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Food Affordability and Double Catastrophe in Early Life: Lessons from the 1974–75 Bangladesh Famine*

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We study the educational outcomes of the 1974–75 Bangladesh famine among early life survivors using the 1991 Bangladesh micro-census data. We find that famine adversely affected survivor children in areas that experienced higher rice prices relative to labour wages. However, children living in wealthy households in famine-stricken areas escaped the adverse effects and had similar educational outcomes as those with no famine exposure. We also find that, surprisingly, exposure to a double catastrophe (i.e., concurrent famine and flood) in early life had weaker effects on survivor children's education than exposure to a single catastrophe. We show that disaster-alleviation mechanisms were more effective in districts affected by double disasters.

I Introduction

One frequent explanation for the large observed cross-country variations in economic performance is disparities in human capital between countries (Mankiw *et al.*, 1992; Barro & Lee, 1996; Kalaitzidakis *et al.*, 2001). A corpus of studies suggests that health is a key factor in human capital formation (Bleakley, 2010; Madsen, 2018), with early childhood malnutrition being one of the principal drivers (Ampaabeng & Tan, 2013). Several studies also document strong relationships between early life conditions and later life outcomes, such as life expectancy (Doblhammer *et al.*, 2013). Adverse effects appear to be acute for individuals who experienced a malnutrition episode *in utero* or in their first 2 years of life (Bryce *et al.*, 2008).

Investigations into the impact of early life malnutrition on later life outcomes confront the problem of establishing a causal relationship because exposure to any shock is often not random (Chen & Zhou, 2007). To address this drawback, a recent strand of the literature exploits nationwide famines, because famines provide exogenous variation in exposure to malnutrition (Neelsen & Stratmann, 2011). The first approach in this literature uses only the cohort variation, whereby researchers compare birth cohorts that experienced the shock just before or just after birth. This strategy is commonly used when the duration of the famine is very short given that short duration allows less chance of selection bias that might arise from the impacts of

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famine on fertility (Song, 2009; Gørgens *et al.*, 2012). However, estimates from this approach may be questionable because cohorts might experience several other shocks over their lifetime. To tackle this issue, researchers use the cohort variation together with the regional variation pertaining to the intensity of famine exposure. In this setting, a growing body of literature uses famine-related mortality at the regional level as a famine severity measure. This is done in combination with cohort fixed effects to control for any shock that each birth cohort might experience later in life.[1]

The primary objective of the present paper is to investigate the effects of early life malnutrition on survivor children's education outcomes using the 1974–75 Bangladesh famine as a natural experiment. We depart from the existing literature that examines the long-term outcomes of famine-led malnutrition in two major respects. First, we study the early childhood effects of famine by exploiting a famine severity indicator that is tightly linked to the genesis of the famine, which is food affordability at the district level: the percentage decline in the rice-exchange rate of labour (i.e., the price of rice relative to the wage of a unit amount of labour). Our approach differs from many studies that use famine-related mortality to measure famine severity based on the premise that mortality only measures the outcome of an extreme form of malnutrition, while other levels of malnutrition can also afflict famine victims, especially children. Measuring famine-driven mortality in a famine environment is also notoriously difficult (Ravallion, 1997). Second, given that the 1974–75 Bangladesh famine coincided with deadly floods in some districts, we explore the novel question of whether and how a double catastrophe in early life, compared with a single catastrophe, affects later life outcomes. To the best of our knowledge, the question of 'double catastrophes' has hitherto not been investigated in the economics literature.[2] Our objective is to examine not only the later life outcomes of single *versus* double catastrophe, but also how disaster response and recovery mechanisms may have worked differently in the two different disaster scenarios.

In most cases, 'food availability decline' is believed to be the principal trigger of famines. However, in his Nobel Prize-winning work, Sen (1981) posited the 'entitlement approach', arguing that disproportionate access to the available food supply due to hoarding, and more generally, weak distribution mechanisms, were the major causes of famine in Bangladesh. In other words, people lost their entitlement to food despite greater food production because food prices began to rise sharply due to hoarding and speculation about future price increases (see also Ravallion, 1985; Dyson, 1991; Lin & Yang, 2000; Hernández-Julián *et al.*, 2014). While we cannot directly test the food availability and food entitlement explanations because of a lack of proper hoarding data, our famine severity indicator, the rice-exchange rate of labour, measures food affordability based on a key relative price set in the rice and labour markets, and therefore permits gauging the economic foundations of malnutrition. The indicator exhibits strong geographical variation.

Using the 1991 Bangladesh micro-census dataset, which includes 10.58 million individual observations and represents a random 10 per cent sample of the country's population, our results document that the famine-affected cohorts have significantly lower levels of literacy and are more likely to be primary school dropouts later in life. More specifically, famine adversely affected the educational outcomes of survivor children later in life in areas that experienced increased rice prices relative to the wage of a unit amount of labour. We also probe further into this result by exploring whether this effect varied according to socio-economic background. Using two income/wealth indicators available in the census dataset that are good measures of affluence in the Bangladesh context

[1] In this connection, the Chinese famine of 1959–61 has received substantial attention in the literature for its long-term effects (Chen & Zhou, 2007; Huang & Zhou, 2013; Kim *et al.*, 2014; Kim *et al.*, 2017). Findings suggest that the Chinese famine had significant negative effects on survivors' education, height, cognitive skill, labour supply and earnings in adulthood. Neelsen and Stratmann (2011) examined the 1941–42 Greek famine and documented its adverse effects on education and labour market performance for its survivors. For the effects of the Dutch hunger famine in 1944–45, see Scholte *et al.* (2015). See Ampaabeng and Tan (2013) for the Ghanaian famine. See Barker (1998) for the foetal origins hypothesis.

[2] Notably, in a recent study, Leppold *et al.* (2022) undertake a scoping review of the public health impacts of multiple disaster exposures and find suggestive evidence that the public health risks of multiple disaster exposures can exceed those of single-disaster exposure.

– whether the children live in brick-walled houses and whether they live in concrete-roofed houses – we find that the adverse effects of famine are significantly lower for children who live in a concrete-roofed house. Surprisingly, those children had similar educational outcomes compared with children *with no famine exposure at all*. These results suggest that children living in wealthy households during famine were advantaged, which could indicate the hoarding and distribution problems experienced during famine. At the very least, this advantage could be interpreted as evidence for greater food access by the wealthy population and lack of food entitlement for the broader population during famine.[3]

Turning to our second contribution, the 1974–75 Bangladesh famine coincided with deadly floods in some districts. This raises the novel question of whether and how a double catastrophe in early life affects later life outcomes compared with a single catastrophe. The significant geographical differences in famine severity and floods in Bangladesh generate spatial variation to disentangle the effects of these two disasters. We measure flood severity using three novel indicators at the district level: height (ft) of the flood inundation, flood duration (months), and the proportion of flood-affected area to the total area of a district. Our findings indicate that famine is a more dominant adverse factor than flood in explaining later life educational outcomes. Moreover, and surprisingly, educational outcomes of early life malnutrition are more adverse in 'only famine-affected' districts than those in the 'flood- and famine-affected' districts. This result shows that children who experienced two disasters (i.e., famine and flood) had less severe later life consequences than children who faced only one catastrophe (i.e., famine). We provide evidence that this outcome could be explained by the greater number of *langarkhanas*[4] in double- than in single-disaster areas. That is, alleviation mechanisms for disasters were stronger in districts shocked by two catastrophes than those experiencing only one disaster, suggesting that the government provided more resources to areas where the disaster exposure was the greatest.

The remainder of the paper is organised as follows. Section II provides an overview of the 1974–75 Bangladesh famine and 1974 Bangladesh floods. Section III explains the data and measurement of key variables, Section IV describes the empirical approach. Section V discusses the empirical results. Section VI examines the effect of double catastrophe *versus* single catastrophe. Section VII concludes.

II The 1974–75 Famine and the 1974 Flood in Bangladesh

(i) The 1974–75 Bangladesh Famine

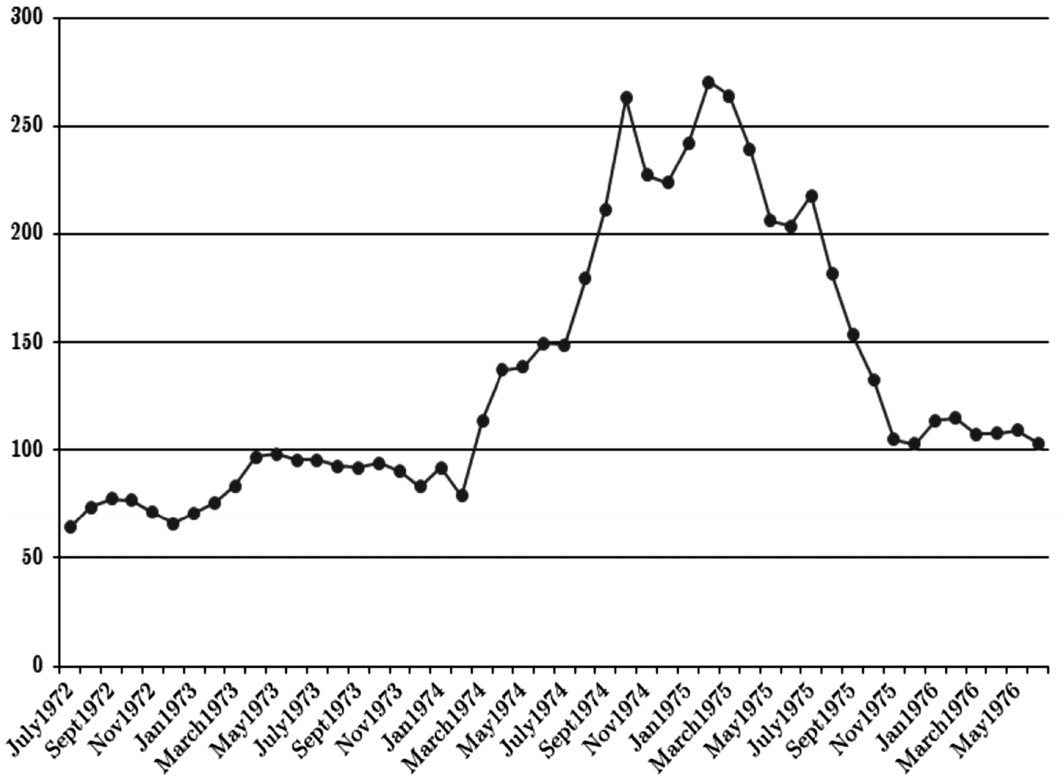
Just three years after the liberation war in 1971, Bangladesh experienced one of the worst famines in recent world history.[5] The famine struck in June 1974 and began to wane by the end of that year; it officially ended in July 1975 (Kagy, 2012). Importantly, the most severe period was between July and October 1974 (Alamgir, 1980; Hernández-Julián *et al*., 2014). Within this short duration, the famine led to the death of arguably around 1.5 million people (Alamgir, 1980; Van Schendel, 2009). Famine occurred in all districts of Bangladesh, but was worse in some districts, particularly those affected by concurrent floods (Sen, 1981). The three most famine-affected districts were Mymensingh, Rangpur and Sylhet, according to the first famine survey by the Bangladesh Institute of

[3] This finding partly accords with one of the early studies by Lin and Yang (2000), who find that both food availability and an urban biased ration system explain the Great China Famine of 1959–61. In a more recent study by Meng *et al*. (2015), weak distribution mechanisms due to central planning were found to be one of the major causes of the Great China Famine. Both studies investigate the problem using macro-level (*i.e*., province and county) mortality and grain production data, while we use person-level census data and study the problem in the context of the 'foetal origins' hypothesis.

