Effects of prenatal and early life malnutrition: Evidence from the Greek famine

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ABSTRACT

This paper examines the long run education and labor market effects from early-life exposure to the Greek 1941–1942 famine. Given the short duration of the famine, we can separately identify the famine effects for cohorts exposed in utero, during infancy and at 1 year of age. We find that adverse outcomes due to the famine are largest for infants. Further, in our regression analysis we exploit the fact that the famine was more severe in urban than in rural areas. Consistent with our prediction, we find that urban-born cohorts show larger negative impacts on educational outcomes than rural-born cohorts.

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1. Introduction

Several hundred million children, most of which grow up in developing or middle-income countries, suffer from hunger (Victoria et al., 2008). Recent research suggests that such nutritional deprivation in childhood may have long-term effects not only on health status but also educational and labor market performance. The adverse effects appear to be strongest for individuals that are exposed to undernourishment in utero or during the first 2 years of life (Bryce et al., 2008). Grantham-McGregor et al. (2007) argue that because of hunger, more than 200 million of today's children will fail to reach their full development potential in the future.

Despite the recent growth in interest in the long-term consequences of early life undernourishment, causal evidence on the relationship remains scarce (Rasmussen, 2001; Walker et al., 2007). Ignoring that exposure to early life undernourishment is often not random can lead to biased estimates of the effects of malnourishment on health, education, and other outcomes.

To address this shortcoming, scholars have begun to examine the consequences of nation-wide famines which can be viewed as quasi experiments. The 1959–1961 Chinese famine is the most prominent case (Luo et al., 2006; Chen and Zhou, 2007; Almond et al., 2007; Gørgens et al., 2007; Meng and Qian, 2009). While the Chinese famine studies are an important contribution to the literature, the 3-year duration of the Chinese famine makes it difficult to distinguish between the consequences of exposure in the second as opposed to the first year of life or in utero.

For instance, quantifications of the effect of famine exposure in the first year of life for the Chinese 1961 birth cohort are complicated by the fact that this cohort not only experienced the famine during the first year of life but was already exposed in utero. Further, the long duration and severity of the Chinese famine had substantial impacts on both mortality and reproductive behavior which gives rise to concerns of selection bias (Song, 2009). For example, concern regarding selection arises if high mortality among individuals in the treatment group changes the characteristics of the survivors in the treatment group relative to the individuals in the comparison group which does not experience comparable mortality (Gørgens et al., 2007; Song, 2009).
In this paper we study the long-term effects of early life exposure to the Greek famine which started in the Fall of 1941 and lasted until early 1942.\(^1\)\(^2\) Examining the Greek famine offers several advantages. Relative to the Chinese famine, selection issues are less severe because the Greek famine was rather short in duration (6–8 months) and thus permitted for lesser selectivity in reproductive behavior (Song, 2009). Therefore, in comparison to other famines, the Greek cohorts with famine exposure are more similar to the Greek cohorts without famine exposure. Moreover, the short duration of the famine allows us to distinguish the effects of undernourishment at specific ages, that is, during the second year of life, the first year of life, and in utero. In the following we refer to those exposed in the second year of life as 1-year-olds, to those exposed in the first year of life as infants, and to those exposed in utero as fetuses. Another advantage of studying the effects of the Greek famine on long-term outcomes is that the Greek famine did not coincide with epidemics of infectious disease for which effects on long-run outcomes might work quite differently than for undernourishment (Hionidou, 2006).

We find that undernourishment of 1-year-olds, infants and fetuses impairs the development of human capital in the long run. Being exposed to famine at young age lowers the likelihood of being literate, the likelihood of upper secondary schooling and the number of years of education. For most of our specifications, the effects are largest for the cohorts exposed as infants followed by the cohort exposed as 1-year-olds. The differences, however, are in most cases not statistically significant. Relative to these two cohorts we find only small effects for the cohort exposed as fetuses.

In the next section, we review earlier work on the nexus between early life health shocks and later-life health and socioeconomic outcomes. In Section 3 we provide a description of the Greek 1941–1942 famine. We describe our empirical strategy in Section 4. Section 5 contains results and Section 6 concludes.

2. Early life undernourishment and later life outcomes

Barker’s fetal origins hypothesis suggests a causal relationship between health in utero and later in life (e.g. Barker, 1998). Empirical tests of this hypothesis have typically used birth weight as a proxy for in utero development. The results from these tests show a strong negative association between birth weight and the risk of chronic disease in adults, especially after the primary reproductive age. Scholars suspect that in order to increase the chance of survival in the face of fetal undernourishment, blood and nutrients are diverted to the brain rather than other vital organs. The development of these organs then would either be impaired or permanently adapted to the nutritional deprivation, creating so called thrifty phenotypes (Hales and Barker, 1992; Barker and Hanson, 2004). Under improved nutritional conditions later in life, thrifty phenotype individuals would face a higher chance of metabolic disorders such as type-II diabetes (Hales, 1997).\(^3\)

In utero malnutrition impairs cardiac health (Hoet and Hanson, 1999) and kidney functioning (Brenner and Chartow, 1994). Further, Walker et al. (2007) present evidence that low birth weight babies have inferior cognitive skills, problem solving abilities, developmental levels, and are less active, happy, cooperative, and more inhibited. Studies by Case et al. (2002) and Almond (2006) show that the negative long-run effects of impaired fetal development do not stop at health, but translate into lesser educational attainment, employment opportunities and income. Twin studies that control for unobserved factors like genetic endowments as possible confounders find support for the negative correlation of birth weight and educational and labor market performance (Behrman and Rosenzweig, 2004; Almond et al., 2005; Oreopoulos et al., 2008; Black et al., 2007).\(^4\)

