



# The long-term and intergenerational effects of early-life hunger experience on human capital and labor market outcomes

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## ABSTRACT

Using individual-level retrospective data on early-life hunger experience from China, we investigate the long-term effects of early nutritional deprivation on one's own and adult children's human capital and labor market outcomes. With an instrumental variable approach, we find that hunger experience lowers educational attainment and job quality of the first generation. But this negative impact on human capital does not transmit to the second generation, possibly due to parents' compensatory behavior of human capital investment and extra attention given to children. Our findings imply that the negative impact of non-extreme nutritional adversities on human capital and labor market outcomes are likely to decrease over generations.

## 1. Introduction

Early-life nutritional deprivation is found to be an important factor influencing human capital accumulation. The “Fetal Origins Hypothesis” (e.g., [Barker, 1990, 1997, 2007](#)) suggests that prenatal nutritional adversities, which change the metabolic system and increase the hazard of diseases, can hinder cognitive ability and educational outcomes.<sup>1</sup> Early-life food shortages can also change individual preferences and behaviors in human capital investment and further influence later-life outcomes (e.g., [Almond, Edlund, Li, Zhang, Lee & Ogawa, 2010](#), [Almond, Currie, & Duque, 2018](#), [Bertoni, 2015](#), [Chen & Zhou, 2007](#), [Cheng & Shi, 2019](#), [Cui, Smith, & Zhao, 2020](#), [Kesternich, Siflinger, Smith, & Winter, 2015](#), [Kim, Fleisher, & Sun, 2017](#), [Lay & Norling, 2020](#), [Meng & Qian, 2009](#), [Mu & Zhang, 2011](#), [Neelsen & Stratmann, 2011](#), [Shi, 2011](#), [Scholte, Van den Berg, & Lindeboom, 2015](#), [Van den Berg, Pinger, & Schoch, 2016](#), etc.).

Such effects of nutritional adversities could be intergenerational through biological and socioeconomic channels. Medical literature suggests that parents experiencing malnutrition in utero (e.g., [Drake & Walker, 2004](#); [Franklin et al., 2010](#)) or during the “slow growth period”<sup>2</sup> (e.g., [Pembrey, 2010](#); [Pembrey, 2006](#)) may have defects in metabolism and reproduction systems. These health adversities can be transmitted to their children and affect children's education and labor market performance. On the other hand, early-life experience of nutritional adversities may alter parents' socioeconomic status as well as their behaviors of nurturing children. These non-biological pathways can affect human capital and labor market outcomes of the next generation in ambiguous directions (e.g., [Kim](#)

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<sup>1</sup> See [Almond and Currie \(2011\)](#) for a comprehensive overview.

<sup>2</sup> Slow growth period refers to ages 8–10 for girls and 9–12 for boys. This is a sensitive period for the development of the reproduction system.

& Fleisher, 2010, Kim, Deng, Fleisher, & Li, 2014, Tan, Tan, & Zhang, 2014, Li & An, 2015, Van den Berg, & Pinger, 2016, Li & Menon, 2021, etc.).

The majority of studies on the long-term or intergenerational impact of nutritional adversities exploit variations in exposure across regions and cohorts (e.g., famine cohort, regional death rate) to extreme nutritional shocks, such as famines or wars (e.g., Chen & Zhou, 2007; Van den Berg, & Pinger, 2016; Jürges, 2013; Kim et al., 2014; Lay & Norling, 2020; Li & Menon, 2021; Scholte et al., 2015). Such aggregate-level exposure measures are limited in two aspects. First, it is difficult to extrapolate the findings on extreme nutritional shocks to settings with more general and non-extreme cases, such as evaluating an early-life nutritional intervention policy in non-famine time. In this sense, the difference-in-differences approach exploiting famine variations identifies an upper bound of the effect of nutritional adversities. Second, exposure measures estimate an intention-to-treat effect of extreme events rather than an average treatment effect of individual nutritional deprivation. There may be discrepancies between regional or cohort exposure measures to famines and actual individual hunger experience. The intention-to-treat effects of extreme events can be confounded by channels other than individual food shortages.

An emerging strand of literature investigates the long-term effect of nutritional deprivation by exploiting individual-level retrospective data on hunger experience (Bertoni, 2015; Kesternich et al., 2015). In the Chinese context, Cui et al. (2020) first exploits the self-reported hunger variable from the life history survey of the China Health and Retirement Longitudinal Study (CHARLS). They provide reliable evidence for negative long-term effects of hunger experience between ages 0 and 17 on physical health, mental health and cognitive ability in old age. Using the same retrospective information on hunger experience as Cui et al. (2020), our paper extends their study by investigating the long-term and intergenerational effects on educational outcomes and labor market outcomes.

We contribute to the literature in the following ways. First of all, we focus on the link of individual hunger experience to completed educational attainment and labor market outcomes, which is different from recent studies with retrospective data primarily focusing on later-life health (Cui et al., 2020; Van den Berg et al., 2016; Deng & Lindeboom, 2021). Our finding is that even with our more general measure than famine exposure, nutritional deprivation still has a significant negative impact on human capital and job quality, which echoes the findings of Cui et al. (2020).<sup>3</sup>

Second, we add to the relatively scarce literature on the intergenerational effects of parental hunger experience on children's human capital, and provide the first evidence using individual retrospective hunger information. Most studies on the intergenerational effects of nutritional adversities focus on children's health, while only a few study children's educational attainment and labor market outcomes. For instance, Kim et al. (2014) and Tan et al. (2014) study how parental exposure to the Great Chinese Famine affects young children's cognitive development and possibility of entering middle school. Lay and Norling (2020) study how parental exposure to severe famines influences children's human capital and labor market outcomes in adulthood. With the individual-level hunger measure, we find that the adverse effects of hunger on human capital and labor market outcomes attenuate over generations.

Third, we attempt to deal with the potential endogeneity in the self-reported hunger variable with an instrumental variable (IV) approach. Van den Berg et al., 2016 and Deng and Lindeboom (2021) instrument hunger experience by famine exposure dummies or famine intensity measures, whereas our IV is the aggregate hunger rate in the birth city within the 5-year birth cohort, which captures systematic factors influencing local food shortages in both famine time and non-famine time.<sup>4</sup> The main assumption is that the hunger rate of the reference group affects later-life individual outcomes only through individual hunger incidence. Our IV passes the over-identification test when we introduce famine intensity as a second IV. And our results are insensitive to city specific time-varying confounders and household-level confounders around birth years. With the IV approach, we can identify a local average treatment effect of early-life hunger experience on human capital and labor market outcomes of one's own and their children.

Lastly, we contribute to understanding the potential pathways of the attenuating hunger effect over two generations. The attenuating intergenerational effects on human capital can be related to parental compensatory behaviors of education investment (Almond et al., 2018; Almond et al., 2009) and extra attention to disadvantaged children, implying that compensatory behaviors in the nurturing process may help to bridge the education gap of children caused by parental nutritional deprivation.

To preview our results, early-life hunger experience reduces the first generation's own years of education and the probability of completing middle school or above, and increases the probability of being illiterate. It also lowers job quality, that is, the probability of working in the non-agricultural sector, government or public sector, and firms. For the second generation, the effect of parental hunger experience on human capital is not significantly different from zero. Moreover, heterogeneity analysis shows that the adverse hunger effect on the first generation's human capital is rather universal, though subgroups with higher educational potential (male and the high-SES group) and with severe hunger experience are slightly more affected. The negative impact of parental hunger does not transmit to the second generation for most groups, and the illiteracy rate is even reduced for the low-SES group. In addition, our results are robust to potential sample selection due to marriage, fertility, or mortality.

Our OLS estimates on the first generation's educational attainment are comparable to Cui et al. (2020) which also uses individual-level retrospective hunger measures. Our findings are smaller in size than most previous literature exploiting exposure to the Great Chinese Famine by the difference-in-differences approach, and our main estimates on the second generation are not even significantly different from zero. This is possibly because we investigate more general and non-extreme cases of food shortages instead of an extreme case such as the Great Famine.

<sup>3</sup> In the heterogeneity analysis of Cui et al. (2020), to explain the differential cognitive outcomes by gender, they look at the education attainment and find that early-life hunger is negatively associated with educational attainment, especially for women. We compare our results with theirs later in the paper.

<sup>4</sup> Throughout the paper, "city" refers to "prefecture-level city" in China.

Exploring the potential mechanisms, we find that parents with early hunger experience have lower health capital, possibly impeding human capital accumulation and labor market performance. Furthermore, parents with hunger experience tend to invest more in children's health and education. They also have more interactions with children, for example, receiving more monetary transfers and providing more care to grandchildren. Compensatory behaviors in human capital investment and extra attention to children might explain the attenuating effects of food shortages over generations.

The rest of the paper is organized as follows: Section 2 describes the data. Section 3 presents the identification strategy. Section 4 presents the main results, analyses of potential pathways and heterogeneous effects. Section 5 provides sensitivity analyses, and Section 6 concludes.

