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ABSTRACT

The Long Run Health and Economic Consequences of Famine on Survivors: Evidence from China's Great Famine*

In the past century, more people have perished from famine than from the two World Wars combined. Many more were exposed to famine and survived. Yet we know almost nothing about the long run impact of famine on these survivors. This paper addresses this question by estimating the effect of childhood exposure to China's Great Famine on adult health and labor market outcomes of survivors. It resolves two major empirical difficulties: 1) data limitation in measures of famine intensity; and 2) the potential joint determination of famine occurrences and survivors' outcomes. As a measure of famine intensity, we use regional cohort size of the surviving population in a place and time when there is little migration. We then exploit a novel source of plausibly exogenous variation in famine intensity to estimate the causal effect of childhood exposure to famine on adult health, educational attainment and labor supply. The results show that exposure to famine had significant adverse effects on adult health and work capacity. The magnitude of the effect is negatively correlated with age at the onset of the famine. For example, for those who were one year old at the onset of the famine, exposure on average reduced height by 2.08% (3.34cm), weight by 6.03% (3.38kg), weight-for-height by 4% (0.01 kg/cm), upper arm circumference by 3.95% (0.99cm) and labor supply by 6.93% (3.28 hrs/week). The results also suggest that famine exposure decreased educational attainment by 3% (0.19 years); and that selection for survival decreased within-region inequality in famine stricken regions.

JEL Classification: I10, I2, J1

Keywords: health, education, famine, China

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1 Introduction

In the twentieth century, more individuals perished from famine than from the two World Wars combined (Sen, 1981).[1] An estimated 16.5-30 million people died in China's Great Famine (1959-1961) alone. While much attention has been paid to those that die from famine, the impact of famine on those who survive has received surprisingly little coverage. Understanding the long run effects of famine on survivors is directly relevant to policy today, as it may affect long run economic growth.[2] More generally, it can help shed light on the long run effects of childhood malnutrition. As recently as 2004, World Bank Indicators reported that, worldwide, 30% of children under the age of five are estimated to be severely malnourished.[3] For famine survivors, exposure has two potentially offsetting effects. It can adversely affect fetal and early childhood development, which can in turn affect adult health, educational attainment and/or labor market outcomes.[4] On the other hand, a reduction in cohort size may have positive effects on later outcomes due to reduced competition for family resources or in the labor market.[5] This paper estimates the net of these two effects in its investigation of the long run impact of famine on survivors.

[1] See Sen (1981) and Ravallion (1997) for a detailed description.

[2] The correlation between improved health status and economic factors have been found in studies by Fogel (1994), Fogel and Costa (1997), and Smith (1999). Bloom et al. (2001) find a correlation between longer life expectancy and higher economic growth rates.

[3] Prevalence of child malnutrition (height for age) is the percentage of children under five whose height for age is more than two standard deviations below the median for the international reference population ages 0 to 59 months. For children up to two years of age, height is measured by recumbent length. For older children, height is measured by stature while standing. The reference population adopted by the WHO in 1983 is based on children from the United States, who are assumed to be well nourished.

[4] Poor health in children has been associated with lower education and/or labor market outcomes in the U.S. (Case et al., 2004), Canada (Currie and Stabile, 2004), Great Britain (Case et al., 2003; Kuh and Wadsworth, 1993; Marmot et al., 2001) and many developing countries (Behrman, 1996; Bleakley, 2002; Brinkley, 1994; Glewwe and Jacoby, 1995; Glewwe et al., 2001; Miguel and Kremer, 2004; and Strauss and Thomas, 1998). See Curie and Madrian (1999) and Curie and Hyson (1998) for a review of studies linking health to educational attainment and labor market outcomes. The latter focuses on the effects of low birth weight. Reduced height has been associated with lower income in Brazil by Strauss and Thomas (1998), and in Ghana by Schultz (2001).

[5] Easterlin (1980) discusses how the size of a generation affects the personal welfare of this members through family and market mechanisms. See Becker and Lewis (1973), Becker and Tomes (1976), Galor and Weil (2000), Hazan and Berdugo (2002) and Moav (2005) for theoretical discussions of the quantity-quality tradeoff; and see Angrist et al. (2006), Black et al. (2004), Qian (2006), Rosenzweig and Zhang (2006), and Schultz (2005) for recent empirical evidence on the quantity-quality tradeoff.

Specifically, we examine the causal effect of childhood exposure to China's Great Famine (1959-1961) on adult health and labor market outcomes almost thirty years afterwards.

There has been almost no studies to date within the economics literature on the long run impact of famine on survivors. Meanwhile, the evidence provided by medical and epidemiological studies are conflicted.[6] In general, studies of the impact of famine face two main difficulties. First, it is difficult to find appropriate control groups for famine victims. Comparing exposed cohorts with unexposed cohorts is problematic in that the results cannot disentangle the effects of famine from other changes over time. Within cohort comparison is made difficult by the general lack of data on the cross-sectional variation of famine intensity. Second is the problem of joint determination. For example, villages with poor institutions may be more likely to have both low grain reserves and poor provision of schooling, leading to increased famine intensity and reduced educational attainment for famine survivors. Then, the observed correlation between famine intensity and educational attainment for survivors will reflect the effects of the underlying institution on each outcome rather than the causal effect of exposure to famine on survivors. Alternatively, if governments target famine-stricken regions with post-famine investments such as school and hospital construction, the observed correlation will confound the effects of the famine with the effects of the subsequent programs.

The principal contribution of this paper is to address both of the problems mentioned above. First, we use the size of the surviving cohort in the county of birth to proxy for the intensity of the famine. When these data were collected, there had been little migration since the famine era due to strict migration controls. Hence, the more intense the famine, the smaller the surviving cohort. This provides a measure of regional famine intensity. Next, we address the problem

[6]Epidemiological studies on the long run impact of the Dutch Famine (1944-1945) find that famine is positively correlated with psychological disorders in adulthood (Neugebauer et al., 1999; Brown et al., 2000; Hulshoff et al., 2000); obesity (Ravelli et al., 1999); and glucose intolerance (Ravelli et al., 1998). However, these results are inconsistent with the findings from Stanner et al.'s (1997) study of a sample of approximately 600 survivors of the Leningrad siege (1941-1944). Recently, economists Gorgens et al. (2002) and Luo et al. (2006) have examined the impact of China's Great Famine. More generally (outside of the famine context), epidemiological studies have observed incomplete "catch-up" after adverse childhood nutritional shocks (Krueger, 1969; Hoddinott and Kinsey, 2001).

of joint determination by using a heretofore unknown (or unmentioned) observation about the Great Famine: famine intensity is *positively* correlated with non-famine *per capita* grain production. This is consistent with the evidence that the direct cause of the famine was over-expropriation of grain from rural areas. (See the Background section for detailed explanation). We exploit the cross-sectional variation in non-famine levels of *per capita* grain production in combination with cohort variation in famine exposure to estimate the causal impact of famine on adult outcomes. This strategy also allows us to correct any potential measurement error that may arise from using cohort size to proxy for true famine intensity.

The main analysis uses data from the *1990 Population Census*, *1989 China Health and Nutritional Survey*, the *1997 Agricultural Census*, historical climate data from China's permanent weather stations, and GIS soil and geographical data from the Michigan Data Center. The analysis is restricted to rural areas to avoid confounding effects from the Cultural Revolution (1966-1976), which was mainly an urban disturbance (Meng and Gregory, 2002; Giles and Park, 2006).[7] The results show that survival rates are *negatively* correlated with normal *per capita* grain production. The younger a child was at the onset of famine, the less likely he/she was to survive.[8] The results for adult outcomes of survivors show that childhood exposure to famine had significant negative effects on adult health and labor supply. For example, for individuals who were one year of age at the onset of famine, the famine on average decreased height by 2.08% (3.34cm), weight by 6.03% (3.38kg), weight-for-height by 4% (0.01 kg/cm), upper arm circumference by 3.95% (0.99cm) and labor supply by 6.93% (3.28 hrs/week). The results also suggest that famine reduced educational attainment by approximately 0.2 years on average. The findings show that exposure to famine has significant negative long run effects on survivors; and that the adverse effects of childhood malnutrition outweigh any potential benefits from reduced cohort

[7]See Background section for further discussion.

[8]We are unable to examine the impact of the famine on mortality rates for individuals who were elderly during the famine. The elderly, like the very young, are more vulnerable to health shocks and were likely to have experienced higher mortality rates relative to other individuals. Because this cohort would be approximately 100 years old in 1990, they do not appear in the 1990 Census.

sizes.

In addition to the main results, we investigate the possibility of selection bias such that the determinants of survival may also affect later outcomes in life. In particular, we investigate the possibility that famine survivors may be "naturally" healthier than individuals in the control group. We use the intuition that determinants of health (absent the famine) are transmitted to children whereas famine exposure is not (Gorgens et al., 2002). The findings show that taller individuals were more likely to survive the famine. Hence, selection bias may cause the main results to underestimate the adverse impact of famine. We also examine the distributional effects of famine exposure. The results show that famine decreased within-region inequality in health outcomes. This is perhaps not surprising if selection for survival has removed individuals from the left hand tail of the "health" distribution amongst survivors of the famine.

This study has several advantages over previous work. First, setting the study in China avoids confounding influences from events often correlated with the occurrence of famines, such as political conflict. Observing individuals in 1990, when China had strict migration controls, allows us to also avoid potentially confounding effects of migration. Second, the data are substantially better than those used in past studies. Disaggregated data and large sample sizes allow us to exploit cross-sectional variation in addition to cohort variation, and increase the precision of our estimates. The use of historical climate data from weather stations allow us to show that there was no "natural" disaster during the famine.[9] Third, the empirical strategy, which relies on using the level of non-famine grain output, avoids issues of measurement error faced by studies using retrospectively constructed output data for famine years. The CHNS provides rich data on health for both parents and children, which allows us to investigate the possibility of selection bias. Fourth, our explanation of the causes of the stark cross-sectional inequity of famine exposure does not rely on hard-to-explain differences in local political institutions. Finally, the results of this paper contribute to the growing literature on the long run effects of child-

[9] Past studies used official data on historical climate or recalled data from survivors. See section on background for details.

hood health shocks.[10] As an evaluation of the long run effects of childhood malnutrition, this study avoids confounding the effect of malnutrition with the effects of parental characteristics.

The paper is organized as follows. Section 2 discusses the background. Section 3 discusses the data. Section 4 presents the empirical strategy and results. Section 5 provides an interpretation for the results. Section 6 offers conclusions.

2 Background

2.1 The Great Famine

During China's Great Famine (1959-1961), an estimated 16.5 to 30 million individuals died (Coale, 1981; Yao, 1999; Peng, 1987; Ashton et al., 1984; and Banister, 1987). Figure 1A plot the population by birth year from the 1990 Population Census. The vertical band indicates the years of the famine. It shows a significant decrease in fertility and survival rates for cohorts born during (and closely before) the famine.[11] Officially, the cause of the famine was a fall in grain output due to bad weather. Several recent studies have argued that although there was a fall in output (see Figure 2A), the "three years of natural disasters" (*san nian zi ran zai hai*), was largely driven by a set of misguided policies (Kueh, 1995; Li and Yang, 2005; Peng, 1987; Yao, 1999; Yang, 1996; Chang and Wen, 1997; Perkins and Yusuf, 1984; Lin, 1990). Using official aggregated data on historical weather conditions, Kueh (1995) finds that although bad weather was a contributing factor, it was unlikely to have caused the full extent of the grain reduction necessary to explain the severity of the famine.[12] However, as Li and Yang (2005) point out, "Given the party line explanation of the GLF [Great Leap Forward] disaster, it is plausible that crop failures caused by other factors, such as the GLF policies, may have been attributed to bad

[10] Recent studies on long run effects of health shocks during childhood include studies by Almond and Mazumder (2005), Almond et al. (2005), Berhman and Rosenzweig (2005), Black et al. (2005), Bleakley (2002), Case et al. (2004), Glewwe et al. (2001).

[11] For cohorts born during the famine, we will overestimate infant mortality because we cannot distinguish increased mortality from reduced fertility. To address this, the empirical analysis will be conducted on a sample that excludes individuals born during the famine as well as the full sample of individuals.

[12] Kueh uses official data on sown area covered and affected by disasters, see Table AA.8., Appendix A (pg. 299) in Kueh, 1995.

weather".[13] They attempt to mitigate the government bias by using recalled weather data from villagers who were alive during the famine. They find that adverse weather conditions explain at most 12.9% of the reduction in agricultural production during the famine. However, recall data may suffer from large systematic biases. Survivors may not recall weather conditions from 40 years ago very accurately. And their recollections may have been influenced by the official explanation.

We obtained historical climate data from China's 205 permanent weather stations and county level data on non-famine grain output.[14] Figure 3A plots the annual mean precipitation and mean temperature by year in the 8 provinces included in this study.[15] There is no noticeable difference during the famine years. The relationship between natural conditions and grain output can be examined more directly. We use county-level grain output and weather conditions for non-famine years to estimate the correlation between natural conditions and output. We then use these estimates and climate data from 1959-1961 to predict output during the famine years. If the famine was caused by natural conditions, the predicted output for famine years should be significantly different from normal output. Instead, we find that the predicted output is highly correlated to actual non-famine output.[16] Alternatively, we can also examine the correlation between survival and historical weather conditions. Figure 3B plots a proxy for survival at the county level (the ratio of famine birth cohort population in 1990 to non-famine birth cohort population in 1990) against weather conditions during the famine relative to normal periods (the ratio of famine period rainfall to non-famine rainfall, and the ratio of famine period temperature to non-famine temperature).[17] There is no visible correlation. These results all show that the famine was unlikely to have been caused by "natural" disasters.

Past studies have suspected the causes of the famine to include labor and

[13] Li and Yang (2005), footnote 23.

[14] See section on data for details.

[15] The geographic scope of this study was determined by the CHNS, which surveyed 8 provinces in 1989.

[16] See Appendix for details.

[17] Strict migration controls largely prevented rural individuals from leaving their region of birth until the 1990s. Hence, the population of the famine cohort in each county can be interpreted as those who survived. We divide this number by non-famine cohort sizes in order to normalize for county size.

acreage reductions in grain production (e.g., Peng, 1987; Yao, 1999), implementation of radical programs such as communal dining (e.g., Yang, 1996; Chang and Wen, 1997), reduced work incentives due to the formation of the people's communes (Perkins and Yusuf, 1984), and the deprivation of peasants' rights to exit from the commune (Lin, 1990).[18] A recent study by Li and Yang (2005) improved upon past studies by compiling a panel of province level data that included conventional variables in the production function, nutritional status of agricultural workers, climate, and institutional variables in order to quantify the relative contributions of various hypotheses to the collapse of grain output. They find that the major contributing factors to this collapse of grain production were over-procurement of grain from rural areas and diversification of resources away from agriculture.

Over-procurement in 1959 led to a decrease in rural workers' physical capacity to produce grain. The reduction in work capacity along with the consumption of inputs such as seeds in the winter of 1959 prolonged the famine. In 1960, the central government had decreased procurement and returned rural workers back into the agricultural labor force (Li and Yang, 2005). But the famine did not end until 1961, when the central government distributed national grain reserves and accepted food aid. Production soon recovered to pre-famine levels.

An obvious question is why over-procurement occurred in the first place. Anecdotal evidence and recent studies have formed a consensus on the occurrence of over-reporting of grain output by rural areas. In 1958, the central government promised that collectivization would increase Chinese grain yields dramatically, and that this grain would be used to support the urban industrial sector and other communist countries. Popular confidence in this program was boosted by bumper harvests during 1955-1958 (see Figure 2A).[19] Local leaders were under pressure to maintain high levels of grain delivery to the central government. According to anecdotal evidence, in 1959, when output returned to

[18]Lin (1990) argues that the removal of exit rights of hard workers destroyed reduced work incentives for shirkers, and hence decreased overall grain production.

