Essays on the Contextual Determinants of Demographic Processes and Family Dynamics

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A thesis submitted for the degree of
Doctor of Philosophy

Hilary Term 2023

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Abstract

This thesis consists of three self-contained articles studying whether and how different social contexts shape population and family dynamics. In the first article, I retest the Trivers–Willard hypothesis, which argues for a negative correlation between maternal stress and sex ratio at birth (SRB), with 243 years of time series data from Sweden. I find no supportive evidence for the hypothesis because the associations of SRB with most of the covariates used as proxies for maternal stress are not statistically significant, and in many cases the level of maternal stress is indeed positively correlated with SRB. In the second article, I exploit quasi-experimental variations in the duration of exposure to a school stipend project to identify the effect of maternal education on child mortality in Bangladesh. Using birth history data from the Demographic and Health Surveys, I find that an additional year of maternal schooling reduces both under-five and infant mortality by about 20%. I also document a number of mechanisms, including greater wealth and literacy, positive assortative mating, lower fertility, delayed marriage and childbearing, greater health-related knowledge, better health-seeking behaviours, and female empowerment, but not female employment. In the third article, I combine individual-level data from the Multiple Indicator Cluster Survey and province-level data on wartime bombing to assess the long-term impact of the Vietnam War on Vietnamese women's attitudes towards intimate partner violence (IPV). To establish a causal link, I use a province's distance to the arbitrarily drawn border between North Vietnam and South Vietnam as an instrument for bombing intensity in that province. I find that women living in provinces that were heavily bombed during the Vietnam War are more likely to accept IPV, reflecting the normalisation of and desensitisation to violence in the private sphere among those who were exposed to conflict violence.

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In June 2010, I sat the National College Entrance Examination (*Gaokao*). My performance on the tests was terribly poor, and there was no chance that I could get admitted to any decent colleges in China. At that time, I could hardly imagine that one day I would be able to read for a doctoral degree. I do have a lot of people to thank because this accomplishment wouldn't have been possible without their help.

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To mum and dad

献给爸爸妈妈

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Introduction

This thesis is a collection of three empirical research papers on the determinants of demographic processes and family dynamics. Instead of looking into individual and micro-level factors, I study whether and how different macro-level social contexts—economic and environmental shocks, educational expansion, and armed conflict—can shape offspring sex ratio, child mortality, and opinions about family violence, with a focus on uncovering the causality and causal mechanisms underlying these demographic and attitudinal outcomes.

I begin by presenting a macro-demographic analysis of variation in human sex ratio at birth (SRB). In 1973, American scientists Robert Trivers and Dan Willard proposed a well-known hypothesis, arguing from a natural selection perspective that a temporary decline in (male-to-female) SRB will be observed in places where mothers are subject to unfavourable situations that cause prenatal exposure to physical and mental stress. The Trivers–Willard hypothesis (TWH) is one of the most influential and controversial theories of twentieth-century evolutionary biology and has been tested extensively by researchers, the empirical evidence so far is, however, mixed. Furthermore, prior research on this topic faces some common limitations: (1) the duration of the period under investigation is relatively short; (2) the covariates used as proxies for maternal stress are often unidimensional and coarsely constructed; and (3) the underlying mechanisms have rarely been examined.

In Chapter 1, I reconsider the TWH that maternal stress leads to more female births in the context of Sweden, which is ideal for testing the TWH given the country's high level of political and social stability and detailed historical population registry. To do so, I compile a comprehensive data set of annual time series spanning two and a half centuries from multiple sources, which provides rich information on Swedish population dynamics, economy and living standard, and climate. I then model annual change in Swedish SRB and its associations with a range of economic and meteorological indicators reflecting different aspects of maternal stress using autoregressive integrated moving average (ARIMA) for three periods: (1) 1749–1991, the maximum time span for which SRB data are available; (2) 1749–1861, the pre-industrial era of Sweden; and (3) 1862–1991, the study period of a past paper testing the TWH and also using Swedish data. According to the TWH, I expect to observe a decreased SRB when pregnant women were exposed to stressful conditions, such as economic contraction, increased cost of living, and abnormal temperature.