## 2. Data

### 2.1. Data source and sample restriction

We use data from "The China Health and Retirement Longitudinal Study" (CHARLS). It is a biennial longitudinal survey starting from 2011 on a representative sample aged 45 or older and their spouses in mainland China. It collects rich information on respondents' health, education, work, demographic characteristics, family information, and information about their children. See [Zhao, Strauss, Yang, and Giles \(2013\)](#) for a detailed introduction of the data and sampling. In addition to the biennial core survey, a special wave was conducted in 2014 to collect respondents' life history information, among which we draw hunger experience information and early life conditions. We use parents' and children's information from the third wave of the core survey in 2015, combined with life history data.

For the first-generation sample (the parents), we restrict our sample to those who were born between 1930 and 1970, which leaves us with a sample of 11,148 individuals from 6693 households.<sup>5</sup> We further use the information of their children to construct the sample for the second generation. We restrict our sample of the second generation to children alive and aged between 30 and 50. We focus on this age range for several reasons. First, children older than 30 and younger than 50 usually have completed education and have not yet reached retirement ages.<sup>6</sup> This age range allows us to make a sensible analysis of children's completed education and labor market performance. Second, we keep children born later than 1965 to avoid the direct impact of extreme nutritional shocks like the Great Chinese Famine (from 1959 to 1961) and other earlier historical events on children.<sup>7</sup> This sample restriction results in 11,348 children from 4765 households.

### 2.2. Variables

Our key explanatory variable *hunger experience* of the first generation is a dummy variable indicating whether an individual had experience of food shortages before age 17. *Parental hunger experience* of the second generation is a dummy variable indicating whether an individual's mother or father had experience of food shortages before age 17.

For outcome variables of the first generation, we consider educational outcomes such as *years of education*, *illiterate or not*, *completing middle school or above*, and the following labor market outcomes: *having a job in the non-agricultural sector*, *having a job in the government or public sector*, and *having a job in a firm*.<sup>8</sup> Similarly, for outcome variables of the second generation, we consider *years of education*, *illiterate or not*, *completing middle school or above*, *currently working or not*, *current annual household income* and *being partnered or not*.<sup>9</sup>

For mechanism analysis, we consider the following outcomes for the first generation: we check how hunger influences parents' health status, which can further influence their ability to learn and work, such as *self-reported health*, *being sick or not in the last month*, *cancer incidence* ("incidence" defined as a dummy for having a disease or not), *stomach disease incidence*, *liver disease incidence*, and objectively measured *incidence of being overweight* (BMI  $\geq$  23). We further consider parents' household expenditure and

<sup>5</sup> The majority of our sample was born between 1930 and 1966, as CHARLS surveys individuals aged 45 and above (and their spouses) in the 2011 baseline wave. Yet, 0.34% of the sample was born between 1967 and 1970. They can be a small number of newly surveyed respondents who were just above age 45 and were added to the sample in wave 2013 and 2015. Or they can be the main respondent's younger spouse aged between 41 and 45 in 2011.

<sup>6</sup> In urban China, the mandatory retirement ages are 50 for blue-collar female workers, 55 for white collar female workers, and 60 for male workers. In rural China, there is no mandatory retirement age. People tend to work until their health permits. See [Giles, Lei, Wang, Wang, and Zhao \(2021\)](#) for a detailed description of retirement system in China.

<sup>7</sup> See [Li and Yang \(2005\)](#) for a detailed introduction of the Great Chinese Famine.

<sup>8</sup> The dummy for "*having a job in the non-agricultural sector*" indicates whether one ever had non-agricultural employment, non-agricultural self-employment and in the army. The dummy for "*having a job in the government or public sector*" indicates whether one was ever employed by the government, public institutions and NGOs. Please refer to Online Appendix A for further details. We also have a dummy variable "*ever worked or not*" for the first generation, indicating whether one has ever worked during the lifetime. But since >99% of the first-generation sample has ever worked, hunger or not has no impact on this variable in any specification or subsample. The results are available upon request.

<sup>9</sup> Ideally, we would like to include individual income information. Due to the data limitation, we can only obtain the total wage range of the child and his/her spouse.

intergenerational interaction to help understand how the effects are passed onto the next generation, such as *food, health, and education expenditure* (we also look at the *incidence of spending any money on health, education*), and their *log shares relative to total household expenditure*,<sup>10</sup> the *incidence and intensity of taking care of grandchildren*, and the *incidence and amount of monetary transfer from and to children*. Online Appendix A gives a detailed description of how all these variables are defined.

For the first generation, we additionally control for their early-life socioeconomic status (SES) variables predetermined before hunger experience. For the second generation, we control for their maternal and paternal early-life SES. The definitions of these variables are listed in Table A.1 in Online Appendix A.

### 2.3. Descriptive statistics

Table 1 shows summary statistics of outcomes and demographic characteristics by (parental) hunger incidence. 75.4% of the first generation have hunger experience before age 17. Most of the first-generation individuals were born in rural areas with an average age of about 65. Their parents are mostly illiterate. Their own average years of education is <6 years. People with hunger experience tend to have fewer years of education. They are more likely to be illiterate and less likely to finish middle or higher education. In terms of labor market outcomes, they are less likely to have non-agricultural jobs, government or public sector jobs, or jobs in a firm.

The second-generation individuals, on average aged 39, are still mainly from rural areas. 91.1% of them have *either* parent having experienced hunger before 17.<sup>11</sup> These children still appear to have less education, but the magnitude of difference is rather small. For other outcomes, children with parental hunger experience are more likely to work and less likely to have a partner, and their household income is lower. Table B.1 in Online Appendix B further summarizes statistics for mechanism variables.

## 3. Set-up of the analysis

### 3.1. Regression Model

We investigate the effects of hunger experience on the human capital of the first generation and second generation. To estimate the long-run effects of the first generation, we employ the following model in Eq. (1):

$$Y_i = \alpha_1 + \beta_1 \text{Hunger}_i + \text{Family}_i' \theta_1 + \delta_1 \text{Male}_i + \text{Age}_i' \gamma_1 + \delta_c + \varepsilon_i \quad (1)$$

$\text{Hunger}_i$  refers to a dummy indicating whether a first-generation individual  $i$  experienced hunger during age 0–17.  $\beta_1$  is the parameter for the long-run effects of hunger experience.  $\text{Family}_i$  refers to a vector of individual  $i$ 's time-invariant family characteristics, including mother's education level, father's education level, Hukou status at birth and number of siblings at birth. In addition, we control gender  $\text{Male}_i$ , as well as  $\text{Age}_i$ , a vector for age and age squared of individual  $i$  in 2015.  $\delta_c$  refers to birth city fixed effects.  $Y_i$  refers to a number of outcomes related to human capital and labor market, including years of education, dummies for education level (illiterate/middle school and above), dummies for whether one has ever had a job in the non-agricultural sector, in the government or public sector, or in a firm.

$$Y_j = \alpha_2 + \beta_2 \text{Parental\_Hunger}_j + \text{Parental\_Family}_j' \theta_2 + \text{Parental\_Age}_j' \gamma_2 + \delta_1 \text{Male}_j + \text{Age}_j' \gamma_1 + \delta_{c,mom} + \varepsilon_j \quad (2)$$

In Eq. (2), we estimate the intergenerational effects of parental hunger experience. Now  $\text{Parental\_Hunger}_j$  is a dummy indicating whether a second-generation individual  $j$ 's mother or father experienced hunger during age 0–17.  $\beta_2$  is the parameter for the intergenerational effects of parental hunger incidence.  $\text{Parental\_Family}_j$  refers to a vector of time-invariant parental family characteristics, including the highest education level of  $j$ 's mother's parents, the highest education level of father's parents, mother's and father's Hukou status at (mother's or father's) birth, and mother's and father's number of siblings at (mother's or father's) birth. We also control a vector for mother's and father's age and age squared,  $\text{Parental\_Age}_j$ . In addition, we control  $j$ 's gender, a vector for  $j$ 's own age and age squared  $\text{Age}_j$ , and mother's birth city fixed effects  $\delta_{c,mom}$ .<sup>12</sup> As dependent variables of the second-generation analysis,  $Y_j$  are  $j$ 's outcomes reported by parents, including years of education, education level (illiterate/middle school and above), current working status, annual household income and a dummy for whether child  $j$  currently has a partner.

### 3.2. Identification strategy

As we rely on self-reported life history information, measurement error of individual hunger experience may arise, which yields downward biased estimates. In addition, unobserved heterogeneity may lead to upward biased estimates for the effect of hunger incidence on human capital and labor market outcomes. To account for the potential endogeneity in the self-reported hunger mea-

<sup>10</sup> For example, we define "log share of education expenditure relative to total household expenditure" as "log (household education expenditure +1) – log (household total expenditures +1)". See Online Appendix A for detailed definitions of these log shares.

<sup>11</sup> In sensitivity check, we alternatively define "parental hunger" as *both* parents having food shortage experience before 17. With this definition, 63.4% of children have "parental hunger" equal to 1.

<sup>12</sup> Our results are robust to replacing (own/parental) age and age squared to (own/parental) birth year fixed effects, and to add father's birth city fixed effects. >80% of the second-generation sample have both parents born in the same city and their age gap is smaller than 5 years. To keep the baseline model parsimonious and avoid high correlation among covariates, we avoid adding too many fixed effects.