[19]The current official explanation for the bumper harvest is good weather (as the famine is caused by bad weather). Anecdotal evidence suggests that the bumper harvests were at least partially an outcome of increased cropping that wore out the soil and could not be sustained over time.

the normal levels, local leaders exaggerated output. Alternatively, the central procurement agency may have enforced procurement targets that were based on past bumper harvests rather than the actual 1959 harvest. Both are consistent with the data. Columns (1) and (2) of Table 2 show that although output had decreased by 15% from 1958, procurement levels actually *increased* by 23%. The proportion of grain procured increased from approximately 25% of total output in 1958 to almost 40% in 1959 (see Figure 2B). The agricultural procurement policy at the time allowed peasants to retain subsistence-level grains while the central government expropriated all surpluses. Market trade in food stuffs and labor migration were strictly and largely successfully prohibited. Consequently, over-reporting and/or over-targeting, which led to over-procurement, caused the retained amount of food to be below subsistence levels (Johnson, 1997). Figure 2C shows the sharp drop in grain retention per capita from approximately 270 kg/person in 1958 to only 190 kg/person in 1959. Since grain was and is the main source of calories for Chinese laborers, this drop will be reflected in a large drop in overall calorie consumption. Ashton et al. (1984) show that daily food intake fell to 1,500 calories per day by 1960.[20]

This begs the question of how rural leaders determined the amount which to over-report and/or how central authorities determined the procurement quantity. To the best of our knowledge, historical procurement target data from the famine years are not available, and procurement target data at disaggregated levels (e.g., county) could not be found for any years. To gain insight on how procurement targets are set, we relied on anecdotal evidence and procurement target data at the national level from the 1980s, when grain targets were still set for central procurement. Anecdotal evidence suggests that production targets for each region were set as a proportion of historical production in order to mitigate problems of peasants hiding grain from (under-reporting output to) central authorities (Oi, 1989). This is consistent with observations that the early communist government implemented many severe measures in order to

[20]Based on the food content table provided by the Institute of Nutrition and Food Hygiene of China, 1 kg grain (simple average of rice, wheat flour, and other grain) has approximate 3587 calories. Hence, 190kg=1867 calories per day, which is 11% lower than the 2100 calorie per person per day the minimum nutrition requirement.

appropriate grain that peasants wished to hide. It is also consistent with the fact that, in the 1980s, procurement targets, as a proportion of past production, were increasing with historical production (Figure 2D).

The pattern of proportional appropriation is also consistent with anecdotal evidence we gathered from a series of interviews conducted with survivors from the famine.[21] Interviewees recalled grain production, reported grain production, and the remaining grain after expropriation. While we cannot take the numbers reported so many years after the famine literally, the anecdotal evidence did give two insights. First, villages seemed to have systematically over-reported grain production proportional to actual production. Second, regions that produced more grain suffered from the famine more. The second fact follows from the first. If regions over-report proportional to actual output and all surplus production is expropriated such that the government takes the difference between reported output and subsistence needs (which are largely fixed over the span of a few years), then regions with higher actual production will retain less grain.

Table 1 provides an illustrative example of this phenomenon. Assume that counties A and B have the same subsistence needs, 200 units of grain. But county A produces 200 units, whereas county B produces 300 units because of better climate and terrain. (Note that this scenario assumes that migration is restricted such that workers cannot move from county A to county B). If both counties over-report production by 10%, the reported yields from counties A and B are 220 and 330. Then, the amounts the government will procure from each are 20 and 130. Counties A and B will retain the difference between their true yields and the procured amount, which will be 180 and 170 units. Consequently, county B, which normally produces more grain will be 15% below subsistence level whereas county A will only be 10% below subsistence level. Later in the paper, we investigate whether the anecdotal evidence reflected the situation at large by regressing survival on non-famine grain production for all the counties in the 8 provinces of our study. Note that we do not attempt to distinguish the hypothesis of proportional production target setting by the central authority from proportional over-reporting by the local authorities. Most likely, both

[21] The authors of this paper conducted 6 interviews with famine survivors living in villages in Hebei and Anhui provinces in 2003 and 2001, respectively.

occurred simultaneously as a result of central pressures to increase grain output.

Next, we examine how much over expropriation was necessary to produce the Great Famine under this hypothesis. We use aggregate data on production and retention presented in Li and Yang (2005), as shown in Table 2. Given the likelihood of misreporting for famine period data, the following calculations should be interpreted very cautiously. For convenience, we convert the aggregate measures in Li and Yang (2005) to county-level measures by dividing the former by the number of provinces in their sample (21) and the number of counties per province (approximately 100). On average, each county produced approximately 80,952 tons of grain on average. We use the minimum of the reported per capita retained grain for the nine years prior to the famine as the subsistence level (228 kg/person in 1954). In 1959, the rural population in the Li and Yang (2005) sample is approximately 549 million people. We multiply that by the annual per capita subsistence and divide by the total number of counties to find that counties on average needed approximately 56,667 tons for subsistence. The reported data on grain retention in column (3) of Table 2 show that there was approximately a 18% decrease in grain retention in 1959 from 1958. Column C of Table 1 shows that local leaders needed to have over-reported (or the central government needed to have over-procured) by 13% on average to generate the observed 18% decrease. The lack of historical data on the difference between grain output and projected output makes it difficult to ascertain whether 13% over-reporting is plausible. But given reports of wastes of entire villages' annual grain output due to systematic inefficiencies from this period, one can see how a village leader would not view 13% over-reporting as too much out of the norm.[22]

It is important to note that urban areas were largely insulated from rural areas. Although food was severely rationed in urban areas during the famine, the extent of the famine there was far less than in rural areas. Almost certainly, information controls prevented individuals living in urban areas from realizing the full extent of the famine. There is very little written history about the

[22]Studies of the collectivizations cite incidences of huge systematic inefficiencies. For example, local leaders were punished if grain was not put by side of roads for pick up after harvest. But they were not punished if the grain was rained out and destroyed. Hence, local governments would spend weeks piling the year's grain output by the road. And if there was bad weather, they would lose the grain (Oi, 1999).

perceptions of the Great Famine within China. Anecdotal evidence suggests that urban residents had very little knowledge of the severity of the famine. The strict control on information may partially explain the observation that there was little out-migration from famine-stricken areas. Unlike previous famines in Chinese history, there is little evidence of begging in cities by individuals from famine-stricken areas.[23]

Despite being isolated from the full extent of the famine, urban areas cannot be used as a comparison group for rural areas because both they are subject to different policies which may produce confounding results. One such factor is the Cultural Revolution (1966-1976) which, as we have stated previously, primarily affected urban areas. The Cultural Revolution caused widespread closings of schools for approximately 5 years. Children who survived the famine will be in school during the Cultural Revolution. Hence, comparing the famine cohort between urban and rural areas would compare outcomes for two different treatments rather than a treatment and a control. We therefore restrict the sample to individuals living in rural areas. Our empirical strategy will be robust to the occurrence of school disruptions in rural areas as long as school closings were not correlated to famine intensity.

2.2 Conceptual Framework

Exposure to famine at young ages affects adult health and labor market outcomes through two main channels. First, it adversely affects childhood health,

[23] There are many questions for which we can only have speculative answers because information is limited. One is why peasants allowed the government to take away so much of their grain. A potential explanation is that collectivization (the pooling of all output and eating from a communal cafeteria) decreased individual accountability and the accuracy of individual information about production. In other words, people did not realize how little food they would have left. Alternatively, collectivization may have given people the delusion that the government would provide for them in return for what was expropriated. A third explanation provided to the authors of this paper through personal interviews is that some villagers did protest the expropriation, knowing that they would not have enough food left but were overcome by force. Another question is why there was no migration to other less affected rural areas or to cities. Urban residents and rural residents outside of famine regions had no idea of the severity of the famine until very recently. One possibility is that effective information controls convinced people that no one had food. However, anecdotal evidence from survivors sometimes suggests that migrants in search of food were prevented from leaving their villages by local officials and militia. There is little or no evidence that the central government mobilized non-local forces such as the People's Liberation Army to prevent migration or to enforce procurement.

which is a product of genetic endowment, fetal health (in utero nutrition), nutrition and other forms of investment (e.g., health care) during childhood. The famine potentially also reduced the quality and/or quantity of other forms of investment into children by reducing the health status of parents. Childhood health can in turn affect adult outcomes directly and indirectly (Kuh and Wadsworth, 1993). Poor health during childhood can have a direct effect on adult health, and through adult health can affect work capacity and labor supply. Barker (1995) and Ravelli et al. (1998) have found that nutrition in utero can affect health status in middle age, through its impact on chronic conditions such as coronary heart disease and diabetes.[24] Poor childhood health could also decrease educational attainment by decreasing returns to education or by increasing the costs of school attendance (Curie and Madrian, 1999; Kremer and Miguel, 2004, Maccini and Yang, 2005). This may in turn affect labor supply and/or wages later in life. Second, exposure to famine could potentially have a positive effect by reducing the cohort size of exposed individuals and hence reducing labor market competition and competition for family resources.

This paper will estimate the net effect of exposure to famine: the sum of the adverse effect of malnutrition and the potentially positive effects from smaller cohort sizes.

3 Data

This paper matches the 1% sample of the 1990 *Population Census* with the 1989 *China Health and Nutritional Survey* (CHNS), the 1997 Agricultural Census and GIS data on natural conditions at the county level. The 1990 *Population Census* contains 32 variables including birth year, region of residence, whether an individual currently lives in his/her region of birth, sex, and relationship to the head of the household. The data allows children to be linked to parents. Because the identification is partially derived from the region of birth, the sample is restricted to individuals who reported living in their birth place in 1990. The CHNS uses a random cluster process to draw a sample of approximately

[24]Experimental work by Ozanne and Hales (2004) using laboratory mice find that lab mice that are underfed *in utero* but who are well-fed after birth catch up rapidly. However, they die earlier than mice that are also well-fed in utero.

3,800 households with a total of 16,000 individuals in eight provinces that vary substantially in geography, economic development, public resources, and health indicators. The survey includes a physical examination of all individuals as well as information on labor supply, work intensity and wages. It also allows us to link children to parents as long as the children are living in the same household as their parents.

The GIS data is provided by the Michigan Data Center. The climate data contains monthly historical data from 205 permanent weather stations in China. The variables include monthly mean temperature, precipitation and days of sunshine. We use GIS to calculate the distance from each county to the nearest weather station. Weather conditions at the nearest station are used to proxy for the weather conditions of each county. The GIS data also include information on terrain. (Hilliness has a significant effect on the cost of producing grains. For example, hilly areas must invest in terraces in order to produce paddy rice).

The 1997 *Agricultural Census* is the only available data that gives a consistent measure of output at the county-level. Using grain production in 1997 as a proxy for non-famine grain production in the 1950s and 1960s will introduce measurement error. But more problematic is the possibility that the central government targeted post-famine agricultural investments towards regions that suffered more during the famine. Then, 1997 grain production will be an outcome of the famine rather than an indicator of pre-famine output. We investigate this possibility directly by examining how much of 1997 per capita grain output can be explained by natural conditions. Table 3 shows the results from regressing county-level per capita grain output on climate variables such as mean rainfall and temperature; the standard deviation of rainfall and temperature; and other geographic controls (see equation (3) in the Appendix). Column (1) shows that the adjusted R-Square estimate is 0.73. This suggests that the variation in 1997 production was largely driven by natural conditions and was not likely to be an outcome of the famine.

The data are collapsed and matched by county and birth year. The number of individuals in each county-birthyear cell is retained so that all regressions are population weighted. To avoid confounding the estimates with the impacts of family planning policies and the Cultural Revolution, the sample is restricted to individuals living in rural areas who were born during 1943-1966. The shaded counties in Map 1 are the counties for which the 1990 Census data can be matched to the grain production data from the 1997 Agricultural Census. Figure 1A plots the 1% sample of the total population by birth year. It shows that, overall, the cohort size for individuals born during the famine is approximately 50% of other cohorts. To see the precision of the variation in cohort size, we regress cohort size on birth year dummy variables. The coefficients are plotted in Figure 1B with 95% confidence intervals. For the empirical analysis, the logarithm of cohort size for each county and birth year will be used as the measure of famine intensity for individuals born in that county and birth year. The benefit of using cohort size as a measure of famine intensity is that it is a measure that can be easily obtained from any famine. (For individuals born during the famine, increased infant mortality rates cannot be separated from reduced fertility rates. Hence, the analysis will use a restricted sample excluding individuals born during the famine in addition to the full sample).

To observe the cross-sectional distribution of famine intensity, we calculate the ratio of cohort size of individuals born during the famine and the mean cohort size of individuals born during non-famine years. This fraction is decreasing in famine intensity. Figure 4 shows that famine intensity varied widely. In some counties, famine cohort sizes were only 25% of normal cohort sizes, whereas other counties were relatively unaffected. In some counties, famine cohort sizes are actually larger than non-famine cohort sizes. To see the geographic dispersion of famine intensity, we transform this measure into 20 categories of famine intensity, each represented by a different shade of color on Map 1. Lighter shades represent counties that suffered higher famine intensity. We see that famine intensity varied widely within provinces. Many neighboring counties experienced very different levels of famine intensity.

Table 4 shows the descriptive statistics for the full sample and restricted sample which exclude those born during the famine. The descriptive statistics are very similar. The average age of the sample in 1990 is 45. The sample is disproportionately male, which is consistent with the general boy-biased sex imbalance in China.[25] Households on average have five individuals. The average years of education is approximately six. Women are less educated than men. Height is commonly used as a measure of the stock of nutritional investments during childhood (Fogel et al., 1982; Fogel, 1994; Steckel, 1986; Micklewright and Ismail, 2001). Average height is approximately 160cm, four centimeters less than the average height of the same cohort in Japan. Another potentially interesting measure of health is blood pressure. The commonly used threshold for high blood pressure is 140/90 mmHg (millimeters of mercury).[26] The sample systolic and diastolic blood pressure measurements are within the normal range and do not indicate high blood pressure, which may indicate an increased likelihood of heart disease. Upper arm circumference is a crude measure of "wasting", the body's inability to retain body mass. On average, upper arm circumference in approximately 25cm, similar to that of the same cohort in Japan. The main economic outcome we examine is hours worked per week as a reflection of work capacity. Adults in the sample work approximately 50 hours per week, on average. (We do not examine wages because they did not reflect the marginal product of labor in rural China when the data was collected).

4 The Long Run Impact of Exposure to Famine

4.1 Identification

Region and year of birth jointly determine an individual's exposure to the famine. The Ordinary Least Squares (OLS) specification uses a simple fixed effects model. Like differences-in-differences, changes across cohorts which affect different regions similarly are differenced out by the comparison across regions. Cohort invariant differences between regions are differenced out by the comparison across cohorts. For example, if regions with bad institutions are more prone to famines and institutions do not change over short periods of time, then differences in institutions will be controlled for by region fixed effects. There are two potential concerns for this strategy. First, cohort size may measure famine

[25] See Qian (2005) for a detailed description of sex selection in China.

[26] The systolic pressure measures the pressure in arteries when the heart is forcing blood through. The diastolic pressure shows the pressure in arteries when the heart relaxes.

intensity with error. This will most likely bias the OLS estimate towards zero (see Appendix). Second, the intensity of the famine and adult outcomes of survivors may both be outcomes of unobservable factors such as regional economic variables. For example, poorer regions with less grain reserves may be more susceptible to adverse food shocks, and have faster economic growth (e.g., mean reversion) which has a direct effect on investment into health and education for famine survivors. Then, the observed correlation between famine intensity and outcomes for survivors will reflect the effect of the underlying economic variable rather than the causal relationship between famine intensity and survivors' outcomes. Alternatively, the government may have targeted post-famine public investments that are beneficial for survivors (e.g., schools, health clinics) at the most adversely affected regions. In both cases, OLS will underestimate the true impact of famine.