Individuals that experience severe undernourishment in their early childhood as opposed to in utero also experience negative long-term health outcomes. In their review of medical evidence from Asian, African, and South American developing countries, Walker et al. (2007) and Victoria et al. (2008) show that, similar to undernourished fetuses, undernourishment of children in the first 24 months of life is associated with reduced adult height, higher blood glucose concentrations, increased blood pressure, harmful lipid profiles, deficits in cognitive skills and an increased chance of mental illness. Just as for malnourished fetuses, the negative effects from exposure to severe undernourishment at early childhood translate into disadvantageous socioeconomic outcomes, such as lower educational attainment and reduced labor income. The translation of early-life nutritional deprivation into poor educational and socioeconomic outcomes can occur both directly and indirectly (Case and Paxson, 2006): The direct pathway is by impairments of cognitive ability through early-life malnutrition that harms school success and, subsequently, labor market outcomes. On an indirect pathway, early life malnutrition translates into poor child health, that reduces both school attendance and attainment. This in turn will worsen adult socioeconomic outcomes.

To address the possibility that early life nutritional levels and genetic endowment are simultaneously determined, scholars have recently begun to examine famines. These studies have produced mixed evidence when investigating the long-run effects of in utero exposure to malnutrition for cohorts born shortly after or during episodes of severe nutritional deprivation.

St. Clair et al. (2005) find that in utero exposure to the Chinese famine of 1959–1961 is associated with higher levels of adult schizophrenia. For the same famine Luo et al. (2006) find that in utero exposure is associated with increased rates of obesity among women, and Meng and Qian (2009) suggest that in utero exposure results in negative effects on height, but has no impact on coronary or metabolic conditions. Almond et al. (2007) provide evidence that Chinese men exposed to the famine in utero are less likely to be literate, to work, and to be married than surrounding cohorts.

Stanner et al. (1997) and Stanner and Yudkin (2001) examine the consequences of the German siege of Leningrad (1941–1944) which led to severe starvation of the city’s inhabitants. They find that in utero exposure had no effect on metabolic or cardiac conditions later in life. Finally, the results by Kannisto et al. (1997) suggest no impact on longevity among cohorts conceived during or shortly before the Finnish famine of 1866–1868 that killed up to 8% of the country’s population. One possible explanation for the lack of findings in these studies is that because of the length and severity of the Leningrad and Finnish famines, the absence of health effects among individuals with in utero exposure is caused by posi-

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1 The Greek famine has received little attention in the famine and other academic literatures. Further, there is “a complete absence of reference to the food crisis of the occupation years in the collective memory of the Greek population, no collective of even official memory of the famine, let alone collective trauma such as that relating to the Irish famine” (Hionidou, 2006, pp. 30–31).

2 Valaoras’ (1946) analysis of short-term somatometric impacts (height, weight, weight-for-height) for a small sample of Athens-born famine exposed children forms the exception to an absence of quantitative studies.

3 According to Stocker et al. (2005) the relative lack of metabolic disorders in African countries is caused by the fact that poor in utero nutrition is typically followed by nutritional scarcity after birth.

4 The evidence for the fetal origins hypothesis in epidemiological studies is, however, not unambiguous. Maccini and Yang’s (2009) results, for example, indicate that variations in rainfall in the year prior to birth do not affect adult health, education and socioeconomic outcomes among Indonesians born 1953–1974.
tive selection into fertility and survival during and after nutritional crises. If survivors exposed early in life have better genetic endowments than surrounding cohorts, this would bias towards zero any estimates of negative famine effects.

The scope for positive selection was smaller in the Dutch famine of 1944–1945 in which nutritional crisis was caused by a 6-month Nazi blockade of the western Netherlands. In a series of papers, Roseboom et al. (1954, 2000a,b) present evidence that in utero famine exposure is associated with impairments of the central nervous system, worse self-reported health, and coronary heart disease. Neugebauer et al. (1999) find a higher prevalence of antisocial personality disorder during adulthood, and the results by Ravelli et al. (1998, 1999) propose increased glucose resistance later in life and higher rates of obesity in cohorts exposed during the fetal stage. However, the unique dataset that was collected to investigate the long-run health effects of prenatal exposure to the Dutch famine does not contain socioeconomic outcome measures like educational attainment or labor market performance.

In contrast to the fetal origins hypothesis, the number of famine studies that investigate the long-run effects of postnatal malnutrition is limited. For example, Meng and Qian (2009), Chen and Zhou (2007) and Gørgens et al. (2007) find negative effects for individuals with early childhood exposure to the Chinese famine for height, weight, weight-for-height, education and labor supply, income and housing space. Alderman et al. (2006) use a panel dataset from Zimbabwe and find that early life exposure to drought and civil war lowers adult height and the number of grades of schooling completed. Maccini and Yang (2009) show that female Indonesians with lower adult height and the number of grades of schooling compared to those born in 1957–1967 are healthier, taller, better educated, and are wealthier adults than peers with less rainfall in infancy.

3. The Greek 1941/1942 famine

On April 30th 1941, only 24 days after Nazi Germany had joined the invasion of Greece by Italian forces, open warfare ended with the Greeks’ unconditional surrender and the country’s occupation by German, Italian, and Bulgarian troops. The Allied forces responded with a full naval blockade, cutting off all imports to Greece, including foods.

Immediately following victory, the occupying forces divided the country into 13 zones between which any movement of goods and people was strictly prohibited. Also within the zones, the confiscation of fuels and all means of transportation including fishing boats and pack animals reduced mobility to a minimum. The occupiers seized strategic industries, and appropriated or bought all stocks of commodities like tobacco, olive oil, cotton, and leather and transferred them to their home countries.