[4] *Langarkhana* were a type of gruel kitchen that distributed free cooked meals of modest size to people who were exposed to either flood or starvation. In 1974, the Bangladesh government opened roughly 6,000 gruel kitchens and fed almost 4 million people (Muqtada, 1981).

[5] Other famines around the world in the 20th century include those in the Soviet Union, Ethiopia, Sudan, Mozambique, Nigeria, Niger, Angola, Zaire, Uganda, Somalia and Liberia. Many of these happened after 1980 (Ravallion, 1997).

FIGURE 1
Average Retail Price of Rice in Bangladesh (Taka per Mound).



Source: Hernández-Julián *et al*., 2014.

Development Studies (BIDS) in 1974.[6] Figure 1 presents the average price of rice in Bangladesh for the period July 1972 to June 1976.

In the first few years after independence, Bangladesh was listed as one of the poorest nations in the world. In 1974, nearly 90 per cent of its total population lived in rural areas and the economy was based on agriculture. Although the agriculture sector contributed 60 per cent of the national gross domestic product and supplied employment opportunities to almost 80 per cent of the population, a key idiosyncrasy is that majority of individuals did not own land. In addition, life expectancy was 47 and 49 years for males and females, respectively. Approximately 15 per cent of all children died before the age of five, and more than 50 per cent of households consumed less than the minimum calorie requirements for an adult. Moreover, the nation of 75 million people had just experienced a protracted civil war, and the state machinery was corrupt and incompetent (Quddus & Becker, 2000). It is noteworthy that from March 1974, the price of rice started to upturn sharply (Hernández-Julián *et al*., 2014).

[6] This survey was conducted in November 1974 in 19 greater districts and the most affected districts were identified in terms of the maximum depth of flood inundation, 6 feet and above, the maximum periods of flood, and the number of persons seeking relief during the famine (Alamgir, 1980).

With the above-mentioned fragile socio-economic conditions, even the slightest shock could lead to starvation across the nation. Heavy rainfall and a series of troubling floods along the Brahmaputra River started in June 1974 and destroyed a major part of the *aus* (the principal rice crop harvested in July–August). Flood also washed away the seedlings of the *aman* (the second most important rice crop), as its transplantation period was between July and September. It also affected a part of *boro* (another major rice crop), as its harvest period was April–June. Another important issue was that the import of food grains from abroad was approximately 28 per cent lower in 1974 than in the preceding year (Alamgir, 1980). Like many other developing countries, Bangladesh had been receiving regular food aid from the US. However, this food aid was severely threatened in 1974 when the US withheld 2.2 million tons of food aid and the then-US ambassador to Bangladesh made it clear that the US would probably not continue to commit food aid to Bangladesh due to its exporting of jute to Cuba. These combined issues led to a food crisis in Bangladesh and provide some background for the food availability decline approach.

Despite these events, the quantity of rice production in Bangladesh did not decline, nor did the quantity of food grains. The total production of rice in 1974 was roughly 13 per cent more than it had been in 1973, and the amount food grains in 1974 was 12.36 million tons, almost 7 per cent more than it had been in 1973 (Alamgir & Salimullah, 1977; Alamgir, 1980; Sen, 1981).[7] Further, there was a substantial increase in rice production in the three most affected districts – Mymensing, Rangpur and Sylhet (22 per cent, 17 per cent and 10 per cent, respectively) – between 1973 and 1974 (Alamgir & Salimullah, 1977; Alamgir, 1980).

Taking into consideration the increased rice production and food grain supply, a shortfall in food availability, commonly known as the food availability decline approach, cannot alone explain the 1974–75 famine in Bangladesh. Thus, Sen (1981) put forward a compelling argument that the increased price of rice due to hoarding problems and reduced purchasing power of the people resulted in the famine, commonly known as the 'entitlement approach'. Sen argued that the price of rice increased and rice purchasing power of the daily wage declined during the famine.

Additional supportive evidence for increasing rice prices comes from Ravallion (1985). Using monthly time series of rice prices in Bangladesh from 1972 to 1975, Ravallion finds that newspaper reports of future crop damage resulted in high rice prices in the country during the famine. The rice markets worked poorly and were unable to reflect future scarcities because of the assumptions made by speculators, which resulted in excessive hoarding of rice. In particular, Ravallion believes that hoarders may have had overoptimistic expectations for rice prices during the famine, or they may have expected rationing in the future. By both accounts, the markets failed to deliver their primary function by equilibrating excess demand with price adjustments (p. 28).

Although the reasons for the uncontrolled increase in the rice price are difficult to fully articulate, the key contributing factors are argued to be distributional, including inflation and speculation about future price increases (Sen, 1981; Dyson, 1991; Hernández-Julián *et al.*, 2014). Floods and the cessation of food aid from the US created rumours of famine, which led to hoarding of the available food and the escalation of rice prices in the market. Exploring the data on the rice-exchange rate of labour between June and October 1974 at the district level, Sen (1981) suggests that the purchasing power of rice with respect to rural labour declined most strongly in the three districts (Mymensingh, Rangpur and Sylhet) that experienced the most severe famine. That is, the greater the decline in the rice-exchange rate of labour in a district, the greater the severity of famine in that district. Importantly, the 'crude death rate' among landless families was three times higher than families with three or more acres of land (Chowdhury & Chen, 1977; Currey & Hugo, 2012), suggesting that famine mostly affected wage labourers and landless labourers.

(ii) A Concurrent Disaster: The 1974 Flood in Bangladesh

Flood is a frequent natural catastrophic event in Bangladesh (Mustafi & Azad, 2003; Rayhan, 2010). It occurs every year because of

[7] The data for food production in 1974 included the *aman* crop harvested in the preceding year, that is, November 1973–January 1974, when there were no floods (Sen, 1981). For the chronology of the events for the 1974–75 Bangladesh famine, see Table S1.

monsoonal rain falling in upstream rivers. Evidence suggests that approximately 60 per cent of the land in Bangladesh is flood-prone, and 25 per cent is inundated by monsoon floodwater between June and October every year (Siddique *et al*., 2000).

The 1974 flood was the first major flood to occur after Bangladesh's independence. Despite regular floods in Bangladesh, the 1974 flood was unprecedented for several reasons. First, unlike most floods, which inundate large parts of the land for several days or weeks between July and August every year, the 1974 flood began in June and lasted until October (Siddique *et al*., 2000; Mustafi & Azad, 2003). Second, the 1974 flood was the fifth largest flood between 1954 and 2004 in terms of flood-affected area as a percentage of the total area of the country, and covered 36.84 per cent of the country's land (Hofer & Messerli, 2002). Third, the scale of the flood was huge; nearly 2000 people died (Alamgir & Salimullah, 1977), more than 1 million were injured and millions remained homeless between June and October 1974 (Davis, 2010). Further, the 1974 flood resulted in a considerable drop in production. It destroyed a major part of the *aus* rice crop, the seedlings of the *aman* and a part of the *boro*. It is worth noting that the harvesting periods of *aman* and *boro* are July–August and April–June, respectively, and the transplantation period of *aman* is July–September (Alamgir, 1980; Sen, 1981; Dyson, 1991).

According to Hofer and Messerli (2002), the 1974 flood hit the country in three phases. At the end of June, it attacked the districts situated in the north-western (i.e., Rangpur and Pabna), north-eastern (i.e., Sylhet) and central-eastern (i.e., Mymensingh and Comilla) areas. At the beginning of August, it reached its most severe phase and affected almost all districts. The final phase was concentrated in the north-western part of the country (i.e., Rangpur). Based on the available data for flood severity measures across districts, the maximum depth of inundation was recorded as 15 ft in Mymensingh followed by 13 ft in Sylhet and Chittagong; it persisted for the longest period in Pabna, followed by Dhaka, Rangpur and Mymensingh, for 5 months. In terms of the proportion of flood-affected areas as a percentage of the total area of the district, the highest value was recorded in Comilla (88.51 per cent), followed by Dhaka and Tangail (Alamgir & Salimullah, 1977). The damage and loss estimates due to the 1974 flood in Bangladesh are presented in Table S2.[8] Figure 2 shows the snapshot of the flooded (greater) districts in Bangladesh.

III Data

The main data used in this study are from the Bangladesh Population and Housing Censuses available on the Integrated Public Use Microdata Series (IPUMS) website (Minnesota Population Center, 2014). The IPUMS dataset provides person-level data from the 1991 Bangladesh census round, including 10.58 million person-observations, representing a random 10 per cent sample of the Bangladesh population.[9] This census, conducted on 2 March 1991, includes basic demographic and socio-economic information for everyone in the household.

The 1991 census wave does not include information on place of birth. This might be a challenge for our estimations because the individuals included in our regression sample might have migrated across regions. In particular, the specification without controlling for birthplace information might bias the literacy effects of famine upwardly. However, data on inter-district net migration for the years 1974–81, published by the Bangladesh Bureau of Statistics (1984) and reported by Nabi (1992), allow us check whether migration is likely to affect our results.

The 1991 census dataset does not contain the birth year information of individuals but provides the age information. We compute the year of birth as:

$$\text{Year of birth} = \text{Census year} - \text{Age} - 1.$$

Age reporting is a common problem in developing countries and is mostly linked with lower

[8] Considering the maximum depth of flood inundation, 13 districts experienced no flood, but famine was present in those districts. In terms of the duration of the flood, 10 districts were not affected by flood, but they were affected by famine. Finally, in relation to the proportion of flood-affected areas as a percentage of the total area of the district, seven districts were affected by the famine only. For the flood-affected and non-flood-affected districts under each category of flood severity measure, see Tables S3 and S4.

[9] The IPUMS also include the two latest census rounds, the 2001 and 2011 Bangladesh census waves containing microdata. However, we do not use these two rounds because quantifying the educational outcomes, such as literacy and primary school dropout rates, may have been affected by recent nationwide interventions.

FIGURE 2
Flooded (Greater) Districts in Bangladesh, 1974. [Color figure can be viewed at wileyonlinelibrary.com]

Source: http://www.banglapedia.org/httpdocs/Maps/MF_0103A.GIF.

cognitive ability (Baten *et al.*, 2014). Also called the 'age heaping' problem, this issue does not seem to be severe in our case because we find that the total size of birth cohorts reporting ages ending in 0 and 5 is not substantially larger as compared with their preceding and successive years.[10] However, this does not rule out a possible age reporting issue around the census date, 2 March 1991, for which reason we subtracted 1 from the reported age to compute the birth year more accurately.[11] Since we construct the year of birth this way, we use the birth cohorts born between 1967 and 1981 in the IPUMS dataset in our analysis and do not impose any restrictions on our analysis sample.