**Table 1**  
Summary statistics of outcomes and demographic characteristics.

Variables	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
<i>First generation</i>						
<u>Hunger = 1</u>				<u>Hunger = 0</u>		
Years of education	8402	4.533	4.018	2746	5.061	4.207
Illiterate	8402	0.319	0.466	2746	0.293	0.455
Middle school or above	8402	0.247	0.431	2746	0.307	0.461
Non-agricultural job	8402	0.448	0.497	2746	0.495	0.500
Government job	7871	0.126	0.332	2467	0.150	0.358
Firm job	7871	0.199	0.400	2467	0.266	0.442
Male	8402	0.484	0.500	2746	0.426	0.495
birth year	8402	1950.530	7.294	2746	1950.249	9.394
Rural or not at birth	8335	0.933	0.249	2713	0.878	0.327
Number of siblings at birth	8402	1.271	1.159	2746	1.255	1.166
Mother's education	8402	1.065	0.475	2746	1.121	0.568
Father's education	8402	1.500	0.942	2746	1.565	0.994
<i>Second generation</i>						
<u>(Either) Parental hunger = 1</u>				<u>(Either) Parental hunger = 0</u>		
Years of education	10,176	8.207	4.062	993	8.767	4.197
Illiterate	10,342	0.049	0.216	1006	0.042	0.200
Middle school or above	10,342	0.586	0.493	1006	0.642	0.480
Work or not	9081	0.915	0.279	881	0.896	0.306
Household income	8147	40,007.487	42,798.898	787	45,766.836	49,668.871
Partnered or not	10,182	0.916	0.277	997	0.924	0.265
Male	10,271	0.517	0.500	1003	0.523	0.500
Birth year	10,342	1976.288	5.543	1006	1975.651	6.256

Note: the household income for the second generation includes zero values. 5.47% of the observations are zeros.

tures, we use aggregate measures of hunger incidence as an IV. Specifically, our IV is calculated as the average hunger rate in the birth city within the 5-year birth cohort based on the sample, which essentially measures the intensity of malnutrition for the respondent's reference group. This aggregate measure can capture systematic shocks to local food shortages in the corresponding reference group, thus correlating with individual hunger incidence. It is exogenous to individual human capital outcomes in the future, given the assumption that the aggregate hunger rate of the reference group affects long-term outcomes only through individual hunger incidence conditional on controlled observables. With the IV approach, we can identify a local average treatment effect (LATE) of more general hunger experience throughout childhood and adolescence, rather than an intention-to-treat effect of exposure to extreme events, such as famines and wars. The intention-to-treat effect may capture many confounding factors other than food shortages. In this sense, our LATE estimates can provide cleaner empirical evidence of the hunger effect. For the first-generation analysis, the first-stage equation of the 2SLS model is as follows, where  $\overline{Hunger}_{c,5}$  refers to the aggregate hunger rate in the birth city within the 5-year birth cohort.

$$Hunger_i = \alpha_{10} + \beta_{10} \overline{Hunger}_{c,5} + Family'_i \theta_{10} + \delta_{10} Male_i + Age'_i \gamma_{10} + \delta_c + u_i \tag{3}$$

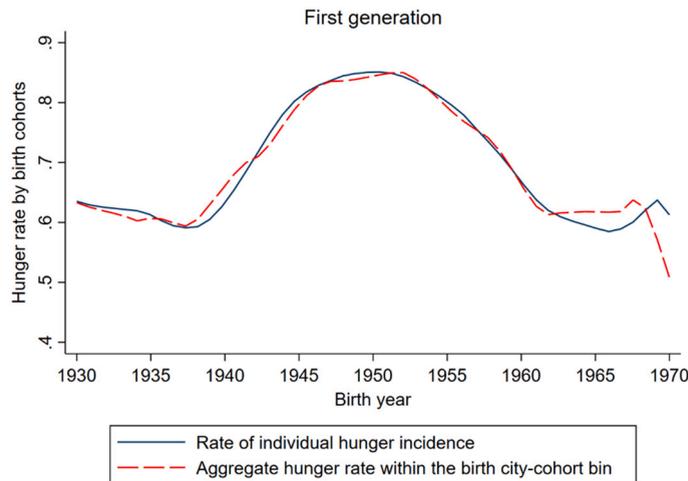
For the second-generation analysis, we first calculate the aggregate hunger rate in the birth city within the 5-year birth cohort for mothers and fathers, respectively. Then, we take the simple average of the maternal measure and the paternal measure to obtain the aggregate parental hunger rate  $\overline{Parental\_Hunger}_{c,5}$ , assuming that the influence from the maternal side and that from the paternal side is of equal importance. In a sensitivity check, we also separately control for mother's and father's hunger experience and find no significantly different effects. For the second-generation analysis, the first-stage equation is as follows.

$$Parental\_Hunger_j = \alpha_{20} + \beta_{20} \overline{Parental\_Hunger}_{c,5} + Parental\_Family'_j \theta_{20} + \delta_{20} Male_j + Parental\_Age'_j \gamma_{20} + Age'_j \gamma_{10} + \delta_{c,mom} + u_j \tag{4}$$

To lend support to the relevance condition of the IV, we show the pattern of individual hunger incidence by birth year for the first generation in Fig. 1 and the second generation in Fig. 3. Besides the relevance condition, the exclusion restriction assumption is needed for a valid IV. Although this assumption is not directly testable, in Section 5, we employ extra IVs to test over-identifying restrictions and show evidence for the joint validity of these IVs. In Section 5, we also check if potential city-specific time-varying confounders and early household characteristics would matter to our IV validity. We show that our results are robust to adding birth city-specific cohort trends, household characteristics at birth, and residential city-specific quadratic age trends.

### 3.3. Stylized facts

Fig. 1 shows the individual hunger rate by birth cohort of the first generation, as well as the aggregate hunger rate within the birth city and 5-year birth cohort bin. As expected, the two variables show very similar patterns, indicating that the aggregate hunger rate is a relevant IV. Early-life hunger incidence is prevalent among the first-generation sample, as there was a combination of chronic malnutrition and periodic severe food shortages in China (Cui et al., 2020). In particular, individuals born in the 1940s and 1950s are much more likely to experience hunger than their earlier and later counterparts, which is consistent with the fact that the Anti-Japanese War (1937–1945), the Henan Famine (1943–1944), the Chinese Civil War (1945–1949), and the Great Chinese Famine



**Fig. 1.** Hunger rate by the first-generation birth cohort.

Note: The “rate of individual hunger incidence” is calculated as the average individual hunger incidence by birth year. The “aggregate hunger rate within the birth city-cohort bin” is calculated as the average  $\overline{Hunger}_{c,5}$  (i.e., the aggregate hunger rate in the birth city within the 5-year birth cohort) by birth year.

(1959–1961) occurred during these years. But when there was no extreme event, nutritional adversities were still prevalent, with about 60% of first-generation individuals born before 1937 or after 1961 who did not experience major wars or famines during childhood still having hunger experience.

Figs. 2a to 2b show regional variations of the first-generation hunger incidence for birth cohorts 1930–1942, 1943–1961 (the Great-Famine cohort), and 1962–1970 in CHARLS, respectively.<sup>13</sup> As the Great Chinese Famine was a major event that causes hunger incidence, comparing Fig. 2a with Fig. 2b, we indeed find that hunger rates in most provinces are much higher for the 1943–1961 birth cohort who experienced the Great Chinese Famine (1959–1961) between ages 0 and 17. In Fig. 2c, the provincial hunger rate drops fast for the young cohort born after the Great Chinese Famine. We also find that hunger incidence is generally less severe in the coastal areas or northeastern provinces for all birth cohorts.

Fig. 3 shows the rate of either parent’s hunger incidence by birth cohort of the second generation and the mean of aggregate maternal and paternal hunger rates (which is the IV used in the baseline). The two curves are parallel, indicating a strong positive correlation between individual hunger incidence and the IV. Individuals born during 1975–1980 have the highest parental hunger rate, which is in line with Fig. 1 that hunger rate peaks among first-generations born in the 1950s.

## 4. Main results

### 4.1. Baseline results

Columns (1) and (3) of Table 2 show OLS estimates for both generations without considering the endogeneity of self-reported hunger experience. For the first generation, hunger incidence between ages 0 and 17 is negatively associated with education outcomes and labor market outcomes. More specifically, having hunger experience is significantly associated with fewer years of education, a higher probability of being illiterate, a lower probability of completing middle school or above, and a lower probability of being employed by firms (considered as a decent job). For the second generation, the point estimates also suggest a negative association between parental hunger experience and children’s outcomes, but all of them are insignificant and small in magnitude.