To address these problems, we exploit three facts: 1) famine intensity was positively correlated with non-famine grain production, 2) that children who were younger at the onset of the famine would have been more vulnerable to disease and malnutrition, and 3) that children born after the famine should not have been affected.[27] Therefore, we use the interaction terms between the amount of non-famine grain production in the county of birth and dummy variables for the year of birth as instruments for famine intensity. Only the combination of the two can be interpreted as exogenous. The key identification assumption for the Two Stage Least Squares (2SLS) estimate is that normal grain levels as measured in 1997 and the adult outcomes of famine survivors in 1990 are not jointly determined by some omitted variable. For example, the exclusion restriction will fail if the government targeted post-famine agricultural investment at regions that suffered proportionally more from the famine. We ruled out this possibility in the previous section by showing that 1997 per capita output is largely explained by natural conditions.[28]

[27]Salama et al. (2001) follow a sample of Ethiopians through a short famine period (December 1999 to July 2000). They find that 80 percent of those who died were children aged less than 14 years of age.

[28]If non-famine grain output is correlated with the quality of institutions, then the correlation is likely to be positive (e.g., richer regions have better schools). If, in addition, institutions persist over time, then the negative impact of famine may be offset by the positive impact of good institutions and our strategy will *underestimate* the true impact of famine on survivors.

The empirical strategy may underestimate the true effect of childhood malnutrition on adult outcomes for two reasons. First, the famine caused a reduction in the cohort size as well as a reduction in the nutritional investment of survivors. If smaller cohort sizes reduce competition in the labor market or for family resources, the main results will estimate the net effect of the *adverse* effects from malnutrition and the potentially *positive* effects from smaller cohort size. Second, there may be positive selection bias for survival such that "stronger" children were more likely to survive the famine. For example, the average "natural" endowment of health will be higher for survivors than for individuals in the control group. We discuss this in detail later in the paper.

4.2 OLS

To estimate the correlation between famine intensity and adult outcomes, we estimate the following equation using left hand side variables of the fraction of males, the logarithms of educational attainment, height, weight, weight-for-height, systolic and diastolic blood pressure, upper arm circumference, arm skin-fold measure, and number of hours worked per week.

$$\log(\text{edu}_{it}) = \beta \log(pop_{it}) + \gamma_i + \rho_t + \varepsilon_{it}$$

We regress the variable of interest on the logarithm of the population in county i, born in year t, $\ln pop_{it}$; county fixed effects, γ_i; and birth year fixed effects, ρ_t. Standard errors are clustered at the county level. β is an elasticity of the left hand side variable with respect to cohort size. Because the famine reduced population, adverse effects of the famine should be reflected in a *positive* β. Table 5 Panel A shows the results using the full sample. The results are not statistically different from zero. Panel B shows the results for the restricted sample excluding individuals born during the famine. This excludes the years for which the data cannot separate mortality from fertility. If selection is negatively correlated with age because younger children are more vulnera-

As a robustness check of the 2SLS strategy, we also use the interaction terms of the variables on natural conditions with the birth year dummy variables as instruments instead of the interactions terms of per capita grain production and birth year dummy variables. The estimates are similar in magnitude to the main results but are not statistically significant. They are not reported in the paper.

ble to adverse nutritional shocks, then the restriction should also mitigate the selection bias. The estimates are generally larger in magnitude in Panel B, which is consistent with the selection hypothesis. However, the estimates are not statistically different from zero.

4.3 Two Stage Least Squares

The OLS estimates will be biased towards zero if famines were more likely to occur in regions with poor institutions that also reduced investment into the health and education of children who were not exposed to the famine. In addition, cohort size may measure true famine intensity with error and cause attenuation bias (see appendix). We address this by exploiting cross sectional variation in normal per capita grain output together with cohort variation in famine exposure to estimate the causal effect of famine exposure on adult outcomes. Map 2 shows non-famine per capita grain output by county. More per capita grain output is reflected in lighter shading. The correlation between per capita grain output and famine intensity can be visually observed by comparing Map 2 with Map 1.

The first stage equation estimates the effect of normal per capita grain production on survival. This equation also tests the hypothesis of over-reporting.

$$\log(pop)_{it} = \sum_{t=2}^{T} \beta_t (\log(grainpc_i) * biryr_t) + \alpha + \gamma_i + \delta_t + \varepsilon_{it} \tag{1}$$

We regress the log of the number of individuals in county ι, of birth year t on: the interaction term between the log of amount of grain produced per capita in county i, $grainpc_i$, and a dummy variable for being born in birth year t, $biryr_t$; county fixed effects, γ_i; and, birth year fixed effects, δ_t. The reference group is comprised of individuals born in 1943. This group and all of its interactions are dropped. All standard errors are clustered at the county level. Proportional over-reporting or over-procurement predicts that β_t should not be statistically different from zero for individuals born after the famine, $t >= 1961$, and for individuals who were too old to be affected by the famine. The key prediction is that β_t should be negative for individuals born during and closely before the famine. If vulnerability to nutritional shocks is negatively correlated with age, then the absolute value of β_t should be larger for those who were younger at the onset of the famine. The estimates are shown in Table 6. The estimates for individuals born between 1955 and 1960 are highly statistically significant. The coefficients and their 95% confidence intervals are plotted in Figure 5. The figure shows that the absolute value of the elasticity was up to 0.1. A 1% increase in normal per capita grain output is correlated with up to 0.1% less in cohort size (survival). Alternatively, a one standard deviation increase in normal grain production caused up to a 8% decrease in cohort size (survival). It also shows that cohorts born 1955-1960 (whose age was approximately one to six years during the famine) were affected. Within the affected cohorts, survival rates were negatively correlated with age.[29]

Next,we estimate the reduced form effect of normal per capita grain production on survivor outcomes.

$$\log(height)_{it} = \sum_{t=2}^{T} \beta_t (\log(grainpc_i) * biryr_t) + \alpha + \gamma_i + \delta_t + \varepsilon_{it} \qquad (2)$$

This is identical to the first stage equation with the exception that the left-hand-side variables are the outcomes of interest for the second stage equation. The results are shown in Table 7. As an illustrative example, the coefficients for height are plotted in Figure 6. If the instruments affect the outcomes through famine, then the pattern of the effect of the instruments on the outcomes of interest should be similar to the pattern of the effect of the instruments on the endogenous right hand side variable. Figure 5 shows that this is the case. It shows that normal per capita grain production has a negative effect on the height of individuals born during 1953-1958. That there is no effect for individuals born during famine years may be due to positive selection for survival if the selection is stronger for younger ages or if survivors are being selected on different attributes.

The 2SLS estimates are shown in Table 8. Panel A shows the results for the full sample, and Panel B shows the results for the restricted sample. The results

[29] We cannot estimate the impact of the famine on individuals who were elderly at the time of the famine. Additionally, because only individuals who survived until 1990 are observed, this analysis cannot disentangle child mortality from a decrease in fertility due to the famine. However, this should only affect the results for individuals born during the famine years, 1959-1961.

are larger in magnitude in the restricted sample for every outcome except the number of hours worked per week. The results in Panel A show that in the full sample, famine exposure as reflected by a reduction in cohort size has a negative effect on all outcomes except education. The estimates are statistically significant for weight, weight-for-height, and labor supply. The estimate for upper arm circumference is almost statistically significant. The estimates for the other outcomes are not statistically different from zero. Panel B shows the estimates using the sample excluding individuals born during the famine. This avoids confounding increased mortality with reduced fertility and mitigates the selection bias if selection for survival is negatively correlated with age at the onset of the famine. Most estimates are larger in magnitude relative to those from the full sample. The estimates show that a famine which reduces cohort size by 1% decreases height by 0.08%, weight-for-height by 0.16%, and upper arm circumference by 0.16%. The results suggest that famine exposure may also reduce educational attainment. But the estimate is not statistically significant. There is no effect on the fraction of males, systolic or diastolic blood pressure, and body fat (skin fold). The estimate for labor supply from using the restricted sample has the same sign as the estimate for the full sample but is smaller in magnitude and not statistically significant.

4.4 Calibration of the Results

To calculate the average effect of the Great Famine on survivors, we construct a cohort trend in population growth to predict cohort sizes in the absence of famine. We use the 1958 birth cohort for this example. Figure A1 plots population by cohort and the de-trended population by cohort. It shows that the size of the actual 1958 birth cohort is approximately 25% less than the size of the predicted cohort size. The 2SLS estimate from Panel B of column (3) in Table 8 shows that a famine that reduces cohort sizes by 1% will reduce height of survivors by 0.083%. The average effect of exposure to famine in percentages is the product of 0.083 and -25. The famine decreased height of survivors by 2.08% on average. To estimate the level effect, we multiply the average percentage effect by the sample mean of the outcome (160.77cm for height) and divide by 100. Hence, for the 1958 cohort, the famine reduced the height of survivors by 3.34 cm on average.

Table 9 shows the calibrated effects of famines for the affected birth cohorts (1953-1960). Only outcomes for which the 2SLS estimates are statistically significant are included. For cohorts born prior to 1959, we show calibrated effects using the 2SLS estimates from the full sample and the restricted sample. For the cohort born during the famine, we only use 2SLS estimates from the full sample. The results show that for the 1958 cohort, exposure to famine decreased height by 3.34cm, weight by 3.38kg, weight-for-height by 0.01 kg/cm, upper arm circumference by 0.99cm and work hours by 3.28 hours per week. For individuals born during the famine, the imputed effects are on average similar with those of the 1958 cohort, with the exception of hours-worked-per-week. Exposure to famine reduces labor supply of the individuals born during the famine by 10.65 hours per week. The calibrations show that the impact of the famine decreases dramatically with age at the onset of the famine. For example, the effect of famine on height was on average almost 500 times larger in magnitude on individuals who were age one at the onset of the famine relative to those who were age two or three.

4.5 Selection Bias

The main results will underestimate the true effect of the famine if the determinants of survival also positively affect health and labor outcomes later in life. In other words, if survivors are, on average, born with more robust health, then a comparison between survivors and the control groups will underestimate the true famine effect.[30] On the other hand, parents may prefer that their children have equal chances of survival and invest more in weaker children. This will offset the effect of being "naturally" healthier. Following the intuition first proposed by Gorgens et al. (2002), we investigate the possibility of selection bias directly by using the fact that factors determining health absent of the famine (e.g., household income, access to medical care) are transmitted to children,

[30] Friedman (1982), using data on slave mortality, observed that shorter slaves experienced higher mortality rates. This suggests that the remarkable catchup in slave height observed by Steckel (1986) may have been biased by excessive deaths of short slaves.

whereas exposure to famine is not. In the *China Health and Nutritional Survey* (CHNS), we link children to their parents.

Figure 7A plots the age-region-adjusted height distribution for individuals born during the famine and individuals born after the famine separately by gender. It shows that there is no observable difference in the mean (or variance) of height between famine survivors and those in the control group. Figure 7B plots the age-region-adjusted height distribution for those children whose parents were both born during the famine, and those children for whose parents were both born after the famine. The figure shows that although the shape of the distribution is similar, children whose parents were born during the famine are on average taller. We find a similar pattern for weight-for-height. The results suggest that there is positive selection for survival and that the pattern is different between male and female children who were exposed to the famine. Using this method, we did not find evidence of selection for other observable outcomes.

4.6 Distributional Effects of Famine

The main analysis examines the effect of exposure to famine on the mean of the distribution of outcomes. These estimates will fail to describe the full distributional impact of famine unless if famine affects both the center and the tails of the distribution in the same way. To investigate the impact of famine on outcomes for the tails of the distribution, we estimate the effect of famine on the 10th, 25th, 50th, 75th and 90th percentiles of the distribution of health and labor market outcomes. The empirical strategy is similar to the main analysis. But instead of averaging the micro data to county-birth year cell means, we aggregate the data to each percentile of each county-birth year cell. Only the left-hand-side variable is affected by this alternative aggregation method because the right-hand-side variable of interest and the instruments vary only at the county-birth year level (it does not vary at the individual level within each county-birth year cell). The advantage of this method over Quantile regressions and Quantile instrumental variables is that we are able to control for county fixed effects.[31]

Appendix Table A2 show the descriptive statistics for each percentile. Table 10 show the OLS and 2SLS estimates. For causal inference, we focus on estimates using the restricted sample excluding individuals born during the famine. (Estimates from the full sample are shown in Appendix Table A3). Panel A shows the estimates for county-birth year cell means from Table 9. Panels B, C, D, E and F show the estimates for the mean effect on the 10th, 25th, 50th, 75th and 90th percentiles of each county-birth year cell. We focus only on the outcomes for which the famine had statistically significant effects on the cell mean. The OLS estimates in columns (1)-(5) are generally increasing in magnitude with the percentile of the distribution. However, they are mostly not statistically significant. The 2SLS estimates for height, weight, weight-for-height and upper arm circumference in columns (6)-(10) exhibit a similar pattern to the OLS estimates. They are larger in magnitude for higher percentiles. They are statistically significant. For example, famine exposure as measured by cohort size has no effect on height for the 10th and 25th percentiles. But for the 50th, 75th and 90th percentiles, a famine which decreases cohort size by 1% will decrease height by 0.07%, 0.08% and 0.11%. The results for upper arm circumference are even more stark. There is no effect at the 10th and 25th percentiles. At the 50th, 75th and 90th percentiles, a famine that decreases cohort size by 1% will decrease upper arm circumference by 0.14%, 0.23% and 0.31%. The estimates for labor supply are not statistically significant at each percentile. The estimates for the 90th and 10th percentile are statistically different from each other only for upper arm circumference.

Next, we use these estimates to calibrate the average effect of famine exposure by percentile. Table 11 shows the estimated impact of famine for the 1958 and 1959-1960 birth cohorts using the 2SLS estimates from Table 10 and the cell means from Table A2. Figures 8A and 8B plot the average calibrated percent-

[31] The main difference is that we are examining the average effect of famine on the tails of the distributions within each county-birthyear cell, whereas Quantile techniques would estimate the effect of famine on the tails of the entire distribution of outcomes. For example, our method can shed light on the effect of famine on inequality within famine regions, while Quantile techniques would give insight to inequality within the entire population. The two methods would be similar if the distribution within each county-birth year cell is identical to the distribution in the population.

age and level effects of famine exposure for the 1958 cohort by percentile. They show that exposure to famine had a larger adverse impact at higher percentiles. In other words, when comparing individuals exposed to famine with individuals not exposed to famine, those in the 90th percentile are relatively worse off than those in the 10th percentile. For example, for individuals in the 1958 cohort, famine reduced upper arm circumference by 7.78% (2.07cm) for those in the 90th percentile compared to 1.65% (0.38cm) for those in the 10th percentile.

5 Interpretation

The main finding of this study is that childhood malnutrition from exposure to famine significantly reduces adult health outcomes and labor supply. Our estimates are likely to underestimate the true effect of childhood malnutrition for two reasons. First, the empirical strategy estimates the total impact of famine on survivors. If reduced cohort sizes have positive effects on survivors (e.g., smaller class sizes), then the adverse effect of malnutrition will be partially offset. Second, the empirical evidence suggests that there was positive selection on survival. Investigating the exact mechanisms of selection would be an interesting avenue for future research.

The fact that the estimates for the restricted sample are larger in magnitude and more precisely estimated suggests that the extent of selection for survival is negatively correlated with age at the time of exposure. Labor supply is the only outcome for which famine exposure had a larger impact on the full sample than the restricted sample. This could reflect a reduction in health status due to the famine that is not reflected in the data, or a reduction in educational attainment due to the famine. However, the results show that exposure to famine had a smaller effect on the educational attainment of individuals born during the famine relative to individuals born before the famine. This is inconsistent with the latter hypothesis. These results together suggest that although individuals born during the famine may be more highly selected on latent health variables as reflected by observable outcomes such as height, there are other dimensions (such as lower work capacity) in which their health status was more adversely affected than children who were slightly older; but this cannot be captured by the health measures reported in the CHNS.

