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Footprints of War and Famine: Intrauterine and Inter-generational Effects of the 1971 Bangladesh Liberation War and the 1974 Bengal Famine

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Abstract

This study utilizes the 1971 Bangladesh liberation war and the subsequent 1974 Bengal famine as a "double hit" design to estimate their impact on the health status of intrauterine birth cohorts. As a novel attempt, the study also investigates whether such "health legacy" is passed onto the next generation. By addressing the selection problem via Heckman two-step estimation and using the rich 1996 Matlab Health and Socioeconomic Survey (MHSS), the study finds that individuals born or conceived during the war, who faced the subsequent famine, had lower health outcomes compared to non-shock exposed cohorts. This provides evidence for the intrauterine growth restriction (IGR) hypothesis. The study also finds that children whose mothers were conceived or born during the war had significantly lower health outcomes with the effect being more pronounced among daughters than sons. The latter finding suggests the possibility of inter-generational transfer of the IGR effect.

1 Introduction

During the liberation war of 1971 and the years that followed, Bangladesh faced several crises with the potential for long-term effects on its population. This study seeks to explore the effect of Bangladesh's liberation war (March 1971-December 1971) and subsequent famine (June 1974-July 1975) on long-term health outcomes of individuals who were *in-utero* during these periods (the first generation). Adding to the literature, the paper also investigates if there is any negative intergenerational health transfer to the offspring (the second generation) of these individuals. Using a rich dataset on a rural area of Bangladesh (Matlab), this is the first time such intergenerational effects of the liberation war and Bengal famine are being studied.

As part of the second Sustainable Development Goal (SDG), Bangladesh aims to eradicate all forms of malnutrition by 2030. In order to attain such a feat, it is essential for policy makers to comprehend the historical legacies of malnutrition persistent in the country. This will permit more efficient allocation of resources to the disadvantaged cohorts to combat both moderate and severe forms of undernutrition. From a policy perspective, the study is thus motivated to enable better targeting of beneficiaries for public health and social safety net programs by understanding whether families who were in-utero during the liberation war and famine are indeed one such disadvantaged cohort compared to their peers.

Bangladesh was formerly 'East Bengal', a part of India during the period of British rule lasting from 1757 to 1947. The partition of India in 1947 created the two-nations of 'East Pakistan' (renamed in 1956 from East Bengal), and 'West Pakistan'. Due to social and political conflicts between East Pakistan and its western wing, the 1971 liberation war commenced at the instigation of the Pakistan armed forces on March 26 (Schendel 2009). The war of independence was short-lived, lasting nine months, ending with Bangladesh's sovereignty in December of 1971. The greatest contention, however, has been the exact death toll. Inaccurate reporting of casualties poses a significant challenge to replicating war casualty figures. While the established government figure places the death toll at over 3 million, varying sources suggest differently (Mascarenhas 1971). Till the time of conducting this study, no internally accepted consensus among critics and historians has been reached.

With the economy in a fragile state, post-war Bangladesh found itself faced with a severe famine triggered in part by floods that began in June 1974. Rice crops were destroyed causing shortfalls which were not felt until harvest time. The resulting unemployment among farmers from the small harvest and inflation in rice prices, served to severely deteriorate purchasing power of low-income rural households, exacerbating the famine's impact (Razzaque, Alam, Wai & Foster 1990). Sen (1981) denies the flood having a significant role in the famine, instead asserting poor allocative efficiency as the prime determinant of the famine. Although official figures differ on the death toll, the famine alone is said to have claimed almost 1.5 million lives (Kagy 2012).

A body of literature exists highlighting how conflicts arise and how conflicts instigate long-term impairment on maternal fertility and child health. While this paper does not delve into the associated conceptual framework in this study, one may look into Lindstrom & Berhanu (1999) and Berrebi & Ostwald (2015) for the pathways through which conflict affects health. Given that the overall outcomes for children *in-utero* in 1971 would only clearly manifest in adulthood, investigation into the long-run lifetime effects on health is necessary. Furthermore, a research gap exists in examining survivors' health and the health of their offspring in the aftermath of war. As part of a growing literature on intrauterine effects, this study will serve to present new findings in the Bangladesh context. The two specific objectives are as follows: (1) understanding the *in-utero* health effects of individuals conceived during crises compared to non-exposed cohorts, and (2) investigating the intergenerational or "echo" health effects, on the offspring of crises exposed mothers.

This paper is structured as follows: section 02 provides a succinct review of past literature, followed by a discourse in section 03 on the self-selection problem inherent in such studies and how this paper aims to address the issue. Section 04 presents the data and outlines the empirical strategy while section 05 presents and discusses the results. Section 06 finally concludes the paper with policy recommendations.

2 Literature Review

The case for adult height as a marker for long-term health outcome is commonly supported (Deaton 2007), which asserts an association between taller height and better health, cognitive outcomes in middle age, greater earning ability, and higher probability of being employed (Case & Paxson 2010; Case, Fertig, & Paxson 2005; Grimard & Laszlo 2014; Gørgens, Meng, & Vaithianathan 2012). As such, this study also employs height and height-for-age z-scores as dependent variables for the investigating intrauterine and intergenerational health effects. In conducting such analyses, however, it is crucial to control for environmental factors. Grimard & Laszlo (2014) serve as a good example: using Peru's civil conflict, the authors posit that mothers who were shielded economically and had access to sanitary and secure living conditions did not have long-term adverse effects on their child's health, even though the children were exposed to civil conflict in their early "critical period" from birth and preceding one year of age. Intrauterine environment, however, in a non-shielded environment is a significant predictor of heath outcome.