As OLS estimates may suffer from omitted variable bias and attenuation bias, we test the endogeneity of (parental) hunger in Online Appendix Table B.2. For most outcomes, the null hypothesis that (parental) hunger can be treated as exogenous is rejected. We therefore consider 2SLS as our preferred method for both generations, as shown in columns (2) and (4) of Table 2. For the first-generation analysis, we instrument individual hunger incidence by average hunger rate within the same birth city and the birth cohort. The first-stage results suggest that the aggregate hunger rate is highly correlated with individual hunger incidence, signifying that hunger incidence is more likely to be caused by macro factors or systematic shocks rather than idiosyncratic family-related factors. The 2SLS estimates generally have a larger effect magnitude, possibly because the IV method has corrected the attenuation bias from measurement errors of individual hunger incidence. Individual hunger incidence during 0–17 significantly decreases years of education by 1.15 years and the probability of having middle school or above by 12.3 percentage points and increases the probability of being illiterate by 4.8 percentage points. In terms of labor market outcomes, in an unreported regression, having early-life hunger

<sup>13</sup> For the readability of figures, we aggregate and present the hunger rate at province level.

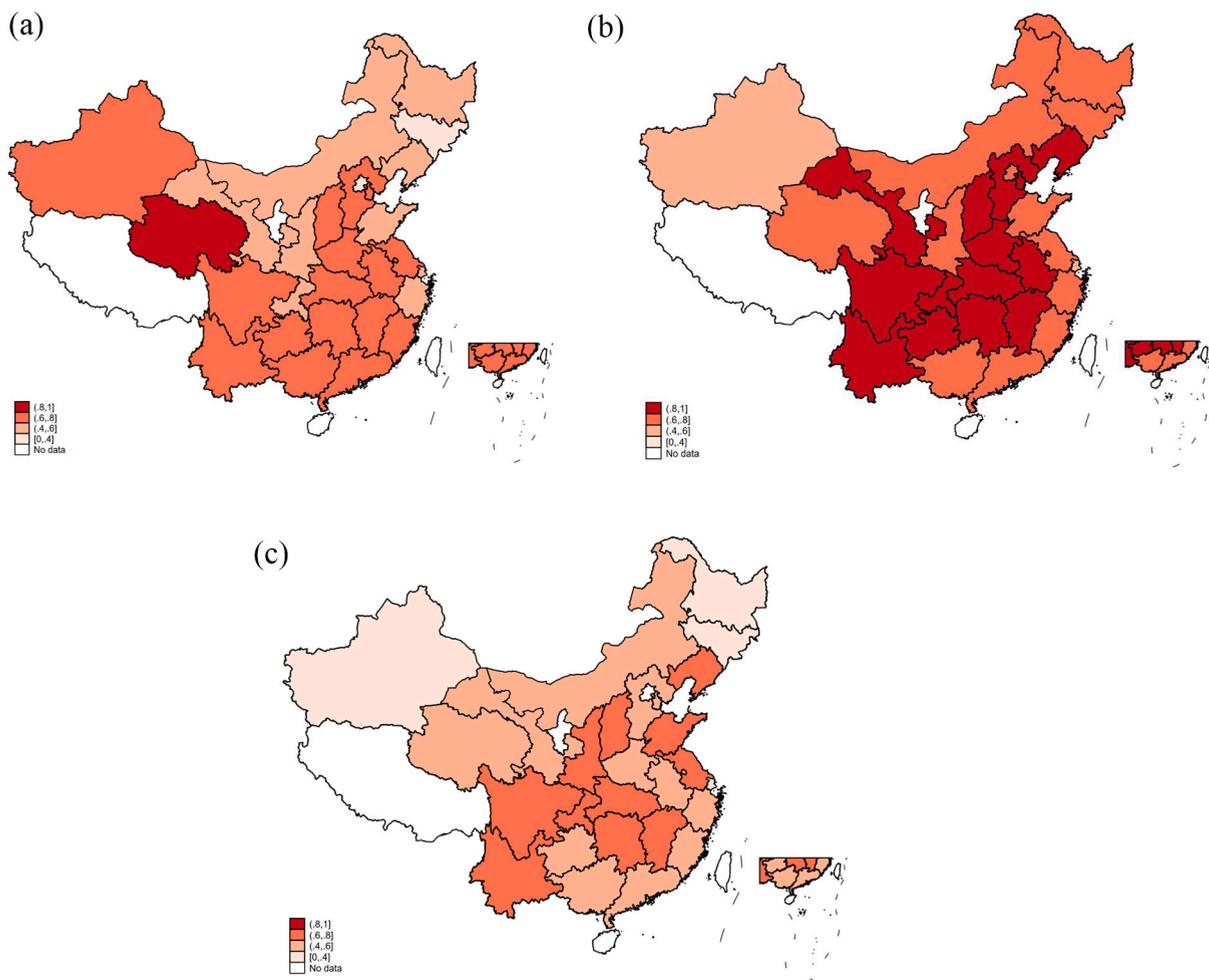


Fig. 2. a). Provincial hunger rate for the 1930–1942 birth cohort. b) Provincial hunger rate for the 1943–1961 birth cohort. Fig. 2c Provincial hunger rate for the 1962–1970 birth cohort.

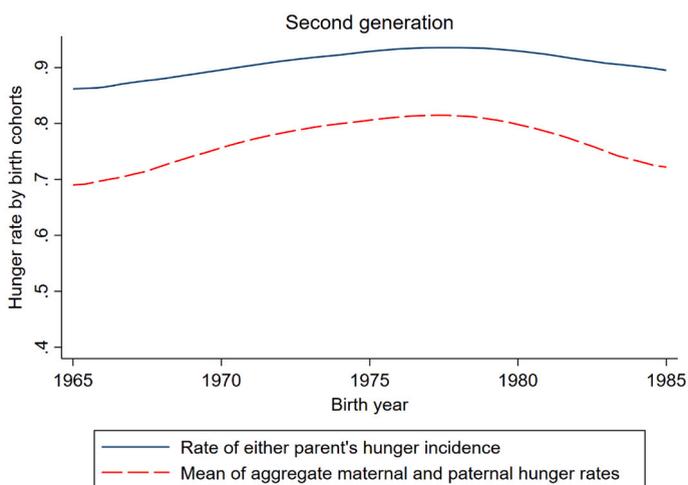


Fig. 3. Parental hunger rate by the second-generation birth cohort.

**Table 2**  
OLS and 2SLS estimates of (parental) hunger for both generations.

	First generation			Second generation	
	OLS	2SLS		OLS	2SLS
Outcome var.	(1)	(2)	Outcome var.	(3)	(4)
Years of education	-0.364*** (0.077)	-1.150*** (0.237)	Years of education	-0.077 (0.123)	0.263 (0.463)
Illiterate	0.028*** (0.009)	0.048* (0.027)	Illiterate	0.003 (0.007)	-0.044* (0.026)
Middle school or above	-0.039*** (0.009)	-0.123*** (0.027)	Middle school or above	-0.005 (0.015)	-0.049 (0.056)
Non-agricultural job	-0.016 (0.010)	-0.102*** (0.029)	Work or not	0.014 (0.011)	0.105*** (0.039)
Government job	-0.011 (0.008)	-0.051** (0.025)	Log (Household income+1)	-0.148 (0.094)	-0.218 (0.378)
Firm job	-0.022** (0.009)	-0.090*** (0.029)	Partnered or not	-0.014 (0.009)	0.031 (0.036)
First-stage coefficient of IV		0.969*** (0.022)	First-stage coefficient of IV	0.709***	(0.030)
First-stage F statistics		1912.94	First-stage F statistics	543.22	

Notes: \*Significant at 10%; \*\* at 5%; \*\*\* at 1%. Numbers in parentheses are robust standard errors. Columns (1) and (3) and columns (2) and (4) show OLS and 2SLS estimates of the effect of (parental) hunger incidence, respectively, on each outcome for both generations. For the first-generation analysis, the IV for hunger incidence is the aggregate hunger rate in the same birth city and the same 5-year cohort bin. Family characteristics, age, age squared, gender, and birth city fixed effects are controlled. For the second-generation analysis, the IV for parental hunger incidence is the average of the aggregate maternal hunger rate and the aggregate paternal hunger rate. Parental family characteristics, child gender, maternal/paternal/own age and age squared, and maternal birth city fixed effects are controlled. The number of observations for each outcome is listed in Table 1.

experience has almost no effect on ever participating in the labor market (with estimated coefficient - 0.001 and standard error 0.006), since >99% of respondents report having ever worked during the lifetime. However, as shown in Table 2, hunger incidence has an adverse impact on job quality. Those experiencing hunger are less likely to have better-paid and more stable jobs, decreasing the probability of having non-agricultural jobs by 10.2 p.p., the probability of being employed by the government or public sector by 5.1 p.p., and that of being employed by firms by 9.0 p.p.