The occupying forces ordered a newly installed central government to reorganize the food supply to the Greek civil population. Farmers had to pay a 10% in kind tax on their produce and sell to the government at fixed prices all production above the government-determined subsistence level. Moreover, the food price controls and rationing that had been in place before the Greek defeat were now tightened. With the low government prices and newly imposed taxes, farmers went to great lengths to hide their produce from the officials and traders pulled their merchandise from the shelves. Additionally, the naval blockade and warfare in surrounding countries severed the foreign trade routes on which Greece traditionally depended for food imports.

The nutritional situation became critical in the summer of 1941 and in the fall turned into a famine. In the Greater Athens area, the calorific value of rations and food provided by public or charity soup kitchens deteriorated from 600 calories per day per person in July of 1941 to 320 in November of 1941. In many places, civil registration records were discontinued during the occupation (Valoras, 1960). Where they were not, the data suggest mortality increases between 300 and 1000% in the winter of 1941–1942 compared to pre-war years. Estimates of a country-wide death toll of the famine vary between 100,000 and 200,000 (Hionidou, 2006) or 1.4 and 2.8% of the Greek population, the large majority of which occurred between October 1941 and March 1942 (Helger, 1949).

Not all parts of Greece experienced equal levels of food scarcity. While comprehensive data on regional famine severity does not exist, the available evidence indicates that with the severe movement restrictions, proximity to agricultural production and the level of urbanization became crucial determinants of famine mortality. Hionidou (2006) estimates that mortality increases in urban areas were on average twice as large as in the countryside. Hence, while certain isolated islands and mountain villages also suffered high losses during the famine, the urban populations took the brunt of the death toll.

Because of the efforts of the Greek diasporas in the US and Britain, the situation of the Greek civilian population soon became widely known in the Allied countries, and increasing public pressure led to the lifting of the naval blockade in February 1942. Wheat shipments soon began and together with the rising temperatures of springtime, this brought down mortality rates. The international relief focused mainly on children. In Athens in February 1942, the Red Cross started to provide daily milk rations, medical services and clothing to children younger than 2 years. From March 1942 onwards, pregnant women and breastfeeding mothers received extra supplies which were further increased for women that were temporarily unable to breastfeed because of undernourishment.

Also in March 1942, the occupiers and Allied forces agreed to the establishment of the Swedish-run Joint Relief Commission to reorganize the public food supply system. The occupiers moreover committed to replace all appropriated agricultural produce with food imports of equal calorific value and relaxed the harshest mobility restrictions and price regulations. As a result, fresh produce from the June harvest and foods that had hitherto been hoarded entered the markets and food shipments further grew in volume. Towards the end of 1942, the nutritional situation had returned to acceptable levels in most parts of the country (Hionidou, 2006).

4. Data and methods

Our data come from the Greek National Population Housing Census from the IPUMS website (Minnesota Population Center, 2009). These are individual level data from the 1971, 1981, 1991, 2001...
and 2001 Greek census waves. Each wave represents a 10% sample of the Greek population. In our regressions, we include the 11 cohorts born between 1936 and 1946. The 1971 census sample therefore includes individuals between 25 and 36 years, the 1981 sample of individuals between 35 and 46 years, the 1991 sample of individuals between 45 and 56 years and the 2001 sample of individuals between 55 and 66 years.

As described above, the medical literature suggests that early life undernourishment has particularly strong long-run effects when experienced in the first 24 months of life or in utero. Therefore, our treatment group consists of the 1940 birth cohort of which the majority was 1 year old when the famine struck, the 1941 birth cohort the majority of which experienced the famine during infancy and the 1942 birth cohort where the majority experienced the famine as fetuses. In comparison, the cohorts born before 1940 had a much lower chance of being exposed to severe malnutrition in the first 24 months of life or in utero. Because the harshest phase of the famine was over in early 1942, the cohorts born in 1943 or later were not affected by the famine.

The 1940, 1941 and 1942 cohorts were between 29 and 31 years of age in the 1971 sample, between 39 and 41 in the 1981 sample, between 49 and 51 in the 1991 sample and between 59 and 61 in the 2001 sample.

We use a regression discontinuity design which exploits the discontinuous change in treatment probability to produce estimates of the mean treatment effects on the cohort level. Using individual data from the Greek census’ 1936–1946 birth cohorts we estimate the model

\[ y_{it} = \text{cons} + \beta_1 1940 + \beta_2 1941 + \beta_3 1942 + \text{sex}_i + yob_1 + yob_2^2 + \epsilon_i. \]  

(1)

The dependent variable \( y_{it} \) represents a set of educational outcomes and, for the working subsample, a measure of socioeconomic status for person \( i \). 1940, 1941 and 1942 are indicators that equal 1 if the individual is born in the respective year and 0 otherwise. The variable \( \text{sex}_i \) is a gender dummy, and \( yob \) denotes the year of birth thereby controlling for linear trends in the outcome variables \( y \). Moreover, to account for non-linearities in the outcome trends we include the squared year of birth \( yob^2 \). (Almond and Mazumder, 2005)

In this model the coefficients \( \beta_1, \beta_2 \) and \( \beta_3 \) represent the departures of the 1940–1942 birth cohorts from a secular outcome trend over the 1936–1946 cohorts. Because the famine lasted only between 6 and 8 months, \( \beta_1, \beta_2 \) and \( \beta_3 \) can be interpreted as the mean effects of undernourishment experienced. In our specification, \( \beta_1 \) represents the departure in outcomes for the 1940 birth cohort exposed to the famine at age 1. Further, \( \beta_2 \) is the departure in outcomes for those born in 1941, that is, those exposed during infancy, and \( \beta_3 \) is the departure in outcomes for those born in 1942 and thus exposed as fetuses.

One of our dependent variables is an indicator whether an individual is literate. Another dependent variable is whether an individual has completed upper secondary or technical school. We compute this variable using the information in the Greek census on the highest level of education achieved. A third dependent variable is total years of education and we compute this variable using the regular time required to gain the reported highest educational attainment. To measure the regular time we use an approximation that is based on the Greek educational system (European Commission, 2009).