Similarly, Arcand & Wouabe (2009) found that higher conflict intensity had stronger adverse effects on a child's height with decreased total fertility for women in Angola. Though it was found that long-term negative effects of conflict intensity on the anthropometrics of the child were economically less significant, their impact remained statistically significant. Agadjanian & Prata (2002), on the other hand, examined war effects on births and reproductive preferences in Angola. They report a decline in fertility during war contingent on a woman's socioeconomic status: women from wealthier households showed a statistically significant dip in birth probability during war. However, the probability showed little variation for women of lower socioeconomic status. By the same token, Eloundou-Enyegue, Stokes & Cornwell (2000), in a study based in Cameroon, found that while there was a decline in fertility during war for both rural and urban areas, the change in fertility was greater in urban areas compared to rural areas.

The fetal origins hypothesis argues that insufficient *in-utero* nutrition can induce adulthood diseases. This linkage was first hypothesized by David J. Barker and his colleagues in the 1990's, when he linked insufficient *in-utero* nutrition leading to increased probabilities of coronary heart disease and related disorders into adulthood (Barker 1995). Following this, a large and recent body of research has focused on long-term adult health outcomes from exposure to intrauterine shocks (Arcand & Wouabe 2009; Grimard & Laszlo 2014; Lee 2014; Tan, Tan & Zhang 2015). The fetal origins hypothesis argument operates through multiple channels, with intrauterine growth restriction (IGR) being one of the critical pathways (Calkins & Devaskar 2011). Fetal growth relies on oxygen and nutrients which, if insufficient, forces the fetus to adapt by slowing down its growth. This is dubbed as the IGR effect. In support of IGR, Widdowson & McCance (1975) showed that short periods of under-nutrition permanently reduced the cell-growth in particular organs.

Lee (2014) explores this hypothesis using the Korean civil war and reports that outcomes of prenatal exposure to the war differ by gender and birth cohorts, using year and place of birth as measures of intensity. A number of studies have also looked at *in-utero* exposure during historical famines and the relationship to adult mortality, generally finding adult mortality to be unaffected (Kannisto, Christensen & Vaupel 1997; Moore et al. 1997; Roseboom et al. 2001). Hernández-Julián, Mansour & Peters (2014) studied effects of the Bangladesh famine and intrauterine malnutrition on children's sex ratios and infant mortality compared to post-famine. When compared with children not *in-utero* during famine, the children who were, had a 32%

likelihood of dying within a month of birth. Women pregnant during the famine experienced higher stillbirths when fertility before the crisis was controlled for. Furthermore, Razzaque, Alam, Wai & Foster (1990) adds that "famine-born" children faced higher mortality rates for up to two years from birth, while "famine-conceived" children's mortality rates remained high for up to one year from birth.

Extending the IGR hypothesis further, this study postulates that the detrimental consequences of war and famine are passed onto the next generation through what Almond, Edlund, Li & Zhang (2007) in their study term as an "echo effect." They find evidence of the Trivers-Willard (1973) hypothesis in their results where mothers who were prenatally exposed to the Chinese famine were more likely to have daughters. This unusual proposition by Trivers & Willard (1973) was based on the finding that the successful birth of males was more "resource-sensitive" – financially solvent parents favored more sons while those in poorer conditions favored daughters. The present study, however, deviates from this hypothesis and aims to study the impact on the height of children born to the war and famine exposed adults.

Adhering to the approach of Tan, Tan & Zhang (2015), this study focuses particularly on the persistence of intergenerational transfer of shocks. Using a dataset from China, Tan, Tan and Zhang (2015) showed that the intergenerational legacy of the famine is observed only in the cognitive ability of children with famine-affected fathers, while those born to famine-affected mothers showed no significant disadvantage. However, they report no significant detrimental effect on the cognitive ability of first-generation men affected by famine, while significance held for mothers.

Gørgens, Meng & Vaithianathan (2012) hint at selective mortality, suggesting that famines may target those with poorer health, already shorter in height, thereby selecting for taller populations suited to survival. Therefore, the resulting height of subsequent generations should also be taller. An ideal study thus should be able to control for such selective mortality and selective fertility in their analyses. I discuss this in more detail in the next section.

3 Self-Selection Bias of War and Famine

The main concern in studying the impact of war and famine shocks is that individuals conceived during war and famine are not a random sample and are prone to issues of survival bias and selective fertility. The health outcome is only observed conditional on survival, which leads to the well-known selection problem through "incidental truncation," where only those children strong enough to survive are around to provide height measures. This would bias the effect of the war and famine towards zero, whereby the estimated coefficient would be a lower bound (and hence, arguably, not as problematic as it would be if it were biased away from zero). Other sources of selection may result from the fact that fertility and parental death rates experienced large changes in response to the war and the famine (Hernandez-Julian, Mansour & Julian 2014). This means that the health outcomes being observed for the 1971 and 1974 cohorts are (a) not a random sample of births, and (b) differentially selected relative to other cohorts that they are being compared against. The endogeneity stemming from this self-selection would cause the ordinary least squares (OLS) estimator to be biased.

Addressing these forms of selection would require a Heckman selection correction, but that in turn requires a credible instrument(s) that might affect fertility and survival probabilities of conflict, but not health outcomes conditional on birth. Another way of phrasing this in terms of instrument variable estimation is that the instrument(s) must affect health outcomes *only* through its impact on survival probabilities of conflict. This means that, in the absence of such an instrument, the under-estimated coefficients could be due to a combination of shorter mothers being more likely to conceive in adverse conditions (for example, because of being less aware of the consequences; a similar form of selection is documented by Agadjanian & Prata (2002) and Eloundou-Enyegue, Stokes & Cornwell (2000)), combined with standard intergenerational transmission of height, which is heritable. Furthermore, the reported coefficients in the first-generation analyses will then be the lower bound effect of war *and* of famine *on the survivors*.