The results of the effect of famine by percentiles yield the perhaps surprising result that famine decreased inequality in long run health and labor supply outcomes within famine-stricken regions. One potential explanation for the reduction in inequality for famine-stricken regions is selection. If individuals in the left tails of the distribution of health endowment do not survive the famine, then the empirical strategy compares individuals in the upper percentiles of the treatment group with those in the upper deciles of the control group. But it compares individuals in the lower percentiles of the treatment group with individuals in the control group who are from a lower decile than in the absence of selection. In other words, for the treatment group, the difference between the observed decile and the actual decile absent of selection is larger in lower deciles; the attenuation bias from selection may be larger for lower deciles.

Amongst famine survivors, we found no evidence to support the Barker Hypothesis, which predicts that individuals exposed to malnutrition will have lower life expectancy due to coronary heart disease even if other health indicators are normal during younger ages. We did not find that exposure to famine increased blood pressure, a commonly used indicator for coronary heart disease. In addition to the main analysis presented in this paper, we also used follow up rounds of CHNS data from 1997 and 2002 to investigate whether the effect of exposure to famine changed as survivors age. We found no evidence to support this hypothesis.[32] One possible explanation is that the interaction effect between age and exposure to famine is non-linear such that we may observe the effects as the survivors reach their 50s and 60s. Another possibility is that famine survivors suffer from coronary heart disease for different reasons than the population at large. Therefore, blood pressure is not a good indicator of the underlying factors that cause famine-related heart disease. The medical literature, to the best of our knowledge, does not shed light on this point. We intend to re-examine the Barker Hypothesis in future waves of the CHNS when the survivors reach their late-middle years.

In addition to the analysis presented in this paper, we also examined the

[32] Results are not reported in the paper.

impact of famine exposure by sex and by age. We found no evidence for sex-differential effects of famine. This is consistent with the main result that differential exposure to famine did not have differential effects on the sex ratios of surviving children. The latter is perhaps surprising considering the overall drop in female survival for cohorts who were very young at the onset of the famine (see Figure A2).[33] These results together suggest that while parents may invest less in girls during bad times, the extent of the famine had no marginal effect on the extent of excess female mortality.[34] This may be an interesting avenue for future research.

6 Conclusion

This paper evaluates the impact of China's Great Famine on survivors almost 30 years after exposure. It resolves problems arising from data limitations and joint determination of famine occurrence and survivor outcomes. The empirical findings show that amongst those who were not elderly at the time of the famine, exposure mostly affected young children. The impact of famine on mortality is negatively correlated with age at the onset of the famine. For survivors, exposure to famine has long lasting negative impacts on health, and significantly reduces labor supply. The results show that the detrimental effects of childhood malnutrition from famine exposure outweigh any potential benefits from reduced cohort sizes.

In addition to the main results, we find that famine decreased long run inequality within affected regions by inducing positive selection for survival. Furthermore, we offer evidence that the Great Famine was not caused by adverse climatic conditions, nor was all of the variation in famine intensity necessarily due to differences in local policies. Because of the procurement scheme used at that time, at least part of the cross-sectional inequity of famine intensity could have arisen even if the behavior of local governments was identical across

[33] There is no evidence of differential mortality for those born during the famine. This is not surprising since fertility was reported to have been extremely low during the famine years.

[34] With the exception of individuals born during the famine years, sex differential survival rates must reflect sex differential mortality since there was no access to pre-natal sex-revealing technology during this period.

regions.

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7 Appendix

7.1 Predicting Grain Output with Natural Conditions

This section establishes that non-famine (1997) grain output is largely explained by weather conditions; and that the occurrence of famine cannot be explained by weather conditions. We estimate the following equation.

$$\log(grainpc_{ip}) = \sum_{j=1}^{2} \rho_{1j} temp_{ip}^{j} + \sum_{j=1}^{2} \rho_{2j} rain_{ip}^{j} + \rho_3 SDTemp_{ip} + \rho_4 SDRain_{ip} \tag{3}$$

$$+ \sum_{j=1}^{2} \rho_{5j} rain_{ip}^{j} * dist_{ip} + \sum_{j=1}^{2} \rho_{6j} temp_{ip}^{j} * dist_{ip} + \rho_7 soil_{ip} + \varphi_p + \varepsilon_i \tag{4}$$

We regress per capita grain production in 1997 for county i province p on a vector of temperature indicators in 1997 for county i province p, $temp$; and a vector of indicators for precipitation in 1997 for county i province p, $rain$;a variable indicating the distance of county i province p to the nearest weather station, $dist$; the standard deviation of temperature and precipitation over the year, $SD(temp)$ and $SD(rain)$; the interaction terms of all the temperature and rain variables with the distance of each county to the nearest weather station, $rain * dist$ and $temp * dist$; a vector of variables on altitude, longitude for county$i$ province p, $soil$;and provincial dummy variables, φ_p. We interact temperature and rainfall variables with distance to the nearest station because it is possible that the further away a county is from the nearest station, the more likely the weather variables are measured with larger errors. To allow for these measurement errors, we specify the variable $dist$ in two ways, one as a continuous variable and the other is a dummy variable for above 100 km away from the nearest station. Note that both $temp$ and $rain$ are measured in two ways, one as the annual mean temperature and rain fall (from January to December) and the other as the average spring temperature and rainfalls (from March to June). The results from both specifications are reported in Table 4. They show that production in 1997 is largely explained by natural conditions with adjusted R squared ranging from 0.68 to 0.76 depending on the model specifications. Using these coefficients and average annual temperature, rainfall and soil variables for the years 1959-1961, we predict grain output per capita for the famine years. If the famine was caused by bad weather, the predicted output should be significantly different from the 1997 normal output. Instead, we observe that the predicted output is highly correlated to the 1997 actual output with the coefficient correlation being 0.80. This high correlation is inconsistent with the argument that famine was caused by abnormal weather conditions.

7.2 Measurement Error

This paper examines the effect of childhood exposure to famine on long run outcomes for survivors (e.g., health, labor supply, education).

$$y_{it} = \beta x^*_{it} + \varepsilon_{it},\ \beta < 0$$

Denote the y_{it} as the average outcome for individuals born in county i and year t as a function of the intensity of famine in county i year t and an error term. Assume that famine intensity is uncorrelated with ε_{it}.

However, we cannot observe the actual intensity of the famine. Instead we use the size of birth cohort t in county i as a measure of famine intensity. Up to 10% of the rural population in China died during the famine. Hence, the more intense the famine, the smaller the cohort size. Denote cohort size as x_{it} such that

$$x_{it} = \pi x^*_{it} + e_{it},\ \pi < 0,\ E[x_{it}e_{it}] = 0$$

or

$$x^*_{it} = \frac{x_{it}}{\pi} - \frac{e_{it}}{\pi}$$

Plugging into the initial regression

$$\begin{aligned} y_{it} &= \beta(\frac{x_{it}}{\pi} - \frac{e_{it}}{\pi}) + \varepsilon_{it} \\ &= \frac{\beta}{\pi}x_{it} + v_{it},\ v_{it} = \varepsilon_{it} - \beta\frac{e_{it}}{\pi} \end{aligned}$$

With the additional assumption of $cov(\varepsilon, e) = 0$, the estimate of $\hat{\beta}$ can be written as

$$\begin{aligned}\hat{\beta}_{OLS} &= \frac{cov(x,y)}{var(x)} \\ &= \frac{\beta}{\pi} - \frac{cov(x,\upsilon)}{var(x)} \\ &= \frac{\beta}{\pi} - \frac{\frac{\beta}{\pi}var(e)}{var(x)} \\ &= \frac{\beta}{\pi} - \frac{\frac{\beta}{\pi}var(e)}{\pi^2 var(x^*) + var(e)} < \frac{\beta}{\pi}\end{aligned}$$

since $\frac{\beta}{\pi} > 0$. Hence, OLS will be biased towards zero.

Table 1: Proportional Over-reporting/Over-expropriation

	A	B	C
True Yield	200	300	80952
Subsistence Amount	200	200	56667
Over Reporting Proportion	10.00%	10.00%	13.00%
Reported Yield	220	330	91476
Procurement	20	130	34810
Retained Grains	180	170	46143
% Deficient	-10.00%	-15.00%	-18.57%

Table 2: Aggregate Grain Output and Agricultural Inputs in China 1952-1977

Year	Grain Output (Million Tons) (1)	Grain Procurement (Million Tons) (2)	Retained Grain per Capita (kg/Person) (3)	Rural Labor (Millions) (4)	Area Sown with Grain (Million Hectares) (5)	Draft Animals (Million Head) (6)	Farm Machinery (Million HP) (7)	Chemical Fertilizer (Million Tons) (8)	% Procured (9)
1952	164	33	260	173	124	76	0.3	0.08	20.12%
1953	167	47	242	177	127	81	0.4	0.12	28.14%
1954	170	51	228	182	129	85	0.5	0.16	30.00%
1955	184	48	256	186	130	88	0.8	0.24	26.09%
1956	193	40	284	185	136	88	1.1	0.33	20.73%
1957	195	46	273	193	134	84	1.7	0.37	23.59%
1958	200	52	268	155	128	78	2.4	0.55	26.00%
1959	170	64	193	163	116	79	3.4	0.54	37.65%
1960	143	47	182	170	122	73	5	0.66	32.87%
1961	148	37	209	197	121	69	7.1	0.45	25.00%
1962	160	32	229	213	122	70	10	0.63	20.00%
1963	170	37	231	220	121	75	12	1	21.76%
1964	188	40	256	228	122	79	13	1.3	21.28%
1965	195	39	261	234	120	84	15	1.9	20.00%
1966	214	41	282	243	121	87	17	2.3	19.16%
1967	218	41	281	252	119	90	20	2.4	18.81%
1968	209	40	261	261	116	92	22	2.7	19.14%
1969	211	38	259	271	118	92	26	3.1	18.01%
1970	240	46	282	278	119	94	29	3.4	19.17%
1971	250	44	293	284	121	95	38	3.8	17.60%
1972	241	39	298	283	121	96	50	4.3	16.18%
1973	265	48	293	289	121	97	65	4.8	18.11%
1974	275	47	303	292	121	98	81	5.4	17.09%
1975	285	53	304	295	121	97	102	6	18.60%
1976	286	49	306	294	121	95	117	6.8	17.13%
1977	283	48	300	293	120	94	140	7.6	16.96%

Source: Columns (1)-(8) from Li and Yang (2005) (Original Source:—Cols. 1, 2, and 4–6 are taken from Ministry of Agriculture (1989); cols. 7 and 8 are taken from Wen (1993); and col. 3 is the result of dividing the difference between cols. 1 and 2 by the rural population); Columns (9)-(10) are calculated from Columns (1)-(3); Column (11) is calculated by subtracting Columns (9) and (10) from 1; Column 12 is calculated from columns (1) and (2).

Table 3: The Effect of Climate on Non-Famine per Capita Grain Output

	Dependent Variable: County Mean Per Capita Grain	
	Annual mean	Spring mean
Temperature	-0.001***	-0.002***
	(0.000)	(0.000)
Temperature Squared	0.000***	0.000***
	(0.000)	(0.000)
Temperature*distance>=100	-0.000	0.000
	(0.000)	(0.000)
Temp squared*distance>=100	0.000	-0.000
	(0.000)	(0.000)
Rainfall	-0.000***	-0.000
	(0.000)	(0.000)
Rainfall squared	0.000***	0.000***
	(0.000)	(0.000)
Rainfall*distance>=100	0.000	0.000
	(0.000)	(0.000)
Rainfall squared*dist>=100	-0.000	-0.000
	(0.000)	(0.000)
Longitude	-0.001***	-0.002***
	(0.000)	(0.001)
Altitude	-0.000***	-0.000***
	(0.000)	(0.000)
Obs	332	332
Adj. R-squared	0.73	0.68

All regressiosn control for province fixed effects.

Table 4: Descriptive Statistics

	I. Full Sample		II. Restricted Sample*	
Variable	**Mean**	**Std. Err.**	**Mean**	**Std. Err.**
Birth Year	1956	0.05	1956	0.05
Sex (Fraction Male)	0.51	0.00	0.51	0.00
Hhsize	4.92	0.01	4.94	0.02
Education (Years)	6.77	0.01	6.67	0.01
Female Education (Years)	5.81	0.01	5.71	0.01
Male Education (Years)	7.70	0.01	7.59	0.01
Height (Cm)	160.84	0.17	160.77	0.18
Weight (Kg)	56.04	0.20	56.03	0.21
WFH (Kg/Cm)	0.35	0.00	0.35	0.00
Systolic (Hg/mm)	111.89	0.31	111.78	0.33
Diastolic (Hg/mm)	73.48	0.23	73.48	0.25
Skinfold (Mm)	13.07	0.23	13.03	0.25
Upper Arm Circumference (Cm)	24.98	0.09	24.98	0.09
Labor Supply (Hrs/Wk)	49.06	0.36	48.88	0.38
Agricultural HH (Fraction)	1.00	0.00	1.00	0.00
County Population (1%)	7116.89	23.96	7125.61	25.62
County-Brithyear Cell Size	115.17	0.48	118.81	0.52

Full sample contains county-birthyear cells for cohorts born 1943-1966.

*Restricted sample excludes cohorts born during the famine, 1959-1961.

Table 5: OLS Results for the Effect of Famine Intensity
Coefficient for the Log of Population by County and Year of Birth

	Dependent Variables									
	Sex	**LnEdu**	**LnHeight**	**LnWeight**	**LnWFH**	**LnSystolic**	**LnDiastolic**	**LnArm**	**LnMM**	**LnWorkHr**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Whole Sample										
Sample Mean (Not in Logs)	**0.506**	**6.768**	**160.843**	**56.035**	**0.348**	**111.895**	**73.478**	**24.977**	**13.074**	**47.536**
LnPop	0.000	-0.005	0.008	0.009	0.000	-0.009	-0.021	-0.004	-0.049	0.037
	(0.003)	(0.005)	(0.007)	(0.014)	(0.009)	(0.018)	(0.026)	(0.012)	(0.068)	(0.102)
Obs	22066	21965	725	725	724	725	725	582	520	423
Panel B. Restricted Sample										
Sample Mean (Not in Logs)	**0.506**	**6.668**	**160.770**	**56.028**	**0.348**	**111.776**	**73.478**	**24.983**	**14.030**	**47.330**
LnPop	-0.003	0.001	0.011	0.018	0.006	-0.004	-0.031	-0.005	-0.071	0.085
	(0.003)	(0.006)	(0.008)	(0.020)	(0.014)	(0.020)	(0.032)	(0.016)	(0.096)	(0.113)
Obs	19317	19229	634	634	633	634	634	513	456	374

All regressions include county and birth year fixed effects.
Standard errors are clustered at the county level.
Restricted sample excludes individuals born during the famine.

Table 6: The First Stage Results
for the Effects of Normal Grain Production on Famine Intensity
Coefficients of the interaction terms of log(grainpc) and birth year dummy variables

Dependent Variable: LnPop			
LnGrainPc*Born 1944	0.012	LnGrainPc*Born 1956	-0.030
	(0.012)		(0.016)
LnGrainPc*Born 1945	0.013	LnGrainPc*Born 1957	-0.038
	(0.013)		(0.014)
LnGrainPc*Born 1946	-0.001	LnGrainPc*Born 1958	-0.053
	(0.013)		(0.015)
LnGrainPc*Born 1947	0.020	LnGrainPc*Born 1959	-0.067
	(0.012)		(0.019)
LnGrainPc*Born 1948	0.003	LnGrainPc*Born 1960	-0.096
	(0.012)		(0.019)
LnGrainPc*Born 1949	0.008	LnGrainPc*Born 1961	0.005
	(0.013)		(0.021)
LnGrainPc*Born 1950	0.002	LnGrainPc*Born 1962	0.017
	(0.014)		(0.015)
LnGrainPc*Born 1951	-0.010	LnGrainPc*Born 1963	0.004
	(0.016)		(0.013)
LnGrainPc*Born 1952	-0.011	LnGrainPc*Born 1964	0.017
	(0.015)		(0.012)
LnGrainPc*Born 1953	-0.014	LnGrainPc*Born 1965	0.001
	(0.014)		(0.012)
LnGrainPc*Born 1954	-0.024	LnGrainPc*Born 1966	0.026
	(0.013)		(0.013)
LnGrainPc*Born 1955	-0.034		
	(0.015)	Observations	16192

All regressions include county and birth year fixed effects.
Standard errors are clustered at the county level.