While the census does not have data on income or wages, it has data that allow us to compute an occupation’s socioeconomic status score which we use as one of our dependent variables. We derive this score from the detailed information on occupation in the Greek census. The census contains more than 400 occupational categories which are in many cases equal to those of the 4-digit ISCO88 scheme. We mapped these categories into socioeconomic status scores. The scores are based on Ganzeboom et al.’s (1992) International Socio-Economic Index of Occupational Status (ISEI). The index uses educational requirements and income to assign each occupation a score that lies between 16 for occupations with the lowest and 90 for occupations with the highest socioeconomic status. Our conversion follows that by Ganzeboom and Treiman (1996). For the occupational categories that did not precisely match the 4-digit ISCO88 codes we applied the ISEI conversions for the more aggregated 3-digit or 2-digit ISCO88 categories. Estimates from Eq. (1) are unbiased if the error term \( \epsilon \) is uncorrelated with the independent variables, including the 1940–1946 birth cohort dummies. This requires that the unobserved characteristics captured in \( \epsilon \) are equally distributed across the 1936–1946 birth cohorts, that is, that selection into the treatment group is random with regards to \( \epsilon \). If this is the case, outcome changes that remain after controlling for secular trends are attributable to the discontinuous changes in treatment probability. However, non-random positive selection into treatment may have occurred through selective mortality and fertility during the famine.

With respect to famine mortality, we suspect that individuals with adverse genetic endowments had a lesser chance of survival if born 1940–1942 than in our control groups. Our control groups consist of the 1936–1939 cohorts that suffered lower mortality than our treatment cohorts (Valaoras, 1946) and the 1943–1946 cohorts that had no direct exposure to the famine at all. If a culling of the weakest did in fact occur and if genetic endowment is correlated

\begin{itemize}
  \item[12] Because the Greek census data only convey an individual’s year of birth and not the precise birth date we cannot disentangle the effects of exposure at different gestational ages and of different length of exposure.
  \item[13] The method to determine literacy status is not the same across the 1971–2001 census waves. While the 1971 census obtained literacy status through direct questioning, the 1981–2001 waves do not contain such a question but instead consider individuals literate if they ever attended school.
  \item[14] Until 1975, compulsory schooling in Greece comprised six years of primary education. Optional higher schooling tracks included three years of lower secondary education (gymnasium) and three years of upper secondary schooling (lyceum).
  \item[15] The ordinal educational attainment variable breaks educational attainment down into more detailed categories in the later census waves. As a result, the estimation results for the years of education variable in Table 2 that we construct from this ordinal variable are not necessarily comparable across the four census waves.
  \item[16] The STATA-code we used to compute these educational outcome measures are available on request.
\end{itemize}
with educational and labor market performance, our estimates \( \beta_1 \), \( \beta_2 \) and \( \beta_3 \) will understate the true effect of early-life famine exposure. The same logic applies to social background as the famine claimed most of its victims among the poor (Hionidou, 2006). In light of these selection issues our estimates identify a lower bound. Positive selection is likely largest for the 1942 cohort. Unlike for the other cohorts, parts of the 1942 cohort were conceived during the famine. Hence, positive selection may not only have occurred through selective mortality but also through selective fertility. For instance, Valaoras (1946) estimates that during the famine up to 70% of females in the Greater Athens area stopped menstruating because of severe undernourishment (famine amenorrhoea). Further, lost births were more frequent among the poor and this reduced the share of individuals with poor parents. If having poor parents is associated with inferior outcomes later in life, the selective fertility reductions give the 1942 cohort better outcomes than the comparison groups. This would in turn cause downward bias in our estimates.18

However, the possible bias in our basic specification (1) may not be unambiguously downward. If, for instance, famine mortality was low in places with poor educational and labor market opportunities, survivors from these places may be overrepresented in the treated 1940–1942 cohorts. In this case, the place of birth is an important control variable since it is correlated with famine intensity and survivorship. Specifications that do not account for birthplaces would therefore have an upward bias. Because the 2001 census includes detailed birthplace information our data allow us to address this selection issue for the 2001 census wave.

The scope for selection is larger and thus comparability between treatment and control groups more limited in more severe and extended famines. The degree of bias from sample selection upon unobserved determinants of educational and labor market performance for our sample is likely lower than for some other famines. For example, while the Greek famine lasted only 6–8 months and claimed 1.4–2.8% of the population (Hionidou, 2006) the siege of Leningrad lasted almost 3 years and killed as much as one third of the city’s inhabitants (Stanner et al., 1997).

5. Results

5.1. Descriptive statistics (Table 1)

Table 1 presents descriptive statistics for Greek citizens in the 1971, 1981, 1991, and 2001 Greek censuses. For each of the censuses, it provides variable means for the 1940–1942 cohorts and for the surrounding 1939 and 1943 cohorts. The table shows that there are no large differences in literacy rates between the 1940–1942 birth years and the adjacent cohorts. However, for upper secondary or technical school education, the 1940–1942 cohorts have a 2% points lower completion rate across all census waves. Further, individuals born 1940–1942 on average have between 1.4 and 2.4 months less education. With respect to labor market outcomes, Table 1 shows that individuals from the 1940–1942 cohorts have jobs with lower socioeconomic status than in the other cohorts.

Moreover, Table 1 shows that across all census waves the share of females is higher among the treated than among the non-treated cohorts, with the difference ranging between 0.2 and 1.8% points. The differences reflect the higher likelihood of male children to succumb to famine that is reported for both the Greek and other famine episodes (Helger, 1949; Jakobovits, 1991). Finally, the last row in Table 1 shows that the share of urban born individuals is 2% points lower in the early-life exposed cohorts than in the surrounding cohorts. This is consistent with Helger’s (1949) finding that the famine was more severe in urban than in rural areas.