Given data limitations, I could not identify an appropriate instrument from the dataset that would permit us to conduct the Heckman two-stage procedure, that is, satisfy the exclusion criteria mentioned above. One potential instrument that, however, carries some possibility of correction is rainfall that has been repeatedly used in recent literature, although arguments both for and against its validity exists. Sarsons (2015) evaluates whether the rainfall exclusion criteria holds for religious conflict and income, finding that rain shocks remain equally strong predictors of conflicts even in the presence of irrigation dams. Similarly, Hsiang, Meng & Cane (2011, p.438) report that the "probability of new civil conflicts arising throughout the tropics doubles during El Niño years relative to La Niña years", providing further support for the argument. On the other hand, Kevane & Gray (2008) report rainfall to only weakly corroborate with conflicts in Darfur. A study by Miguel, Satyanath & Sergenti (2004) became a focal point of discourse where they used rainfall as an instrument to estimate an increase in the likelihood of conflict by one-half from a negative growth shock of five per cent. Ciccone (2011) however show that their result was driven by a counterintuitive positive correlation between conflict in t and rainfall in $t-2$ time periods, where the relationship disappears when conducted using conflict in t and rainfall in $t-1$.

With arguments both for and against using rainfall as a credible instrument, the basic idea is that rainfall affects conflict only through its impact on income. Taking this chain one step further, it is tempting to posit that for predominantly agrarian economies, rainfall affects health outcomes through its impact on conflict. While this may be true, rainfall also impacts income which would affect health outcomes (and hence the chain is not only from conflict to health), rendering such an argument to be weak. Controlling for income or wealth in the estimation procedure will however control this pathway to a certain extent; even in such a case, I consider the instrument to only weakly satisfy the exclusion criteria. There are also further caveats to using rainfall as an instrument for this study.

Firstly, majority of the cases where rainfall is effective as an instrument are mainly in studies concerning civil conflict. Although the Bangladesh liberation war can be considered a civil conflict between then East Pakistan and West Pakistan, the intensity is much higher where political incentives were also strong motivators of the war, along with economic conditions stimuli. Secondly, the dataset does not have a variable measuring the variance in regional intensity of conflict, which would have been a far better variable for conducting the analyses using a difference-in-difference estimator, as done by Lee (2016) when analysing the Korean War. Thirdly, because the data is only on one region of Bangladesh, the Matlab area, this study is only able to use the time variation in rainfall (as opposed to the spatial variance as well). With the aforementioned caveats in mind, even though the Heckman selection results are reported correcting for some of the selectivity, I do not consider it to be a perfect selection correction.

The study also utilizes within-mother variation by incorporating mother fixed-effects in the regression analyses. By clustering siblings, this assumes genetic and environment factors associated with mothers to remain constant over time, wherein each mother serves as her own control. This approach controls for confounding due to unmeasured time-invariant factors and sibling-invariant mother characteristics, such as aspirations for children, thereby reducing bias from unobservable variables (Anekwe, Newell, Tanser, Pillay & Bärnighausen 2015). I further control for a range of potential confounders (detailed in section 4). Finally, while I can control for mothers' height in the intergenerational analyses, parental height data is missing for the first-generation analyses and hence could not be controlled for. Therefore, a pathway for heritable transfer of health remains open for the initial analysis.

While the aforementioned tools, particularly the Heckman selection, may control for selection bias, it is important to keep in mind that the empirical analyses are conducted on the basis of a "double hit" design. By that I emphasize that the presence of two consecutive shocks does not permit disentanglement of the effects of war and famine. While it would be possible to carry out analysis that isolated the effect of those born or conceived during the *famine* only, such is not the objective of this study and has been explored earlier by Hernandez-Julian, Mansour & Julian (2014). The interpretation of the effects of being born or conceived during the war is harder, since all these children were also affected by the famine at some point of their childhood, specifically during ages three to four, where the absence of long-lasting effects cannot be really ruled out. Additionally, the two shocks could have multiplicative, rather than additive, effects. Thus, the coefficients derived in this study are not purely war effects, but rather a combined war-famine effect on long term health.

4 Data

Data for the present study was taken from the 1996 MHSS, conducted in the rural region of Matlab Bazar *thana*, a sub-district located southeast of the capital, Dhaka, consisting of 4,364 households and 2,687 *baris* (Rahman et al. 1999). Two different analysis levels are used for this study. Firstly, an individual-level dataset is used for the first-generation analysis. The window for birth cohorts is between June 1950-April 1976 and the final number of observations stands at 6,368. Secondly, a child-level dataset was constructed for intergenerational analysis of the second-generation with 2,494 observations. The oldest birth year for mothers is 1951, whereas the range for the births of children is between 1981 and 1996.

To test for the fetal origins hypothesis in the first-generation model, I consider height as the dependent variable, following Deaton (2007). Although height-for-age z-scores would have been ideal, the conversion requires the "standard" reference curves which are unavailable for adults. For the intergenerational analysis however, I am able to use the child's height-for-age z-scores as the outcome variable, following Vidmar, Carlin, Hesketh, & Cole (2004).

The fetal origins hypothesis posits that negative shocks of events that persist well into adulthood are magnified when the individual in question is *in-utero* rather than postnatal. As such, those "conceived during the war" (born December 1971-July 1972) and "conceived during famine" (born March 1975-April 1976) should be significantly stunted compared to a cohort that did not face a shock event. Conception during these periods exposes the individuals to the deleterious effects of pre-natal maternal stress. Case & Paxson (2010) argued that early-life health is central to determining health in adulthood. Keeping to their proposition, I also included birth cohorts for individuals "born during war" (March 1971-December 1971) and "born during famine" (June 1974-March 1975). The other birth cohorts range in five-year intervals relative to the famine commencing June 1974, with the exception of the July 1972-June 1974 cohort (aged 0-2 years during famine). Individuals aged 21-25 during famine (born June 1950-June 1955) are kept as the reference cohort for the analysis. Fig. 1 visualizes the timeline and the corresponding birth cohorts constructed to conduct the analyses.