For the second-generation analysis, we instrument parental hunger incidence by the aggregate parental hunger rate, which is calculated as the average of both parents' aggregate hunger rates within the birth city-cohort bin. The IV has a strong association with individual parental hunger incidence, with a large F statistic of 543.2. In contrast to the significant adverse impact of hunger on the first generation's human capital, we find a small and insignificant effect of parental early-life hunger on adult children's education, which is consistent with the pattern from the OLS estimates. Regarding labor market outcomes, children with parental hunger experience are significantly more likely to work, but income per capita is not significantly higher. The point estimate of the effect on

**Table 3**  
2SLS estimates of hunger on behaviors of the first generation.

Outcome var.	(1)	Outcome var.	(2)	Outcome var.	(3)
<i>Health</i>		<i>Expenditure structure</i>		<i>Intergenerational interactions</i>	
Self-reported health	0.063 (0.066)	Log (Food expenditure+1)	0.067 (0.114)	Grandparenting or not	0.093*** (0.033)
Sick	0.067** (0.030)	Log share of food expenditure	0.105 (0.080)	Number of grandchildren taken care of	0.134*** (0.047)
Overweight	0.102*** (0.036)	Having total OOP health costs or not	0.112*** (0.034)	Log (Hours of grandparenting+1)	0.682*** (0.254)
Cancer	0.013** (0.007)	Log (Total OOP health costs+1)	0.869*** (0.268)	Having transfer from children or not	0.086*** (0.024)
Stomach disease	0.043 (0.029)	Log share of health expenditure	0.543* (0.318)	Log (Amount of transfer from children+1)	0.687*** (0.206)
Liver disease	0.031** (0.015)	Having education expenditure or not	0.066** (0.026)	Having transfer to children or not	-0.003 (0.032)
		Log (Education expenditure+1)	0.448** (0.209)	Log (Amount of transfer to children+1)	-0.188 (0.255)
		Log share of education expenditure	0.519** (0.230)	Net transfer from children	3824.432*** (1504.286)

Notes: \*Significant at 10%; \*\* at 5%; \*\*\* at 1%. Numbers in parentheses are robust standard errors. All columns report 2SLS estimates of the effect of hunger incidence on each mechanism variable for the first generation. The IV for hunger incidence is the aggregate hunger rate in the same birth city and the same 5-year cohort bin. For each regression, family characteristics, age, age squared, gender, and birth city fixed effects are controlled. All the expenditures, number and hours, and transfers include zeros. The share of zeros and the number of observations for each outcome are listed in Table B.1.

household income is negative, while the probability of having a partner is higher, though both are insignificant, suggesting a lower income per capita. To summarize, the adverse effect of early-life nutritional shocks on human capital and labor market outcomes seems to attenuate over generations.

#### 4.2. Potential pathways

To shed light on the underlying mechanisms to interpret the adverse long-run impact of early-life nutritional shocks, as well as the attenuating effect on the second generation, we explore how hunger experience alters the first generation's health capital and consumption behavior, which may shape their own human capital accumulation and the way of nurturing the second generation. In Table B.1, we summarize the variables needed for exploring potential pathways of long-run and intergenerational effects.

On the one hand, nutritional adversities can lead to a lower level of health capital, which negatively affects human capital accumulation and labor market outcomes in the long run. In column (1) of Table 3, we check this health pathway by investigating whether early-life hunger experience would transform into negative impacts on one's own objective and subjective health indicators. First, early-life nutritional adversities lead to a higher probability of being overweight, which is constructed with objectively measured BMI. This seems to suggest that people tend to over-compensate for their early nutrition shortages and gain too much weight later in life. This is consistent with Kesternich et al. (2015) which documents that hunger experience during age 0–16 increases the probability of being obese. Second, hunger experience significantly increases both the risk of having liver diseases which is usually related to digestion and overweight and the probability of having cancer. The risk of stomach diseases related to digestion also increases, though not significantly. Third, current subjective health status deteriorates with hunger experience. We find a higher self-reported health status score, which means worse health status, as well as a higher probability of being recently sick. Our finding of adverse impacts on health is consistent with Cui et al. (2020), who also find that retrospective hunger incidence is associated with a higher risk of being overweight and more functional limitations among Chinese elderly individuals. Health, therefore, could indeed be a pathway through which early-life nutrition shortage negatively affects one's education and labor market outcomes for the first generation.

On the other hand, parents experiencing nutritional adversities, as well as the negative shocks on health and education thereafter, may change their behaviors of nurturing the children. According to Almond et al. (2009, 2018), parents' responses to the observed endowment shocks can be either reinforcing or compensating. When investments in human capital are assumed to be substitutable across periods, parents can make compensating later-life investments in a child who suffered from an adverse early-life shock. In our context, parents make extra efforts to improve children's education and health to ensure that the children would not suffer from parental hunger experience, that is, an observed adverse endowment shock. Although we do not directly observe parental investment in children, we can still infer parents' behavioral changes through their expenditure structure and relationship with their children. As shown in columns (2) and (3) of Table 3, with early-life hunger experience, food expenditure relative to total household expenditure increases, though not significantly.<sup>14</sup> The incidence and amount of out-of-pocket (OOP) health expenditure increase by 11.2 percentage points (p.p.) and by 86.9%, respectively. And the share of OOP health expenditures relative to total household expenditures increases substantially by 54.3%. The increasing food expenditure, though insignificant, might suggest that children are provided with more nutrition by parents with hunger experience, which is also indicated by Kesternich et al. (2015). In addition, although the increasing health expenditure is consistent with the deteriorating health status caused by early-life hunger, it may also suggest that the awareness of investing in health is enhanced by hunger experience. This would in turn influence children's development in a positive way. Therefore, parental early-life hunger incidence may not always impede children's health and human capital accumulation.

In an unreported regression, when we estimate the effect of parental hunger on children's health status, the estimate is not significant, with a coefficient of 0.185 and a standard error of 0.113. Combined with the results from column (1) of Table 3, it further suggests that the adverse health impact of hunger on the first generation might not be strong enough to pass onto the next generation.

Furthermore, according to column (2) of Table 3, parents with hunger experience significantly increase the probability of spending money on education by 6.6 p.p., the amount of household expenditure on education by 44.8%, and the share of education expenditure relative to total household expenditure increases by 51.9%. Although we do not have information on whether the education expenditure is spent on their children, grandchildren, or even parents themselves, it is clear that parents with hunger experience tend to make compensatory investments in education. The increased education investment may help to attenuate the potential negative impact of parental hunger on children's human capital accumulation. The net effect of parental hunger on child education might even become positive if parents largely increase compensatory behaviors. The intergenerational effect of early-life hunger incidence seems to be not harmful to the current extent.

Column (3) of Table 3 further shows that parents with hunger experience have more interactions with children. They provide much more help in taking care of grandchildren, which may in part explain why their adult children's probability of working significantly increases. Meanwhile, they receive significantly more monetary transfers from children. The intensive interaction may indicate a closer relationship between parents and their children, which might come from the persistent attention to children's food, health, and education from parents experiencing early-life hunger. Taken together, we conjecture that the potential negative effects of parental hunger incidence on children might be compensated by the extra expenditure and care to children from parents, which leads to a null

<sup>14</sup> The expenditure, number and hours of grandparenting, and transfer variables in columns (2) and (3) of Table 3 include zero values. There are 0.96%, 73.12% and 81.22% zero observations in food expenditure, OOP health cost and education expenditure. Therefore, we add the incidence of OOP health cost and that of education expenditure as dependent variables to check effects at the extensive margin for these variables with substantial share of zeros. Please refer to Online Appendix A for definition of these variables and Table B.1 for the descriptive statistics.

effect on educational attainment of the second generation.

### 4.3. Heterogeneous effects

To better understand who is more affected by early-life nutritional adversities, we investigate heterogeneous effects by subgroup. Table 4 reports the gender heterogeneity. For the first generation, early-life hunger experience negatively affects both genders' educational outcomes, but males' outcomes to a larger extent. This might be because females born during 1930–1970 have a very low educational attainment on average (46% women illiterate vs. 14% men illiterate) according to our sample, and hunger incidence would not further negatively shock their educational attainment much. But women's job quality decreases more. They are significantly less likely to have non-agricultural jobs, such as in the government or public sector. For the second generation, the adverse effect of parental hunger on education is no longer significantly different from 0 for both genders. Daughters' probability of working increases, possibly related to the fact that parents with hunger experience offer more help in grandchildren care, which frees daughters to work.

Moreover, Table 5 reports heterogeneous effects by family socioeconomic status (SES). A first-generation individual is categorized into a low-SES group if both her/his parents are illiterate and her/his household had no access to clean water nor electricity before her/his birth. Otherwise, she/he belongs to a high-SES group. Similar to the pattern shown in gender heterogeneity, early-life hunger experience reduces years of education for both the low- and high-SES groups. But it reduces more for the high-SES group, possibly because they have potentially higher educational attainment when there is no nutritional deprivation. In terms of education structure, the low-SES group is 6 p.p. less likely to complete middle school or above while 7.8 p.p. more likely to be illiterate, while 23.8% of the high-SES group shift from middle school or above to a lower education level. Both the low and high-SES groups are about 11 p.p. less likely to have a non-agricultural job and about 9 p.p. to have a firm job, respectively, and the high-SES group's probability of having a government job decreases by 8.8 percentage points.

For a second-generation individual, being in a low-SES group means that both parents belong to the low-SES group. Otherwise, she or he belongs to a high-SES group. Columns (3) and (4) of Table 5 report the 2SLS estimation results. Parental hunger has no negative effects on educational attainment for either group, implying a rather prominent compensatory effect of hunger. It even reduces the likelihood of being illiterate by 0.17 and increases the probability of working by 0.24 for the low-SES group. This is possibly because with less potential educational attainment, their loss of schooling is easier to compensate for.

