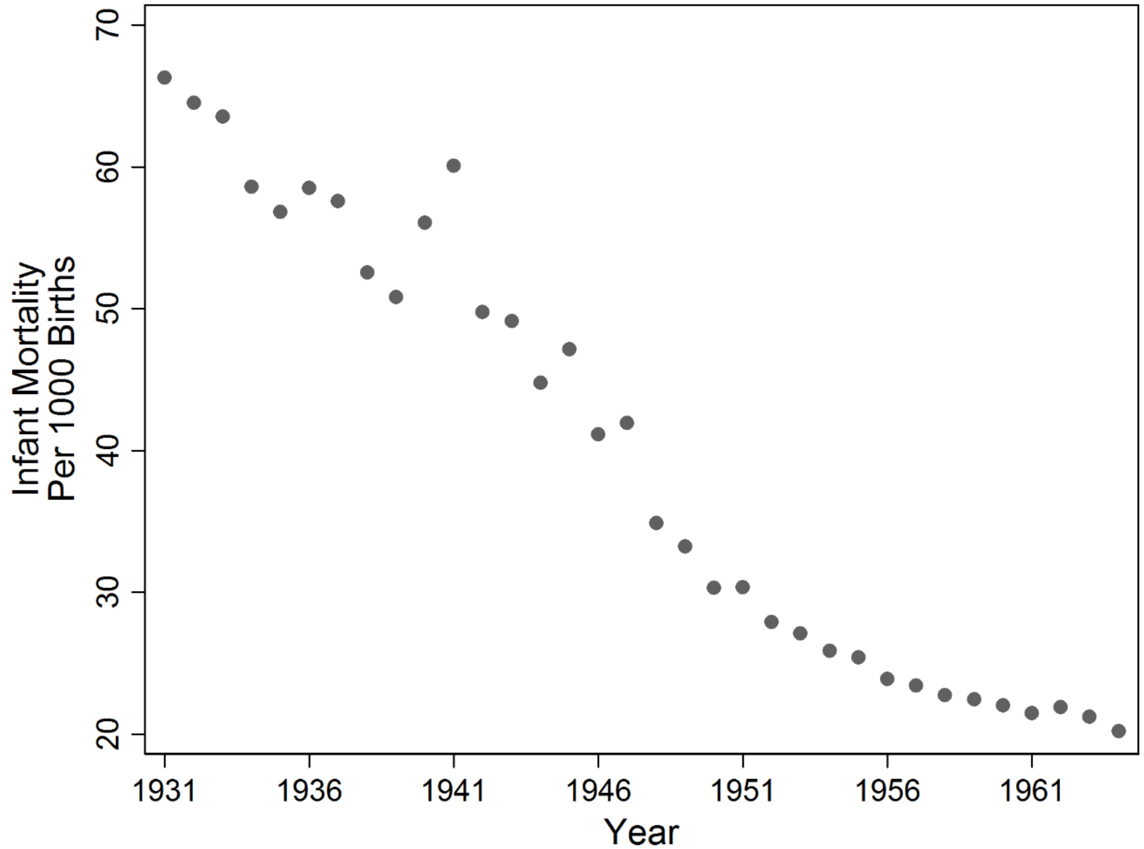


Fig. 1. Infant Mortality (Per 1,000 Births) in England – 1931 to 1964
Source: Authors calculations from table "mort_lgd" in the database of "Birth & Death Statistics for local Government Districts from 1921-1974."
Notes: In the figure we plot average infant mortality by year for those born in England between 1931 to 1964.