Table 7: The Reduced Form Effects of Normal Grain Production on Adult Outcomes

Coefficients of the interaction terms of log(grainpc) and birth year dummy variables

	Dependent Variables									
	Sex	**LnEdu**	**LnHeight**	**LnWeight**	**LnWFH**	**LnSys**	**LnDias**	**LnArm**	**LnMM**	**LnWorkHr**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
LnGrainPC*Born 1943	-0.029	0.003	0.021	0.022	-0.001	-0.050	-0.039	0.000	-0.209	0.030
	(0.018)	(0.009)	(0.012)	(0.014)	(0.020)	(0.020)	(0.019)	(0.028)	(0.132)	(0.104)
LnGrainPC*Born 1944	-0.006	-0.002	0.020	0.015	0.005	0.005	0.013	-0.018	-0.024	0.016
	(0.015)	(0.008)	(0.013)	(0.031)	(0.022)	(0.010)	(0.020)	(0.014)	(0.110)	(0.077)
LnGrainPC*Born 1945	-0.016	0.003	0.016	0.013	0.003	-0.046	-0.020	0.010	-0.065	0.067
	(0.013)	(0.012)	(0.008)	(0.021)	(0.025)	(0.016)	(0.022)	(0.014)	(0.140)	(0.072)
LnGrainPC*Born 1946	-0.024	0.011	0.018	0.006	0.012	0.001	0.015	-0.016	-0.229	0.045
	(0.019)	(0.011)	(0.006)	(0.012)	(0.015)	(0.017)	(0.026)	(0.014)	(0.148)	(0.172)
LnGrainPC*Born 1947	-0.030	0.008	0.023	0.004	0.019	-0.044	-0.049	-0.001	-0.212	0.199
	(0.023)	(0.010)	(0.009)	(0.018)	(0.015)	(0.020)	(0.007)	(0.017)	(0.114)	(0.111)
LnGrainPC*Born 1948	-0.005	0.016	0.018	0.031	-0.013	-0.024	-0.010	0.011	-0.114	0.116
	(0.010)	(0.010)	(0.006)	(0.011)	(0.009)	(0.024)	(0.025)	(0.011)	(0.094)	(0.089)
LnGrainPC*Born 1949	-0.021	0.005	0.022	0.029	-0.007	-0.020	-0.024	0.006	-0.091	0.024
	(0.013)	(0.010)	(0.008)	(0.011)	(0.011)	(0.013)	(0.021)	(0.014)	(0.083)	(0.106)
LnGrainPC*Born 1950	-0.029	0.001	0.013	0.000	0.014	-0.005	-0.002	0.013	-0.145	0.327
	(0.019)	(0.010)	(0.007)	(0.027)	(0.024)	(0.015)	(0.019)	(0.012)	(0.147)	(0.178)
LnGrainPC*Born 1951	-0.021	0.005	0.022	0.046	-0.029	0.010	0.018	0.034	-0.037	0.053
	(0.017)	(0.011)	(0.008)	(0.024)	(0.016)	(0.015)	(0.027)	(0.008)	(0.098)	(0.065)
LnGrainPC*Born 1952	-0.010	0.008	0.011	0.014	-0.003	-0.011	-0.008	0.005	-0.062	0.114
	(0.010)	(0.010)	(0.009)	(0.011)	(0.011)	(0.014)	(0.018)	(0.011)	(0.071)	(0.116)
LnGrainPC*Born 1953	-0.032	0.008	0.014	-0.004	0.019	-0.011	0.005	-0.016	-0.182	0.008
	(0.022)	(0.012)	(0.008)	(0.015)	(0.019)	(0.018)	(0.015)	(0.026)	(0.148)	(0.112)
LnGrainPC*Born 1954	-0.026	0.007	0.012	-0.008	0.019	-0.023	-0.020	-0.005	-0.252	0.025
	(0.020)	(0.013)	(0.009)	(0.015)	(0.018)	(0.013)	(0.018)	(0.018)	(0.140)	(0.064)
LnGrainPC*Born 1955	-0.008	0.023	0.005	-0.019	0.024	-0.007	-0.002	-0.016	-0.153	-0.018
	(0.012)	(0.012)	(0.010)	(0.018)	(0.013)	(0.012)	(0.023)	(0.013)	(0.126)	(0.100)
LnGrainPC*Born 1956	-0.026	0.017	0.006	-0.001	0.008	-0.026	0.008	0.017	-0.153	0.062
	(0.018)	(0.013)	(0.008)	(0.020)	(0.017)	(0.014)	(0.026)	(0.017)	(0.122)	(0.093)
LnGrainPC*Born 1957	-0.017	0.024	0.005	-0.013	0.018	-0.026	-0.017	-0.001	-0.127	0.028
	(0.012)	(0.012)	(0.006)	(0.012)	(0.011)	(0.011)	(0.016)	(0.017)	(0.063)	(0.092)
LnGrainPC*Born 1958	-0.036	0.023	0.018	0.014	0.004	-0.017	-0.012	0.012	-0.139	-0.061
	(0.019)	(0.013)	(0.006)	(0.018)	(0.020)	(0.017)	(0.021)	(0.017)	(0.123)	(0.082)
LnGrainPC*Born 1959	-0.029	0.030	0.019	0.014	0.006	-0.018	-0.016	-0.037	-0.114	0.013
	(0.018)	(0.012)	(0.007)	(0.017)	(0.013)	(0.014)	(0.021)	(0.039)	(0.084)	(0.095)
LnGrainPC*Born 1960	-0.011	0.030	0.008	-0.010	0.019	-0.024	-0.019	0.012	-0.155	0.016
	(0.012)	(0.015)	(0.006)	(0.017)	(0.015)	(0.016)	(0.019)	(0.017)	(0.094)	(0.068)
LnGrainPC*Born 1961	-0.021	0.041	0.019	0.016	0.003	-0.020	-0.019	0.014	-0.160	0.124
	(0.016)	(0.013)	(0.005)	(0.012)	(0.014)	(0.010)	(0.020)	(0.017)	(0.129)	(0.132)
LnGrainPC*Born 1962	-0.021	0.038	0.017	0.018	-0.001	-0.012	-0.026	0.021	-0.043	0.055
	(0.015)	(0.014)	(0.011)	(0.026)	(0.021)	(0.024)	(0.041)	(0.016)	(0.139)	(0.089)
LnGrainPC*Born 1963	-0.017	0.031	0.009	0.015	-0.006	-0.014	-0.010	0.018	-0.047	0.094
	(0.017)	(0.014)	(0.009)	(0.017)	(0.012)	(0.015)	(0.028)	(0.012)	(0.122)	(0.074)
LnGrainPC*Born 1964	-0.024	0.029	0.022	0.022	0.000	-0.027	0.003	0.014	-0.158	0.141
	(0.013)	(0.015)	(0.007)	(0.017)	(0.013)	(0.014)	(0.025)	(0.008)	(0.120)	(0.102)
LnGrainPC*Born 1965	-0.027	0.028	0.019	0.020	-0.001	-0.012	0.010	0.021	-0.141	0.067
	(0.019)	(0.013)	(0.011)	(0.013)	(0.013)	(0.016)	(0.017)	(0.019)	(0.164)	(0.083)
Observations	16192	16139	542	542	541	542	542	443	381	644

All regressions include county and birth year fixed effects.

Standard errors are clustered at the county level.

Table 8: The 2SLS Effects of Famine on Sex Ratios and Educational Attainment for Survivors in 1990

Coefficients of the log of population

	Dependent Variables									
	Sex	**LnEdu**	**LnHeight**	**LnWeight**	**LnWFH**	**LnSys**	**LnDias**	**LnArm**	**LnMM**	**LnWorkHr**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Whole Sample										
Sample Mean (Not in Logs)	**0.506**	**6.768**	**160.843**	**56.035**	**0.348**	**111.895**	**73.478**	**24.977**	**13.074**	**47.536**
LnPop	-0.014	-0.003	0.039	0.139	0.101	0.009	0.018	0.190	0.032	0.448
	(0.019)	(0.033)	(0.022)	(0.057)	(0.040)	(0.059)	(0.061)	(0.105)	(0.344)	(0.149)
Obs	15571	15518	542	542	541	542	542	443	381	322
R-Squared	0.14	0.87	0.24	0.47	0.50	0.40	0.35	0.29	0.45	0.25
Panel B. Restricted Sample										
Sample Mean (Not in Logs)	**0.506**	**6.668**	**160.770**	**56.028**	**0.348**	**111.776**	**73.478**	**24.983**	**14.030**	**47.330**
LnPop	-0.024	0.115	0.083	0.241	0.159	0.010	-0.006	0.158	0.062	0.277
	(0.027)	(0.064)	(0.035)	(0.091)	(0.061)	(0.051)	(0.083)	(0.088)	(0.484)	(0.349)
	13625	13580	474	474	473	474	474	391	334	287
Obs	0.14	0.86	0.10	0.39	0.45	0.41	0.35	0.32	0.46	0.30

All regressions include county and birth year fixed effects.
Standard errors are clustered at the county level.
Restricted sample excludes individuals born during the famine.

Table 9: Calibrated Average Effect of Famine

Cohort			Height (Cm) (1)	Weight (Kg) (2)	WFH (Kg/CM) (3)	Arm (Cm) (4)	WkHr (Hrs/Wk) (5)
1953-55	**Full Sample**	**Effect of Famine (%)**	-0.002	-0.008	-0.006	-0.011	-0.027
		Average Effect	*-0.004*	*-0.005*	*0.000*	*-0.003*	*-0.013*
	Restricted Sample	**Effect of Famine (%)**	-0.005	-0.014	-0.010	-0.009	-0.017
		Average Effect	*-0.008*	*-0.008*	*0.000*	*-0.002*	*-0.008*
1956-1957	**Full Sample**	**Effect of Famine (%)**	-0.005	-0.017	-0.013	-0.024	-0.056
		Average Effect	*-0.008*	*-0.010*	*0.000*	*-0.006*	*-0.027*
	Restricted Sample	**Effect of Famine (%)**	-0.010	-0.030	-0.020	-0.020	-0.035
		Average Effect	*-0.017*	*-0.017*	*0.000*	*-0.005*	*-0.016*
1958	**Full Sample**	**Effect of Famine (%)**	-0.98	-3.48	-2.53	-4.75	-11.20
		Average Effect	*-1.57*	*-1.95*	*-0.01*	*-1.19*	*-5.32*
	Restricted Sample	**Effect of Famine (%)**	-2.08	-6.03	-3.98	-3.95	-6.93
		Average Effect	*-3.34*	*-3.38*	*-0.01*	*-0.99*	*-3.28*
1959-1960	**Full Sample**	**Effect of Famine (%)**	-1.95	-6.95	-5.05	-9.50	-22.40
		Average Effect	*-3.14*	*-3.89*	*-0.02*	*-2.37*	*-10.65*

Effect of famine (%) = the percentage of people "missing" during the famine cohort" * estimated 2SLS coefficients.

Effect (Levels) = Effect of famine (%) * Sample Mean of Outcome

Assume that famine decreased cohort sizes by 6%, 12.5%, 25% and 50% for 1953-54, 1955-57, 1958, 1959-1960 (see Figure A1)

Table 10: OLS and 2SLS Estimates for the Effect of Famine Quartiles –Restricted Sample

	Dependent Variables									
	LnHt	**LnWt**	**LnWFH**	**LnArm**	**LnWkHr**	**LnHt**	**LnWt**	**LnWFH**	**LnArm**	**LnWkHr**
	OLS	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Mean										
LnPop	0.011	0.018	0.006	-0.005	0.023	0.083	0.241	0.159	0.158	0.454
	(0.008)	(0.020)	(0.014)	(0.016)	(0.058)	(0.035)	(0.091)	(0.061)	(0.088)	(0.314)
Obs	634	634	633	513	585	474	474	473	391	446
A. 10th Percentile										
LnPop	-0.007	-0.025	-0.019	-0.055	0.100	0.052	0.176	0.123	-0.066	0.167
	(0.011)	(0.023)	(0.018)	(0.026)	(0.099)	(0.051)	(0.136)	(0.095)	(0.155)	(0.299)
Obs	634	634	633	513	563	474	474	473	391	438
B. 25th Percentile										
LnPop	0.007	0.008	0.000	-0.033	0.052	0.066	0.244	0.176	0.062	0.661
	(0.010)	(0.028)	(0.021)	(0.031)	(0.043)	(0.043)	(0.127)	(0.090)	(0.083)	(0.352)
Obs	634	634	633	513	585	474	474	473	391	446
C. 50th Percentile										
LnPop	0.014	0.021	0.006	-0.004	0.188	0.077	0.208	0.131	0.147	0.458
	(0.008)	(0.019)	(0.015)	(0.016)	(0.078)	(0.029)	(0.090)	(0.069)	(0.071)	(0.321)
Obs	634	634	633	513	585	474	474	473	391	446
D. 75th Percentile										
LnPop	0.012	0.028	0.014	0.005	0.023	0.082	0.211	0.131	0.233	0.370
	(0.010)	(0.023)	(0.018)	(0.020)	(0.058)	(0.039)	(0.069)	(0.047)	(0.105)	(0.351)
Obs	634	634	633	513	585	474	474	473	391	446
B. 90th Percentile										
LnPop	0.020	0.044	0.023	0.041	0.100	0.114	0.370	0.258	0.311	0.167
	(0.012)	(0.032)	(0.022)	(0.023)	(0.099)	(0.041)	(0.143)	(0.107)	(0.109)	(0.299)
Obs	634	634	633	513	563	474	474	473	391	438

All regressions include county and birth year fixed effects.

Standard errors are clustered at the county level.