The main sample for our empirical analyses comes from the 1936–1946 Greek-born birth cohorts in the 2001 Census. For each of these cohorts, Figs. 1 and 2 show the share of higher secondary school graduates, and the average years of education, respectively. The figures also include a linear time trend line. There is an upward trend over time in both measures of educational attainment. Educational outcomes for the 1940–1942 birth cohorts with famine exposure at critical stages of development, however, diverge down-

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18 An alternative hypothesis regarding the direction of bias in our estimates of the 1942 cohort effect is that if couples with more advantageous characteristics for the development of their children adapted more quickly to the new nutritional situation than couples with less advantageous characteristics, the latter group would be overrepresented among the 1942 cohort’s parents. This would result in an upward rather than a downward bias in our estimates. However, evidence by Valaoras (1946) indicates that the fertility reductions in most cases came through hunger-caused physical impairment to conceive rather than deliberate family planning considerations. As food deprivation was more prevalent among the disadvantaged, an upward bias in the 1942 is unlikely.

Table 2

<table>
<thead>
<tr>
<th>Cohort (exposed in)</th>
<th>(1) 1971 census</th>
<th>(2) 1981 census</th>
<th>(3) 1991 census</th>
<th>(4) 2001 census</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient SE</td>
<td>Coefficient SE</td>
<td>Coefficient SE</td>
<td>Coefficient SE</td>
</tr>
<tr>
<td>Literate</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1940 (1-year-old)</td>
<td>−.002 [.022]</td>
<td>−.005*** [.028]</td>
<td>−.005** [.026]</td>
<td>.000 [.024]</td>
</tr>
<tr>
<td>1941 (infant)</td>
<td>−.004* [.025]</td>
<td>−.011*** [.031]</td>
<td>−.007** [.029]</td>
<td>−.007*** [.027]</td>
</tr>
<tr>
<td>1942 (fetus)</td>
<td>.001 [.022]</td>
<td>−.003 [.027]</td>
<td>−.001 [.026]</td>
<td>.002 [.023]</td>
</tr>
<tr>
<td>N</td>
<td>115,368</td>
<td>130,321</td>
<td>129,372</td>
<td>126,214</td>
</tr>
<tr>
<td>Upper secondary education</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1940 (1-year-old)</td>
<td>−.016*** [.045]</td>
<td>−.013*** [.043]</td>
<td>−.013*** [.039]</td>
<td>−.018*** [.042]</td>
</tr>
<tr>
<td>1941 (infant)</td>
<td>−.022*** [.051]</td>
<td>−.024*** [.050]</td>
<td>−.018*** [.047]</td>
<td>−.023*** [.049]</td>
</tr>
<tr>
<td>1942 (fetus)</td>
<td>−.026*** [.050]</td>
<td>−.023*** [.049]</td>
<td>−.021*** [.047]</td>
<td>−.023*** [.048]</td>
</tr>
<tr>
<td>N</td>
<td>117,638</td>
<td>130,321</td>
<td>129,372</td>
<td>126,214</td>
</tr>
<tr>
<td>Years in education</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1940 (1-year-old)</td>
<td>−.147*** [.037]</td>
<td>−.177*** [.048]</td>
<td>−.159*** [.045]</td>
<td>−.148*** [.043]</td>
</tr>
<tr>
<td>1941 (infant)</td>
<td>−.215*** [.041]</td>
<td>−.293*** [.055]</td>
<td>−.173*** [.053]</td>
<td>−.235*** [.050]</td>
</tr>
<tr>
<td>N</td>
<td>117,638</td>
<td>130,321</td>
<td>129,372</td>
<td>126,214</td>
</tr>
<tr>
<td>ISEI-score</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1940 (1-year-old)</td>
<td>−.303** [.215]</td>
<td>−.700*** [.211]</td>
<td>−.296 [.229]</td>
<td>−.456 [.332]</td>
</tr>
<tr>
<td>1941 (infant)</td>
<td>−.521** [.242]</td>
<td>−.533** [.245]</td>
<td>−.235 [.263]</td>
<td>−.839** [.348]</td>
</tr>
<tr>
<td>1942 (fetus)</td>
<td>−.478** [.235]</td>
<td>−.583** [.241]</td>
<td>−.279 [.257]</td>
<td>−.111*** [.324]</td>
</tr>
<tr>
<td>N</td>
<td>69,998</td>
<td>80,993</td>
<td>73,852</td>
<td>43,177</td>
</tr>
</tbody>
</table>

The results in Table 2 are for the subsamples of individuals in the respective census waves that are Greek citizens. In addition to the indicators for whether born in 1940, 1941, or 1942, all specifications include year of birth, year of birth squared and a sex indicator. Huber–White robust standard errors are reported in brackets. The table reports only point estimates and standard errors for the 1940, 1941 and 1942 indicators.

5.2. Basic regressions (Table 2)

Table 2 shows the coefficient estimates for our basic model in Eq. (1). We estimate separate regressions for each of the four 1971–2001 census waves.

For the 1940 cohort, that was exposed to the famine as 1-year-olds, we find a half a percentage point decline in literacy in the 1981 and 1991 censuses. For this cohort, the reductions in the chance to complete upper secondary or technical school range between 1.3% points in the 1981 and 1991 censuses and 1.8% points in 2001. Years of education are also lesser for 1-year-olds and the reductions are between 1.2 and 1.4 months in all four censuses. Finally, the ISEI score is 0.5 points, or 1.5%, below the trend for 1-year-olds in the 1971 census and 0.7 points, or 1.9%, in the 1981 census. The ISEI reductions in the 1991 and 2001 are not statistically significant.