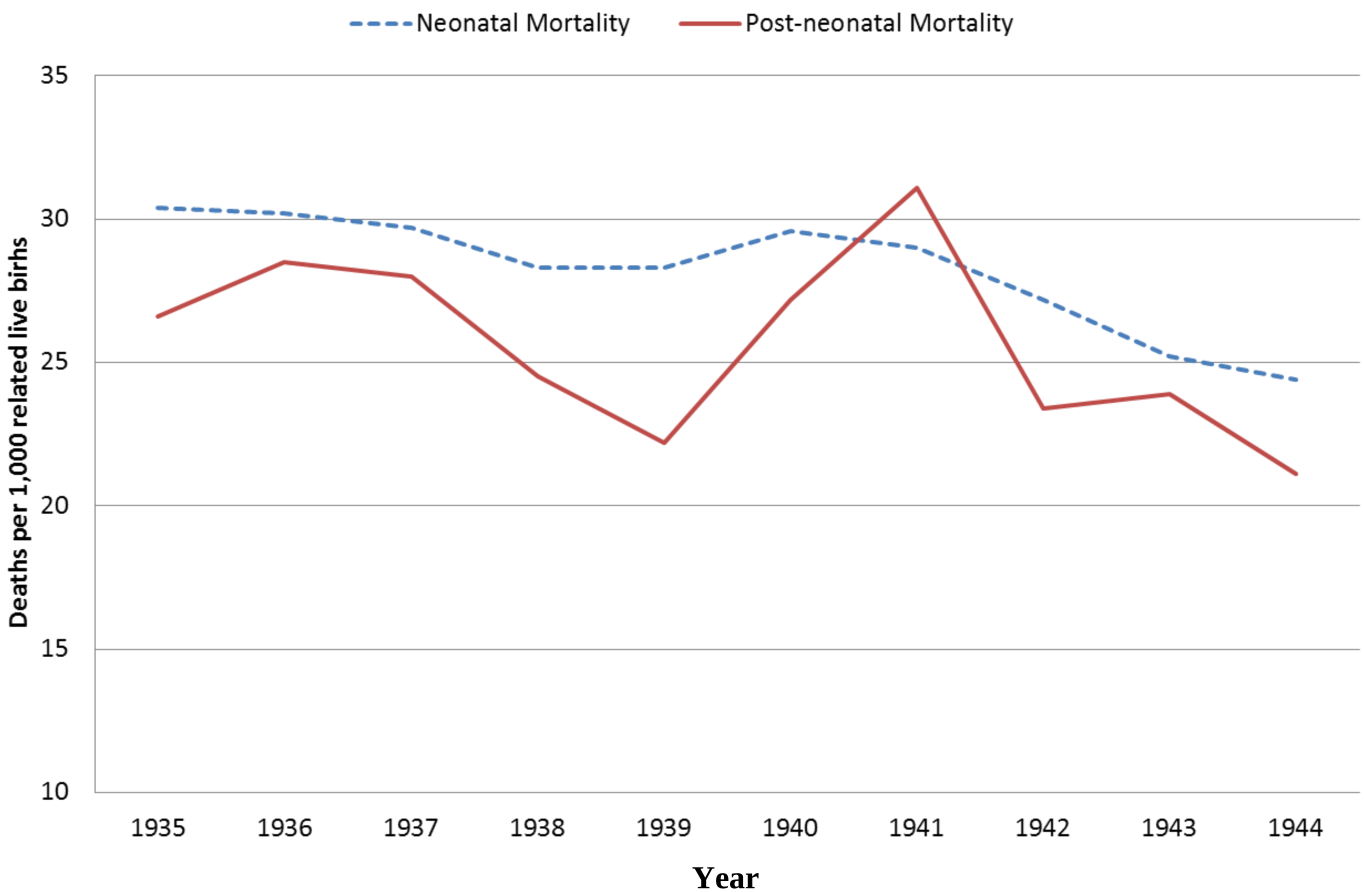


Fig. 2 Infant Mortality by Age, Deaths per 1,000 related live births, England and Wales

Source: Authors calculations based on data from On the State of the Public Health During the Six Years of War

Notes: This figure plots neonatal (0 to 1 month) and post-neonatal mortality (1 to 12 months) mortality in England and Wales. The figure illustrates that the aggregate spike observed in Figure 1 was driven largely by an increase in post-neonatal mortality.

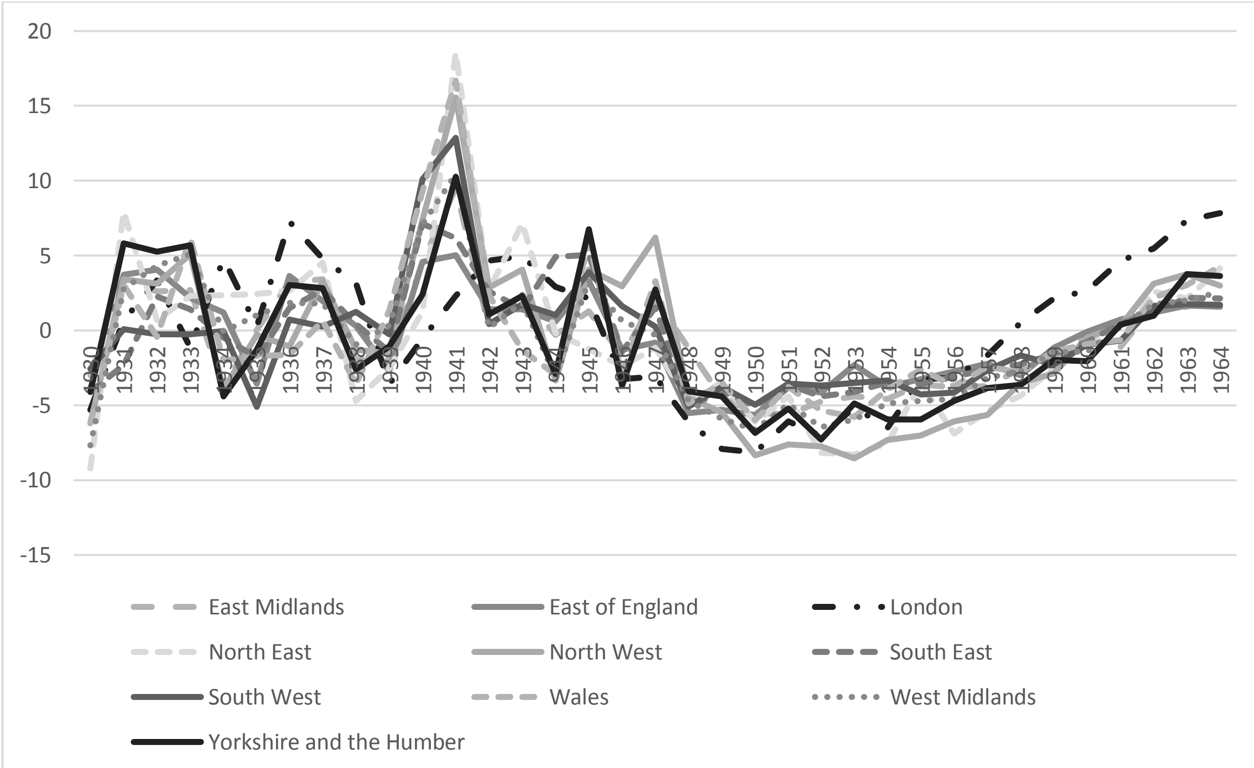


Fig. 3 Deviations in Infant Mortality (Per 1,000 Births) from within-Region Trend – 1930 to 1964

Source: Authors calculations from table "mort_lgd" in the database of "Birth & Death Statistics for Local Government Districts from 1921-1974."

Notes: This figure plots deviations from a region-specific linear trend in infant mortality from 1930 to 1964. It illustrates that there is substantial variation within the country in the extent to which infant mortality increased.