I find that, over any of the three periods under consideration, the vast majority of covariates used as proxies for maternal stress, such as different measures of gross domestic product, consumer price index, crop yield, and temperature anomaly, show no statistically significant associations with SRB. These associations remain non-significant in multivariate models controlling for annual expectancies, total fertility rate, and mean age at childbearing. Apart from the lack of statistical significance, the signs of many regression coefficients are in the opposite direction compared to what is suggested by Trivers and Willard, that is, many ARIMA models predict an increase in SRB as the level of maternal stress increases, providing further evidence against the hypothesis. I continue in this chapter to examine the foetal loss mechanism, which argues that stress-induced prenatal deaths disproportionately affect male foetuses and ultimately cause SRB to decline. Once again, my ARIMA models estimating the correlations between Swedish sex ratio of stillbirths in 1862–1991 and various proxies for maternal stress fail to provide any statistically significant results, suggesting no evidence that maternal stress reduced the SRB by rising male foetal loss.

The next chapter looks at the demographic and health consequences of educational expansion. The social determinants of health perspective suggests that education inequalities are often closely related to health disparities. For instance, the demographic and public health literature has shown that mothers with higher educational attainment are less likely to incur child death. However, it is unclear if the negative association between maternal schooling and child mortality can be given a causal interpretation because some unobserved variables—such as innate ability and family background—may confound both maternal education and child survival and thus cause omitted-variable bias.

In Chapter 2, I exploit a quasi-experiment to identify the causal impact of maternal education on child mortality in the context of Bangladesh, a country that has witnessed a dramatic decline in child mortality during the past half-century. In 1994, the Bangladeshi government launched the Female Secondary School Stipend Project (FSSSP), which made secondary education free and provided extra stipends for girls of secondary-school age residing in rural areas. The staggered implementation of the FSSSP generated variations in the duration of exposure to the project for eligible girls, and I use such plausibly exogenous variations to explore the causal link between Bangladeshi mothers' education and their children's mortality.

I find that an additional year of maternal schooling would reduce both under-five and infant mortality by about 20% in Bangladesh. This effect, which is estimated using two-stage least squares (2SLS), is substantially larger in magnitude than that estimated using ordinary least squares (OLS), indicating that some unobserved factors are likely to influence both maternal education and child mortality and hence bias the OLS estimates upward. I also show that my results are robust to a number of alternative model specifications, including survival models estimated using a control function approach to control for right censoring of child mortality. In addition, I reveal several mechanisms through which maternal education could help improve child survival, including greater wealth and literacy, lower fertility, delayed marriage and childbearing, greater health-related knowledge, better health-seeking behaviours, and empowerment of women. I do not find an effect of education on women's employment. However, I do find that Bangladeshi women with more years of schooling were more likely to marry men with better education and jobs, pointing to positive assortative mating as a causal pathway.

The final chapter investigates the connection between two global issues—violence against women and armed conflict. A growing body of literature has found an elevated risk of experiencing intimate partner violence (IPV) among women who are currently or were previously exposed to armed conflict. However, findings on the correlation between conflict exposure and individual attitudes towards IPV—as measured by whether a person agrees that a husband is justified in beating his wife under some hypothetical circumstances—are inconsistent. What is more, previous studies, which often utilise the number of conflict-related events or deaths in an area as a proxy for conflict exposure for people living in that area, do not fully address the endogeneity issue. Endogeneity can arise from selection; for example, areas where aggressive and combative individuals live are more likely to be targeted in armed conflict and are thus more prone to conflict-related casualties, meanwhile, these individuals are also more likely to normalise and be desensitised to the use of violence in future intimate relationships. Another source of endogeneity is measurement error—as most conflict intensity measures capture the scale of, rather than the true damage caused by conflict violence, the impact of armed conflict tends to be underestimated.

In Chapter 3, I ask this question in the context of Vietnam, a country with a violent history of warfare and a high prevalence of violence against women. Specifically, I assess the impact of exposure to the Vietnam War early in life on Vietnamese women's attitudes towards IPV more than three and a half decades after the end of the war. During the war, Vietnamese people were exposed to large-scale and indiscriminate aerial bombardment conducted by the United States and its allies, which caused millions of casualties, and I leverage the amount of ordnance dropped in a Vietnamese province to represent the intensity of war exposure for people living in that province. To deal with the endogeneity problem, I adopt an instrumental variable (IV) approach that exploits the distance to the border between North Vietnam and South Vietnam as a source of exogenous variation in the geographical distribution of wartime bombing.