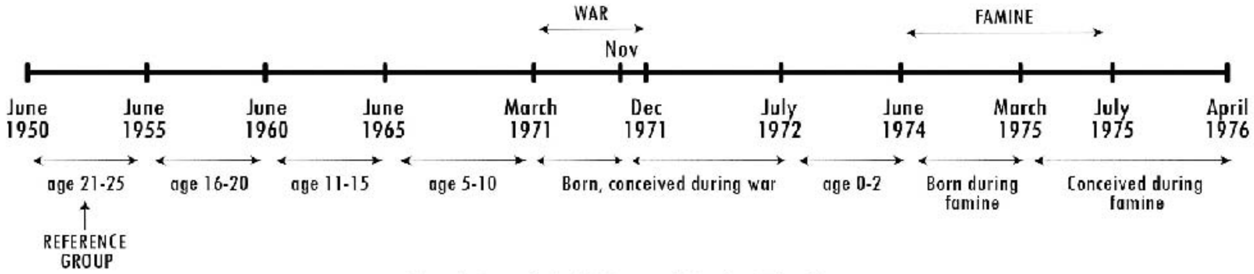

Fig. 1: Bangladesh War and Famine Timeline

Table 1: Descriptive Statistics

Panel A, Selective Statistics for First Generation by Sex

Variable	Male		Female	
	Mean	Std. Dev.	Mean	Std. Dev.
Height (in cm)	161.43	(6.93)	149.91	(6.09)
Weight (in kg)	50	(6.51)	43	(6.69)
Monsoon Season (binary)	0.374	(0.009)	0.380	(0.008)
Dry Season (binary)	0.444	(0.010)	0.458	(0.008)
Hot Season (binary)	0.182	(0.386)	0.162	(0.369)
Primary Education (binary)	0.267	(0.009)	0.312	(0.008)
Secondary Education (binary)	0.265	(0.009)	0.165	(0.006)
Tertiary Education (binary)	0.093	(0.006)	0.019	(0.002)

Panel B, Selective Statistics for First Generation by *uterine* environment

Outcome	*In-utero* during War/ Famine	Std. Dev.	*In-utero* during other years	Std. Dev.	Difference	Std. Error
Female (binary)	0.548	(0.50)	0.587	(0.50)	0.040†	(0.030)
No Education (binary)	0	(0)	0.005	(0.0009)	0.005	(0.0044)
Primary Education (binary)	0.263	(0.026)	0.295	(0.006)	0.032	(0.028)
Secondary Education (binary)	0.395	(0.029)	0.198	(0.005)	-0.197**	(0.025)
Tertiary Education (binary)	0.078	(0.016)	0.048	(0.002)	-0.030*	(0.013)
NGO Participation (binary)	0.168	(0.022)	0.200	(0.005)	0.032†	(0.024)
Log of Individual Assets	11.25	(0.124)	10.86	(0.026)	-0.390**	(0.126)
Employed in Agriculture (binary)	0.445	(0.030)	0.579	(0.006)	0.134**	(0.030)
Tin Roof and Walls (binary)	0.530	(0.030)	0.443	(0.006)	-0.087**	(0.030)
Total Inheritance (in BDT)	58741.34	(8331.64)	50215.66	(1826.90)	-8525.68	(8685.38)
Value of Land Owned (in BDT)	63711.03	(7708.32)	47941.80	(1439.76)	-15769.23*	(6902.31)
Value of Gold/Silver Owned (in BDT)	3970.43	(402.26)	3492.13	(131.79)	-478.30	(619.48)

Table 1: Descriptive Statistics (Contd ...)

Panel B, Selective Statistics for Second Generation by *uterine* environment

	Children born to Mothers *in-utero* during War/ Famine	Std. Dev.	Children born to Mothers *in-utero* during other years	Std. Dev.	Difference	Std. Error
Daughter (binary)	0.545	(0.062)	0.487	(0.010)	-0.058	(0.062)
Log of Individual Assets	10.947	(0.180)	10.417	(0.042)	-0.529*	(0.263)
Tin Roof and Walls (binary)	0.530	(0.062)	0.354	(0.010)	-0.177	(0.060)
Father's Highest Class Completed	6.00	(0.543)	5.87	(0.093)	-0.130	(0.537)
Mother's Highest Class Completed	5.88	(0.391)	4.83	(0.070)	-1.05**	(0.370)
Child Had Major Disease Past Year (binary)	0.076	(0.033)	0.138	(0.007)	0.062†	(0.043)
Child Received Outpatient Care (binary)	0.652	(0.059)	0.387	(0.010)	-0.264**	(0.061)
Delivery at Home (binary)	0.937	(0.031)	0.949	(0.004)	0.129	(0.028)
Parent's Age Difference	8.82	(0.577)	6.23	(0.075)	-2.59**	(0.467)
Height of Mother (in cm)	149.84	(0.705)	150.08	(0.123)	0.247	(0.781)
Height of Father (in cm)	160.368	(0.794)	161.271	(0.139)	0.903	(0.850)

** $p<0.01$, * $p<0.05$, † $p<0.1$

"In-utero during war/famine" refers to individuals born between December 1971-July 1972 in war or April 1975-April 1976 in famine (281 observations). "In-utero during other years" includes all individuals born within 1950-1976, excluding crises-conceived periods (6087 observations). Similarly for second-generation individuals, the same age cohorts are forwarded to the next generation only for females, so that it refers to sons and daughters born to crises-conceived mothers (66 observations) compared to born to mothers in all other years (2428 observations).