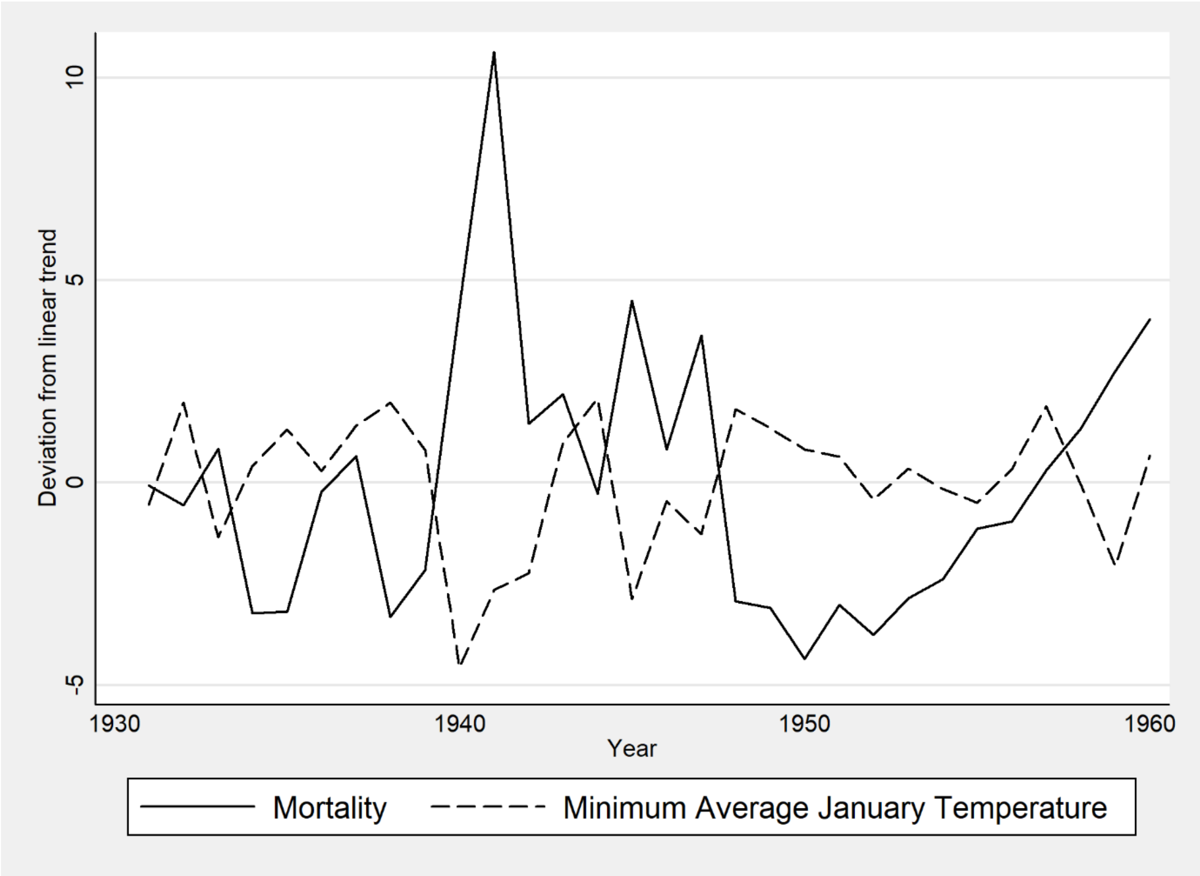


Fig. 4 Deviations in Infant Mortality (Per 1,000 Births) and Minimum January Temperatures in England – 1931 to 1960

Source: Authors calculations from table "mort_lgd" in the database of "Birth & Death Statistics for Local Government Districts from 1921-1974" and data from the MET office on average minimum temperatures available at http://www.metoffice.gov.uk/public/weather/climate-historic/#?tab=climateHistoric (last accessed 7 August 2017).
Notes: This figure plots deviations from a country linear trend in infant mortality and temperature from 1931 to 1960..

Table 1
Causes of infant mortality by year

	Infant mortality by cause of death (measured as infant deaths per 1,000 live births)										
	1939	1940	Change 1940-1939	Share of Total Change 1940-1939	1940	1941	Change 1941-1940	Share of Total Change 1941-1940	1942	Change 1942-1941	Share of Total Change 1942-1941
Bronchitis and pneumonia	8.9	12.7	3.8	60.8	12.7	13.7	1.0	31.4	9.2	-4.5	47.6
Whooping Cough	1.1	0.6	-0.5	-7.9	0.6	2.1	1.5	46.6	0.8	-1.3	13.7
Measles	0.1	0.4	0.3	4.8	0.4	0.6	0.2	6.1	0.2	-0.4	4.2
Tuberculosis diseases	0.5	0.6	0.1	1.6	0.6	0.7	0.1	3.1	0.5	-0.2	2.1
Convulsions	1.2	1.2	0.0	0.0	1.2	1.3	0.1	3.0	1.0	-0.3	3.1
Enteritis and diarrhea	4.3	4.4	0.1	1.4	4.4	4.8	0.4	12.2	5.2	0.4	4.0
Congenital malformations	6.1	6.6	0.5	7.7	6.6	6.6	0.0	0.0	6.5	-0.1	1.4
Premature birth	14.9	14.5	-0.4	-7.1	14.5	14.7	0.2	8.9	13.7	-1.0	10.8
Injury at birth	2.7	2.7	0.0	0.0	2.7	2.6	-0.1	-3.3	2.6	0.0	0.0
Asphyxia, atelectasis	2.1	2.3	0.2	3.3	2.3	2.2	-0.1	-2.8	2.0	-0.2	2.1
Congenital debility	1.7	2.1	0.4	6.5	2.1	2.1	0.0	0.0	1.6	-0.5	5.3
Hemolytic disease	0.5	0.5	0.0	0.0	0.5	0.5	0.0	0.0	0.6	0.1	1.0
Other causes	6.5	8.3	1.8	28.9	8.3	8.2	-0.1	-5.1	6.8	-1.4	14.5
Total	50.6	56.9	6.3	100	56.9	60.1	3.2	100	50.6	-9.5	100

Notes. We present cause-specific infant mortality by year. For instance, in 1939 bronchitis and pneumonia deaths were the cause of 8.9 infant deaths per every 1,000 live births. Between 1939 and 1940, bronchitis and pneumonia deaths rose to 12.7 infant deaths per every 1,000 live births, a change of 3.8. Between 1939 and 1940, the total change in infant deaths per live births was 6.3. As a share of the total change, the rise in bronchitis and pneumonia deaths explain 60.8 percent.