My first-stage result indicates that provinces located closer to the border received heavier wartime bombing than provinces located further away, which is in line with the characteristic of the Vietnam War that most intense fighting took place near the North-South border. I also argue that the border is exogenous because it was arbitrarily drawn at the 17th parallel without taking account of Vietnamese people's opinions and thoughts and did not have any political and economic significance other than being the border between North Vietnam and South Vietnam during the war. After endogeneity is addressed using IV estimation, I find that a 1% increase in the amount of ordnance dropped in a province per square kilometre would rise the possibility that a Vietnamese woman accepted IPV by 12.4%, and this 2SLS estimate is 1.68 times the OLS estimate, implying the OLS result may be biased downward due to measurement error in bombing intensity. When analysing the mechanisms, I do not find evidence that exposure to the Vietnam War made women more tolerant of IPV through education or marital matching. Instead, the war was likely to directly affect women's views about IPV because of the normalisation of and desensitisation to violence in the private sphere.

Chapter 1

Maternal Stress and Sex Ratio at Birth in Sweden over Two and a Half Centuries: A Retest of the Trivers–Willard Hypothesis

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1.1 Introduction

In most human populations, the sex ratio at birth (SRB), also known as the secondary sex ratio, shows both "striking stability across time", hovering around 105 male births per 100 female births (i.e., 51.2% of total births are boys), and "significant variation with a number of variables" (James, 2012, p. 183). Yet, scholars have made several hypotheses on the causes of short-term variations and have suggested dozens of biological, environmental, demographic, socioeconomic, and psychological factors that can affect SRB (Chahnazarian, 1988; James, 1987, 2012; James and Grech, 2017). Generally, these factors produce their effects on SRB either through the alteration of sex ratio at conception or through the modification of sex-selective loss in utero, or both (Chahnazarian, 1988; James, 2012).

A particularly intriguing and influential hypothesis of SRB variations was proposed by Trivers and Willard (1973), suggesting that "natural selection should favor parental ability to adjust the sex ratio of offspring produced according to parental ability to invest" (p. 90), and female mammals including humans are therefore able to adjust offspring sex ratio in response to their maternal condition in order to achieve optimal reproductive payoff. The assumptions made in the Trivers–Willard hypothesis (hereafter the TWH) are that (1) reproductive success of male offspring is more unstable and resource-sensitive than that of female offspring, (2) maternal condition is associated with the offspring's condition, and (3) maternal condition is positively associated with reproductive success. Therefore, parental investment in sons will yield greater reproductive payoff with good maternal conditions but lower reproductive payoff with poor maternal conditions, relative to a comparable investment in daughters. As a result, the TWH proposes that mothers are more likely to give birth to sons when they are in good condition during gestation and to daughters when they are in poor condition. At the macro or population level, the TWH predicts a temporary decline in SRB in places where women are subject to unfavourable situations that can be characterised as poor conditions and lead to prenatal exposure to physical and mental stress. The main mechanism for the TWH is often thought to be the maternal-stress-induced prenatal deaths which disproportionately affect male foetuses (James and Grech, 2017). Accordingly, the TWH also predicts an increase in the percentage of male foetal loss during a stressful period.

The TWH has been tested by biologists, demographers, ecologists, economists, epidemiologists, sociologists, and statisticians using data from different sources and with various research designs and methods (Song, 2015). Considerable literature has related declines in SRB to idiosyncratic shocks, including warfare and armed conflicts (Ansari-Lari and Saadat, 2002; Kemkes, 2006; Valente, 2015; Zorn et al., 2002), political upheavals (Catalano, 2003), severe famines (Song, 2012), terrorist attacks (Catalano et al., 2005b, 2006) and earthquakes (Catalano et al., 2013; Fukuda et al., 1998; Torche and Kleinhaus, 2012), among others. Not all existing research, however, provides supporting evidence for the hypothesis. A number of studies have questioned the previous findings on SRB declines during times of wars (Graffelman and Hoekstra, 2000; Polasek, 2006; Polasek et al., 2005), political upheavals (Schnettler and Klüsener, 2014), and famines (Zhao et al., 2013). These equivocal results indicate that the relationship between SRB changes and idiosyncratic shocks is sensitive to data and the context under study (James, 2009).