The 1941 birth cohort exposed during infancy appears to be the most affected by the famine. It is the only cohort that shows significant reductions in the share of literate individuals over all census waves, ranging from 0.4% points in 1971 to 1.1% points in 1981. The negative effect on the likelihood to complete upper secondary or technical school is statistically significant and ranges between 1.8% points in 1991 and 2.4% points in 1981. A similar picture emerges for years of education where being born in 1941 causes a reduction between 1.4 and 2.4 months in 1991 and 1981 respectively. We also find reductions in the ISEI score that are statistically significant in the 1971, 1981 and 2001 census waves where they range between 0.5 and 0.8 points, or between 1.4 and 2.5% of the sample average, respectively.

In contrast to 1-year-olds and infants, the 1942 cohort treated as fetuses does not display significant reductions in literacy. Instead, in two of the four censuses, the point estimates have positive signs.

Fig. 1. Percent of 2001 census sample of 1936–1946 Greece-born individuals with upper secondary schooling and linear trend.

Fig. 2. Average years of education in 2001 census sample of 1936–1946 Greece-born individuals and linear trend.

19. Over time comparisons of the coefficient estimates for the years-of-education-variable need to take into account that the educational attainment variable that underlies our years-of-education-variable becomes more detailed with each census wave. However, when we use a harmonized years-of-education variable instead, we observe only minimal difference on the estimates’ size. Results for this harmonized years-of-education-variable are available from the authors on request.

20. Because the 2001 census used a different methodology for occupation coding, the magnitudes of the ISEI-score effect are not fully comparable between the 1971–1991 and the 2001 waves.

21. The doubling of the negative impact between the 1971 and 1981 census waves may in part be caused by the methodological differences in creating the literacy variable that we discuss in footnote 13.

22. This finding contrasts with evidence from the Chinese Famine: Almond et al. (2007) find that the male (female) cohorts with the most famine exposure during the fetal stage are 9 (7) percent more likely to be illiterate than the surrounding cohorts.
Reduced with the proximity to agricultural production (Helger, 1949; Hionidou, 2006). This anecdotal evidence is supported in our hypothesis that early life exposure to famine worsens long-term outcomes. No comprehensive data on regional famine intensity exists but contemporary reports indicate that the intensity of birth dummies to the specification in column 1. Relative to the specification in column 2, column 3 adds an urban birthplace indicator and interactions of the 53 prefecture of birth dummies with this urban birthplace indicator. For column 1, Huber–White robust standard errors are reported in brackets. For columns 2 and 3 we report robust standard errors clustered at the prefecture of birth level in brackets.26

One concern regarding the estimates in Table 2 is that they might be biased because of a correlation between regional differences in famine intensity and unmeasured or unobservable characteristics that determine educational and labor market outcomes. No comprehensive data on regional famine intensity exists but contemporary reports indicate that the intensity reduced with the proximity to agricultural production (Helger, 1949; Hionidou, 2006). This anecdotal evidence is supported in our data.24 We hence suspect that for children born between 1940 and 1942 the likelihood of survival was higher in rural areas and that rural-born individuals may therefore be overrepresented in these cohorts. Thus, if being born in rural areas correlates with inferior educational and labor market opportunities then the estimates in Table 2 contain an upward bias.25

### 5.3. Regressions with birthplace controls (Table 3)

It is not clear whether the downward bias that we suspect because of positive genetic and social selection into survival is larger than any upward bias due to rural-born individuals being overrepresented in the 1940–1942 cohorts. As a result, we cannot determine whether the coefficients in our basic model in Table 2 over- or underestimate the true famine effects. However, because the 2001 census contains detailed birthplace information we can control for birthplace and therefore net out the upward bias from positive birthplace selection in the 2001 sample. Table 3 presents results based on the 2001 data. In contrast to Table 2, the results in Table 3 are not for all Greek citizens but for the subsample that the 2001 census identifies as Greece-born. The first column of Table 3 replicates the basic specification (1) for the Greece-born sample. Column 2 adds 53 dummy variables for the prefecture of birth.26 By including the dummies we control for all determinants of education and labor market outcomes that are specific to the prefecture of birth and constant over time. In column 3 we add an indicator for whether a person was born in an urban area.

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24 When we regress the urban birthplace indicator on the explanatory variables in model (1) we find that the likelihood of being born urban is reduced by 0.9% points for the 1940 cohort, 2.2% points for the 1941 cohort and 4.4% points for the 1942 cohort. All reductions are statistically significant.

25 The correlation of birthplace and educational and labor market outcomes may for example be driven by structural differences in access to early life healthcare or parental education.

26 In 2005, the average prefecture had a population of about 200,000, ranging from the most populous Athens prefecture with almost 2,700,000 million people to 23,000 in the Lefkada prefecture. Our results are robust to the inclusion of 156 indicators that break the birthplace information down below the prefecture level.
and interaction terms between the urban birthplace and prefecture of birth dummies. Because of these interaction terms, this last specification not only controls for time-invariant prefecture and urban birthplace effects, but also for prefecture-specific consequences of being born in an urban area. For all models that include birthplace information we cluster standard errors at the prefecture level.

For the 1940 cohort of 1-year-olds, the famine effects for literacy are not statistically significant in column 1, nor in columns 2 and 3 which include birthplace indicators. The estimate of the famine’s impact on completing upper secondary or technical school drops from minus 1.8% points in the basic specification in column 1 to minus 1.2% points in columns 2 and 3. Similarly, the reduction in the number of years in education falls from 1.2 months to 0.8 about months after the inclusion of birthplace indicators. The estimated impact on socioeconomic status also falls in columns 2 and 3 and is no longer statistically significant.

For the 1941 cohort exposed during infancy we find a negative and statistically significant effect for literacy regardless of whether or not we include birthplace controls. The estimated effects on upper secondary schooling and the number of years in education are smaller in columns 2 and 3 than in column 1, and remain statistically significant. Like for 1-year-olds, the coefficients on socioeconomic status falls for infants when including birthplace indicators and the point estimates are no longer statistically significant.