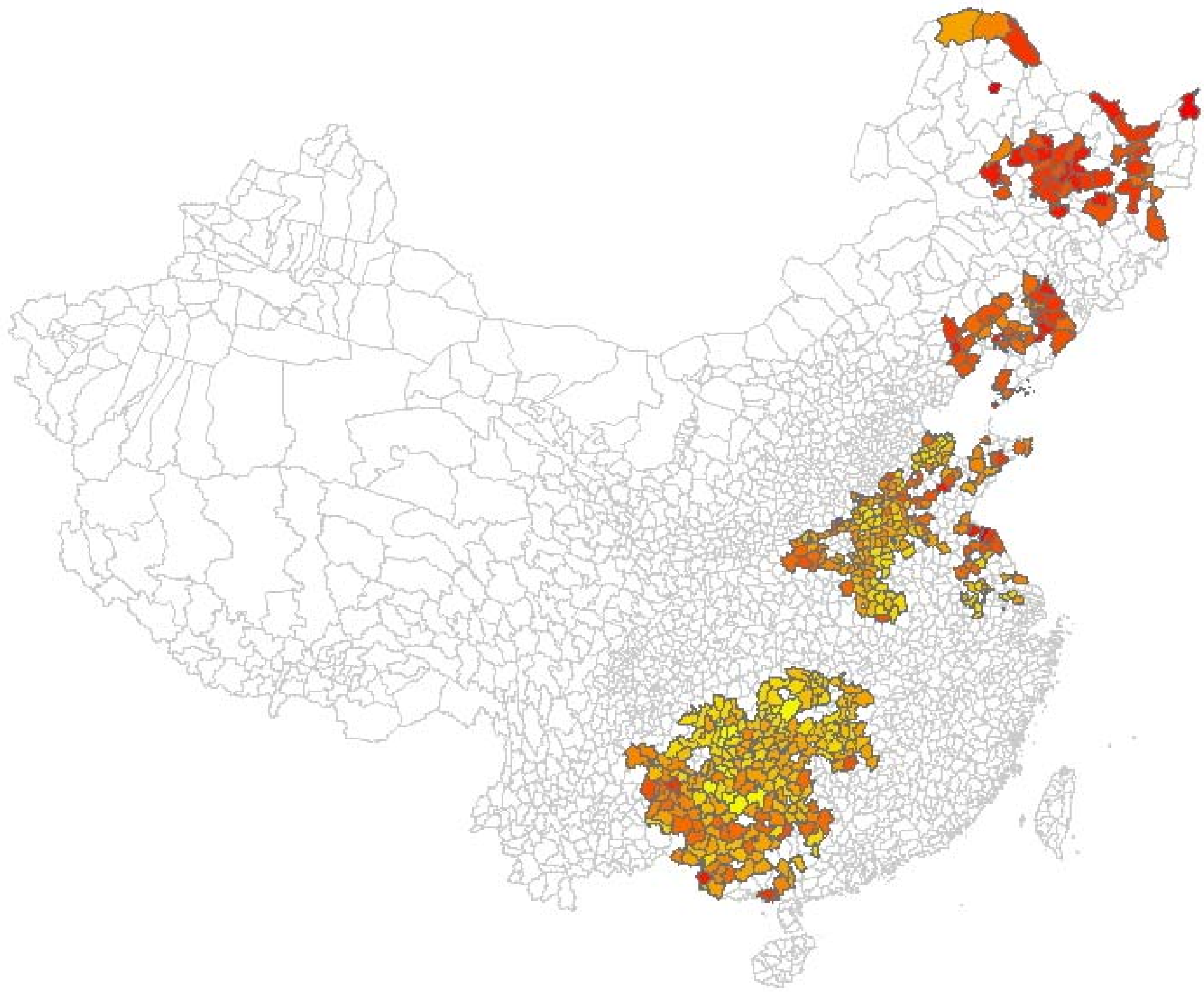
Restricted sample excludes individuals born during 1959-1961.

Table 11: Calibrated Average Effect of Famine by Quartiles for the 1958 Cohort
Restricted Sample excludes individual born 1959-1961

		Height (Cm) (1)	Weight (Kg) (2)	WFH (Kg/Cm) (3)	Arm (Cm) (4)	WorkHr (Hrs/Wk) (5)
Panel A. 1958 Cohort						
Mean	%	-2.075	-6.025	-3.975	-3.950	-11.350
	level	*-3.336*	*-3.376*	*-0.014*	*-0.987*	*-5.372*
10	%	-1.300	-4.400	-3.075	1.650	-4.175
	level	*-2.019*	*-2.134*	*-0.010*	*0.381*	*-1.754*
25	%	-1.650	-6.100	-4.400	-1.550	-16.525
	level	*-2.609*	*-3.166*	*-0.014*	*-0.368*	*-7.767*
50	%	-1.925	-5.200	-3.275	-3.675	-11.450
	level	*-3.107*	*-2.829*	*-0.011*	*-0.905*	*-5.543*
75	%	-2.050	-5.275	-3.275	-5.825	-9.250
	level	*-3.383*	*-3.048*	*-0.012*	*-1.505*	*-4.850*
90	%	-2.850	-9.250	-6.450	-7.775	-4.175
	level	*-4.760*	*-5.797*	*-0.025*	*-2.071*	*-2.352*
Panel B. 1959-1960 Cohort						
mean	%	-4.150	-12.050	-7.950	-7.900	-22.700
	level	-6.672	-6.751	-0.028	-1.974	-10.744
10	%	-2.600	-8.800	-6.150	3.300	-8.350
	level	-4.038	-4.268	-0.019	0.763	-3.507
25	%	-5.700	-18.500	-12.900	-15.550	-8.350
	level	-9.015	-9.602	-0.042	-3.691	-3.925
50	%	0.000	0.000	0.000	0.000	0.000
	level	0.000	0.000	0.000	0.000	0.000
75	%	-4.100	-10.550	-6.550	-11.650	-18.500
	level	-6.766	-6.096	-0.023	-3.010	-9.699
90	%	-5.700	-18.500	-12.900	-15.550	-8.350
	level	-9.519	-11.594	-0.049	-4.143	-4.704

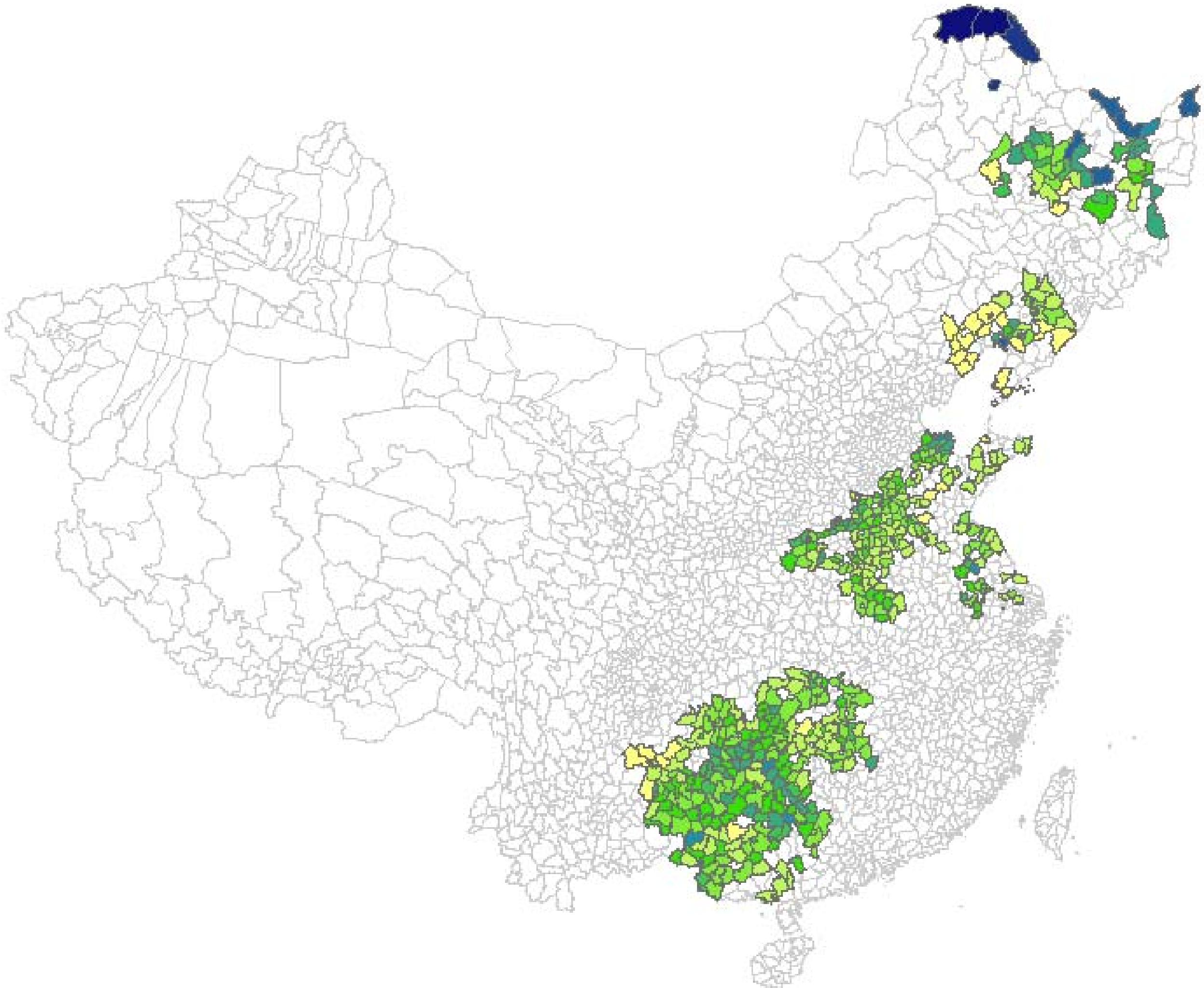
Effect (%) = (the percentage of people "missing" during the famine cohort" -- see Figure 1) * estimated 2SLS coefficients.
Effect (Levels) = Effect of famine (%) * Sample Mean of Outcome

Map1: Famine Intensity by County



Note: *Lighter* shading reflects greater famine intensity.

Map 2: Non-famine Grain Sown



Note: *Lighter* shading reflects more non-famine grain production.

Figure 1A: Sample Population by Birth Year in 1990

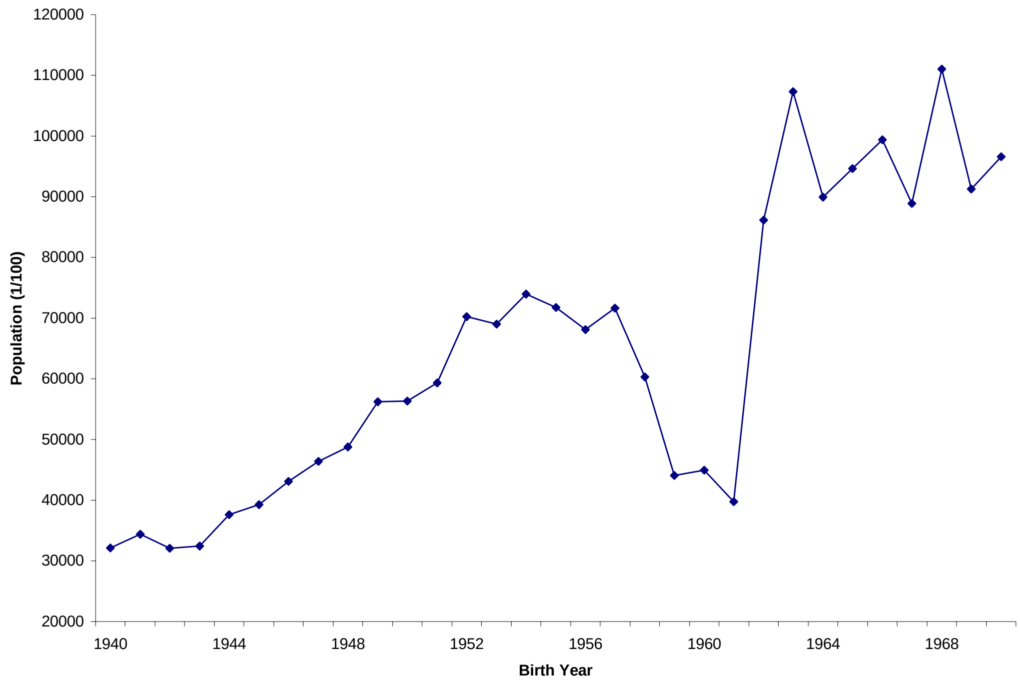


Figure 1B: Changes in County Population Over Birth Years
Coefficients of birth year dummy variables and the 95% confidence intervals

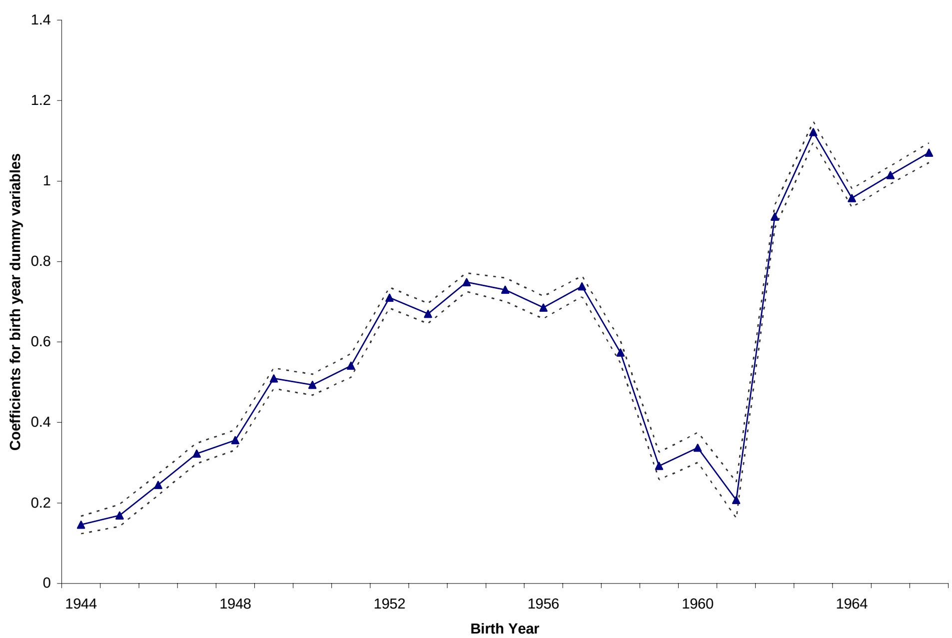
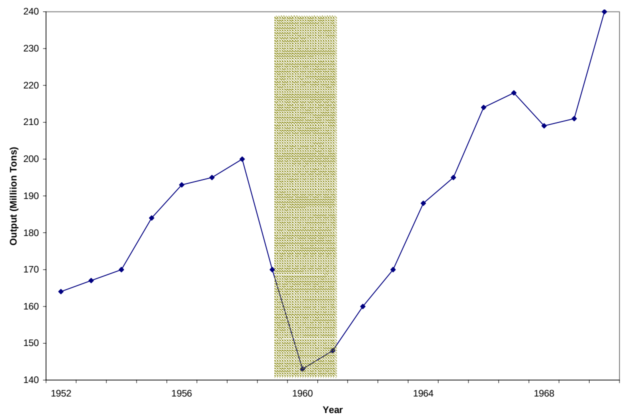
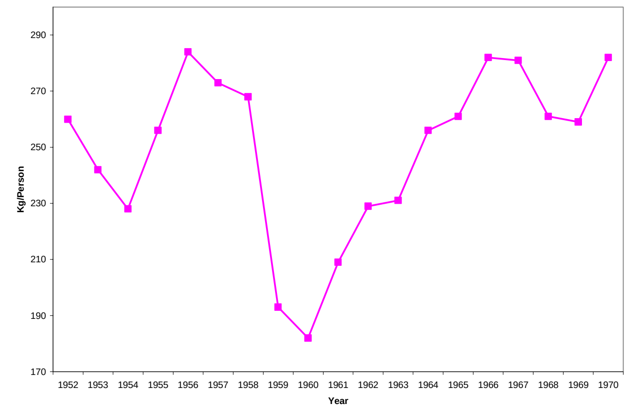


Figure 2A: Historical Grain Production



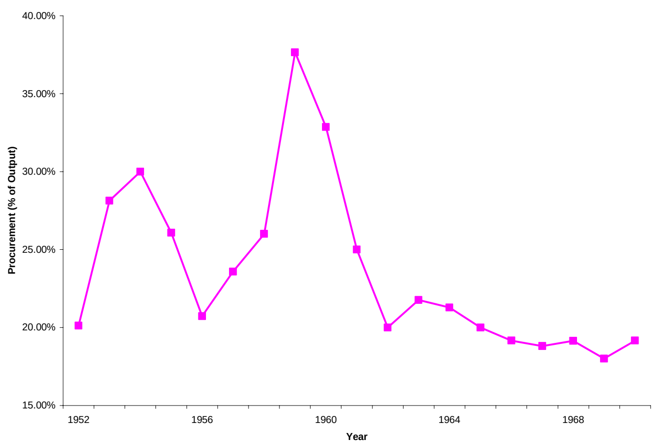
Source: Table 3 Column (1)