Another line of research highlights the effects of a more common and robust type of maternal stress, economic contraction, on SRB. Based on the TWH, observed SRB in a population is presumed to decline when the population undergoes a contracting economy in which households consume fewer goods and services than needed or expected, which may sufficiently stress women and hence lower SRB. In a seminal paper, Catalano and Bruckner (2005) employed a time series model to 129 years of demographic and macroeconomic data from Sweden and reported a positive relationship between percentage change in private consumption and SRB in 1862–1991. In other words, Swedish SRB was higher when the economy expanded and lower when the economy contracted, which is in line with the TWH. Several time series analyses for other countries have been conducted following their work, but the results are rather mixed. Helle et al. (2009) found a non-significant relationship between SRB and percentage change in real gross domestic product (GDP) in Finland in 1865–2003. Another study showed no relationship between consumption and SRB in Poland when using annual data for the period 1956–2005 (Żadzińska et al., 2007), while a later investigation conducted by the same researchers using quarterly data for the period 1995–2007 reported a reduction in SRB in Poland four quarters after the occurrence of economic decline (Żadzińska et al., 2011). An integrative review article suggested that the association between economic stress and SRB presented in former research remained largely speculative and acknowledged the needs for more research on this topic (Margerison Zilko, 2010).

In addition to economic stress, another typical but less examined maternal stress is climatic shock. A handful of studies have discovered a relationship between temperature and SRB (Catalano et al., 2008; Fukuda et al., 2014; Helle et al., 2007, 2009), while others have not (Dixson et al., 2011, 2013a,b). Most of these studies have usually relied on limited measures of temperature recorded in just a few locations as a proxy for climatic shock to the entire population. For example, Catalano et al. (2008) used annual average temperature in the Tornedalen region of Sweden and Stockholm to represent climatic shock to populations of four Nordic countries. Such proxies may be problematic as they cannot reflect the average situation relating to climate and temperature in a geographically sparse area. In this study, we use a series of refined temperature records.

A few studies have also examined the relationship between maternal stress and foetal death sex ratio. Bruckner et al. (2010) reported an increase in the percentage of male foetal deaths in the United States in September 2001 when 9/11 induced widespread feeling of distress among American women. Using monthly data from the state of California, Catalano et al. (2005a) found that the ratio of male to female foetal deaths increased during months in which the unemployment rate also increased. The existing research has only looked over a relatively short period because historical data on prenatal deaths are not recorded in many places of the world.

The objective of this study is to retest the TWH that maternal stress leads to decline in SRB with a comprehensive data set that covers a longer period than previous studies and includes a range of indicators reflecting different aspects of maternal stress. Our data set combines demographic, socioeconomic, and meteorological time series for nearly two and a half centuries for Sweden covering the period from 1749 to 1991. We conduct time series analyses to explore the relationship between Swedish SRB and a set of proxies for maternal stress in the period 1749–1991 as well as in two sub-periods 1749–1861 and 1862–1991. We also examine the relationship between Swedish sex ratio of stillbirths and maternal stress in 1862–1991 as an underlying mechanism for the TWH. To the best of our knowledge, this study provides, hitherto, the most comprehensive macro-level analysis of national SRB trends in terms of the duration of the period and the number of covariates under investigation.

1.2 Materials and Methods

1.2.1 Study Setting

Sweden provides an ideal setting for testing the relationship between economic and climatic stress and SRB for two main reasons. First, the country "has enjoyed a relatively high level of political and social, if not economic, stability" (Catalano and Bruckner, 2005, p. 538), for which we can focus on the impacts of economic and climatic stress and minimise the consequences of idiosyncratic shocks. Moreover, Swedish population registers provide sex-specific demographic data for a very long period that can date back to the mid-18th century, allowing us to examine SRB trends in a pre-industrial period. The data quality of Swedish population registers is also thought to be "satisfactorily good" during pre-industrial time and almost 100% complete and accurate after 1860 (Fellman and Eriksson, 2011; Sundbärg, 1907).

1.2.2 Outcome Variables

The main outcome variable is annual SRB for the whole Swedish population. Instead of using the conventional expression for sex ratio as the male-to-female ratio, we calculated SRB as the percentage of male births in a given year, which is recommended by methodological literature on statistical analysis of SRB to avoid confusion in interpretation of the results and to facilitate comparisons between studies (Wilson and Hardy, 2002). Annual number of live births by sex is drawn from the Human Mortality Database (HMD; University of California, Berkeley (USA) and Max Planck Institute for Demographic Research (Germany), 2018), from which we calculate annual SRB as the percentage of male births between 1749 and 1991. The HMD compiles detailed demographic data, such as age- and sex-specific births, deaths, and population counts, from Swedish population registers.