(i) Dependent Variables

In examining the effects of famine on education, we use literacy as one of our main outcomes of interest. A person is considered literate if s/he reads and writes. We use a binary measure that is coded 100 (for easy coefficient interpretation) for literate persons, and 0 otherwise. We choose literacy as an educational outcome for two main reasons. First, it helps to understand the level of human capital by gauging the economic value of an individual's skill set. More precisely, literacy facilitates better employment prospects and better socio-economic status for individuals and the economy (Desai, 2012). The extant literature also suggests a nexus between malnutrition in early life and the literacy of adult survivors (Neelsen & Stratmann, 2011). Second, the data on literacy for 10.58 million individuals are available in the 1991 census dataset and enable us to perform a rigorous and systematic analysis of the long-term effects on famine survivors. As the second outcome variable, we use 'being a primary school dropout', which is defined as 100 if a person dropped out of primary school, and 0 otherwise. This is based on the extant literature and anecdotal evidence that shows early childhood malnutrition relates to the discontinuation of a child's schooling (Leiva *et al.*, 2001; Alderman *et al.*, 2006; Molina, 2012).

(ii) Famine Severity: Measuring Food Affordability

We use the percentage decline in the rice-exchange rate of labour as a proxy for food affordability. This measure captures the increase in the price of rice relative to the wage of a unit amount of labour. In this study a greater percentage decline in the rice-exchange rate of labour refers to higher rice prices relative to rural income, more limited food access and therefore greater famine severity. Our regressions use (the absolute value of) the percentage decline in the rice-exchange rate of labour between June and October 1974 for each district in Bangladesh. The data on this variable are compiled by the BIDS (1974), reported in Alamgir and Salimullah (1977) and calculated by Sen (1981, table 8).[12] Figure 3 shows the inter-district variation of the decline in the rice-exchange rate of labour.[13]

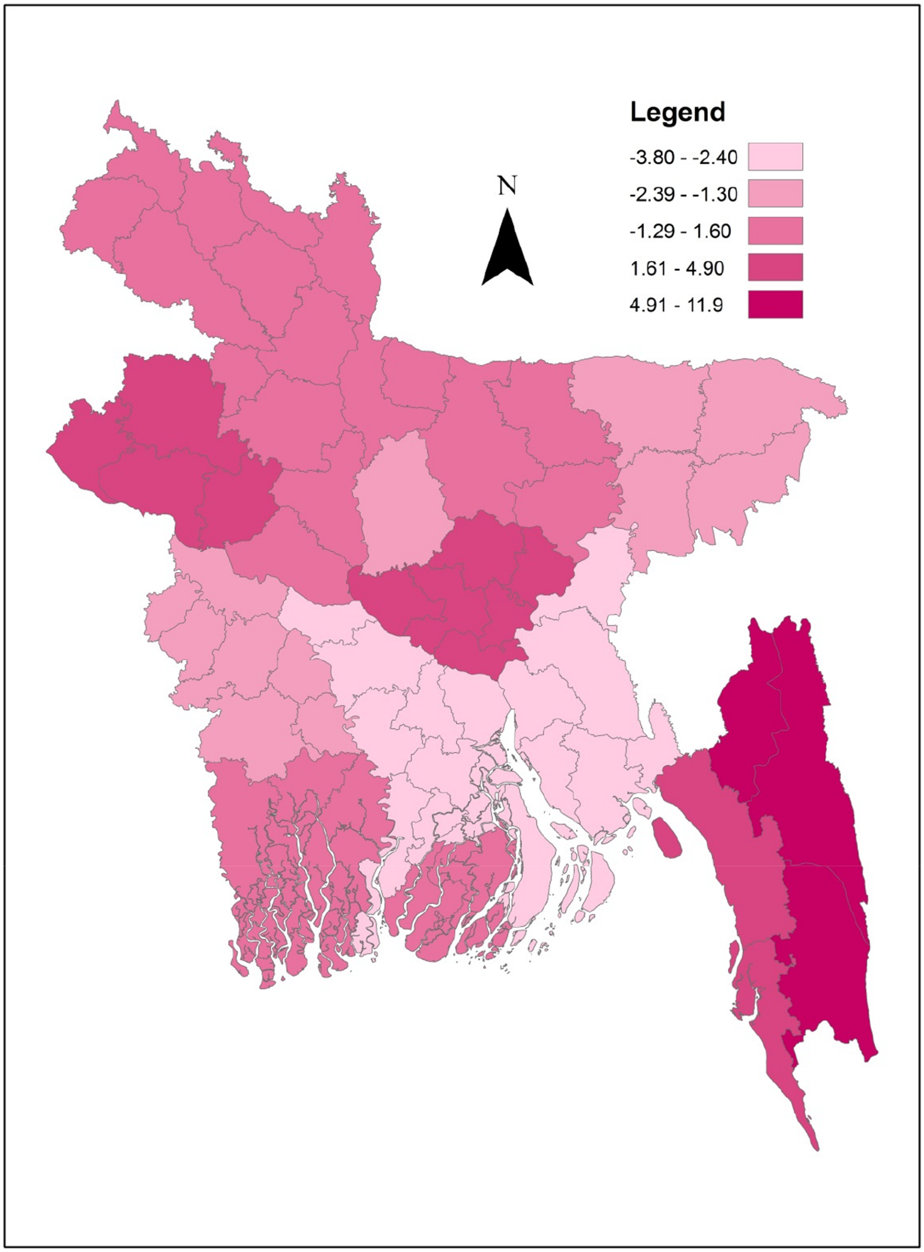
[10] This is possibly because individuals in our sample are young. We notice that the older birth cohorts in the census, such as those above 40 years of age, present an acute age-heaping problem in Bangladesh, as observed in the 1991 census (the age distribution table is available from the authors upon request).

[11] The reliability of age information in the census data depends on many factors, for instance, whether the respondent knows their actual date of birth and if the respondent takes the time to remember their age. Moreover, it is not unlikely in Bangladesh that people report themselves to be younger than they indeed are. We subtract 1 to identify the year of birth for several reasons. For example, the 1991 census in Bangladesh was conducted on 2 March 1991. If a person is born in April 1970, they are more likely to casually report their age as 20 years on the census day, despite their actual age, that is, almost 21 years. Likewise, if a person is born in December 1970, they may choose to report their age as 20 years as well. One issue may arise if this person is born in the first quarter of 1970 if their birthday is just before the census survey and they report their actual age as 21 years. In this case, our formula erroneously calculates their year of birth as 1969 instead of 1970. Also, even if individuals remember their birthday correctly, our approach of subtracting 1 from the census year is likely to provide the appropriate year of birth for the majority of the cohort born between 3 March and 31 December in a given year. However, we exercise caution in our empirical specification by using an age-correction dummy in case this subtraction is not accurate.

[12] Sen (1981), table 8) reports the data as the *decline* in the rice-exchange rate of labour, which is provided in absolute value terms. From now on, the decline in the rice-exchange rate of labour is referred to in this paper in absolute value terms.

[13] We acknowledge that our measure does not consider intra-regional rice trade flows, which may be a limitation in the determination of rice prices (Ravallion, 1985).

FIGURE 3
Decline in the Rice-Exchange Rate of Labour across Districts, June–October 1974. [Color figure can be viewed at wileyonlinelibrary.com]



Source: Data obtained from Alamgir and Salimullah (1977); and Sen (1981).

To elaborate on the economics behind this variable, it is not difficult to imagine that labour here is largely unskilled agricultural labour. According to Ravallion (1997), arguably the rice-exchange rate of rural labour is the single most important relative price determining the probability of starvation during famine. It can measure the high and unstable prices, which can reflect hoarding and speculative behaviour, which in turn depends on miscalculations by hoarders or misreporting by newspapers related to the future scarcity of food. Therefore, the rice purchasing power of agricultural wages in each area is a reasonable measure of food affordability for households, and thus for famine severity. Ravallion also provides evidence that this relative price is correlated with the mortality rate during famine, but argues that measuring mortality in a famine environment is notoriously difficult,[14] while this relative price is easier to measure and therefore much more reliable. Notably, this famine severity measure is reported at the 19 greater district levels in Bangladesh. To obtain district-level famine severity measures, we first identify the regions that each district belonged to and then assign the severity measures for greater districts to the 64 current districts.

(iii) The 1974 Flood Severity Measures

Our flood severity variables are the maximum depth of flood inundation (ft), the period of flood inundation (months) and the proportion of flood-affected areas in the total area of the district. These measures capture different aspects of societal exposure to flood waters, including floodwater height, aerial extent of flooding, and the length of time of the flood. Simple spatial extent does not inform about the flood depth, while floodwater can dry out in a short period of time if the topography allows. Lengthy inundation can cause diseases and interruptions to food networks, agricultural activity and transportation. Typically, farmers welcome moderate flooding because it brings alluvial soils, but excessive flooding is damaging for economic activity. Therefore, it is important to consider the different dimensions of flood severity.

The data were accessed from the Government of Bangladesh Water Development Board Annual Report on Flood in Bangladesh 1974 and 1975. These were compiled by Alamgir and Salimullah (1977) and were also reported at the 19 greater district levels. We follow the same procedure above to convert the data at the 19 greater district levels to the 64 districts. Figures 4–6 show the inter-district variation of flood intensity for each measure, respectively.