Finally, in Table 6, we check heterogeneity by the severity of hunger. As hunger incidence is a subjective measure in CHARLS, the actual severity of hunger can be very different among respondents who report experiencing hunger. Though we cannot measure the severity of self-reported hunger, we can proxy it by provincial severity of the Great Famine which was the most severe nutritional shock to the first-generation sample. More specifically, the hunger severity in respondent's birth province  $p$  is measured by the average excessive death rate (AEDR) from 1959 to 1961 (the famine years), that is,  $AEDR_p = \frac{\sum_{i=1959}^{1961} EDR_{ip}}{3}$ . The excessive death rate (EDR) is calculated as the excessive deaths per 10,000 people in year  $i$  and province  $p$  compared to the average pre-famine deaths, that is,  $EDR_{ip} = \left( ADR_{ip} - \frac{ADR_{1956,p} + ADR_{1957,p} + ADR_{1958,p}}{3} \right)$ , and  $ADR_{ip}$  refers to the annual death rates (deaths per 10,000 people) in province  $p$  and

**Table 4**  
2SLS estimates of (parental) hunger for both generations by gender.

	First generation			Second generation	
	Male	Female		Male	Female
Outcome var.	(1)	(2)	Outcome var.	(3)	(4)
Years of education	-1.704*** (0.394)	-0.634** (0.295)	Years of education	0.008 (0.694)	0.600 (0.612)
Illiterate	0.085** (0.039)	0.007 (0.038)	Illiterate	-0.038 (0.023)	-0.045 (0.043)
Middle school or above	-0.188*** (0.047)	-0.071** (0.032)	Middle school or above	-0.043 (0.086)	-0.047 (0.073)
Non-agriculture job	-0.089* (0.048)	-0.118*** (0.037)	Work or not	0.065 (0.043)	0.139** (0.066)
Government job	-0.027 (0.045)	-0.086*** (0.027)	Log (Household income+1)	-0.323 (0.529)	-0.252 (0.567)
Firm job	-0.080* (0.048)	-0.095*** (0.035)	Partnered or not	-0.008 (0.061)	0.048 (0.040)
First-stage coefficient of IV	0.925*** (0.036)	1.004*** (0.031)	First-stage coefficient of IV	0.660*** (0.043)	0.752*** (0.043)
First-stage F statistics	649.41	1046.17	First-stage F statistics	233.26	307.99
Observations	5125	5802	Observations	5740	5370

Notes: \*Significant at 10%; \*\* at 5%; \*\*\* at 1%. Numbers in parentheses are robust standard errors. The columns show 2SLS estimates of the effect of (parental) hunger incidence on each outcome for both generations by gender. For the first-generation analysis, the IV for hunger incidence is the aggregate hunger rate in the same birth city and the same 5-year cohort bin. Family characteristics, age, age squared, gender, and birth city fixed effects are controlled. For the second-generation analysis, the IV for parental hunger incidence is the average of the aggregate maternal hunger rate and the aggregate paternal hunger rate. Parental family characteristics, child gender, maternal/paternal/own age and age squared, and maternal birth city fixed effects are controlled.

**Table 5**  
2SLS estimates of (parental) hunger for both generations by SES.

	First generation			Second generation	
	Low-SES	High-SES		Low-SES	High-SES
Outcome var.	(1)	(2)	Outcome var.	(3)	(4)
Years of education	-0.817*** (0.317)	-1.878*** (0.396)	Years of education	1.600 (0.982)	-0.556 (0.548)
Illiterate	0.078* (0.041)	0.034 (0.038)	Illiterate	-0.171** (0.072)	0.019 (0.023)
Middle school or above	-0.060* (0.035)	-0.238*** (0.045)	Middle school or above	-0.010 (0.131)	-0.055 (0.063)
Non-agriculture job	-0.107** (0.043)	-0.114** (0.045)	Work or not	0.240*** (0.086)	0.046 (0.049)
Government job	-0.033 (0.034)	-0.088** (0.042)	Log (Household income+1)	-0.131 (0.669)	-0.254 (0.484)
Firm job	-0.089** (0.040)	-0.089* (0.048)	Partnered or not	0.067 (0.085)	-0.015 (0.043)
First-stage coefficient of IV	0.958*** (0.033)	0.987*** (0.036)	First-stage coefficient of IV	0.578*** (0.053)	0.796*** (0.040)
First-stage F statistics	852.89	750.14	First-stage F statistics	121.08	400.96
Observations	6097	4189	Observations	4248	6160

Notes: \*Significant at 10%; \*\* at 5%; \*\*\* at 1%. Numbers in parentheses are robust standard errors. Columns (1) and (2) show 2SLS estimates of the effect of hunger incidence on each outcome for the first generations by the first generation's social economic status (SES). SES is defined as "low" if the first-generation respondent's household had no clean water nor electricity before birth and both parents are illiterate, otherwise, SES is defined as "high". Columns (3) and (4) show 2SLS estimates of the effect of parental hunger incidence on each outcome for the second generation by SES. "SES (2nd gen.)" is defined as "low" if a second-generation individual's both parents belong to low-SES group. Otherwise, "SES (2nd gen.)" is defined as "high". The IVs and control variables are the same as those in Table 2.

**Table 6**  
2SLS estimates of (parental) hunger for both generations by severity of hunger.

	First generation			Second generation	
	Severe	Non-severe		Severe	Non-severe
Outcome var.	(1)	(2)	Outcome var.	(3)	(4)
Years of education	-1.664*** (0.448)	-0.879*** (0.279)	Years of education	-1.353 (1.064)	0.945* (0.517)
Illiterate	0.084 (0.052)	0.026 (0.031)	Illiterate	0.050 (0.063)	-0.072*** (0.028)
Middle school or above	-0.127** (0.050)	-0.112*** (0.031)	Middle school or above	-0.209 (0.127)	0.006 (0.061)
Non-agriculture job	-0.111** (0.054)	-0.088** (0.034)	Work or not	0.035 (0.086)	0.132*** (0.044)
Government job	-0.013 (0.043)	-0.059* (0.030)	Log (Household income+1)	-0.636 (0.909)	-0.040 (0.416)
Firm job	-0.048 (0.051)	-0.106*** (0.035)	Partnered or not	0.084 (0.082)	0.009 (0.039)
First-stage coefficient of IV	0.957*** (0.041)	0.972*** (0.026)	First-stage coefficient of IV	0.619*** (0.057)	0.749*** (0.036)
First-stage F statistics	533.25	1374.07	First-stage F statistics	118.27	442.59
Observations	3516	7411	Observations	3709	7401

Notes: \*Significant at 10%; \*\* at 5%; \*\*\* at 1%. Numbers in parentheses are robust standard errors. Columns (1) and (2) show 2SLS estimates of the effect of hunger incidence on each outcome for the first generation by hunger severity, measured by the average excessive death rate (AEDR) from 1959 to 1961 in the birth province. Hunger is severe if AEDR > 0.8‰ and non-severe otherwise. Columns (3) and (4) show 2SLS estimates of the effect of parental hunger incidence on each outcome for the second generation by parental hunger severity. Parental hunger is severe if the AEDR of parents > 0.8‰ and non-severe otherwise. The IVs and control variables are the same as those in Table 2.

year *i* (Kim et al., 2014; Shi, 2011). Then we calculate the national average AEDR during the Great Famine and split the sample by it. A first-generation individual is considered to have experienced severe hunger if she is born in a province with an AEDR above the national average, 0.8‰, and vice versa. Comparing column (1) with (2), we find that more severe food shortages have a much larger negative impact on years of education, while the negative impact does not necessarily transform into reduced job quality in the labor market.

We conduct a similar exercise regarding the second generation in columns (3) and (4) of Table 6. We define a second-generation individual as "severe parental hunger" if the mean of the AEDR in her mother's and father's birth provinces exceeds the national average. None of the children's outcomes are affected in the "severe" group, while children's educational attainment and working status are improved in the non-severe group. The results indicate that the compensatory behaviors of intergenerational human capital

investment are more likely to happen to parents experiencing less severe hunger.

In an unreported analysis, we split the sample by Hukou status. Despite a very small urban sample<sup>15</sup> and possibly imprecise estimates, early-life hunger experience still significantly reduces both groups' educational attainment. Hunger has a more pronounced effect on the educational attainment of the urban sample, which is similar to our analysis of SES heterogeneity.

In summary, the adverse effect of hunger incidence on first-generation education is rather universal, though subgroups with higher educational potential and more severe hunger are slightly more affected. The job quality reduction is mostly driven by females and residents from regions with a less severe famine. For the second generation, most groups show a null effect on education, except that the low SES group even reduces the probability of being illiterate. The increased probability of working is mainly driven by females, the low SES group, and residents from non-severe regions.