The summary statistics for the outcome variable and key covariates are presented in Table 1. To note, *in-utero* during war/famine cohorts contain only 281 observations. There is strong evidence of both men and women in Matlab being chronically malnourished, also reported in studies by Menken & Phillips (1990) and Pebley, Huffman, Chowdhury & Stupp (1985). On average, men and women in the sample weighed 50 kgs and 43 kgs, with mean height 5'3" and 4'11" respectively. The mean of the binary variables represents percentage and table 01 reports that births peaked in November with 45.8% of females and 44.4% of males being born in the dry season. This is the harvest time for *Aman* crop that supplies half of the annual rice production in Matlab (Curlin, Chen & Hussain 1976). Hence, these individuals are more likely to have better outcomes with their early nutrition requirements being fulfilled by correspondingly well-fed mothers (Hernandez-Julian, Mansour & Julian 2014). Pertaining to this evident seasonality in birth patterns, three seasonal dummies are included to control for potential season trends: monsoon (June-October), winter/dry (November-February) and summer (March-May).

Table 1 also reports mean comparison tests of key demographic and socioeconomic characteristics by segregating birth cohorts into two groups to reflect the dynamics of fetal origins hypothesis in first- and second-generation individuals. There is consistent indication of selection for the pool of individuals who were *in-utero* during the crises. These individuals have significantly higher secondary and tertiary education, assets, greater land ownership, are mostly employed in agriculture, and are more likely to live in houses made of tin roofs and walls. This is indicative of higher socioeconomic standing, suggesting that surviving individuals who were *in-utero* during crises had better socioeconomic protection, and were thereby selected to survive, compared to their counterpart. Similar tabulation for second-generation individuals yield characteristics of selection carried forward by children born to mothers who were *in-utero* during crises. Additionally, these children faced significantly lower chances of contracting major diseases (in the past year) while they still received higher outpatient care, compared to children born to mothers who were *in-utero* during other years.[1]

4.1 Empirical Model: First Generation

As discussed under section 2, in addition to the OLS estimation, which would be biased towards zero due to endogeneity from non-random selection, I also report the Heckman two-step selection estimator. The following cohort-based OLS regression, similar to Kagy (2012) and Grimard & Laszlo (2014), is estimated as the baseline model:

$$
\begin{aligned}
Y_i = \; & \beta_0 + \beta_1 BornWar_i + \beta_2 ConcWar_i + \beta_3 BornFam_i \\
& + \beta_4 ConcFam_i + \sum_{t=July1972}^{June1974} \beta_5 I_i \\
& + \sum_{t=June1965}^{March1971} \beta_6 I_i + \sum_{t=June1960}^{June1965} \beta_7 I_i \\
& + \sum_{t=June1955}^{June1960} \beta_8 I_i + \beta_S Season_i + \beta_X X_i' + \varepsilon_i
\end{aligned}
$$

The years and months in the above equation correspond to figure 01 provided earlier. Y_i is the outcome of interest for individual i, representing height. The variables of interest, war and $famine$, contain dummies for individuals born or conceived during the periods with β_1 to β_4 as their respective coefficients. As aforementioned, the time frame for each of these cohorts are as follows: "conceived during the war" (born December 1971-July 1972), "born during war" (March 1971-December 1971), "conceived during famine" (born March 1975-April 1976), and "born during famine" (June 1974-March 1975). The coefficients β_5 to β_8 captures whether the individual was born in the respective cohort as indicative by the summation intervals. $Season$ contains birth dummies for monsoon and dry season, with hot season as the reference. X is a vector of controls to account for demographic, socioeconomic, and community level variation including education, employment, marital status, non-governmental organization (NGO) participation, individual wealth, religion, household environment, and distance to health center. I further include yearly birth fixed effects and mother fixed effects (as discussed in section 3) to control for other heterogeneous factors arising from an individual's birth year and from differential parental behavior of mothers.

The problem with the OLS estimator is the omitted variable determining how people were selected into the sample. This is the well-known problem of "incidental truncation," where the outcome is being observed for only those who were selected. The Heckman two-step selection attempts to correct this by predicting the likelihood of conflict survival in the first stage using a probit model with an instrument satisfying the exclusion restriction. The inverse Mills ratio is then calculated for each observation which is used for the second stage estimation. As discussed under section 2, within data limitations and aforementioned caveats, I use rainfall as the instrument in the predicting the first stage. Should some selection be corrected for, the coefficients should at least be larger than the OLS coefficients which were biased towards zero. The basic form of the Heckman equations is as follows:

Selection Equation: $Y_2 = \propto Z + \delta$

Equation of Interest: $Y_1 = \beta_0 + \beta_1 X + \sigma \rho_{\varepsilon\delta} \lambda (\mathrm{T} - \propto -) + \sigma' \varepsilon'$

The selection equation is estimated using probit where Y_2 is a binary representing conflict survival with Z being the independent (instrument) variable and δ is the error term. This probit estimation provides the predicted values retained as $\mathrm{T} - \propto -$ from which the inverse Mills ratio is estimated. The inverse Mills ratio is then included as a regressor in the equation of interest, represented by $\rho_{\varepsilon\delta} \lambda (\mathrm{T} - \propto Z)$ with σ being its coefficient. In the equation of interest Y_1 is height, X is a vector of covariates (including the cohort dummies) presented in the previous OLS equation and ε is the error term. This second stage is an OLS estimation which is consistent when the assumptions of normality of (δ hi ε) are met, where $\rho_{\varepsilon\delta}$ is the correlation coefficient of the two errors, and (δ , ε) is independent of X and Z.