Figure 2C: Historical Rural Grain Retention



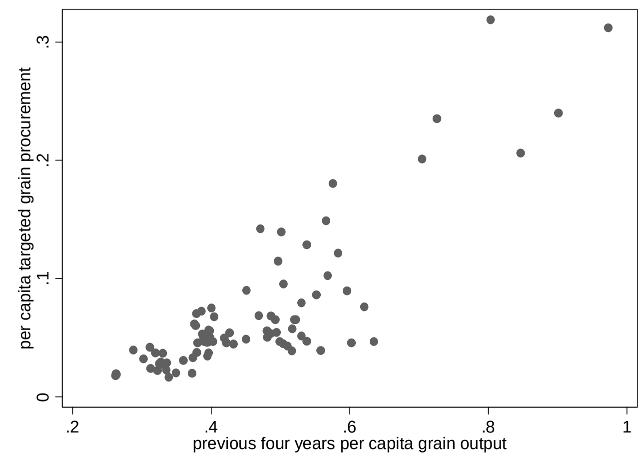
Source: Table 3 Column (3)

Figure 2B: Historical Rural Grain Procurement



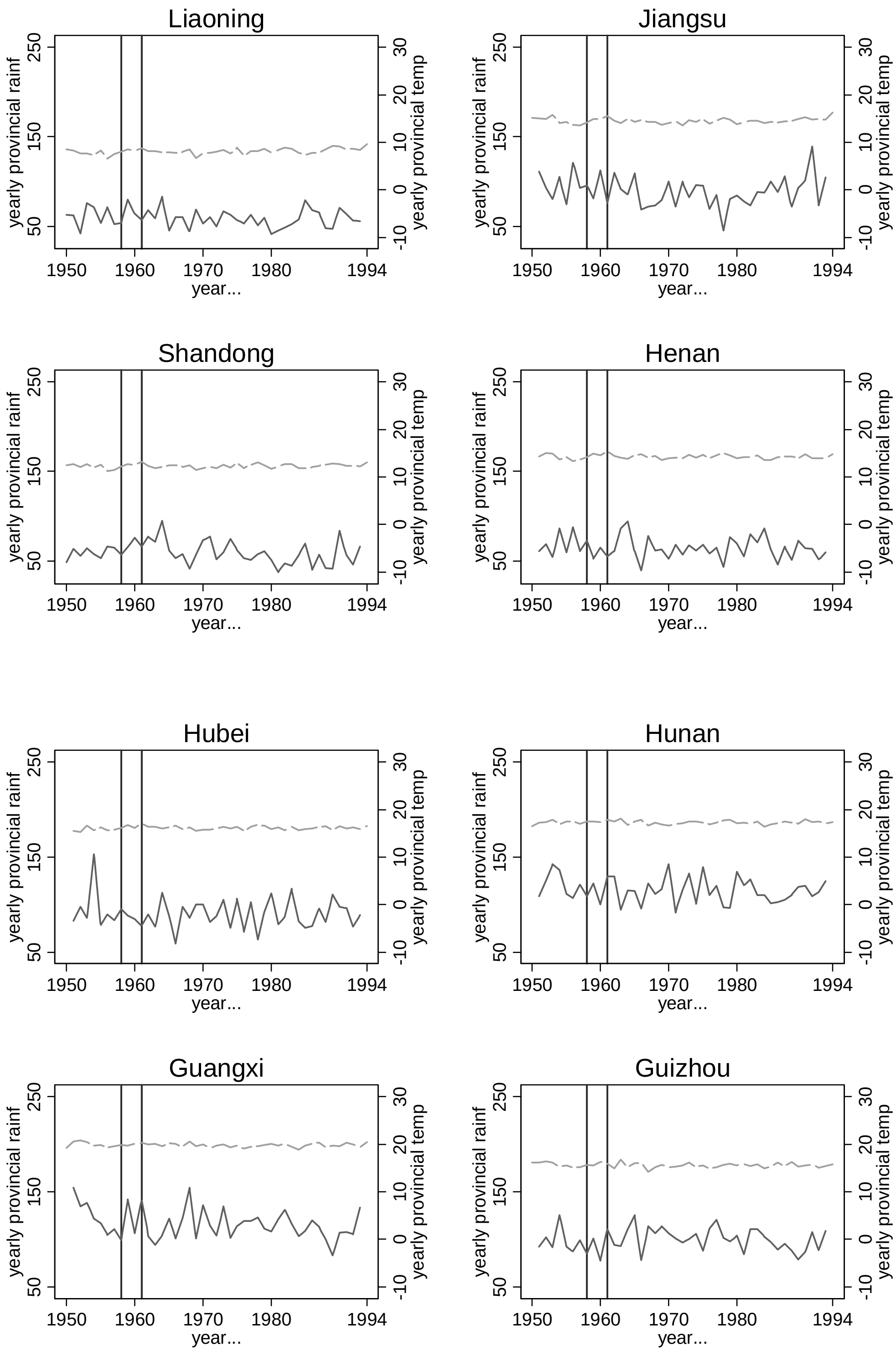
Source: Table 3 Column (2)

Figure 2D: Grain Procurement Targets



Source: ????

Figure 3A: Mean Annual Precipitation and Temperature



Notes: Rainfall is represented as the solid line. Temperature is represented as a dashed line. The vertical lines indicate the famine years.

Figure 3B: The Correlation between Famine Survival and Weather Conditions

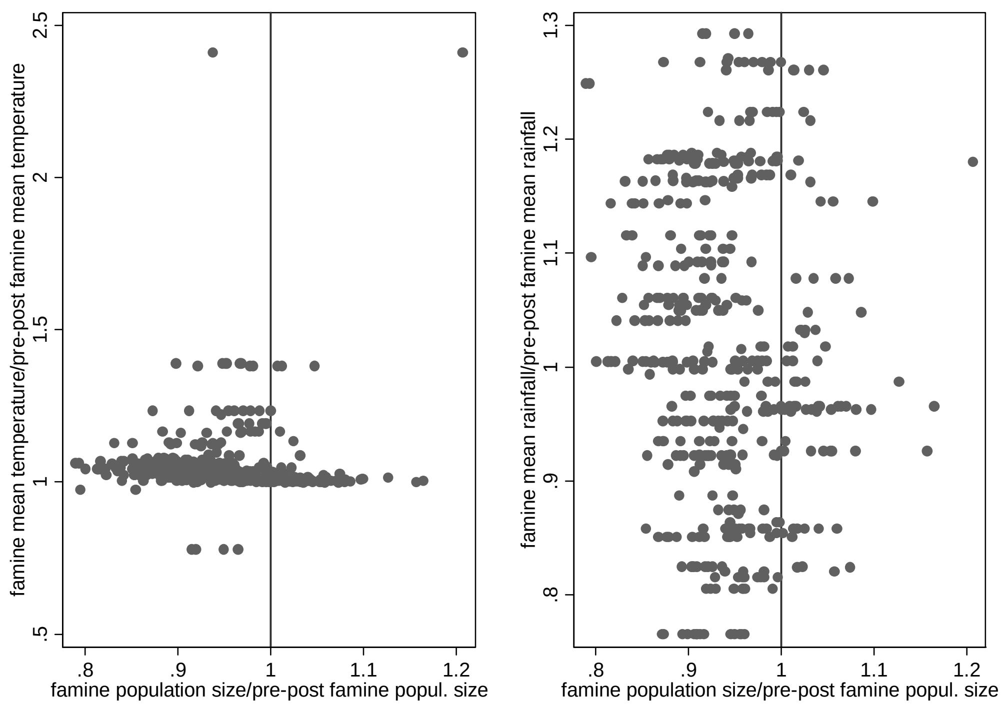


Figure 4: Famine Intensity across Counties

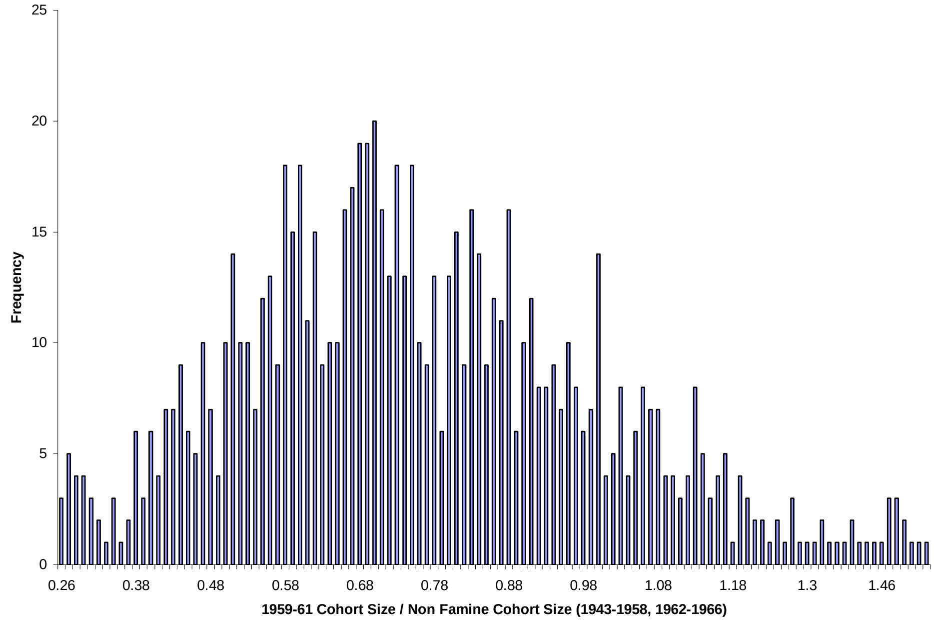


Figure 5: The First Stage Effects of Normal Grain Production on Famine Intensity

Coefficients of the interaction terms of log(grainpc) and dummy variables for birth years and the 95% confidence intervals

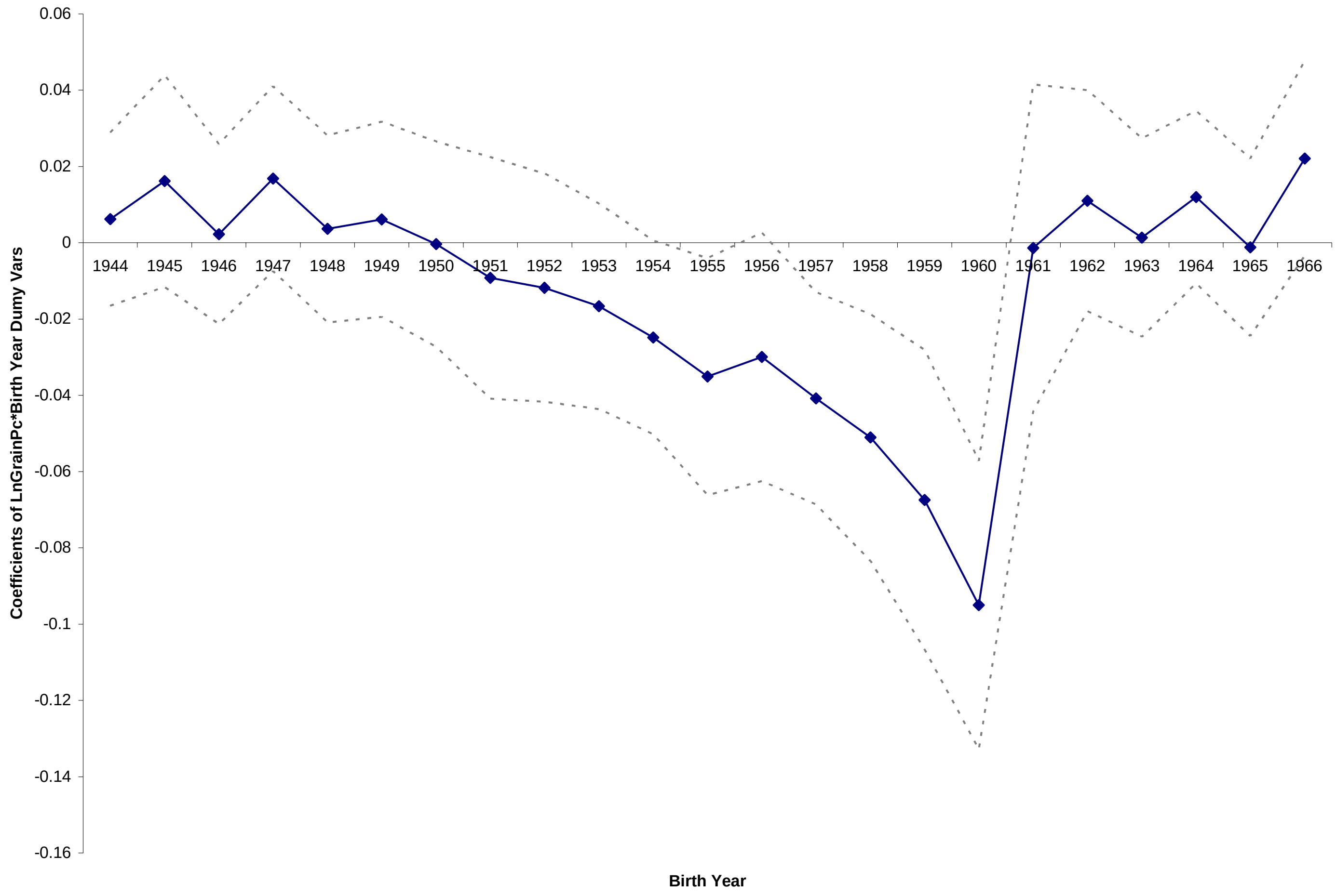


Figure 6: The Reduced Form Effect of Famine on Height of Survivors
Coefficients of the interaction terms of log(grain) and birth year dummy variables

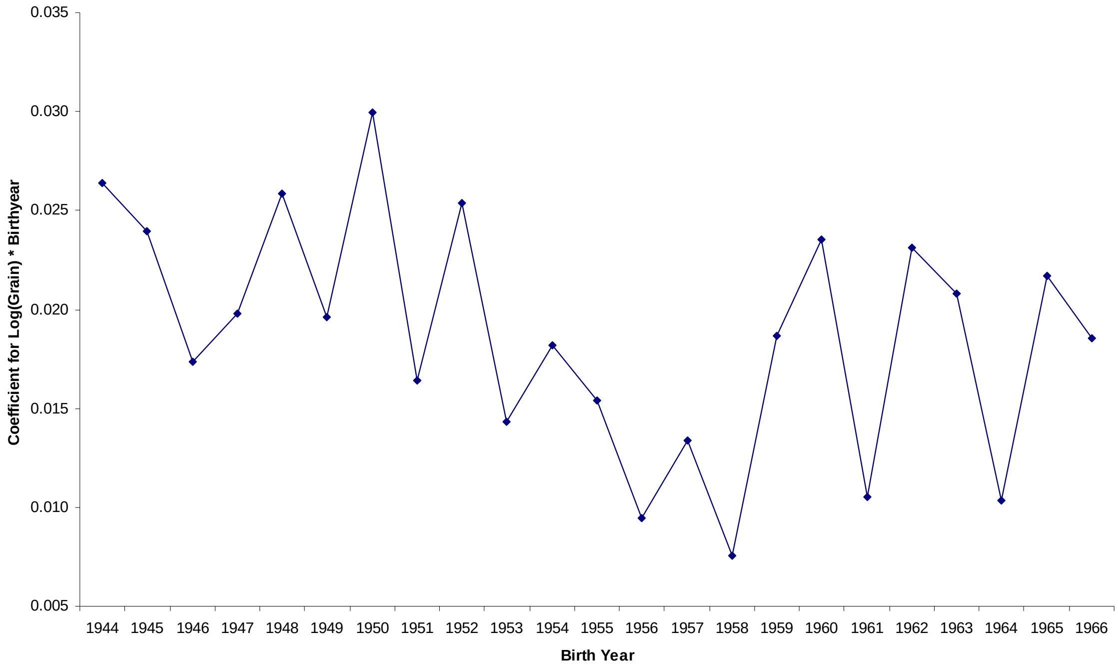


Figure 7A: Age, Sex and Regions Adjusted Height for Individuals Born during the Famine and Individuals born after the Famine