In columns 2 and 3, the 1942 cohort has a lower chance of having completed upper secondary education and fewer years of education than those not exposed to the famine. However, the point estimates are not statistically significant. Further, in column 3 fetal exposure is associated with a statistically significant half percentage point increase in the likelihood to be literate. Unlike in the basic specification, the reductions in the ISEI score for the 1942 cohort are small and not statistically significant.

The reduction in the point estimates resulting from the inclusion of birthplace controls is consistent with the hypothesis that those born in urban areas and Column 3 does so for individuals born outside Greece. For columns 1, 2 and 3 we report robust standard errors clustered at the prefecture of birth level in brackets. For column 4, the robust standard errors reported in brackets are clustered at the country of birth level.

The reductions are largest for the 1942 cohort is consistent with the hypothesis that this cohort experienced additional negative birthplace selection through fertility. For this paragraph (and a corresponding paragraph in the conclusions) we wrongly assumed that the majority of the 1942 cohort was exposed in the later stages of gestations. However, since the period of severe malnutrition lasted at least six months and started in the fall of 1941, only those born January–March 1942 were exposed late in gestation. The rest was “treated” already in the first trimester. The paragraph’s conclusion that the absence of effects may be explained by a large share of the cohort only being exposed in less vulnerable stages of gestation is therefore wrong. We apologize for this mistake.

5.4. Regressions for urban, rural, and foreign born subsamples (Table 4)

Because famine severity differed between urban and rural areas it is likely that the long-run effects on the 1940–1942 cohorts are also different for urban and rural born cohorts. We account for this possibility by estimating separate models for the urban- and rural-born populations. By obtaining separate estimates of the 1940, 1941 and 1942 cohort effects for the urban and rural samples we can distinguish the long-run effects of severe famine exposure from those of milder famine exposure.

For comparison purposes, Table 4, column 1 replicates the results for the 2001 sample of individuals born in Greece that we show in column 2 of Table 3. Table 4, column 2 has estimates for the 2001 sample of urban-born Greeks, and column 3 presents estimates for the 2001 sample of rural-born Greeks.

Table 4, columns 2 and 3, shows that for 1-year-olds, the estimates for literacy do not differ between those in urban and rural areas. In contrast, the chance of completing upper secondary school is reduced by 2.1% points in the urban-born cohort as opposed to 0.8% points in the rural-born cohort. The reduction in years of education is 1.6 months for the urban-born sample while there is a small and statistically insignificant reduction for years of education for the rural-born sample. In terms of socioeconomic status we

| Table 4 | OLS estimates of departures from 1936 to 1946 cohort trend for different subsamples of the 2001 census. |
|-----------------|---------------------------------|-----------------|---------------------------------|---------------------------------|
| Cohort (exposed in) | (1) Prefecture FE Full Sample | (2) Prefecture FE Urban Sample | (3) Prefecture FE Rural Born Sample | (4) Country FE Foreigner Born Sample |
| Coefficient | SE | Coefficient | SE | Coefficient | SE | Coefficient | SE |
| Literate | | | | | | | |
| 1940 (1-year-old) | .000 | [.0019] | −.003 | [.0029] | .001 | [.0025] | −.006 | [.0115] |
| 1941 (infant) | −.007*** | [.0024] | −.007* | [.0040] | −.006** | [.0031] | −.016* | [.0069] |
| 1942 (fetus) | .004 | [.0024] | −.001 | [.0034] | .005** | [.0027] | −.018** | [.0105] |
| N | 123,793 | 40,053 | 83,740 | 5,811 |
| Upper secondary education | | | | | | | |
| 1940 (1-year-old) | −.012*** | [.0043] | −.021*** | [.0074] | −.008* | [.0042] | .021 | [.0208] |
| 1941 (infant) | −.016* | [.0065] | −.033*** | [.0109] | −.004 | [.0054] | .013 | [.0269] |
| 1942 (fetus) | −.006 | [.0075] | −.012 | [.0129] | .001 | [.0069] | −.010 | [.0258] |
| N | 123,793 | 40,053 | 83,740 | 5,811 |
| Years in education | | | | | | | |
| 1940 (1-year-old) | −.092** | [.0430] | −.188** | [.0779] | −.050 | [.0429] | .287 | [.2492] |
| 1941 (infant) | −.172*** | [.0653] | −.342*** | [.1006] | −.058 | [.0627] | .107 | [.1847] |
| 1942 (fetus) | −.017 | [.0639] | −.034 | [.0956] | .027 | [.0642] | .056 | [.3056] |
| N | 123,793 | 40,053 | 83,740 | 5,811 |
| ISEI-score | | | | | | | |
| 1940 (1-year-old) | −.282 | [.3289] | −.159 | [.5290] | −.177 | [.3339] | −.378 | [.1123] |
| 1941 (infant) | −.563 | [.5257] | −.262 | [.8925] | −.288 | [.5309] | −.807 | [.5900] |
| 1942 (fetus) | −.493* | [.2826] | −.044 | [.5338] | −.377 | [.3616] | .802 | [.1217] |
| N | 43,177 | 12,859 | 29,426 | 2,229 |

In all specifications we use data from the 2001 census that contains birthplace information. In addition to the indicators for whether born in 1940, 1941, or 1942, all specifications include year of birth, year of birth squared and a sex indicator. In addition, the specifications in columns 1–3 include prefecture of birth dummies and the specification in column 4 country of birth dummies. Column 1 reproduces column 2 of Table 3. Column 3 provides estimates for the same specification as in column 2 but for individuals born in urban areas and Column 3 does so for individuals born in rural areas, column 4 does for individuals born outside Greece. For columns 1, 2 and 3 we report robust standard errors clustered at the prefecture of birth level in brackets. For column 4, the robust standard errors reported in brackets are clustered at the country of birth level.
do not find statistically significant effects for urban- or rural-born individuals.