Table 2
Descriptive Statistics

	Mean	Std. Dev.	N
Infant Mortality per 1,000 Live Births	45.76	11.78	35604
Birth Rate per 1,000 total population	17.30	2.26	35604
Good or Excellent Self-Reported Health	0.68	0.47	35538
Poor or Very Poor Health	0.12	0.32	35538
Has Recent Inpatient Visits	0.09	0.29	35597
Has Reported Health Problems	0.69	0.46	35460
Has Arm/Leg/Hand Problem	0.38	0.49	35466
Has Chest/Breathing Problem	0.13	0.34	35466
Has Heart/BP Problem	0.23	0.42	35466
Disabled	0.10	0.29	35539
Has a Job	0.56	0.50	35597
Real Annual Labor Income	10,616.62	15,214.03	34540
Owns Home	0.83	0.38	35050
Female	0.54	0.50	35604
Age	55.99	6.78	35604
Birth Year	1943.4	4.45	35604
Birth Month	6.52	3.42	35604

Notes. We present average sample characteristics for our sample analysis sample.

Table 3
Relationship between Infant Mortality and Adult Health and Socioeconomic Status

	(1)	(2)	(3)	(4)
Very Good/Excellent Health	0.0191	0.0213	0.0189	0.0140
(mean = 0.68)	(0.0296)	(0.0287)	(0.0289)	(0.0334)
Poor/Very Poor Health	0.0013	0.0004	0.0034	0.0072
(mean = 0.12)	(0.0169)	(0.0164)	(0.0166)	(0.0202)
Has Recent IP Visits	0.0002	0.0005	0.0009	0.0134
(mean = 0.09)	(0.0103)	(0.0102)	(0.0102)	(0.0125)
Has Reported Health Problems	0.0059	0.0031	0.0026	0.0189
(mean = 0.69)	(0.0267)	(0.0269)	(0.0269)	(0.0318)
Has Arm/Leg/Hand Problem	0.0261	0.0225	0.0268	0.0359
(mean = 0.38)	(0.0332)	(0.0329)	(0.0337)	(0.0394)
Has Chest/Breathing Problem	0.0163	0.0122	0.0133	0.0032
(mean = 0.13)	(0.0212)	(0.0210)	(0.0211)	(0.0265)
Has Heart/BP Problem	-0.0030	-0.0050	-0.0062	-0.0103
(mean = 0.23)	(0.0259)	(0.0260)	(0.0258)	(0.0329)
Disabled	0.0350*	0.0361*	0.0390**	0.0436*
(mean = 0.10)	(0.0183)	(0.0185)	(0.0187)	(0.0237)
Has a Job	-0.1028***	-0.0900***	-0.0934***	-0.0983***
(mean = 0.56)	(0.0284)	(0.0264)	(0.0266)	(0.0310)
Real Annual Labor Income	-2485.57***	-2001.32***	-2181.88***	-2832.12***
(mean = 10,616.48)	(884.62)	(758.78)	(775.50)	(872.86)
Owns Home	-0.0214	-0.0212	-0.0242	-0.0005
(mean = 0.83)	(0.0282)	(0.0285)	(0.0277)	(0.0362)
Additional Controls	N	Y	Y	Y
Birth Rate	N	N	Y	Y
Region-specific linear trends	N	N	N	Y

Notes. Each estimate comes from a separate regression. The key independent variable, infant mortality, is standardized to be mean 0 and standard deviation 1. All regressions control for region, year, and month of birth. Additional controls include dummies for current age, sex, and current region of residence. In column 3 we add a control for the birth rate which varies by region and cohort of birth. In column 4 we add controls for birth-region linear time trends. For real annual labor income, we report the marginal effect following estimation using a GLM procedure. Standard errors clustered by region-by-year of birth are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 4
Relationship between Infant Mortality and Adult Health and Socioeconomic Status By Childhood Socioeconomic Status

	(1) Low SES	(2) High SES	(3) P-value for test of equality
Very Good/Excellent Health	0.0182	0.0439	0.1298
(mean = 0.68)	(0.0289)	(0.0336)	
Poor/Very Poor Health	0.0046	-0.0057	0.3454
(mean = 0.12)	(0.0168)	(0.0197)	
Has Recent IP Visits	0.0011	-0.0003	0.8058
(mean = 0.09)	(0.0103)	(0.0109)	
Has Reported Health Problems	0.0027	0.0113	0.6286
(mean = 0.69)	(0.0271)	(0.0342)	
Has Arm/Leg/Hand Problem	0.0274	0.0294	0.9033
(mean = 0.38)	(0.0342)	(0.0379)	
Has Chest/Breathing Problem	0.0140	-0.0010	0.2249
(mean = 0.13)	(0.0212)	(0.0247)	
Has Heart/BP Problem	-0.0063	0.0022	0.6184
(mean = 0.23)	(0.0261)	(0.0312)	
Disabled	0.0390**	0.0353*	0.6436
(mean = 0.10)	(0.0188)	(0.0204)	
Has a Job	-0.0982***	-0.0576*	0.0144**
(mean = 0.56)	(0.0268)	(0.0295)	
Real Annual Labor Income	-2197.7***	-2632.7***	0.4483
(mean = 10,616.62)	(771.6)	(968.2)	
Owns Home	-0.0276	-0.0005	0.0777*
(mean = 0.83)	(0.0276)	(0.0328)	