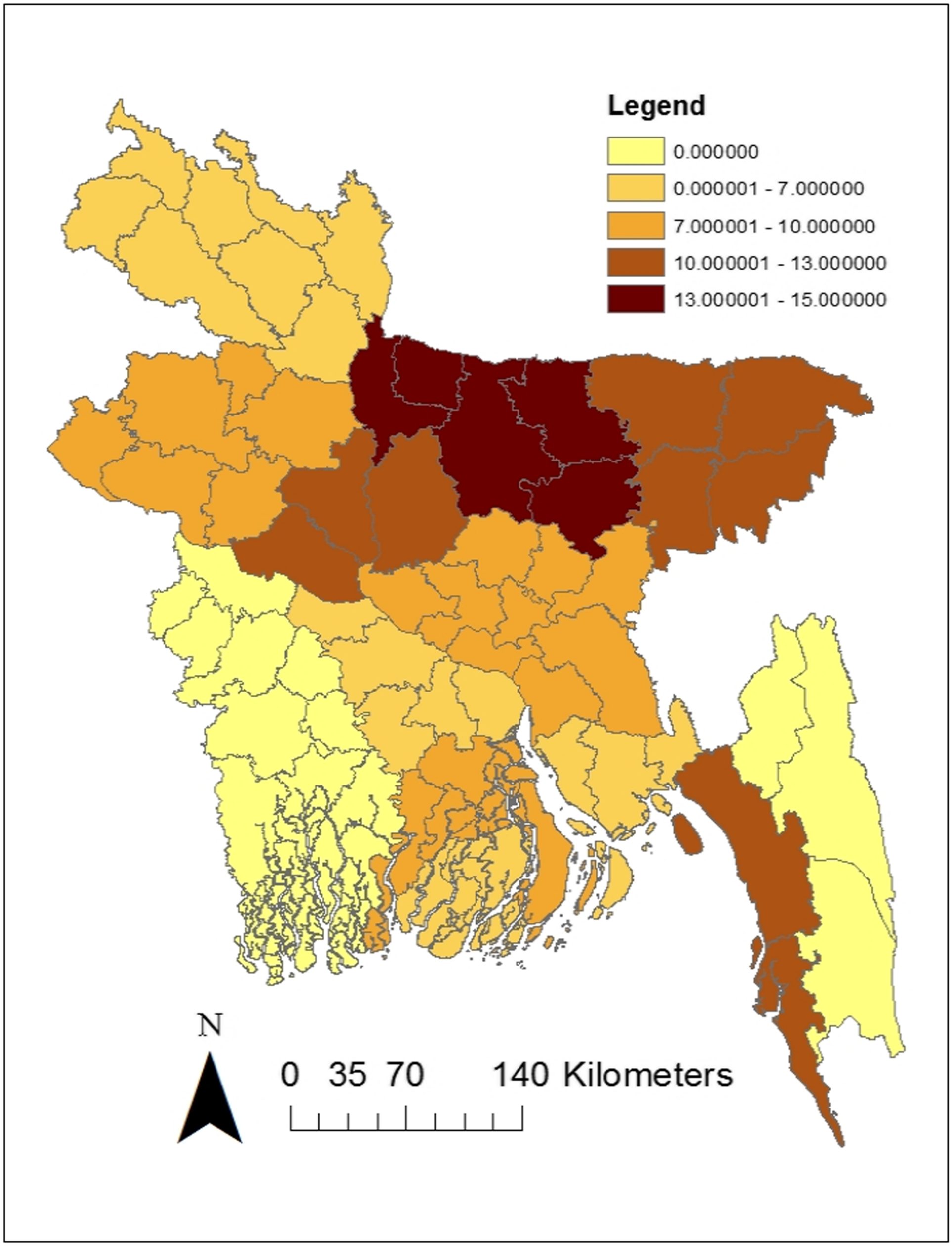
(iv) Definition of the Treatment and Control Groups

To quantify the long-term consequences of the 1974–75 Bangladesh famine among child survivors, we consider the birth cohorts born during the famine and the birth cohorts born within the four years on either side of the famine period as our regression sample. In particular, we construct three treatment groups. Supported by most of the literature, the Bangladesh famine began in June 1974 and ended in July 1975, with the harshest period being between July and October 1974 (Alamgir, 1980; Hernández-Julián *et al.*, 2014). Following Bryce *et al.* (2008), Victoria *et al.* (2008) and Neelsen and Stratmann (2011), it is plausible to assume that the effects of early life undernourishment are experienced in the first three years of life. Unlike the census rounds from other countries such as Greece, the 1991 Bangladesh census does not have the month of birth information. Thus, we consider the identified effects in our regressions as 'early childhood effects'. The 1972–75 birth cohort was between 15 and 18 years of age in the 1991 census round. This treatment group is our benchmark group. The 1972 birth cohort was exposed to famine in their second year; the 1973 birth cohort experienced the famine in their first and second years; the 1974 birth cohort experienced famine *in utero* and in the first year; and the 1975 birth cohort experienced the famine as a foetus. Finally, we confine the birth cohorts to four years before and after the famine period as our comparison group, because these cohorts were not exposed to famine in their first two years of life or *in utero*; namely, the 1968–71 and 1976–79 birth cohorts.

To check the robustness of the estimated measures, our second treatment group uses the

[14] Deaths occur during famine not only because of starvation but also due to the disease environment, health management and healthcare, and suicides. There are also non-linearities associated with mortality and high grain prices (income loss) and consumption during famine, which are difficult to quantify. For example, Ravallion (1987, ch. 2) shows that mortality was an increasing concave function of food grain price during two severe famines in South India in 1877 and Bangladesh in 1974–75.

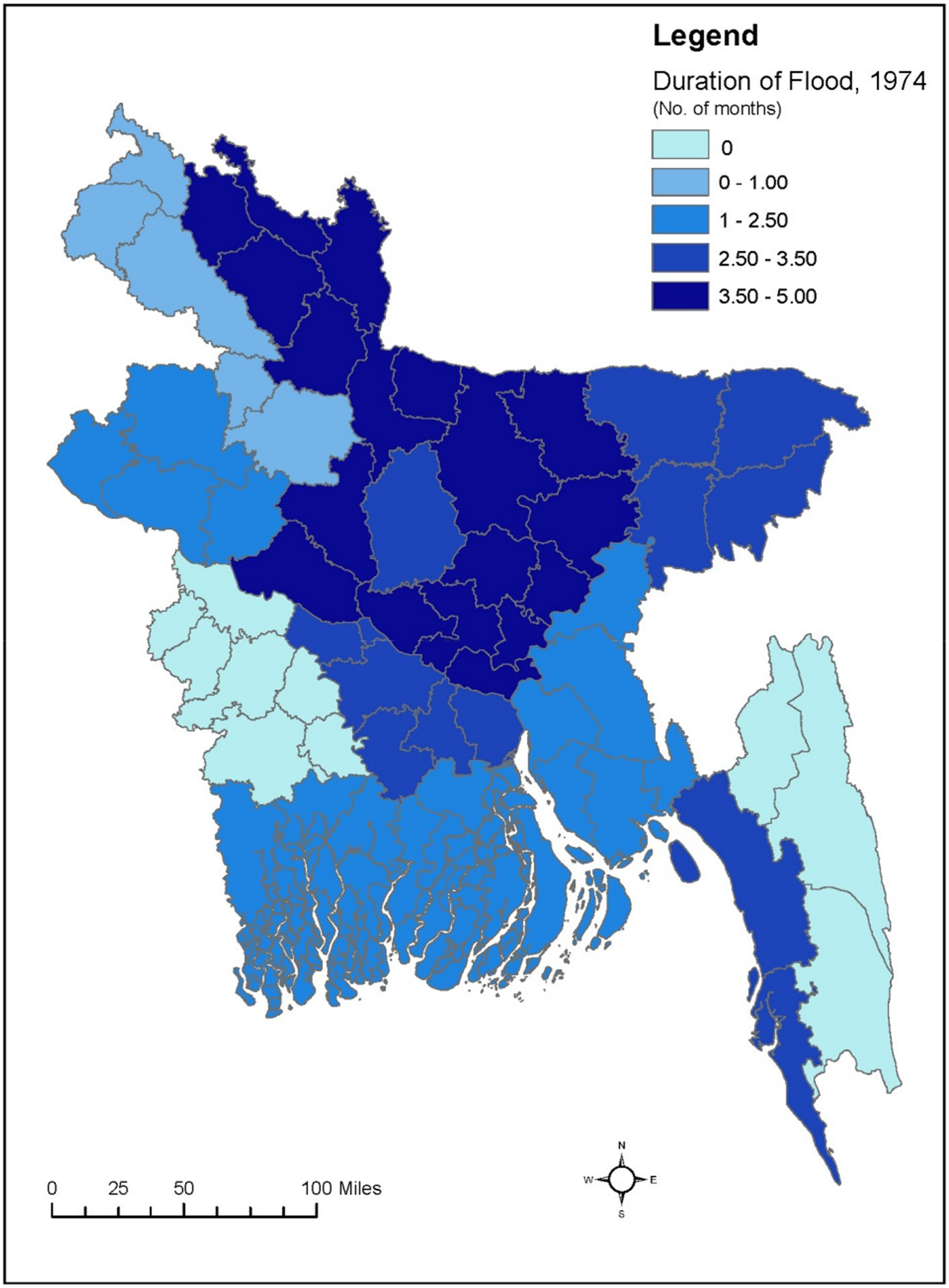
FIGURE 4
Maximum Depth of Flood Inundation (ft) in Districts, 1974. [Color figure can be viewed at wileyonlinelibrary.com]



Source: Data obtained from Alamgir and Salimullah (1977).

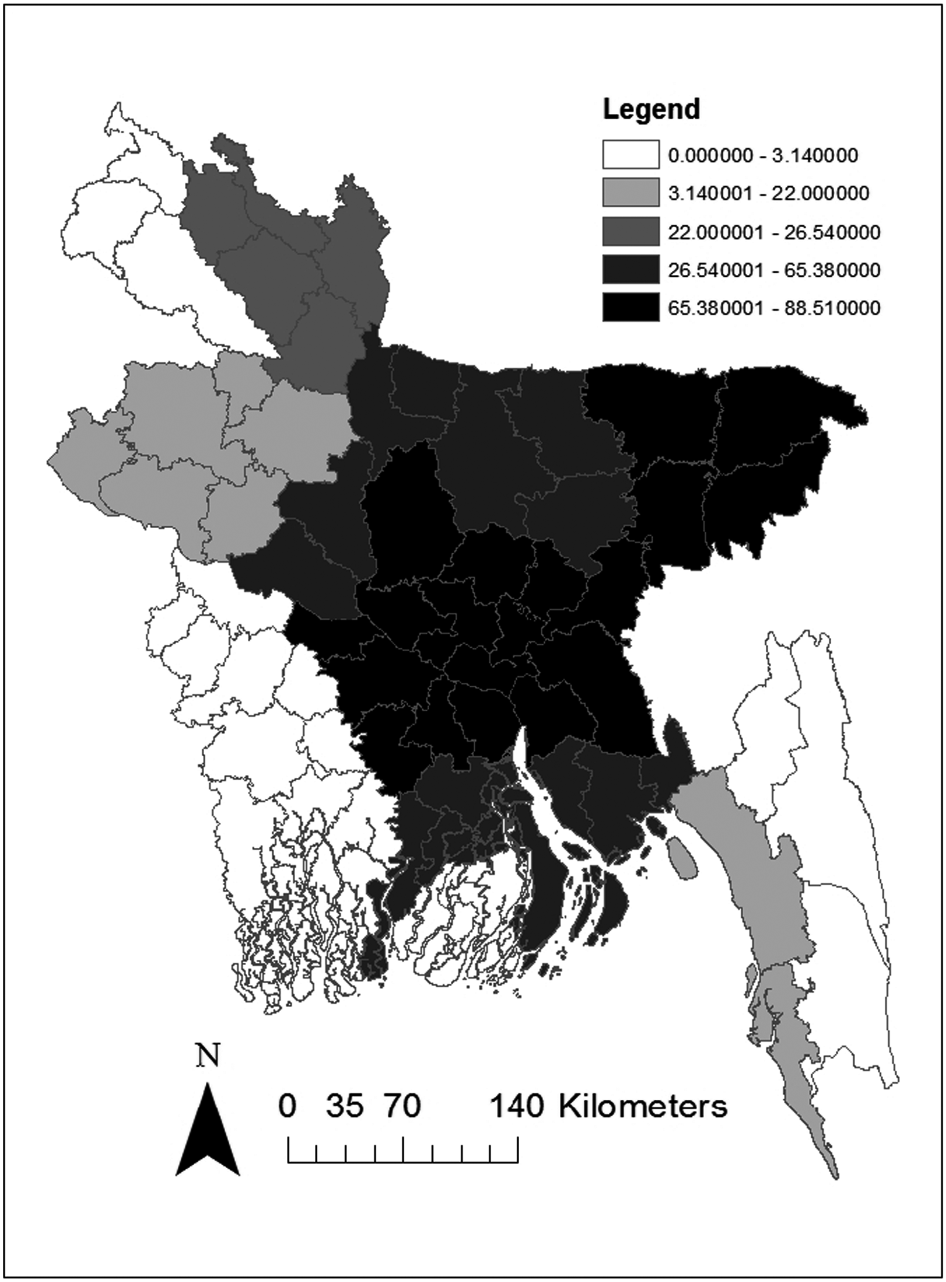
FIGURE 5

Duration of Flood Inundation (Months) in Districts, 1974. [Color figure can be viewed at wileyonlinelibrary.com]



Source: Data obtained from Alamgir and Salimullah (1977).