For infants, the small statistically significant reduction in the literacy rate for the full sample in column 1 is also present in the urban and rural samples in columns 2 and 3. Like for the 1940 cohort, the 3.3% point reduction in upper secondary schooling and the 2.9 months reduction in years in education for the urban-born cohort are multiple times larger than the statistically insignificant reductions for the rural-born cohort. Also, as in the majority of specifications, the adverse effects on upper secondary schooling and years in education are larger for infants than for 1-year-olds. We find no significant effect on socioeconomic status in the urban and rural subsamples.

For rural-born fetuses we find a 0.5% point statistically significant increase in the likelihood to be literate. The remaining estimates for the 1942 cohort are small in magnitude and not statistically significant. The direction of the coefficient on literacy in the 1942 rural-born cohort is puzzling if positive selection into fertility is stronger, and thus, any positive effect larger, in the urban sample than in the less-famine exposed rural sample.

Because members of the 1940–1942 foreign-born cohorts are unlikely to have experienced systematic undernourishment early in life, we predict no significant departure in outcomes from the cohort trend for this subsample. We test this hypothesis in Table 4, column 4 which shows results for Eq. (1) for the foreign-born individuals in the 2001 census.

We find significant reductions in literacy for those born in 1940 and 1941. We however do not find effects on upper secondary schooling, years in education and the SEI score in any of the 1940–1942 cohorts. The sum of the findings in column 4 is therefore consistent with our prediction in that foreign-born individuals were not systematically negatively affected by the famine. That is unless they immigrated to Greece in their first 2 years of life.27 Our results for Greece-born individuals in Tables 2, 3 and 4 are thus unlikely to be a mere statistical anomaly.

A concern with our identification strategy may be that there were events other than the 1941–1942 famine that affected socioeconomic outcomes exclusively for the 1940–1942 birth cohorts. A possible candidate for such an event could be the Greek civil war. While hostilities began as early as 1942, warfare was most intense between 1946 and 1949. The 1940–1942 birth cohorts with early-life famine exposure experienced this period of destabilization between age 4 and 9. For the cohorts in our control group, the 1936–1939 cohorts lived through the civil war between age 7 and 13, and the 1943–1946 cohorts between age 0 and 6. Assuming any disruptions in the educational system because of the civil war have more severe consequences on educational outcomes for children exposed at school-age than for children exposed earlier, any negative civil war cohort effects would be largest for the 1936–1939 cohorts. In our models, we, however, do not find that educational outcomes for the 1936–1939 cohorts diverge from the 1936–1946 outcome trends. Furthermore, our results for the 1940–1942 cohorts do not change. If, in an alternative specification, we limit the control group to individuals born 1930–1939. This result indicates that the negative divergences from trend in the 1940–1942 cohorts are not driven by better educational opportunities for the later-born cohorts that lived through the civil war at an older age.

Our results show that the famine had a negative effect on education, and this finding allows us to form a rough estimate of the famine’s long-run economic cost. To obtain this cost, we utilize the estimated effects on years of education from our basic model in Table 2 and assume that a work life in Greece extends from age 16 to 61. We find that the famine-related economic loss is about $1.4 billion (in 2005 US dollars), or 1.1% of Greece’s 2003 GDP, the year in which the 1942 cohort turned 61 years of age.28

6. Conclusions

This paper is the first to examine the long-run education and labor market effects from early-life exposure to the Greek 1941–1942 famine. In our baseline specification and across all four 1971, 1981, 1991 and 2001 census waves, we find statistically significant reductions in years in education and in the likelihood to complete upper secondary schooling for those exposed to the famine as fetuses, infants and 1-year-olds. In addition, reductions in socioeconomics status are statistically significant in the 1971, 1981 and 2001 censuses. The adverse long-run effects are typically larger for the cohorts exposed as infants and fetuses than for the cohort exposed as 1-year-olds.

We reduce any upward selection bias that is due to an overrepresentation of individuals born in areas with inferior educational opportunities in the 1940–1942 cohorts using the 2001 census, which contains detailed birthplace information. By controlling for birthplace we seek to identify a lower bound of the true famine effect. In comparison to the baseline model, birthplace controls lower the education effects for the 1-year-olds and infants by about one third but remain statistically significant. By contrast, the effects for cohorts exposed as fetuses are very small and not statistically significant after the inclusion of birthplace controls. Rather than providing evidence against the fetal origins hypothesis, this finding may be driven by positive selection in the 1942 cohort. Because the cohort was conceived in a situation of severe nutritional strain, positive selection in this cohort likely occurred not only through survival but in addition through fertility. That is, during the famine, fertility was lower for those at the lower distribution of income and skills than for those in the upper distribution.

Finally, contemporary sources suggest that the famine was more severe in urban than in rural areas. We exploit this cross-sectional variation by estimating separate models for the urban- and rural-born subsamples of the Greek 2001 census. In line with our prediction, the urban-born cohorts suffer larger negative impacts on educational outcomes than the rural-born cohorts. For 1-year-olds and infants born in urban areas the likelihood to complete upper secondary school reduces by 2.1 and 3.3% points, respectively. Years of educational are lower by 1.6 months for individuals exposed as 1-year-olds and by 2.9 months for those exposed during infancy.

We are now aware of earlier studies that link early childhood famine exposure to reductions in educational attainment (Maccini and Yang, 2009) and therefore would like to delete this statement from the paper. Our results highlight the role of appropriate nutri-

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27 We obtain similar results for estimations based on the subsample of Albanian-born individuals that form the largest single group of immigrants in the 2001 census. Results are available from the authors on request.
tion in the formation of human capital in particular in the first 2 years of life. They indicate that developmental lags in countries that frequently experience nutritional crisis may in part be due to lacking nutrition.

References