## 5. Sensitivity analyses

### 5.1. Additional instrumental variables and over-identification tests

Although we cannot directly test the exclusion restriction for the IV, we can employ additional IVs to conduct over-identification tests. For the first generation, we add an indicator for exposure to the Great Famine as the additional IV to hunger incidence. The IV is constructed as the interaction term of the provincial excessive death rate during 1959–1961, which measures the intensity of local famine (e.g., Kim et al., 2014, 2017), and a dummy for whether one is exposed to the Great Famine during age 0–17 (measured as birth year between 1942 and 1961). The Great Famine, as the most adverse and unexpected nutritional shock for our sampled cohorts, can provide additional information to our primary measure of general hunger experience. For the second generation, we replace the average parental hunger rate with two IVs, the aggregate average hunger rate within the birth city and the 5-year cohort bin of mothers and fathers, respectively. Though using multiple IVs can be undesirable, as it can make the monotonicity assumption needed for interpreting the 2SLS estimates as a positively weighted average of LATE for multiple compliers less likely to hold (Mogstad, Torgovitsky, & Walters, 2021), we can still show indirect evidence of IV validity by testing the over-identifying restrictions with this exercise. We conduct a similar 2SLS estimation as in the baseline. The results presented in Table 7 are close to the baseline results. The Hansen J tests of over-identifying restrictions show that the joint validity of the two IVs cannot be rejected at the 10% significance level for all outcomes.

### 5.2. Adding time-varying controls at the city level and household characteristics

We add further controls to check if our results are confounded by potential city-specific time varying factors or household-specific factors. In Table 8, we additionally control for birth city specific birth year linear trends in column (1) and maternal birth city-cohort trends for the second-generation results in column (4). This can account for, for example, trends in economic growth or local public expenditure on education and health across birth cities.

In columns (2) and (5), on top of the city-cohort trends, we further control for characteristics of early-life household health-related infrastructure and educational support. More specifically, for the first generation, we control for “whether the household has access to clean water at birth”, “whether the household has electricity at birth”, “whether the respondent's mother passed away around birth”, and “whether the respondent's father had passed away around birth”. For the second generation, we control for both mother's and father's access to clean water and electricity at (mother's or father's) birth, and whether the mother's (or father's) parent had passed away around (mother's or father's) birth.

In columns (3) and (6), we further control for “residential city-specific quadratic age trends” (interacting age and age<sup>2</sup> with residential city dummies) and “parental residential city specific quadratic child age trends” (interacting child age and child age<sup>2</sup> with parental residential city dummies) for the first and second generations, respectively. This can account for the potentially differential trends of regional development, especially those time-varying factors that are associated with education and labor market outcomes in a nonlinear way. The results are rather robust, suggesting minimal threats to the identification of these city-specific time varying confounders or household-level confounders.

### 5.3. Alternative construction of IV

We experiment with alternative IV construction methods. Columns (1) and (3) of Table B.3 construct an alternative IV as “aggregate (parental) hunger rate in the same birth city and 10-year cohort bin”. Columns (2) and (4) use the “aggregate (parental) hunger rate in the same birth province and 5-year cohort bin” as the IV. This is because a larger reference group can lower measure errors when we calculate the aggregate hunger rate based on the sample. Table B.3 shows that the baseline results are robust to the two alternative IV construction methods.

<sup>15</sup> In the first-generation sample, 885 individuals are urban residents out of 11,048 individuals with Hukou information. In the second-generation sample, 414 out of 11,338 individuals with complete parental Hukou information have at least one parent with urban Hukou.

**Table 7**  
2SLS estimates of the first-generation analysis with an additional IV.

	First generation		Outcome var.	Second generation	
	2SLS	Hansen J (P)		2SLS	Hansen J (P)
Outcome var.	(1)	(2)	Outcome var.	(3)	(4)
Years of education	-1.153*** (0.236)	0.852	Years of education	0.231 (0.461)	0.275
Illiterate	0.049* (0.027)	0.627	Illiterate	-0.045* (0.025)	0.669
Middle school or above	-0.122*** (0.027)	0.841	Middle school or above	-0.055 (0.055)	0.143
Non-agriculture job	-0.105*** (0.029)	0.178	Work or not	0.102*** (0.039)	0.490
Government job	-0.050** (0.025)	0.665	Log (Household income+1)	-0.237 (0.378)	0.418
Firm job	-0.092*** (0.029)	0.385	Partnered or not	0.034 (0.036)	0.387
First-stage coefficient of IV			First-stage coefficient of IV		
Average hunger rate (city,5 yr)	0.964*** (0.022)		Average maternal hunger rate	0.388*** (0.026)	
Famine exposure	0.002** (0.001)		Average paternal hunger rate	0.327*** (0.023)	
K.P. Wald rk F stat.	959.21		K.P. Wald rk F stat.	273.12	

Notes: \*Significant at 10%; \*\* at 5%; \*\*\* at 1%. Numbers in parentheses are robust standard errors. The number of observations for each outcome is listed in Table 1. Columns (1) and (3) show 2SLS estimates of the effect of (parental) hunger incidence on each outcome for both generations. For the first-generation analysis, the two IVs for hunger incidence are “aggregate hunger rate in the same birth city and the same 5-year cohort bin” and the additional IV “famine exposure”. The latter is measured as an interaction of the “dummy variable indicating whether birth year is between 1942 and 1961” and AEDR. For the second-generation analysis, the IV for parental hunger incidence is the aggregate maternal hunger rate and the aggregate paternal hunger rate separately. The control variables are the same as those in Table 2. Columns (2) and (4) report the p values for the Hansen J test.

**Table 8**  
2SLS estimates of (parental) hunger for both generations accounting for city specific time varying and household characteristics.

	First generation			Outcome var.	Second generation		
	City-cohort	City-cohort + household	City-quadratic age		City-cohort	City-cohort + household	City-quadratic age
Outcome var.	(1)	(2)	(3)	Outcome var.	(4)	(5)	(6)
Years of education	-1.283*** (0.256)	-1.251*** (0.254)	-1.044*** (0.243)	Years of education	0.433 (0.515)	0.507 (0.513)	0.680 (0.489)
Illiterate	0.059* (0.030)	0.057* (0.030)	0.031 (0.028)	Illiterate	-0.058** (0.028)	-0.056** (0.028)	-0.074*** (0.028)
Middle school or above	-0.145*** (0.029)	-0.141*** (0.029)	-0.131*** (0.028)	Middle school or above	-0.010 (0.063)	-0.007 (0.063)	0.010 (0.060)
Non-agricultural job	-0.101*** (0.033)	-0.100*** (0.033)	-0.082*** (0.030)	Work or not	0.112** (0.048)	0.109** (0.048)	0.119*** (0.043)
Government job	-0.055** (0.028)	-0.056** (0.028)	-0.053** (0.026)	Log (Household income+1)	-0.452 (0.449)	-0.384 (0.449)	-0.116 (0.412)
Firm job	-0.095*** (0.032)	-0.087*** (0.032)	-0.081*** (0.029)	Partnered or not	-0.012 (0.040)	-0.013 (0.040)	0.027 (0.039)
First-stage coefficient of IV	0.958*** (0.027)	0.958*** (0.027)	0.966*** (0.024)	First-stage coefficient of IV	0.703*** (0.034)	0.705*** (0.034)	0.702*** (0.032)
First-stage F stat.	1303.10	1300.58	1556.10	First-stage F stat.	433.55	441.55	489.09

Notes: \*Significant at 10%; \*\* at 5%; \*\*\* at 1%. Numbers in parentheses are robust standard errors. All columns show 2SLS estimates of the effect of (parental) hunger incidence on each outcome for both generations. For the first-generation analysis, the IV for hunger incidence is the aggregate hunger rate in the same birth city and the same 5-year cohort bin. Family characteristics, age, age squared, gender, and birth city fixed effects are controlled. The number of observations for each outcome is listed in Table 1. Column (1) further controls for birth city specific birth cohort linear trends. Column (2) further controls for birth city-specific birth cohort linear trends, and household characteristics at birth (whether the household has clean water at birth, whether the household has electricity at birth, whether mother dead around birth, and whether father dead around birth). Column (3) further controls for residential city specific quadratic age trends. For the second-generation analysis, the IV for parental hunger incidence is the average of the aggregate maternal hunger rate and the aggregate paternal hunger rate. Parental family characteristics, child gender, maternal/paternal/own age and age squared, and maternal birth city fixed effects are controlled. Column (4) further controls for maternal birth city specific birth cohort linear trends. Column (5) further controls for maternal birth city specific birth cohort linear trends, and both parents’ household characteristics at birth (whether mother(father)’s household has clean water at birth, has electricity at birth, whether mother(father)’s mother(father) dead around mother(father)’s birth.) Column (6) further controls for parental residential city-specific quadratic child age trends.

#### 5.4. Is there survival bias of the first generation?

Another concern is that severe hunger incidence may correlate with the death rate of the first generation, which might lead to the survival bias of both generations. For instance, the surviving individuals from the Great Famine may have better health and human capital, which underestimates the negative hunger effects. In this sense, our baseline estimates provide a lower bound of the hunger effect on the population. Though we cannot observe individuals who died before the survey or directly test the selectivity of the first-generation sample, we can still provide some indirect evidence to show how this potential survival bias would influence the results.

In the retrospective survey, respondents are asked “During 1958–1962 (around the period of the Great Famine), had any of your family (including your grandparents, parents, siblings, children and so on) starved to death?” Out of 10,797 observations who answered this question, 1276 (11.8%) chose “yes”. The dummy for family members’ death proxies the chance of survival during extreme events of food shortages. Individuals without any family member dead of hunger will have a lower mortality risk and suffer less from survival bias; thus, hunger experience is expected to have smaller adverse impacts on them. When we restrict the sample to these individuals without much survival bias, according to column (1) of Table 9, the negative effect of hunger still remains, and they are only slightly smaller in magnitude than the baseline estimates. This indicates that the survival bias may not be large and that our baseline estimates tend to be conservative if survival bias exists.