Figure 6
Total Flood-Affected Area in the Total Surface Area of the Districts (%), 1974.



Source: Data from Alamgir and Salimullah (1977).

Table 1
Descriptive Statistics: Means and Standard Errors

	Benchmark sample (1)	Treatment group (both famine and flood) (2)	Treatment group (only famine) (3)	Control group (4)
Treatment dummy (No = 0, Yes = 1)	0.324			
	(0.468)			
Individual characteristics				
Age (years)	16.316	16.558	16.546	16.200
	(3.557)	(1.240)	(1.213)	(4.236)
Birth year	1973.684	1973.442	1973.453	1973.800
	(3.557)	(1.240)	(1.213)	(4.236)
Literacy (No = 0, Yes = 100)	45.118	43.492	42.721	48.505
	(49.761)	(49.575)	(49.467)	(49.978)
Primary school dropout (No = 0, Yes = 100)	61.750	63.939	64.381	57.190
	(48.600)	(48.018)	(47.887)	(49.480)
Male (No = 0, Yes = 1)	0.504	0.513	0.501	0.499
	(0.500)	(0.500)	(0.500)	(0.500)
Rural (No = 0, Yes = 1)	0.779	0.781	0.794	0.778
	(0.415)	(0.414)	(0.403)	(0.415)
Famine severity measure				
% decline in the rice-exchange rate of labour (in absolute value)	41.777	41.797	33.252	41.768
	(15.472)	(15.428)	(6.472)	(15.493)
Flood severity measures				
Maximum depth of flood inundation (ft)	8.510	8.529		8.501
	(4.395)	(4.383)		(4.401)
Period of flood inundation (months)	2.747	2.748		2.745
	(1.320)	(1.321)		(1.318)
Proportion of flood-affected areas as a % of the total area of the district	45.555	45.550		45.558
	(30.384)	(30.359)		(30.396)
Number of observations	2,475,054	802,663	108,199	1,672,391

Notes: Column 1 includes individuals who were born between 1968 and 1979 and resided in Bangladesh in 1991. Column 2 includes individuals born in 1972–75 who experienced both flood and famine in their first 24 months of life or *in utero*. Column 3 includes individuals born in 1972–75 who experienced only famine in their first 24 months of life or *in utero*. Column 4 includes individuals born during 1968–71 and 1976–79 who experienced neither flood nor famine in their critical age.

1972–76 birth cohort. We add the 1976 birth cohort to the benchmark treatment construction because although the famine was over in July 1975, persons born in early 1976 were exposed to famine *in utero* in 1975. Given that the treatment group includes a five-year window now, our new control group is expanded to include five yearly windows and covers the birth cohorts born between 1967–1971 and 1977–81.

Our third treatment group construction consists of the 1972–74 birth cohort as the treatment group and the 1976–78, 1968–69 and 1971 birth cohorts as the control group. We exclude the 1971 and 1976 birth cohorts from our regression sample as another way of taking caution in case subtracting 1 from age was not suitable in calculating the year of birth.

(v) Descriptive Statistics

Table 1 presents the descriptive statistics for the key variables. Our benchmark regression sample includes 2,475,054 people, of whom 802,663 were born between 1972 and 1975 (i.e., the treatment group), 1,672,391 were born between 1968 and 1971 and between 1976 and 1979 (i.e., the comparison group). The average age of the regression sample is about 16 years. A total of 50 per cent of the sample is male and 78 per cent of the sample lives in rural areas. On average, 45 per cent and 62 per cent of the sample are literate and primary school dropouts, respectively, with a sizeable variation in both literacy and primary school dropout rates, as shown by the standard deviations of almost 50 and 49, respectively. The mean percentage decline in the rice-exchange rate of labour in our sample is 41.777 per cent. This figure is very close to the Bangladesh's mean reported by Sen (1981, table 8): 42.2 per cent. The means of flood severity measures are as follows: the maximum depth of inundation was 8.5 ft, the period of inundation was 2.75 months and the proportion of flood-affected areas in the total district area was 45.6 per cent.

Turning to the treatment and control group statistics, both groups exhibit large similarities in their average demographic characteristics, such as age, gender and rural residence. However, the control group consists of more literate individuals than the treatment group (48.5 per cent *versus* 43.6 per cent) and a lower proportion of primary school dropouts (57.2 per cent *versus* 63.9 per cent). These differences are not entirely unexpected and show that even in the raw data, famine-affected birth cohorts score less favourable educational outcomes than unaffected cohorts. In the raw data, the education outcomes are also slightly less favourable in 'only famine-affected' areas than in 'both famine and flood-affected' areas.

IV Econometric Approach

To empirically quantify the educational effects of famine on the early life survivors, we estimate the following model:

$$Y_{ijt} = \lambda_0 + \lambda_1\left(\text{Treatment}_i \times \text{FamineSeverity}_j\right) + \lambda_2(\text{Treatment}_i \times \text{AgeCorrection}_{it}) + \mathbf{X}_i'\boldsymbol{\beta} + \alpha_j + \gamma_t + \epsilon_{ijt} \quad (1)$$

where Y_{ijt} is the educational indicators described above, employed in alternate regressions for individual i, born in district j in year t. Treatment_i is a binary variable that indicates whether individual i was born during the famine. The benchmark treatment group is equal to 1 if a person was born in 1972–75 and 0 if born in 1976–79 or 1968–71. FamineSeverity_j represents our food affordability measure, the absolute value of the percentage decline in the rice-exchange rate of labour in district j. Turning to the control variables, even though we try to address the age heaping problem in our calculation of birth year and treatment construction, as a further caution we interact the treatment indicator with $\text{AgeCorrection}_{it}$ that takes 1 for the birth cohorts whose ages end with '0' or '5', in case this subtraction is not accurate. The matrix $\mathbf{X}_i$ includes further control variables for individual i, including gender (which takes 1 if the individual i is male, and 0 otherwise), religion (which takes 1 if the individual i is non-Muslim, and 0 otherwise), and rural residence as indicated by 1, and 0 otherwise. We also include the year of birth of individual ? and the squared year of birth (not shown in Eqn 1) to address possible non-linearities in outcome trends (Almond & Mazumder, 2005; Neelsen & Stratmann, 2011). $_{\alpha}$ denotes the district fixed effects and is used to control the time-invariant factors that vary across districts. The inclusion of $_{\gamma}$, cohort fixed effects, ensures that possible common shocks that each cohort might have experienced in their later lives (e.g., change in education policies, teaching availability and quality, and other demographic and economic shocks) are controlled for in terms of their effects on the educational outcomes. The key variable of interest is the interaction variable between Treatment_i and FamineSeverity_j. We present the estimates of $_{\lambda}$ from Equation (1), which is expected to be negative if famine indeed adversely affected the literacy rates and positive if it increased the primary school dropout rates.

In another set of regressions, we estimate Equation (1) using the control group born only after the famine. In other words, we drop the cohort born before famine from the control group in case there is the possibility that children above 2 years of age were also affected by famine.

To probe further into food affordability, we also use a three-way interaction term among the treatment indicator, famine severity indicator and two wealth indicators. The wealth indicators available in the census include whether individuals live in a brick-walled house, and whether they live in a concrete-roofed house. We estimate the following model:

$$Y_{ijt} = \eta_0 + \eta_1\left(\text{Treatment}_i \times \text{FamineSeverity}_j\right) + \eta_2\left(\text{Treatment}_i \times \text{FamineSeverity}_j \times \text{W1}_i\right) + \eta_3\left(\text{Treatment}_i \times \text{FamineSeverity}_j \times \text{W2}_i\right) + \eta_4(\text{Treatment}_i \times \text{AgeCorrection}_{it}) + \mathbf{W}_{\mathbf{ij}}'\Omega + \mathbf{X}_{\mathbf{i}}'\pi + \alpha_j + \gamma_t + \epsilon_{ijt} \quad (2)$$

where W1_i and W2_i stand for the two wealth variables,[15] and the matrix $\mathbf{W}_{\mathbf{ij}}$ includes the

[15] It is assumed that the individual lived in the same type of house during the famine and in the census year 1991.

appropriate components of the triple interaction terms (i.e., each variable in composite triple interaction terms is included in the regression on their own and in double interaction with the other two variables). The key objective of the three-way interaction is to identify whether the effects of famine varied with respect to socio-economic status within districts. Those households that live in a brick-walled house or a concrete-roofed house are more likely to be able to afford the available rice given their higher income and wealth status. In our benchmark regression sample, 12 per cent lived in brick-walled houses, while 6 per cent lived in concrete-roofed houses as of 1991.[16] To estimate a meaningful effect of one wealth variable by controlling for the other, we include both variables in the specification. We present the estimates of $_{\eta}$, $_{\eta}$ and $_{\eta}$.

To investigate the role of double catastrophe in early life on educational outcomes, we augment Equation (1) by additionally controlling for the interaction between treatment indicator and flood severity across districts as follows:

$$\begin{aligned} Y_{ijt} = \delta_0 + \delta_1 \Big(\text{Treatment}_i \times \text{FamineSeverity}_j\Big) \\ + \delta_2 \Big(\text{Treatment}_i \times \text{FloodSeverity}_j\Big) \\ + \delta_3 (\text{Treatment}_i \times \text{AgeCorrection}_{it}) + \mathbf{X}_\mathbf{i}' \varphi \\ + \alpha_j + \gamma_t + \epsilon_{ijt} \end{aligned} \quad (3)$$

where FloodSeverity_j denotes, in alternate regressions, the three flood severity measures mentioned above. We only use the benchmark treatment construction in this estimation and report the estimates of δ_1 and δ_2. This approach would indicate whether our results related to famine are driven by floods or whether they still exert independent effects. In an alternative estimation, we estimate Equation (1) by splitting the sample into two groups: 'both flood- and famine-affected' and 'only famine-affected', to identify the dominant factor resulting in early life malnutrition that led to adult educational outcomes.

All models are estimated using ordinary least squares where robust standard errors are clustered at the district level.

V Empirical Results

(i) Benchmark Results

Table 2 presents the estimated coefficients from Equation (1) using the interaction variables between famine severity and alternative treatment constructions in separate regressions. Panel A presents the coefficients of interaction terms for two outcomes – literacy and primary school dropout – by making use of the 1968–79 sample, while panel B reports the same by using the 1972–79 sample; that is, by including only those born after famine in the control group. The results with individual control variables **X** are not greatly different than those without, hence we present the former, but for brevity, we do not present their coefficients in the tables.