Notes. Each row represents a separate regression. We interact standardized infant mortality with indicators for if your father worked in a lower or higher SES occupation when you were 14. We include the main effect of "High SES Occupation." The main effect of standardized infant mortality is subsumed by the interactions. All regressions control for region, year, and month of birth. Additional controls include dummies for current age, sex, and current region of residence, and birth rates which vary by region and cohort of birth. For real annual labor income, we report the marginal effect following estimation using a GLM procedure. Standard errors clustered by region-by-year of birth are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 5
Relationship between Infant Mortality and Adult Health and Socioeconomic Status By Age

	(1) Ages 40-53	(2) Ages 54-59	(3) Ages 60-73
Very Good/Excellent Health	0.0432	0.0227	0.0156
(mean = 0.68)	(0.0306)	(0.0294)	(0.0289)
Poor/Very Poor Health	-0.0170	0.0098	0.0012
(mean = 0.12)	(0.0188)	(0.0179)	(0.0163)
Has Recent IP Visits	-0.0037	-0.0007	0.0020
(mean = 0.09)	(0.0113)	(0.0104)	(0.0108)
Has Reported Health Problems	-0.0022	0.0066	0.0008
(mean = 0.69)	(0.0292)	(0.0267)	(0.0276)
Has Arm/Leg/Hand Problem	0.0083	0.0285	0.0269
(mean = 0.38)	(0.0353)	(0.0344)	(0.0337)
Has Chest/Breathing Problem	-0.0076	0.0084	0.0169
(mean = 0.13)	(0.0236)	(0.0221)	(0.0207)
Has Heart/BP Problem	-0.0159	-0.0087	-0.0044
(mean = 0.23)	(0.0272)	(0.0261)	(0.0267)
Disabled	0.0217	0.0271	0.0461**
(mean = 0.10)	(0.0195)	(0.0189)	(0.0189)
Has a Job	-0.0475	-0.0870***	-0.0994***
(mean = 0.56)	(0.0289)	(0.0288)	(0.0252)
Real Annual Labor Income	-1458.64*	-2240.97***	-2190.99***
(mean = 10,616.62)	(866.05)	(837.96)	(769.42)
Owns Home	-0.0098	-0.0209	-0.0268
(mean = 0.83)	(0.0306)	(0.0285)	(0.0277)

Notes. Each row represents a separate regression. We interact standardized infant mortality with terciles of age. All regressions control for region, year, and month of birth. Additional controls include dummies for current age, sex, and current region of residence, and birth rates which vary by region and cohort of birth. For real annual labor income, we report the marginal effect following estimation using a GLM procedure. Standard errors clustered by region-by-year of birth are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 6
Randomization Inference

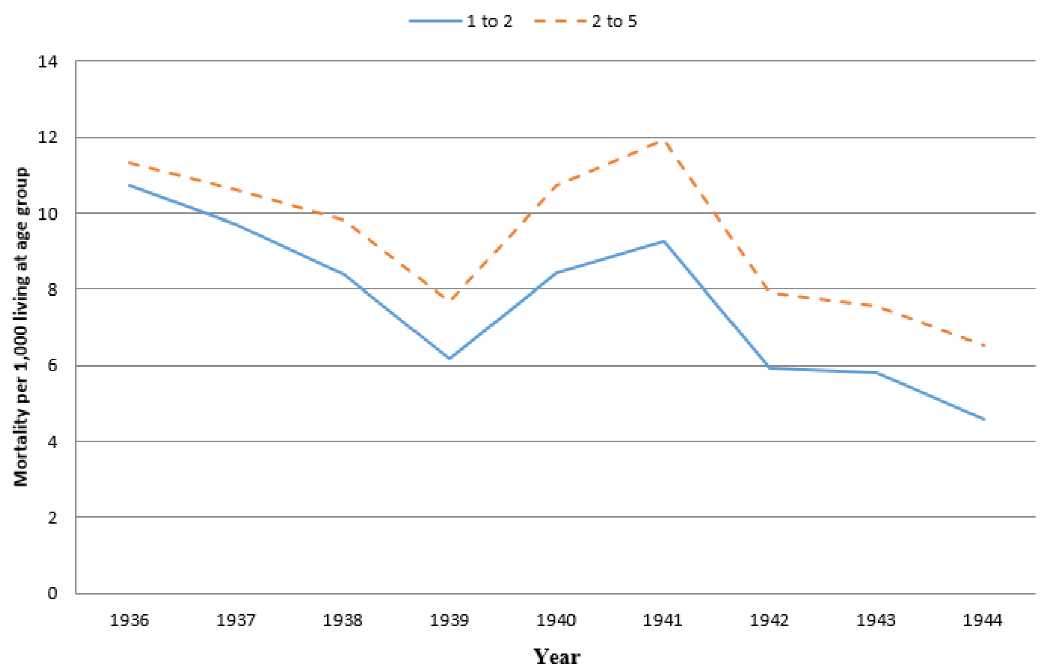
	Original Estimate and Standard Error	Randomization p-values	
		Randomly reassign birth regions	Randomly reassign birth cohorts
Very Good/Excellent Health	0.0189	0.566	0.572
(mean = 0.68)	(0.0289)		
Poor/Very Poor Health	0.0034	0.895	0.878
(mean = 0.12)	(0.0166)		
Has Recent IP Visits	0.0009	0.976	0.943
(mean = 0.09)	(0.0102)		
Has Reported Health Problems	0.0026	0.925	0.931
(mean = 0.69)	(0.0269)		
Has Arm/Leg/Hand Problem	0.0268	0.428	0.429
(mean = 0.38)	(0.0337)		
Has Chest/Breathing Problem	0.0133	0.591	0.546
(mean = 0.13)	(0.0211)		
Has Heart/BP Problem	-0.0062	0.788	0.830
(mean = 0.23)	(0.0258)		
Disabled	0.0390**	0.059*	0.047**
(mean = 0.10)	(0.0187)		
Has a Job	-0.0934***	0.037**	0.002***
(mean = 0.56)	(0.0266)		
Real Annual Labor Income	-2181.88***	0.161	0.040**
(mean = 10,616.62)	(775.50)		
Owns Home	-0.0242	0.426	0.404
(mean = 0.83)	(0.0277)		