#### 5.5. Is there survival bias of the second generation?

In addition to parental survival bias, one may also be concerned that parental hunger experience may correlate with children’s death rate and lead to sample selection bias for the second generation, which attenuates the negative hunger effect on children’s outcomes. We can show evidence by testing whether parental hunger experience affects children’s mortality. We add a sample of dead children reported by parents back to the second-generation sample and check whether parental hunger incidence affects children’s mortality. Column (4) of Table 9 shows that parental hunger experience hardly has any effect. Similar to the analysis about the first generation’s survival bias, column (3) of Table 9, based on a second-generation sample whose parents have no family member dead during 1958–1962, also shows very robust results to the main estimation in column (4) of Table 2. Therefore, the children’s survival bias is minimal here.

#### 5.6. Is the sample selected by parents’ marital and fertility choices?

One may be concerned that nutritional deprivation in early life may correlate with the first generation’s marital and fertility decisions, which could lead to sample selection bias for the second generation. To check this, we include back a sample of first-generation individuals without any child during the lifetime, and we no longer restrict on children’s age, which leaves us a sample of 17,006 observations. Then, we conduct a similar 2SLS analysis as in Section 4.1, but the outcomes are whether one has had a partner or had no child during the lifetime. The results in column (2) of Table 9 show that hunger experience before age 17 has no significant effect on marital decision (have a partner or not) or on fertility decision (being childless or not). Thus, the potential selection into marriage or fertility is minimal in this context.

#### 5.7. Alternative functional form of parental hunger incidence

In the second-generation analysis, instead of defining parental hunger incidence as “either parent experienced hunger during 0–17”, we apply an alternative definition: *both* parents experienced hunger during 0–17. Under this new definition, children with only one parent having hunger incidence, who belong to the group of “parental hunger = 1”, are now categorized to the baseline group of “parental hunger = 0”; thus, we expect the effect of parental hunger to be smaller. We use the same IV and control variables as in Section 4.1 and report the results in columns (1) and (2) of Table B.4. As expected, the magnitude of point estimates is generally smaller, yet the pattern remains unchanged: parental hunger has an almost null effect on education and increases the probability of working significantly.

Another exercise is to assume that mothers’ and fathers’ hunger experience would influence children’s outcomes separately. Thus, the independent variables would be two dummies, “mother having hunger incidence during 0–17 or not” and “father having hunger incidence during 0–17 or not”, which are instrumented by maternal and paternal aggregate hunger rates. Columns (3) and (4) of Table B.4 report the 2SLS results. The point estimates are qualitatively the same as the main analysis, though most of them are insignificant. The coefficients of maternal hunger incidence are not significantly different from those of paternal hunger at the 10% significance level for all outcomes. Therefore, we find no strong evidence for separate effects of maternal and paternal hunger on child outcomes in the sense that maternal hunger and paternal hunger are strongly correlated through assortative marital match.

**Table 9**  
2SLS estimates for both generations to check sample selection and survival bias.

Sample	First generation		Second generation		
	No death during Great Famine	With childless individuals	No death during Great Famine	With dead child	
Outcome var.	(1)	(2)	Outcome var.	(3)	(4)
Yrs. of edu.	-1.010*** (0.247)		Yrs. of edu.	0.376 (0.498)	
Illiterate	0.031 (0.028)		Illiterate	-0.047* (0.028)	
Middle school or above	-0.113*** (0.028)		Middle school or above	-0.045 (0.060)	
Non-agri. Job	-0.082** (0.030)		Work or not	0.083** (0.042)	
Gov. job	-0.042 (0.026)		Log (hinc. +1)	-0.207 (0.399)	
Firm job	-0.076** (0.030)		Partnered or not	0.003 (0.038)	
Partner		0.022 (0.020)	Death of children	-0.010	(0.022)
Having no child		0.006 (0.006)			
1st-stage coef. of IV	0.998*** (0.024)	0.951*** (0.020)	1st-stage coef. of IV	0.742*** (0.035)	0.712*** (0.029)
1st-stage F stat.	1763.57	2206.91	1st-stage F stat.	447.20	593.95
Observations	9331	17,006	Observations	8297	11,904

Notes: \*Significant at 10%; \*\* at 5%; \*\*\* at 1%. Numbers in parentheses are robust standard errors. The table reports 2SLS estimates of the effect of (parental) hunger incidence on each outcome for both generations with alternative samples to check sample selection and survival bias. Column (1) is based on the first-generation sample having no family member who died during 1958–1962. Column (2) adds the first-generation sample that has no child to the original sample. Column (3) is based on the second-generation sample, with both mothers and fathers having no family member who died during 1958–1962. Column (4) adds the second-generation (children) sample who passed away to the original sample. The IVs and control variables are the same as those in Table 2. The detailed definitions of the variables “partner” and “having no child” for the 1st generation, and the variable “death of children” for the 2nd generation can be found in Online Appendix A.

### 5.8. Weighting the sample with sampling weights

The last exercise is to show how results would change if we weigh the sample with sampling weights provided by CHARLS. Though the primary interest of this paper is to estimate the causal effect of hunger experience on the sampled group (elderly people born between 1930 and 1970 and their adult children), rather than extrapolating our parameter estimates to the whole population, it would be useful to show whether weighting would largely influence the interpretation of results.<sup>16</sup> Table B.5 suggests that the results are essentially robust to weighting with the sampling weights provided by CHARLS.

## 6. Conclusion

We study the long-run and intergenerational effects of early-life nutritional deprivation on one’s own and adult children’s human capital and labor market outcomes using unique individual retrospective data on hunger experience from ages 0 to 17 from China.

We find that early nutritional deprivation has a negative impact on one’s own completed education and job quality. It significantly reduces years of education and the probability of having middle school or above and increases the probability of being illiterate. This reduces the chance of having non-agricultural jobs such as government or firm jobs. For the second generation, parental early hunger experience essentially has a null effect on adult children’s human capital. In terms of labor market performance, children with parental hunger are more likely to work, but their household income is not significantly affected.

Regarding the potential mechanisms, we find that early-life hunger significantly decreases the health capital of the first generation, which may impede their human capital accumulation and the development of job skills. Moreover, individuals with hunger experience tend to spend more money on food and healthcare and have a higher share of household expenditure on education. They have more interactions with their children as well. This compensatory behavior of investing in human capital and extra attention given to children might explain the attenuating effects of hunger over generations.

Heterogeneity analysis suggests that the adverse hunger effect on the first generation’s human capital is rather universal, and subgroups with higher educational potential (male and high SES groups) or from regions with more severe famine are slightly more affected. The job quality reduction is mostly driven by females and residents from regions with less severe famine. For the second

<sup>16</sup> We cannot exclude the possible heterogeneous effects of the very old first-generation cohorts, or the very young second-generation cohorts that we dropped from our sample. So, it is unclear whether simply weighting with sampling weights will yield the right population local averaged treatment effect or not (Solon, Haider, & Wooldridge, 2015).

generation, the negative impact of parental hunger does not pass on to children for most groups, and children of the low SES group even have a lower probability of being illiterate. The increasing probability of working is mainly driven by female, low SES group, and adult children from non-severe regions.

Compared with recent studies exploiting exposure to the Great Chinese Famine, our results are smaller in size for both generations. Lay and Norling (2020) find that being born in a severe famine area reduces completed years of schooling by 3.8 years, while our IV estimates suggest that childhood hunger reduces one's years of education by 1.15 years. This is not surprising because we investigate more general cases of nutritional deprivation instead of an extreme case such as severe famines. Our OLS estimates on the first generation's educational attainment are similar to Cui et al. (2020) which also studies more general cases of nutritional deprivation. More importantly, even a non-extreme indicator like self-reported hunger experience in our study would also have negative effects on human capital for the first generation, which implies the necessity of early-life nutritional interventions (e.g., Fang & Zhu, 2022; Lundborg, Rooth, & Alex-Petersen, 2022).

In terms of the second-generation effect, Kim et al. (2014) find that a one standard deviation increase in maternal in-utero exposure to the Great Famine reduces the probability of entering junior secondary school by 5 to 7 percentage points. Li and Menon (2021) find that daughters of rural mothers with severe in-utero famine exposure are 14.7 p.p. less likely to have high school education or above. However, by using a general measure of nutritional adversities and a sample covering urban and rural regions, we do not find a significant persistent impact on the human capital of the next generation. Our findings imply that the human capital effects of non-extreme nutritional adversities are likely to attenuate over generations, yet early-life nutritional interventions might still help bridge the educational gap caused by parental hunger experience.

Limited by the data, we do not have much information on the children's outcomes (e.g., cognitive ability, objective health measures, individual wage, etc.). More information on children would further help uncover the pathways of nutritional shocks over generations. Future evidence from other countries or cultural and economic contexts would also complement the understanding of the long-run and intergenerational effects of nutritional deprivation.

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## Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.chieco.2022.101914>.

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