In panel A of Table 2, the estimated coefficient for the interaction term between the benchmark treatment indicator – the 1972–75 cohort – and the percentage decline in the rice-exchange rate of labour is −0.057 for literacy and – 0.064 for primary school dropout (columns 1 and 4). This means that, at the mean percentage decline in the rice-exchange rate of labour in a district (41.777 per cent), individuals of that district are 2.38 percentage points less likely to be literate and 2.67 percentage points more likely to have dropped out from primary school later in life. These results are statistically significant at the 1 per cent level. To gain a better understanding of the economic significance of these results, we translate these coefficients to population effects. Given that our sample consists of a random 10 per cent of the population in Bangladesh in 1991 that was born between 1968 and 1979 and that this sample includes 2,475,054 individuals, the coefficient of 0.057 (in absolute terms) would mean that for each one standard deviation increase in the percentage decline in the rice-exchange rate of labour, an estimated 7,072,100 (= 24,750,540 × 0.324 × 0.057 × 15.472) children born during 1972–75 remained illiterate later in life in Bangladesh. Similarly, the estimate in column 4 for primary school dropout suggests that, for each 1 standard deviation increase in the percentage decline in

[16] We also considered whether agriculture is the main source of income for the household instead of the two wealth indicators. A total of 42 per cent of the individuals in the sample belonged to a household whose main source of income is agriculture. The estimated three-way interaction terms involving agriculture as the source of income are insignificant. One explanation for this finding is that an individual who derives income from agriculture may only be a worker on the land, and hence may not have control over the output/produce, while a wealthy person is likely to be a landowner, and hence may control the output/produce. The results are available from the authors upon request.

Table 2
Long-Term Effects of the 1974–75 Famine on Education Outcomes among Early Life Survivors

Outcome →	Literacy 1972–75 Cohort (1)	1972–76 Cohort (2)	1972–74 Cohort (3)	Primary school dropout 1972–75 Cohort (4)	1972–76 Cohort (5)	1972–74 Cohort (6)
Panel A: Control group includes people born before and after the famine						
Coefficients on ↓						
Treatment dummy × % decline in rice- exchange rate of labour	−0.057***	−0.087***	−0.056***	0.064***	0.090***	0.063***
	(6.234)	(5.947)	(6.069)	(5.491)	(4.585)	(5.074)
Number of observations	2,475,054	3,220,132	1,708,121	2,475,054	3,220,132	1,708,121
R^2	0.078	0.110	0.087	0.075	0.132	0.094
Panel B: Control group includes people born after the famine						
Coefficients on ↓						
Treatment dummy × % decline in rice-exchange rate of labour	−0.103***	−0.138***	−0.119***	0.112***	0.141***	0.132***
	(6.144)	(6.254)	(6.398)	(5.144)	(4.499)	(5.331)
Number of observations	1,708,020	2,366,218	1,650,994	1,708,020	2,366,218	1,650,994
R^2	0.066	0.115	0.085	0.067	0.149	0.100

Notes: Each specification estimates separate regressions exploiting (the absolute value of) the percentage decline in the rice-exchange rate of labour as the famine severity measure interacted with the treatment dummy. A greater percentage decline in the rice-exchange rate of labour refers to higher rice prices relative to rural labour income, more limited food access, and therefore greater famine severity. Only the coefficients on interaction variables are presented. All specifications control for rural, male, birth year, birth year squared, religion dummies, a treatment dummy interacted with the age-correction dummy, district fixed effects and cohort fixed effects. The treatment dummy in columns (1) and (4) is equal to 1 if a person was born in 1972–75 and 0 if born in 1976–79 or 1968–71. The treatment dummy in columns (2) and (5) is equal to 1 if a person was born in 1972–76 and is 0 if born in 1977–81 or 1967–71. The treatment dummy in columns (3) and (6) is equal to 1 if a person was born in 1972–74 but is 0 if born in 1976–78 or 1968–69 and 1971. Robust standard errors are clustered at the district level. t-statistics (in absolute values) are presented in parentheses. *$P < 0.10$, **$P < 0.05$, ***$P < 0.010$.

rice-exchange of labour, an estimated 5,479,700 (= 17,080,200 × 0.3
24 × 0.064 × 15.472) children born during 1972–75 dropped out of school later in life. A 1 standard deviation change in the famine severity indicator from the mean in both directions roughly covers 16 of the 19 greater districts in Bangladesh (Sen, 1981, table 8), so these estimated figures can reasonably be close to the population effects.

We also conduct the same estimations using the two alternative treatment group constructions (i.e., 1972–76 and 1972–74) and interact them with the famine severity measure in columns 2, 3, 5 and 6. We find qualitatively similar results for these sets of treatment indicators, though the 1972–76 cohort yields a higher coefficient estimate (−0.087 and 0.090 for literacy and primary school dropout, respectively). This is likely to indicate that the 1976 cohort is unlikely to be affected by famine (see below). Taken together, the effects of famine are pronounced in districts where individuals lost entitlement to food access.

Previous studies (Islam *et al.*, 2016, 2017) document that early life exposure to shocks may extend beyond 2 years of age. Thus, inclusion of the pre-famine birth cohort in the control group could be problematic because they were also somewhat exposed to famine. Therefore, we estimate Equation (1) using only those born after famine as the control group (i.e., by making use of the 1972–79 period where the 1976–79 is the control group). The estimates in panel B in Table 2 remain strongly significant, confirming that our benchmark results are not driven by control group definition. As expected, the results presented in panel B have higher coefficient magnitudes, possibly because the cohorts born before 1972 might include famine-affected

TABLE 3
Long-Term Effects of the 1974–75 Famine: The Role of Wealth Indicators and the Rice-Exchange Rate of Labour

	Literacy			Primary school dropout		
Outcome →	1972–75 Cohort (1)	1972–76 Cohort (2)	1972–74 Cohort (3)	1972–75 Cohort (4)	1972–76 Cohort (5)	1972–74 Cohort (6)
Treatment dummy × % decline in the rice-exchange rate of labour	−0.057***	−0.085***	−0.057***	0.063***	0.085***	0.064***
	(6.035)	(5.275)	(5.835)	(5.041)	(3.957)	(4.843)
Treatment dummy × % decline in the rice-exchange rate of labour × Living in a brick-walled house	−0.025	−0.031	−0.010	0.034	0.006	0.023
	(0.779)	(0.746)	(0.315)	(1.141)	(0.176)	(0.741)
Treatment dummy × % decline in the rice-exchange rate of labour × Living in a concrete-roofed house	0.039	0.030	0.056*	−0.074**	−0.050	−0.096***
	(1.219)	(0.773)	(1.788)	(2.253)	(1.245)	(3.224)
Number of observations	2,465,749	3,209,195	1,701,590	2,465,749	3,209,195	1,701,590
R^2	0.113	0.141	0.113	0.114	0.166	0.114

Notes: Only the coefficients on interaction variables are presented. The percentage decline in the rice-exchange rate of labour is in absolute values. All specifications control for rural, male, birth year, birth year squared, religion dummies, a treatment dummy interacted with the age-correction dummy, district fixed effects and cohort fixed effects. The treatment dummy in columns (1) and (4) is equal to 1 if a person was born in 1972–75 and 0 if born in 1976–79 or 1968–71. The treatment dummy in columns (2) and (5) is equal to 1 if a person was born in 1972–76 and is 0 if born in 1977–81 or 1967–71. The treatment dummy in columns (3) and (6) is equal to 1 if a person was born in 1972–74 but is 0 if born in 1976–78 or 1968–69 and 1971. Robust standard errors are clustered at the district level. *t*-statistics (in absolute values) are presented in parentheses. *$P < 0.10$, **$P < 0.05$, ***$P < 0.010$.

children (though to a lower degree than those born during famine), and removing them from the control group increases the coefficient.

(ii) The Role of Affluence in Famine Effects

We now estimate Equation (2) to shed further light on our results. Higher wealth means greater access to food, and this might even lead to speculative behaviour. Table 3 presents the results of the three-way interactions between the benchmark treatment indicator, famine severity, and the wealth indicators ‘living in a brick-walled house’ and ‘living in a concrete-roofed house’.

In the regression, the two-way interaction between the treatment indicators and the famine severity is expected to be negative. For the three-way interaction, we expect a positive sign for literacy because the higher wealth might have alleviated the adverse famine effect or even provided an advantage for better education later in life for children, giving them a comparatively better chance of being literate, and a negative sign for being a dropout given that a child from an affluent family has a lower chance of dropping out of school than someone in the associated comparison group.

As anticipated, the estimates of the three-way interactions are positive for literacy and negative for primary school dropout (columns 1–6). While the triple interactions that include living in a brick-walled house as the wealth indicator are insignificant, there is statistically significant evidence to suggest that living in concrete-roofed house may have made a difference to famine effects. The coefficient estimate in column (3), 0.056, was significant at 10 per cent level and obtained with the 1972–74 treatment cohort, suggesting that the probability of being literate increases for a famine-affected person if that person lives in a concrete-roofed house. Crucially, the coefficients of the two-way interactions between treatment and famine severity are similar (in absolute terms) to that of the three-way interaction coefficient, suggesting that the adverse famine effects may have been completely offset in a wealthy household.

We estimate similar regressions for being a primary school dropout, and the three-way interactions are even stronger. The estimates are significant at the 5 per cent and 1 per cent levels with the 1972–75 and 1972–74 treatment group constructions, respectively (columns 4 and 6). The estimated coefficients are slightly larger (in absolute terms) for triple interactions than the two-way interaction between treatment and famine severity. This finding suggests that the probability of being a dropout for a famine-affected person from a district with higher famine severity and living in concrete-roofed house is similar to the counterfactual person with no famine exposure, or alternatively, significantly lower than that of a famine survivor from a less wealthy household.

While not direct evidence, this finding could be interpreted as a support for the argument that wealthier households would have had more access to (or control over) food, which may have been hoarded, such that their children had a better later life educational outcome than a survivor living in a less wealthy household. At the very least, there is evidence that the famine effects did vary with socio-economic status within the districts where the relative price of rice was higher with respect to labour.

(iii) Robustness Checks

Inter-district migration

Despite several measures, selection biases may still prevail in our estimation in several ways. Persons in districts with greater famine severity might migrate to regions that are less famine-stricken, as migration is not restricted in Bangladesh. Moreover, the 1991 census wave does not identify where someone was born, or where they lived during the famine. In a country that has such high migration rates, it may not be plausible to assume that where someone currently resides is where they have always lived. With this point in mind, we analyse the inter-district net migration data for between 1974 and 1981. Specifically, we run regressions of inter-district net migration rate,[17] the food affordability measure, and the three flood severity measures. Table S5 reports that there is no significant relationship between famine and flood severity measures and net migration.[18] While we do not make a blanket conclusion that migration does not affect our results, we take comfort that in our setting there is an insignificant association between migration and famine (i.e., food affordability) and flood severity measures. Nonetheless, a caveat is due when interpreting our results.