Notes. Each estimate comes from a separate regression. For comparison, Column 1 reports the estimates from Column 3 of Table 3. The key independent variable, infant mortality, is standardized to be mean 0 and standard deviation 1. All regressions control for region, year, and month of birth, dummies for current age, sex, and current region of residence, as well as birth rate which varies by region and cohort of birth. Randomization p-values are computed as c/n where c = # times (|Placebo Estimate| >= |Actual Estimate|) and n = number of replications. In each randomization test, n=1000. 1st randomization: randomly reassigns birth regions, so everyone in the same birth region (across all birth cohorts) is assigned a randomly selected birth region. Observations are assigned the infant mortality in the random region in their birth year and subsequent year and then we re-weight infant mortality based on birth month as described in the text. 2nd randomization: randomly reassigns birth cohort, so everyone in the same birth year (across all regions) is assigned a randomly selected birth year. * p<0.10, ** p<0.05, *** p<0.01

Table 7
Sensitivity Analyses

	(1) Original Estimates	(2) Cohorts 1940 to 1950	(3) Cohorts 1935 to 1945	(4) Cohorts 1935 to 1955	(5) Random Wave per person	(6) Drop London
Very Good/Excellent Health	0.0189	0.0271	0.0301	-0.0031	-0.0233	0.0072
(mean = 0.68)	(0.0289)	(0.0305)	(0.0305)	(0.0237)	(0.0407)	(0.0322)
Poor/Very Poor Health	0.0034	0.0096	-0.0130	0.0106	0.0337	0.0097
(mean = 0.12)	(0.0166)	(0.0181)	(0.0188)	(0.0141)	(0.0254)	(0.0190)
Has Recent IP Visits	0.0009	0.0050	-0.0020	0.0053	0.0137	0.0039
(mean = 0.09)	(0.0102)	(0.0111)	(0.0101)	(0.0091)	(0.0222)	(0.0119)
Has Reported Health Problems	0.0026	0.0146	0.0081	0.0054	0.0318	0.0062
(mean = 0.69)	(0.0269)	(0.0301)	(0.0276)	(0.0225)	(0.0303)	(0.0312)
Has Arm/Leg/Hand Problem	0.0268	0.0484	0.0242	0.0319	0.0254	0.0507
(mean = 0.38)	(0.0337)	(0.0391)	(0.0350)	(0.0274)	(0.0376)	(0.0372)
Has Chest/Breathing Problem	0.0133	0.0046	-0.0022	0.0334*	0.0281	0.0115
(mean = 0.13)	(0.0211)	(0.0221)	(0.0231)	(0.0186)	(0.0251)	(0.0236)
Has Heart/BP Problem	-0.0062	0.0139	-0.0049	-0.0009	-0.0082	-0.0029
(mean = 0.23)	(0.0258)	(0.0284)	(0.0289)	(0.0229)	(0.0341)	(0.0285)
Disabled	0.0390**	0.0379*	0.0305	0.0467***	0.0677***	0.0374*
(mean = 0.10)	(0.0187)	(0.0214)	(0.0222)	(0.0150)	(0.0222)	(0.0220)
Has a Job	-0.0934***	-0.0743**	-0.1147***	-0.0729***	-0.1231***	-0.1063***
(mean = 0.56)	(0.0266)	(0.0308)	(0.0272)	(0.0242)	(0.0394)	(0.0296)
Real Annual Labor Income	-2181.88***	-2599.82***	-2140.34***	-1640.25*	-3130.63***	-2571.91***
(mean = 10,616.62)	(775.50)	(952.01)	(655.77)	(844.33)	(931.998)	(879.003)
Owns Home	-0.0242	-0.0245	0.0068	-0.0181	-0.0680**	-0.0224
(mean = 0.83)	(0.0277)	(0.0308)	(0.0347)	(0.0244)	(0.0284)	(0.0329)

Notes. Each estimate comes from a separate regression. For comparison, Column 1 reports the estimates from Column 3 of Table 3. The means of each dependent variable correspond to the means in Table 3 for the birth cohorts between 1935 and 1950. The key independent variable, infant mortality, is standardized to be mean 0 and standard deviation 1. In columns 2-4 we vary the included birth cohorts. In column 5 we randomly select one wave per individual. In column 6, we exclude individuals born in London. Standard errors clustered by region-by-year of birth are in parentheses. * p<0.10, ** p<0.05, *** p<0.01



Appendix Fig. 1 Child Mortality for those Age 1 to 5

Source: Authors calculation from On the State of the Public Health During the Six Years of War, pp 17
Notes: This figure plots child mortality by age which illustrates that mortality also increased for children age 1 to 5