4.2 Empirical Model: Intergenerational Outcomes

The intergenerational health transmission of the mother to the second-generation child due to IGR is estimated using the following OLS regression:

$$
\begin{aligned}
C_i = \; & \beta_0 + \beta_1 MothWar_i + \beta_2 MothFam_i \\
& + \sum_{t=July1972}^{June1974} \beta_3 M_i + \sum_{t=June1965}^{March1971} \beta_4 M_i \\
& + \sum_{t=June1960}^{June1965} \beta_5 M_i + \sum_{t=June1955}^{June1960} \beta_6 M_i \\
& + \beta_S Season_i + \beta_Z Z_i + \varepsilon_i \qquad (3)
\end{aligned}
$$

where C_i indicates the height-for-age z-scores of child i. The birth cohorts constructed for the first-generation analysis (under section 4.1) are simply forwarded to the next generation – now dealing with sons and daughters born to 'war surviving' and 'famine surviving' mothers. I pursue the analysis only for mothers primarily due to the small sample size of men conceived during war and famine who only became fathers by 1996 (collection year of sample).

The vector *Z* contains the following controls: height of parents, mother's education, age (and age squared) of the child, parents' age difference, child's number of siblings, individual wealth, religion, self-reported health status of parents, child's hospitalization history, whether the child suffered any major disease in the past year, child's delivery location, time taken to reach the *thana* and International Centre for Diarrheal Disease Research, Bangladesh (ICDDR,B) hospital from residence, and household environment including presence of waste around house as observed by the interviewer.

5 Results and Analysis

5.1 First Generation Findings

Table 2 presents the results for the first-generation health outcomes. The OLS results are presented in column (1) followed by the Heckman two-step selection in column (2) with individuals aged 21-25 years old during the famine (1974) as the reference group. I also report placebo OLS and Heckman regression results for robustness check in columns (3) and (4) with individuals aged 21-25 years old during 1964 as the reference group.

Table 2: Impact of War and Famine on First Generation's Health Outcomes, Dependent Variable: Height (in cm)

	OLS	Heckman	OLS Placebo 1964	Heckman Placebo 1964	Heckman 1962-1972 War Cohorts	Heckman 1974-1980 Famine Cohorts
	(1)	(2)	(3)	(4)	(5)	(6)
Born during Famine	-0.162	-0.137	-	-	-	-0.0541
	(0.165)	(0.297)	-	-	-	(0.156)
Conceived during Famine	-0.131	-0.147*	-	-	-	-0.259*
	(0.131)	(0.079)	-	-	-	(0.151)
Age 0-2 during Famine	-0.213	-0.230*	-0.748	-0.649*	-	-
	(0.170)	(0.105)	(0.924)	(0.389)	-	-
Born during War (Age 03 during Famine)	-0.378**	-0.406**	-	-	-0.214**	-
	(0.122)	(0.115)	-	-	(0.070)	-
Conceived during War (Age 04 during Famine)	-0.535**	-0.598**	-	-	-0.315*	-
	(0.180)	(0.113)	-	-	(0.152)	-
Age 5-10 during Famine	-0.619	-0.836	-0.703	-0.717	-	-
	(0.641)	(0.897)	(0.937)	(0.637)	-	-
Age 11-15 during Famine	-0.449	-0.555	-0.516	-0.625	-	-
	(0.368)	(0.754)	(0.911)	(0.549)	-	-
Age 16-20 during Famine	-0.252	-0.314	-	-	-	-
	(0.828)	(0.569)	-	-	-	-
Born in 1964	-	-	-0.140	-0.278	-	-
	-	-	(0.0927)	(0.227)	-	-
Conceived in 1964	-	-	-0.0809	-0.104	-	-
	-	-	(0.0898)	(0.096)	-	-
Observations	2,658	2,658	3,497	3,497	1,020	443
R-squared	0.957		0.844		0.823	0.766
LR test of rho=0 (p-value)		0.0003		0.027		

** $p<0.01$, * $p<0.05$, † $p<0.1$

Robust standard errors are in parentheses. All regressions use sample weights, are clustered at the bari level, include yearly birth effects and mother fixed effects as well as controls for education, employment, marital status, NGO participation, individual wealth, religion, household environment, distance to health center and seasonal fixed effects. Individuals aged 21-25 years old during famine is the reference group for columns (1) and (2). Placebo tests in columns (3) and (4) were conducted with individuals aged 21-25 years old during 1964 as reference group. Columns (5) and (6) report 10-year interval robustness checks.

The sign of the coefficients for born/conceived during both the war and famine are negative, albeit only statistically significant for those born/conceived during the war in both the OLS and Heckman coefficients. Expectedly, OLS coefficients are biased towards zero compared to the Heckman coefficients. It is also worthwhile to note that the likelihood ratio test of no selectivity is rejected at 1 per cent, indicating presence of selection bias in the OLS results. As such I elaborate on the Heckman coefficients in the ensuing discussion.

Children born during the war have a lower height, on average, by 0.406 standard deviations while those who were conceived during the war have a lower height, on average, by 0.598 standard deviations compared to those aged 21-25 years old during the famine. It is worthwhile to reiterate that this is the combined war and famine effect as opposed to a purely war effect (as discussed under section 2). This supports the fetal origins hypothesis, although it should be noted that those aged 0-2 during famine also yielded statistically significant coefficients. This is most likely because those aged 0-2 during famine were born right after the war and hence faced the war aftermath. This also provides an argument on the possibility of shock events affecting not only *in-utero* but also in the early formative years as postulated by the 1001 critical days hypothesis (Leadsom, Field, Burstow & Lucas 2013).