Comparability of treatment and control groups

An important concern with the treatment-control design is to ensure the comparability between the two groups. We take several measures to be able to meaningfully compare the birth cohorts born before, during, and after famine. First, using cohort fixed effects ensures that we control for later life shocks that are common to all cohorts across Bangladesh. However, it is possible that cohorts living in different districts could have faced uncommon shocks, reducing comparability between the treatment and control cohorts in different districts. This problem is likely to arise due to differential famine severity faced by different district where different levels of famine severity might be correlated with different levels of future investment in education. To address this possibility, we control for district-specific cohort trends (see Table S6, panel A) and find that our estimates are robust to this issue.

Parental controls and family characteristics.

Our estimated results could be biased due to selection bias caused by parental and/or family characteristics. To address this, we control for parental characteristics (literacy and income sources of both parents) in the regressions as well as family/dwelling fixed effects (see Table S6, panels B and C). Our results remain similar.

Infant/child mortality bias

Neelsen and Stratmann (2011) find that cohorts with stronger genetic endowments are more likely to survive famine shocks. Using data from a small rural sample in Bangladesh, Razzaque *et al.* (1990) find that infant and child mortality were higher in areas affected during 1974–75 famine up to the 24-month period after famine. To our knowledge, no data on district-level infant mortality rates during the famine exist in Bangladesh to allow us to investigate the problem in our context. Therefore, we acknowledge that the 'survival of the fittest' effect (along with the fact that genetic endowments can affect the socio-economic outcomes) could downwardly bias the literacy effects of famine.

Fertility bias

People could respond to famine by differential fertility. If poorer people have fewer children during famine, this could also bias our literacy estimates downwards. In the absence of district-

[17] Interregional migration in Bangladesh is mostly from rural to urban areas, so we can assume that net migration is dominated by out-migration from rural areas.

[18] The results do not change appreciably when we remove two major cities, Dhaka and Chittagong, from the sample.

level data on births and deaths during famine, we estimate regressions to understand whether our food affordability indicator can predict sample size differences between the treatment and control group in the census data (see Table S7). The results showed that famine effects are unlikely to be driven entirely by cohort size differences.[19]

Treatment construction

We prefer to construct the treatment and control groups over a block of time, with the 1972–75 period being the benchmark, because the census dataset is prone to reporting and recording issues related to birth year. Also, although the most severe period of Bangladesh famine was July 1974–October 1974, the famine officially ended in July 1975. This suggests that from the foetal origins perspective, it is not entirely clear which birth cohorts were *in utero* or newborn, and so on. Nonetheless, constructing the treatment indicator with year-by-year treatment for the years 1972, 1973, 1974 and 1975, delivers essentially similar results. Table S8 shows that the coefficients of the years 1972, 1973 and 1974 all interacted with famine severity, are statistically significant, with relatively comparable estimates. The effect of 1975 is insignificant.[20]

VI Double Catastrophe in Early Life

The coefficients of our benchmark model in Equation (1) could be biased by one of the deadliest floods in 1974 in Bangladesh that occurred concurrently with the 1974–75 famine. Hence, whether the effects of malnutrition in 1974 on the early childhood birth cohorts are engendered from the first disaster (i.e., famine), or whether they are biased by the second disaster (i.e., flood), are valid concerns.[21]

The double catastrophe also provides unique opportunity to understand and disentangle the effects of two concurrent disasters on later life outcomes.[22] Crucially, the duration of severe famine (i.e., July–October 1974) coincided with the duration of heavy floods, yet the spatial intensity of both floods and famine in 1974–75 varied greatly across districts in Bangladesh (Alamgir, 1980; Hernández-Julián *et al*., 2014). While the famine affected all of the districts in Bangladesh, the floods only affected a subset of districts (Alamgir & Salimullah, 1977; Sen, 1981). Thus, we exploit the spatial variation in both flood and famine severity to quantify and compare the effects of these two concurrent exogenous shocks. In this query, we estimate Equation (3) using three different flood severity measures in alternate regressions.

(i) Results

The estimation results of Equation (3) are reported in Table 4. Importantly, we verify that the effects of famine remain similar after controlling for the effects of floods in 1974. The coefficients of the two-way interaction terms for famine effects are roughly similar to those reported in Table 3, indicating that famine effect is relatively independent from floods. The table also reports the coefficients of the two-way interaction between the benchmark treatment indicator and the flood severity measures. We find that the coefficients of the interaction between treatment and the flood severity measures are comparatively weakly significant. Of the flood severity measures, the period of inundation (months) is negative and significant at 10 per cent, suggesting that the districts where inundation was longer by one month reduces likelihood of literacy in that district by 0.32 percentage points. This could be because lengthy inundation might have caused adverse effects on children due to the diseases or interruptions to food network, agricultural activity and transportation. The maximum depth of inundation and the share of flooded districts have no explanatory power on literacy. We also find almost similar results for the primary school dropout, with the interactions between treatment indicator and the period of

[19] R^2 from this regression = 0.009.

[20] As an additional robustness check, clustering the standard errors at the 19 greater-district level instead of 64 districts makes little change to our key results. With the 19 greater-district clusters, the number of groups is not sufficiently large to address the Moulton problem. For these results, see Table S9.

[21] Floods are one of the world's deadliest disasters. They account for the loss of almost 53,000 human lives over the past decade (EM-DAT, 2011). Several studies using floods in developing countries investigate their early life malnutrition effects (Del Ninno & Lundberg, 2005; Goudet *et al*., 2011; Alderman *et al*., 2012). Floods are the most common type of annual catastrophe in Bangladesh (Mustafi & Azad, 2003; Rayhan, 2010).

[22] Alderman (1996) finds that the effects of successive shocks are stronger than a single shock on households' consumption smoothing.

TABLE 4
Double Catastrophe in Early Life: Long-Term Effects of Concurrent Famine and Floods (1972–75 Birth Cohort)

	Literacy			Primary school dropout		
Outcome →	(1)	(2)	(3)	(4)	(5)	(6)
Treatment dummy × % decline in the rice-exchange rate of labour	−0.0575*** (6.02)	−0.0541*** (4.68)	−0.0459*** (4.09)	0.0664*** (5.88)	0.0587*** (4.33)	0.0567*** (4.32)
Treatment dummy × Flood-affected areas/total district area	0.00170 (0.22)			−0.00645 (0.90)		
Treatment dummy × Maximum depth of flood inundation (ft)		−0.0204 (0.45)			0.0396 (0.84)	
Treatment dummy × Period of flood inundation (months)			−0.321* (1.89)			0.213 (1.36)
Number of observations	2,475,054	2,475,054	2,475,054	2,475,054	2,475,054	2,475,054
R^2	0.078	0.078	0.078	0.075	0.075	0.075

Notes: Only the coefficients on interaction variables are presented. The percentage decline in the rice-exchange rate of labour is in absolute values. All specifications control for rural, male, birth year, birth year squared, religious, treatment dummy interacted with the age-correction dummy, district fixed effects and cohort fixed effects. The treatment dummy is equal to 1 if a person was born in 1972–75 and is 0 if born in 1976–79 or 1968–71. Robust standard errors are clustered at the district level. *t*-statistics (in absolute values) are presented in parentheses. *$P < 0.10$, **$P < 0.05$, ***$P < 0.010$.

inundation having a *t*-statistic 1.36, with the expected positive sign.

(ii) Single versus Double Catastrophe: 'Only Famine' versus 'Flood and Famine' Districts

Given that the flood and the famine in Bangladesh occurred at the same time and that famine affected all of the districts in Bangladesh but flood did not, it is plausible to expect that the long-term effects of early life malnutrition on the 1972–75 cohort differs between the sample from both the flood- and famine-affected districts and the sample from districts affected by the famine only. To investigate that, we estimate separate regressions for each educational outcome by splitting our sample into two groups. The first subsample includes the individuals in 'flood- and famine-affected' districts, and the second subsample includes the individuals in the 'only famine-affected' districts. We create these subsamples separately with respect to each flood measure.

Table 5 presents the results. In the estimation we *a priori* expect the effects to be stronger for the sample of 'flood- and famine-affected' individuals than the 'only famine-affected' individuals, as the former group was shocked by the double catastrophe. However, our findings in columns (1) and (2) in panel A indicate that, at the mean percentage decline in the rice-exchange rate of labour, the chance of being literate is reduced by around 2.17 percentage points in the sample of 'flood- and famine-affected' individuals, while it is reduced by a much larger rate, 6.6 percentage points, for 'only famine-affected' group. The associated coefficients for both groups (−0.052 and − 0.159, respectively) are statistically significant at the 1 per cent level. Moreover, the coefficients are statistically different across the two samples as shown by the *F*-tests in the table. Turning to the primary school dropout rate in columns (1) and (2) in panel B, at the mean famine severity, the probability of being a dropout in almost 2.5 percentage points higher for the sample of 'flood- and famine-affected' individuals while it is 5.97 percentage points higher for the 'only famine-affected' individuals, compared with the control group. The results in columns 3 and 4 and columns 5 and 6 using the other two flood severity measures are similar.

Overall, the results in Table 5 are contrary to our initial expectation mentioned above. This is not only because the effects are larger in the sample of 'only famine-affected' individuals

TABLE 5
Double Catastrophe in Early Life: Split-Sample Approach

	Maximum depth of flood inundation (ft)		Period of flood inundation (months)		Proportion of flood-affected areas	
Flood severity measure→	(1) Experienced both Flood and famine	(2) Experienced Only famine	(3) Experienced both Flood and famine	(4) Experienced Only famine	(5) Experienced both Flood and famine	(6) Experienced Only famine
Panel A: Outcome → literacy						
Treatment dummy × % decline in rice-exchange rate of labour	−0.052*** (5.392)	−0.159*** (4.212) [100.09***]	−0.052*** (5.368)	−0.125*** (3.261) [2172.18***]	−0.054*** (5.810)	−0.124* (2.308) [1324.38***]
Number of observations	2,137,141	337,913	2,259,129	215,925	2,333,820	141,234
R^2	0.079	0.079	0.079	0.065	0.080	0.063
Panel B: Outcome → primary school dropout						
Treatment dummy × % decline in rice-exchange rate of labour	0.060*** (4.898)	0.143*** (3.574) [51.11***]	0.060*** (4.918)	0.144*** (3.319) [1623.93***]	0.062*** (5.184)	0.154** (2.602) [937.07***]
Number of observations	2,137,141	337,913	2,259,129	215,925	2,333,820	141,234
R^2	0.075	0.077	0.075	0.068	0.076	0.066

Notes: Only the coefficients on interaction variables are presented. All specifications control for rural, male, birth year, birth year squared, religious, treatment dummy interacted with the age-correction dummy, district fixed effects and cohort fixed effects. The treatment dummy is equal to 1 if a person was born in 1972–75 and 0 if born in 1976–79 or 1968–71. Robust standard errors are clustered at the district level. *t*-statistics (in absolute values) are presented in parentheses. *F*-statistics testing the equality of the coefficients are presented in square brackets. *P < 0.10, **P < 0.05, ***P < 0.010.

compared with 'flood- and famine-affected' individuals, but also because, in many cases, the effects are at least twice as strong.