Year

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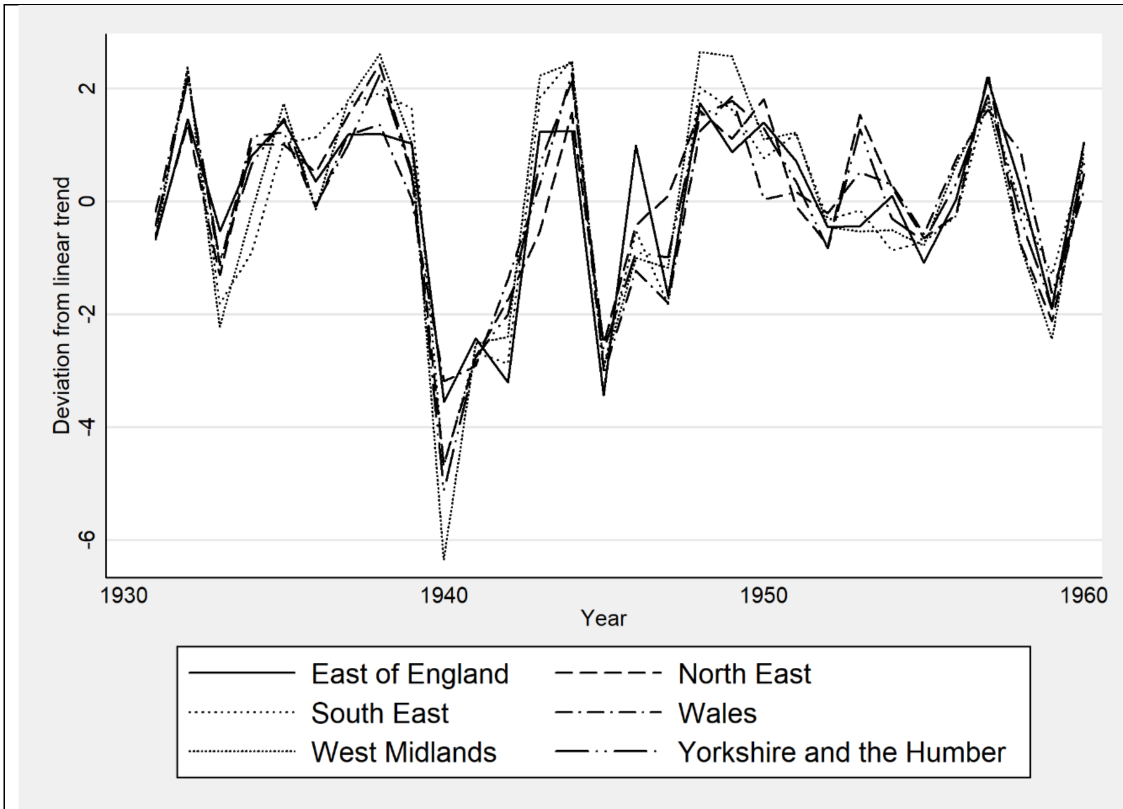
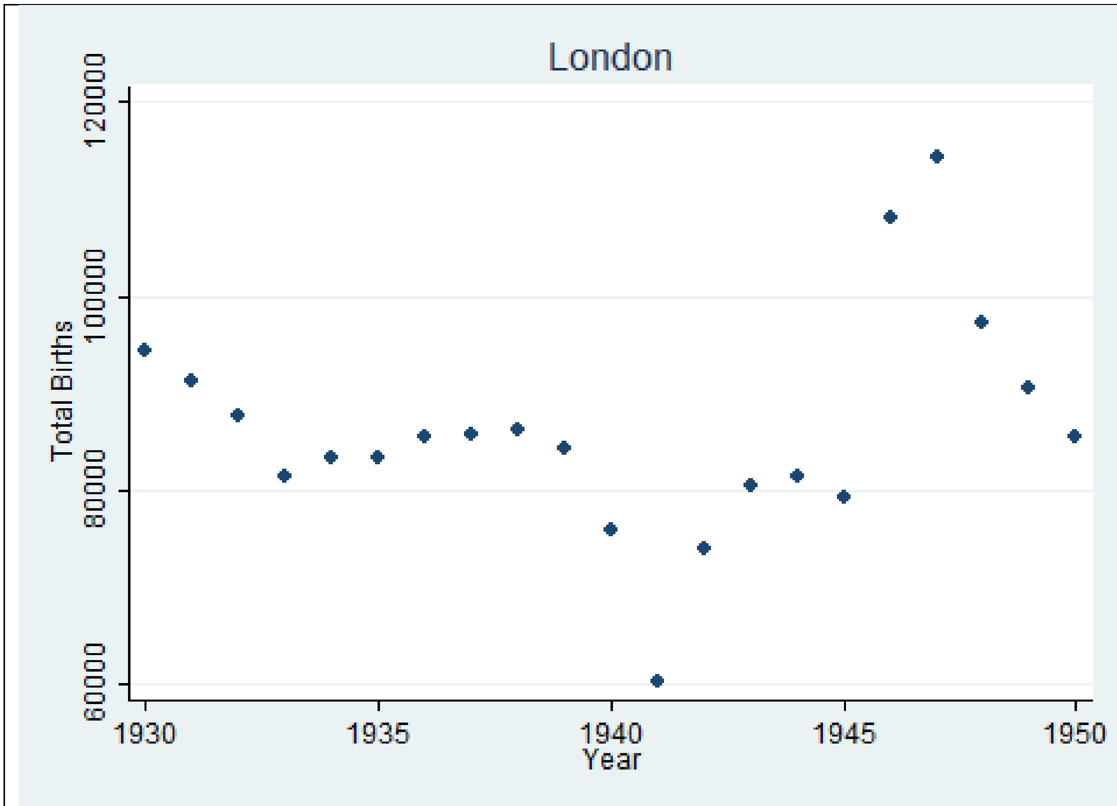