Children conceived during famine are seen to be statistically significant for the Heckman correction where we see that the famine conceived children have a lower height, on average, by 0.147 standard deviations compared to those aged 21-25 during famine. Expectedly, this effect is smaller than the coefficient on born/conceived during war, as those conceived during famine should not carry any shock from the war. Thus, while the coefficients on those born/conceived during war reflect the combined war and famine effect (the double hit design), the coefficients on those born/conceived during famine reflect only the *in-uterine* shock from famine (a single hit). It, however, remains unexplained as to why the coefficient on those born during famine remains insignificant. The simplest explanation is that, perhaps, being conceived during famine bestows a stronger effect than being born during famine. This lends credence to the possibility for *in-uterine* effects being stronger than post *in-uterine* effects, at least in this scenario.

In order to ensure the results are not spurious, I report a placebo test using a restricted cohort from 1939-1965, with individuals aged 21-25 in 1964 as the reference group in columns (3) and (4). This selected range provides a 10-year window from the famine, unaffected by conflict, famine, or any other significant catastrophic event. Thus, if the results in table 02 are not driven by spurious factors, the primary variables of interest should be insignificant. Expectedly, neither those born nor conceived in 1964 are significant in the placebo regressions, contrasting results from the main model. Unlike the main model where I exploit the twin crises, the low coefficient magnitude and insignificance from placebo results further stresses that the war and famine are indeed key drivers in the primary results. Note that the coefficient "Aged 0-2 during Famine" is negative and significant in column 4 (as in column 2), which, as previously discussed, is simply capturing the effect of the famine during the early formative years as postulated by the 1001 critical days hypothesis (Leadsom et al. 2013) compared to individuals aged 21-25 in 1964.[2]

It should be noted that in all the analyses I employ a "cohort-based" approach, where by keeping one age cohort as the reference, I obtain coefficients for the other age cohorts. As a robustness check, I also present the method employed by Hernández-Julián et al. (2014), of keeping a 10-year window for treatment and control groups for war and famine respectively. Therefore, for war the treatment dummies are those "born during war" and "conceived during war" and the year of birth is limited within 1962-72, in order to eliminate the effects of the consecutive famine in 1974. This allows me to infer whether those who were *in-utero* during the war had lower health outcomes in reference to all other births during 1962-72. Similar analysis was done for famine, keeping the reference window within 1974-1980. I find that even though the magnitude changes, the results are still consistent. I report only the Heckman selection estimates and the results are provided under columns (5) and (6) in table 02.

The final robustness check performed is by alternating reference cohorts with the same set of controls. I run a series of Heckman estimations by repeatedly alternating each age cohort as the reference point. Table 03 presents the results indicating consistency to a large extent in the findings. The general trends derived in the main model regression reappear and the signs of the coefficients remain unchanged. Additionally, I review the consistency of the results by excluding all January birth individuals from the regressions. This is because on the unavailability of specific month of birth information, the fieldworkers often coded the birth as occurring in January (Rahman et al. 1999), causing the data to include an inaccurately large number of January births. As reported in column (5) of table 03, even after excluding all January births, the results are fairly robust with the findings in the main model, albeit with a larger coefficient.

Table 3: Robustness Check, Dependent Variable: Height (in cm)

	Ref: Age 16-20	Ref: Age 11-15	Ref: Age 5-10	Ref: Age 0-2	Excluding January Births
	(1)	(2)	(3)	(4)	(5)
Born during War	-0.546*	-0.417†	-0.285	-0.573**	-0.651**
	(0.220)	(0.223)	(0.229)	(0.209)	(0.251)
Conceived during War	-0.452*	-0.372†	-0.290*	-0.382**	-0.424*
	(0.183)	(0.192)	(0.105)	(0.137)	(0.196)
Born during Famine	-0.126	-0.0902	-0.0525	0.000577	-0.224
	(0.167)	(0.173)	(0.180)	(0.170)	(0.198)
Conceived during Famine	-0.119**	-0.110**	-0.0998	-0.0780	-0.0617
	(0.032)	(0.034)	(0.136)	(0.132)	(0.151)
Observations	2,658	2,658	2,658	2,658	2,185
R-squared	0.957	0.957	0.956	0.957	0.959

** p<0.01, * p<0.05, † p<0.1

Robust standard errors are in parentheses. All regressions use sample weights, are clustered at the bari level, include yearly birth effects and mother fixed effects as well as controls specified under table 02. Age cohort dummies are included in all regressions, with the respective reference cohort mentioned in each column.

Table 4: Impact of War and Famine on Second Generation's Health Outcomes, Dependent Variable: Height-for-Age (z-scores)

	Intergenerational Model		
	All Mothers	Sons	Daughters
	(1)	(2)	(3)
Mother born in War	0.601†	0.718*	-0.167
	(0.311)	(0.331)	(0.232)
Mother conceived in War	0.232	0.0428	0.189
	(0.156)	(0.166)	(0.219)
Mother born in Famine	0.673*	1.207†	-0.141
	(0.309)	(0.716)	(0.192)
Mother conceived in Famine	-0.462**	-0.288**	-0.842**
	(0.075)	(0.085)	(0.040)
Mother aged 0-2 in Famine	0.0314	0.184	-0.332
	(0.189)	(0.229)	(0.304)
Mother aged 5-10 in Famine	-0.0182	0.0340	-0.161
	(0.0731)	(0.105)	(0.141)
Mother aged 11-15 in Famine	-0.000664	-0.00220	-0.0718
	(0.0703)	(0.0948)	(0.125)
Mother aged 16-20 in Famine	0.00933	0.0116	-0.0754
	(0.0648)	(0.0793)	(0.131)
Observations	677	346	331
R-squared	0.909	0.917	0.916

** p<0.01, * p<0.05, † p<0.1

Robust standard errors are in parentheses. All regressions use sample weights, are clustered at the bari level, include yearly birth effects and mother fixed effects as well as controls for height of father and mother, mother's education, age and age squared of child, parents' age difference, number of siblings of child, individual wealth, religion, self-reported health of father and mother, if child was ever hospitalized, if the child had any major disease in the past one year, child's delivery location, time taken to reach the thana and icddr,b hospital from residence and household environment including presence of waste around house and if the yard is kept "clean" as observed by the interviewer. Mothers aged 21-25 years old during famine is the reference group for all regressions.