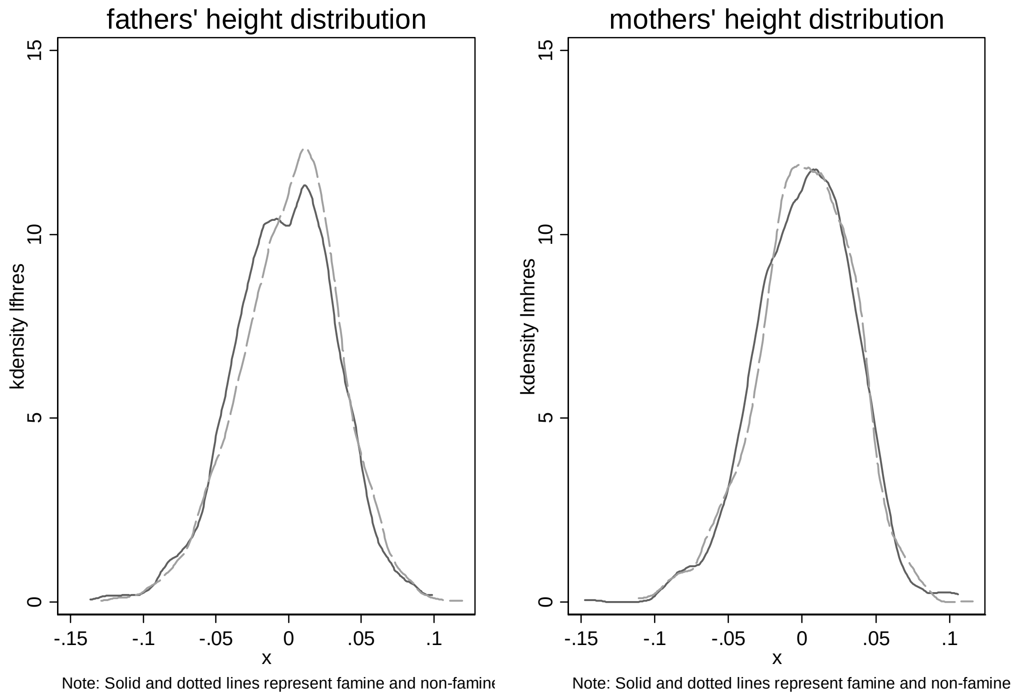


Figure 7B: Age, Sex and Region Adjusted Height for Children of Famine Survivors and Children of Individuals born after the Famine

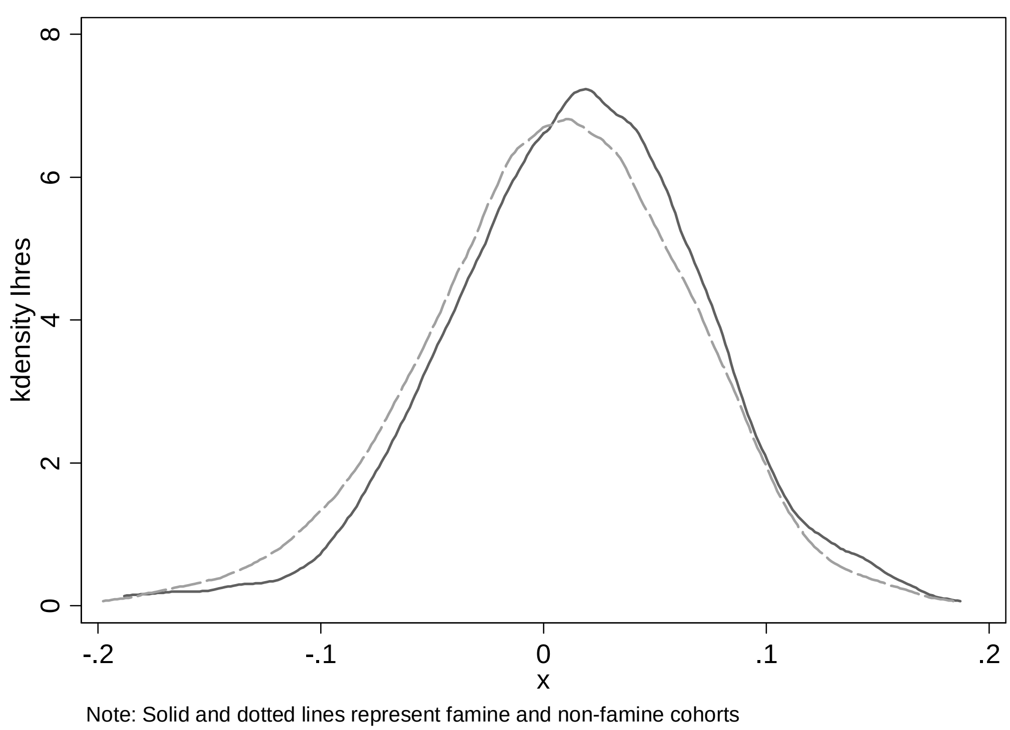


Figure 8A: Calibrated Average Effect of Famine Exposure by Percentiles for 1958 Cohort (%)

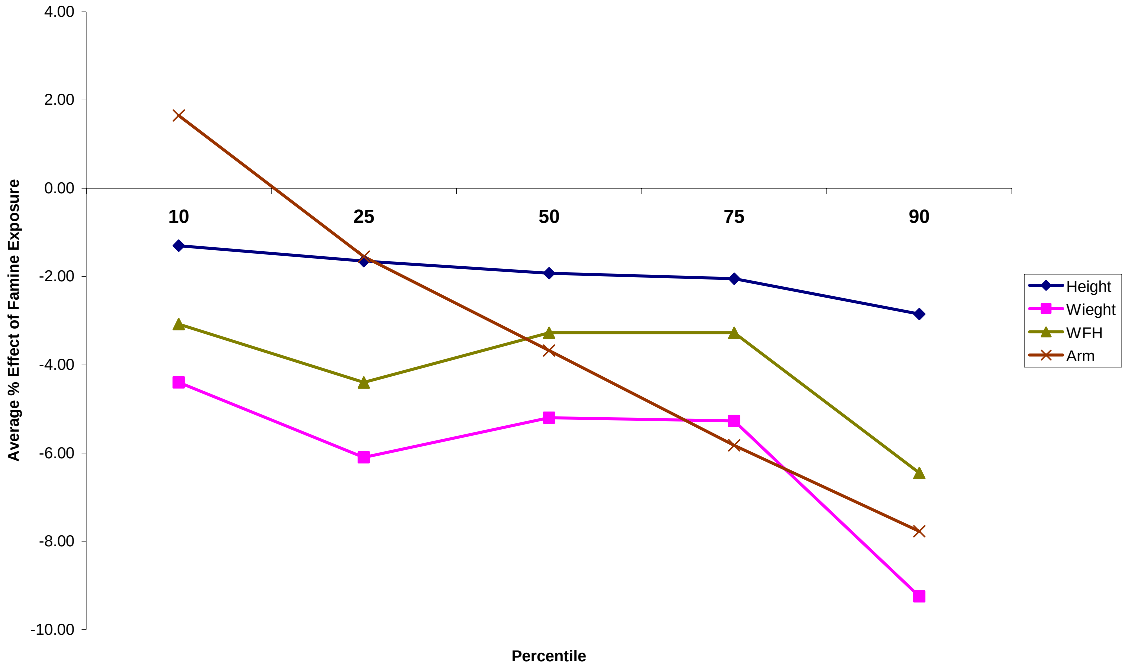


Figure 8B: Calibrated Average Effect of Famine Exposure by Percentiles for 1958 Cohort (Level)

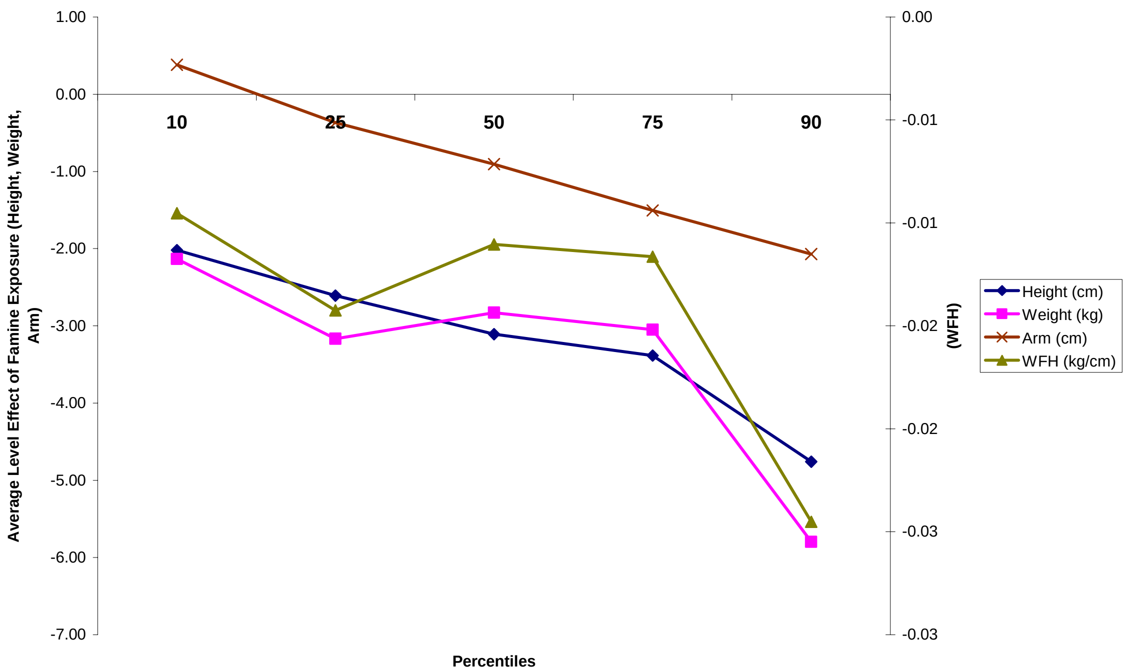


Table A1: Descriptive Statistics by Percentiles

	Height (Cm)	Weight (Kg)	WFH (Kg/Cm)	Arm (Cm)	Hrswk
	(1)	(2)	(3)	(4)	(5)
			I. Full Sample		
10th	155.25	49.60	0.32	23.25	39.00
25th	157.98	52.11	0.33	23.95	45.60
50th	160.97	55.60	0.34	24.97	48.00
75th	163.94	59.44	0.37	26.03	53.50
90th	166.41	63.50	0.39	27.00	59.00
			II. Restricted		
10th	155.30	48.50	0.31	23.11	42.00
25th	158.15	51.90	0.32	23.73	47.00
50th	161.40	54.40	0.34	24.63	48.41
75th	165.02	57.78	0.36	25.83	52.43
90th	167.00	62.67	0.38	26.64	56.33

Restricted sample excludes individuals born during 1959-1961.

Table A2: The OLS and 2SLS Estimates for the Effect of Famine by Percentile – Full Sample

	Dependent Variables									
	LnHt	**LnWt**	**LnWFH**	**LnArm**	**LnWkHr**	**LnHt**	**LnWt**	**LnWFH**	**LnArm**	**LnWkHr**
	OLS	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Mean										
LnPop	0.008	0.009	0.000	-0.004	0.023	0.039	0.139	0.101	0.190	0.254
	(0.007)	(0.014)	(0.009)	(0.012)	(0.058)	(0.022)	(0.057)	(0.040)	(0.105)	(0.203)
Obs	725	725	724	582	585	542	542	541	443	509
B. 10th Percentile										
LnPop	-0.010	-0.032	-0.022	-0.038	0.097	-0.000	0.073	0.072	0.056	0.171
	(0.009)	(0.019)	(0.015)	(0.020)	(0.089)	(0.035)	(0.094)	(0.069)	(0.110)	(0.234)
Obs	725	725	724	582	645	542	542	541	443	501
C. 25th Percentile										
LnPop	0.004	0.006	0.002	-0.019	0.055	0.022	0.159	0.137	0.127	0.399
	(0.008)	(0.023)	(0.017)	(0.021)	(0.032)	(0.022)	(0.086)	(0.068)	(0.063)	(0.223)
Obs	725	725	724	582	668	542	542	541	443	509
D. 50th Percentile										
LnPop	0.009	0.014	0.004	-0.002	0.160	0.029	0.126	0.098	0.176	0.256
	(0.007)	(0.014)	(0.011)	(0.014)	(0.060)	(0.019)	(0.071)	(0.057)	(0.115)	(0.210)
Obs	725	725	724	582	668	542	542	541	443	509
E. 75th Percentile										
LnPop	0.010	0.010	-0.000	0.001	0.041	0.052	0.113	0.064	0.238	0.165
	(0.008)	(0.017)	(0.013)	(0.014)	(0.043)	(0.025)	(0.040)	(0.026)	(0.124)	(0.236)
Obs	725	725	724	582	668	542	542	541	443	509
F. 90th Percentile										
LnPop	0.019	0.033	0.013	0.029	0.097	0.081	0.221	0.144	0.287	0.171
	(0.010)	(0.023)	(0.015)	(0.018)	(0.089)	(0.024)	(0.064)	(0.045)	(0.116)	(0.234)
Obs	725	725	724	582	645	542	542	541	443	501

All regressions include county and birth year fixed effects.

Standard errors are clustered at the county level.

Figure A1: De-trended Log Population

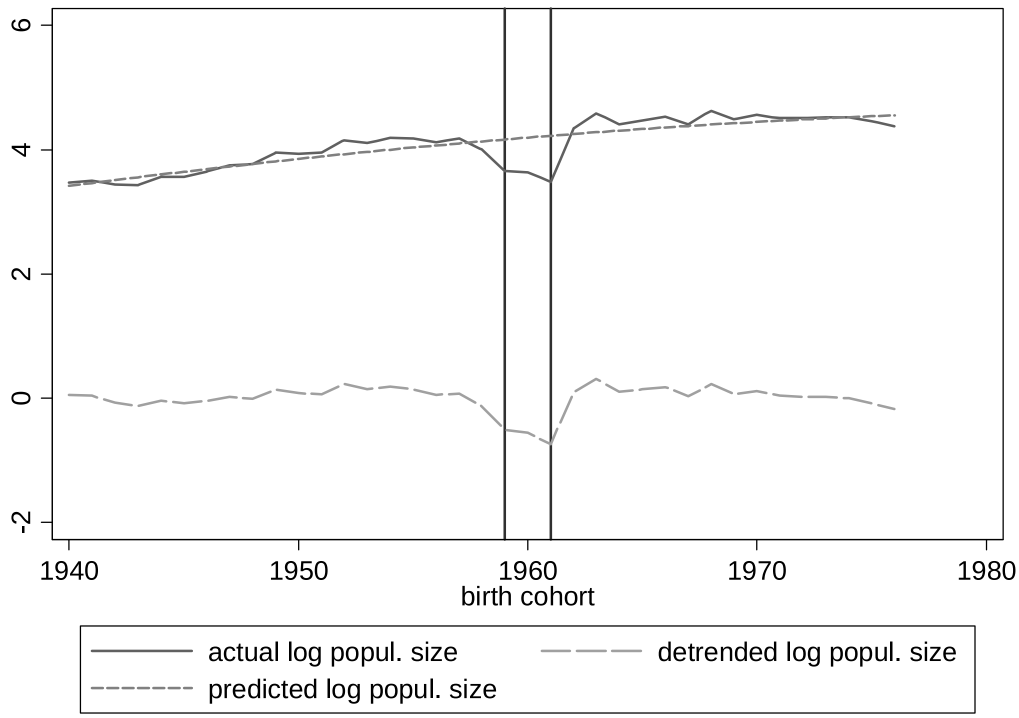


Figure A2: Fraction of Females by Birth Year in 1990