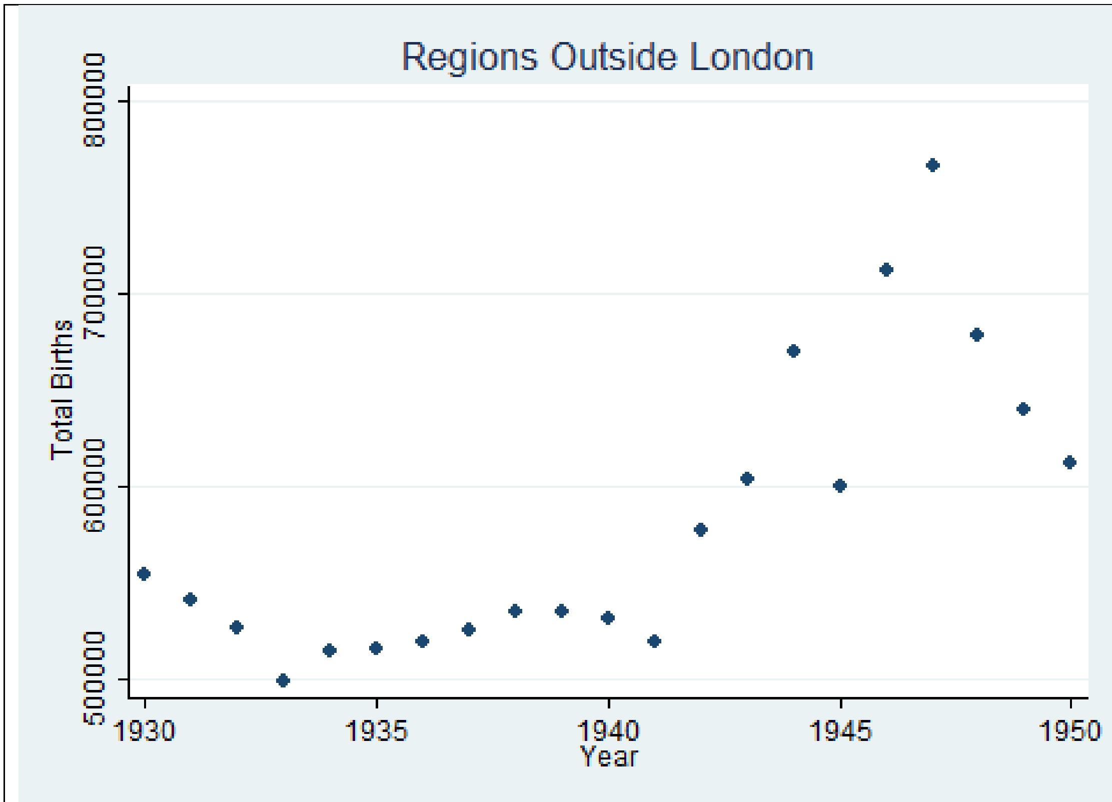
Appendix Fig. 2 Deviations in Minimum January Temperatures from within-Region Trend – 1931 to 1960

Notes: This figure deviations from a region-specific specific linear time trend in minimum average January temperatures. The data were obtained from the MET office at http://www.metoffice.gov.uk/public/weather/climate-historic/#?tab=climateHistoric (last accessed 7 August 2017).



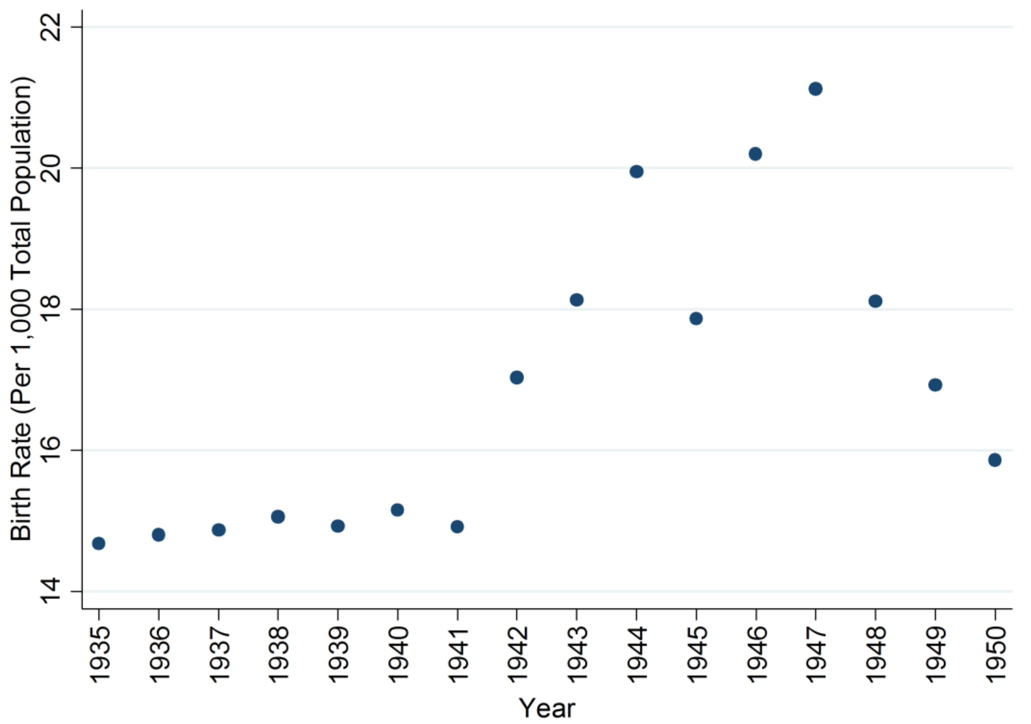
Appendix Fig. 3 Total births recorded in London, 1930-1950

Source: Authors calculations from table “mort_lgd” in the database of “Birth & Death Statistics for Local Government Districts from 1921-1974.”
Notes: This figure plots total births in London by year.



Appendix Fig. 4 Total births outside London, 1930-1950

Source: Authors calculations from table "mort_lgd" in the database of "Birth & Death Statistics for Local Government Districts from 1921-1974."
Notes: This figure plots total births in regions of England outside of London.



Appendix Fig. 5 Birth Rates, UK, 1935-1950

Source: Authors calculations from table "mort_lgd" in the database of "Birth & Death Statistics for Local Government Districts from 1921-1974."
Notes: This figure plots birth rates in England by year.