5.2 Second Generation Findings

The discussion now shifts to the long-term effects of prenatal and postnatal exposure to crises on the second generation. Columns (1) to (3) in table 04 present the estimation results for height-for-age z-scores of the children for 'scarred' and 'un-scarred' mothers of war and famine. Since there is no selection of the second-generation children (whose height-for-age z-scores is used as the dependent variable) the OLS estimates are reported.

While the results do provide evidence for the fetal origins hypothesis transferred to the second-generation by famine-conceived mothers, the estimation does not yield significant coefficients for the offspring of war-conceived mothers. This finding is interesting because Tan, Tan & Zhang (2015) do not find significant intergenerational effect of famine-conceived mothers while studying the Great Famine in China, but instead do find so in the case of fathers. Children born to mothers conceived during famine are likely to be 0.462 standard deviations lower in height-for-age compared to the reference group cohort with the effect being more pronounced on daughters (column 3) than on sons (column 2).

Is this finding an indication towards the son preference culture? In order to test this, it would be required to run a separate set of regressions on households that has only sons or only daughters (but not both) so that gender-biased parental investment can be controlled for. Unfortunately, that is not possible for two reasons: (1) sons and daughters would be differentially selected which would have to be accounted for but is not possible due to data limitations, and (2) there are 57 only son households – such a small sample size would not provide any reliable estimates.

Furthermore, while the intergenerational fetal origins effect seem to hold for famine-conceived mothers, we don't find similar results for war-conceived mothers. A possible explanation for such a result could be that while famine directly affects the nutrition received by the fetus to have long-term negative health outcomes, there are several pathways through which war can affect the fetus. Grimard & Laszlo (2014, p.140) identified the following pathways: "(i) shocks to nutrition resulting from the death of income earners and the loss or theft of assets, (ii) shocks to health because of unsanitary environments during displacement, and (iii) prenatal psychosocial stress shocks". In contrast to extreme nutrition deficiency faced during famine, war effects can be short-term and reversible via positive life cycle effects, thereby potentially causing the effect to remain unobserved in the offspring's long-term health outcomes.

However, inferences for the intergenerational effects need to be made with caution. One issue that arises when comparing offspring of "scarred" and "unscarred" mothers is "assortative matching" in the marriage market. This leads to "scarred" survivors marrying least fit males (Tan, Tan & Zhang 2015). The average age difference of parents in the MHSS dataset is 6.30 years, indicating that famine-conceived females are unlikely to end up with the same aged male cohorts. I am, however, unable to compare the outcome of survivors who married scarred vs. non-scarred spouses. While such robustness checks would strengthen the analyses, the findings are still indicative of intergenerational transmission of IGR from the mother to the fetus. I do however recommend further work on richer datasets of other such shocks to validate its consistency.

6 Conclusion

This study adds to the growing literature on the fetal origins hypothesis by inspecting "intergenerational transmission" (IGT) effects from the perspective of a developing nation. First generations exposed to the combined effect of war and famine are found to have significantly lower health outcomes compared to non-exposed cohorts. Intergenerational analysis yielded that pathways of IGT are most pronounced among the offspring of mothers conceived during famine, signifying that an acute lack of nutrition of these mothers *in-utero* passes on such an undesired legacy.

It is worthwhile to reiterate that a number of factors did limit the study. First, the absence of an "ideal" variable for the Heckman selection may have resulted in lingering selectivity in the analyses. Second, without data on the intensity of conflict, it was not possible to carry out a difference-in-difference estimation which would be a better approach than the cohort comparison estimated conducted. Additionally, other factors constituting the "life-cycle" of an individual such as quality of parental after-birth care, family member deaths and financial upheavals were not controlled for. The exclusion of such controls may result in a bias for the *in-utero* cohorts. It is also to be noted that the findings are characteristic only of the Matlab area of Bangladesh and are not necessarily nationally representative.

Altogether, the findings support "intrauterine growth restriction," as proposed by Barker (1995), among the first generation of war- and famine-conceived cohorts, with this "legacy" being transmitted to the second generation for famine-conceived mothers. With such health consequences, it is important for policy makers to target children who were born/conceived during the Bangladesh liberation war (1971) and experienced the Bengal famine of 1974, and also children of mothers who were born/conceived during the 1971 war, to address undernutrition. If Bangladesh truly aims to eradicate all forms of malnutrition by 2030 (SDG2), such focused targeting of public health and safety net beneficiaries is essential, and especially so in this case due to its intergenerational transfer property. If left unaddressed, the poor health outcomes resulting from such shocks for a particular cohort, and their subsequent generations, would reduce the nation's productivity and development potential.

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Endnotes

1. Interestingly, the descriptive statistics do not indicate presence of Trivers-Willard hypothesis in the sample, as the female-to-male sex ratio is significantly lower during war/famine (similar findings were obtained by Stein et al., 2004). However, we cannot conclusively state this since we are only dealing with live births and those who are alive in the 1996 survey. In contrast, Hernández-Julián et al. (2014) finds evidence of Trivers-Willard hypothesis when looking into only the 1974 Bengal famine using live and still births.
2. I also consider potential endogeneity issue resulting from reverse causality between health and wealth (Fakir 2016). Applying instrument variables approach I run 2SLS regression as a robustness check, using total inheritance, value of ornaments, and value of radio as instruments, obtaining consistent results.

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