(iii) Are distributional mechanisms at work? Gruel kitchens

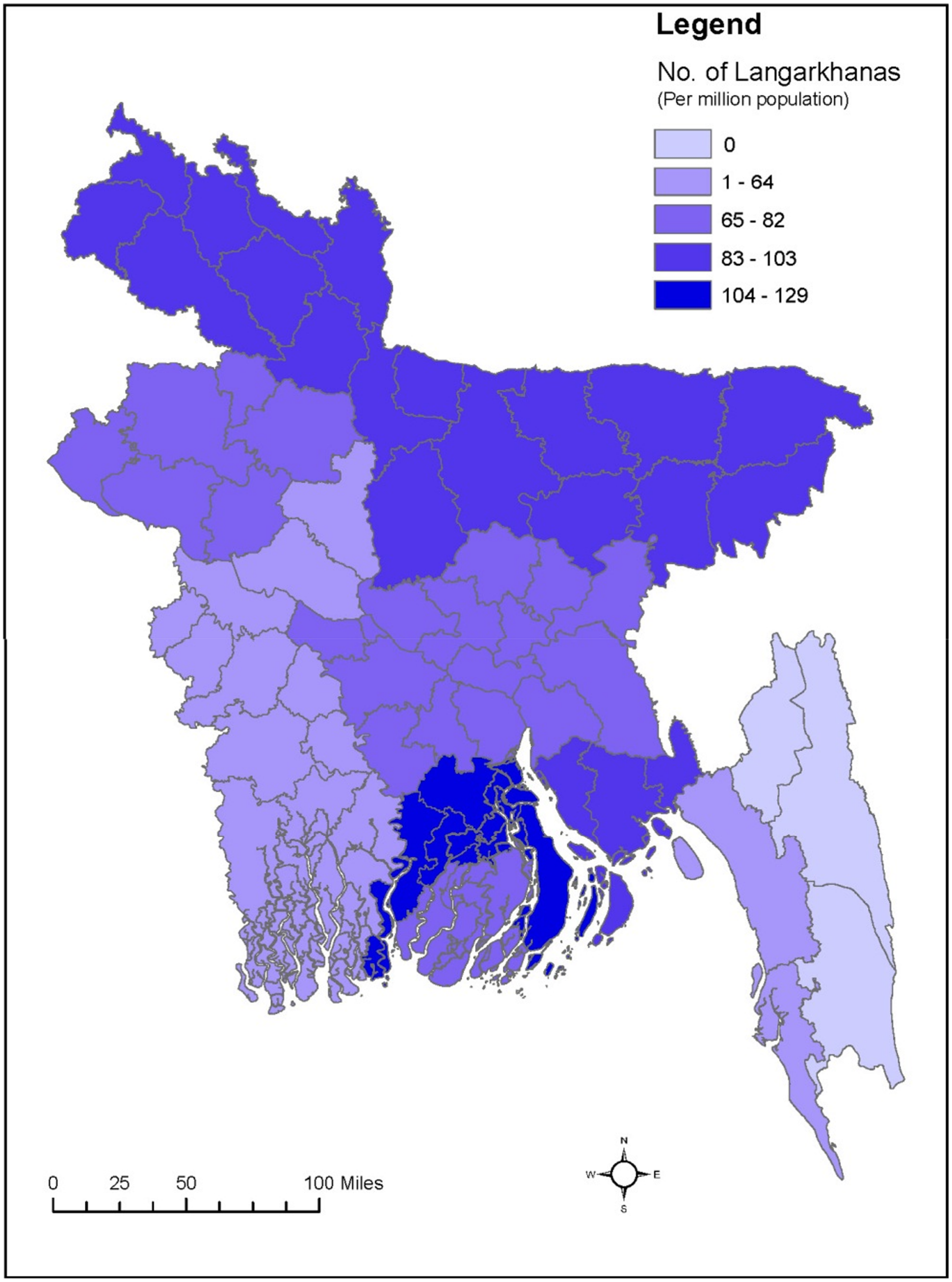
One plausible and measurable explanation for the above surprising finding is related to disaster-alleviation mechanisms.[23] It is likely that interventions to counter the effects of famine, such as *langarkhanas* (gruel kitchens), could differ in 'flood- and famine-affected' *versus* 'only famine-affected' districts. To test this conjecture, we check the data on the number of gruel kitchens (per million people) across districts in 1974 and found that there were fewer, and sometimes none, in 'only famine-affected' districts (Fig. 7).

To formally test if the number of gruel kitchens (per million people) differed in 'flood- and famine-affected' and 'only famine-affected' districts, we estimate some regressions.[24] We present the results in Table 6. Column 1 presents the coefficients of the famine severity measure; columns 2–4 present the coefficients of three

[23] Another plausible explanation might be that a major part of the 'only famine-affected' sample is from the hilly and rugged districts, for example, Rangamati, Khagrachari and Bandarban. Anecdotal evidence suggests that these geographically disadvantaged districts are also poor (Hossain, 2013).

[24] We source the data on the number of gruel kitchens per million people across the districts from Alamgir and Salimullah (1977). Importantly, these data were also obtained at the greater district level, and we employ the same conversion approach as we did to obtain three flood severity measures for 64 districts.

FIGURE 7
Number of Langarkhanas (per Million People) in Districts, 1974. [Color figure can be viewed at wileyonlinelibrary.com]



Source: Data obtained from Alamgir and Salimullah (1977).

TABLE 6
Disaster Severity and Disaster Relief

Independent variable ↓	Outcome: Number of gruel kitchens (Langarkhanas) (per million people)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
% decline in the rice-exchange rate of labour	0.533** (3.35)				−2.179*** (6.97)	−2.330*** (19.47)	−2.686*** (1.07)
Flood dummy 1		46.603*** (6.64)			−53.093*** (4.49)		
Flood dummy 2			49.756*** (6.02)			−54.730*** (7.11)	
Flood dummy 3				54.022*** (5.06)			−75.606*** (12.51)
% decline in the rice-exchange rate of labour × flood dummy 1					2.709*** (8.31)		
% decline in the rice-exchange rate of labour × flood dummy 2						2.883*** (18.14)	
% decline in the rice-exchange rate of labour × flood dummy 3							3.310*** (26.66)
Number of observations	64	64	64	64	64	64	64
R^2	0.107	0.481	0.446	0.389	0.985	0.673	0.637

Notes: Ordinary least squares (OLS) regressions at the district level. Flood dummy 1 is equal to 1 if a district had a strictly positive depth of flood inundation, and 0 otherwise. Flood dummy 2 is equal to 1 if a district had a strictly positive period of flood inundation, and 0 otherwise. Flood dummy 3 is equal to 1 if a district had a strictly positive proportion of flood-affected areas as a percentage of total area of the district, and 0 otherwise. *t*-statistics (in absolute values) are presented in parentheses. $^*P < 0.10$, $^{**}P < 0.05$, $^{***}P < 0.010$.

flood severity dummies: flood dummies 1–3.[25] The coefficients presented are positive and significant.

Crucially, we also estimate a two-way interaction model with the interaction between famine severity and flood dummy, pointing to the existence of two catastrophes in a district. In column 5, the coefficient of the interaction term is positive and statistically significant at 1 per cent, indicating that the 'famine-affected districts' that were also inundated by flood had more gruel kitchens. In particular, in the districts that were affected by double shocks (i.e., both famine and flood), the government seems to have established more gruel kitchens as a disaster effect smoothing mechanism. We also conduct the same exercise using flood dummy 2 and flood dummy 3 (columns 6 and 7) and our results are similar to those in column 5. These results suggest that that the alleviation mechanisms worked better in double catastrophe districts, such that the probability of being literate and not dropping out of primary school was higher in those districts compared with single catastrophe (i.e., 'only famine-affected') districts as of 1991.

VII Conclusions

This paper presents strong evidence on the long-term consequences of early life malnutrition arising from the 1974–75 Bangladesh famine, one of the world's most deadly. Using the 1991 micro-census dataset that contains 10.58 million person-observations, we exploit the variation between individuals exposed to famine-led malnutrition in their early childhood and a counterfactual group with no such malnutrition experience, and then identify whether both groups differ in educational outcomes later in life.

Famines are typically thought to arise from severe food shortages. Sen (1981), in his account of the 1974–75 famine in Bangladesh that ultimately won him the Nobel Prize, claimed that the disproportionate access to food in the time of food shortage, which was induced by weak institutions leading to entitlement issues, was a more crucial factor in famine than the concomitant decrease in food supply. With this compelling explanation in mind, we use a food affordability measure with spatial variation: the percentage decline in the rice-exchange rate of labour in each district. Our approach departs from most of the studies which use famine-related mortality to measure famine severity and is more tightly linked to the genesis of the famine.

Our key finding is that higher rice prices relative to the wage of a unit amount of labour reduced the probability of literacy and increased the likelihood of dropping out of primary school later in life. Probing further into this finding, our investigation shows that children born to wealthier households during famine in famine-affected districts were not affected by the catastrophe and had similar educational outcomes compared with children with no famine exposure. This advantage could point to greater food access by wealthy families, and lack of food entitlement for the broader population.

In a second unique contribution, the paper goes on to study a novel question of the effects of a double catastrophe: concurrent famine and flood experienced in early life. The spatial variations in the distribution of famine and flood presence enable us a unique natural experimental design to study this question. Our findings suggest that although floods had adverse education outcomes for survivors, famine was a stronger cause of early life malnutrition. We also find that, surprisingly, the effects were stronger in 'only famine-affected' districts as compared with 'both flood- and famine-affected' districts. We show that the early life survivors located in double catastrophe areas were thus advantaged because the government accumulated resources in areas where natural disaster exposure was the greatest.

The policy implications of our findings are related to the optimal distribution of post-disaster relief and recovery resources. Policy-makers tend to treat all disasters the same when designing disaster relief and recovery programs, but recent policy work emphasises the role of disaster type, severity and location in optimal formulation of post-disaster interventions

[25] Flood dummies are generated by using the three flood severity measures. They take 1 if the respective flood severity measure is strictly positive, and 0 otherwise. An alternative approach is to use the continuous measure of flood severity across districts. However, since this section is mainly focused on affirming whether the effects of malnutrition in 1974 on the birth cohorts are engendered from famine, or if they are biased by flood, the use of continuous measures of flood severity is not the preferred approach. Importantly, experimenting with the treatment dummy interacting with continuous measure of the flood severity across districts does not pick up any statistically significant effects, suggesting that it is not the severity but the presence of floods that led to the establishment of gruel kitchens in districts.

(Ulubasoglu, 2021). This study introduces a further novel dimension to disaster vulnerability, single *versus* multiple catastrophe experience, which should be considered in the optimal allocation of disaster funds. In addition, our findings suggest that while different types of disasters, such as famines and floods, can and do have different effects on victims, and in a disaster scenario, children are the most vulnerable even in the long run. Therefore, it may not be optimal to amass all available resources in areas that are most severely affected, and an allocation model that prioritises children in any disaster zone is preferable over more skewed alternatives to prevent more adverse long-term consequences of disasters on the society.

Conflict of interest

The authors declare no conflict of interest.

Supporting Information

Additional Supporting Information may be found in the online version of this article:

Tables S1-S9

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