Annual number of stillbirths by sex between 1861 and 1991 is provided by Statistics Sweden, which we use as a proxy for foetal loss. Stillbirths can represent only part of the total prenatal deaths, but this is the most suitable and comprehensive variable we can rely on to examine the foetal loss mechanisms underlying the TWH. In line with the expression for SRB, we calculate the sex ratio of stillbirths as the percentage of male stillbirths in a given year.

1.2.3 Economic Covariates

Eight economic covariates are used in this study to measure the intensity of maternal stress attributable to economic conditions in a given year. First, we use GDP to measure the values of all goods and services produced as an indicator of economic size and development in Sweden. Estimates of annual GDP per capita in 1910/1912 constant price are acquired from the Swedish Historical National Accounts (SHNA; Lobell et al., 2008; Schön and Krantz, 2012). Economic historians have pointed out several pitfalls in the SHNA (Bohlin, 2003; Edvinsson, 2013a; Myrdal and Morell, 2011), therefore we make use of the percentage volume growth in GDP (GDP volume growth) as an alternative measure of economic size (Edvinsson, 2013a,b). We also use consumer price index (CPI) as a measure of changes in price levels of a basket of consumer goods and services purchased by households to better capture the living cost and reflect the level of stress among Swedish people. An estimation of Swedish CPI (index 1914 = 100) is obtained from Edvinsson and Söderberg (2011).

Three economic indicators that are widely employed in historical research are also utilised in this paper. First, we use real wage, the wage adjusted for inflation, as a direct measure of the amount of goods and services that can be bought and a proxy for standard of living in pre-industrial Sweden. We draw on a series of daily real wage of Swedish male labourers from Söderberg (2010). During the pre-industrial period, the predominant part of household consumption was almost always food, and hence the price of food is often treated as another proxy for standard of living. The price of rye, the most important staple food in pre-industrial Sweden, was achieved in Jörberg (1972). Last, CPI, real wage, and rye price are determined partly by crop yields which may be used independently as a proxy for population stress (Eckstein et al., 1985). A crop index was computed by Sundbärg (1907) and quantifies the relative abundance of crops in the calendar year season. An index of 3.0 refers to an average crop yields in a specific year, so any year with an index greater/smaller than 3.0 indicates crop yields above/below the average during that year.

In previous research, Catalano and Bruckner (2005) used private consumption in 1985 prices from the Swedish Macro Data from 1861–1988 (Institute for International Economic Studies, 2019) and Statistics Sweden (1997) as a measure of the capacity of Swedish households to consume goods and services and a proxy for economic stress. This covariate is retained and labelled as consumption (old) in our analysis. In addition, we draw on updated estimates of consumption from the SHNA and term this covariate consumption (new) as an alternative measure of consumption to check whether the previous findings are robust.

Following existing research, economic indicators including GDP per capita, CPI, real wage, rye price, consumption (new), and consumption (old) are expressed as annual percentage change (current value relative to the value in last year). We keep the original values of GDP volume growth since it has already been expressed as a percentage change.

1.2.4 Climatic Covariate

To get a picture of abnormal temperature as a population stressor, we rely on temperature anomalies. A temperature anomaly is the difference from an average or baseline temperature. A positive anomaly indicates that the observed temperature is warmer than the baseline, while a negative anomaly indicates the observed temperature was cooler than the baseline, and therefore temperature anomaly can best capture the variation in ambient temperature over a long period. Temperature anomalies (1961–1990 average temperature as baseline) are obtained from the georeferenced CRUTEM4 data set that provides global historical near-surface air temperature anomalies over land (Jones et al., 2012). From the dissemination of CRUTEM4 data via Google Earth (Osborn and Jones, 2014), we extract time series of annual temperature anomalies over six 5×5 grids on which the Swedish territory is located and average the six values in a given year to represent the temperature anomaly for the whole of Sweden in that year (Moberg and Alexandersson, 1997). Annual temperature anomalies are computed over 21-month intervals, in other words, the value of temperature anomaly in a year is calculated as the mean of monthly anomalies over 9 months (April to December) in the previous year and over 12 months (January to December) in the current year (Brohan et al., 2006; Helle et al., 2009).

1.2.5 Control Variables

Helle et al. (2009) suggested that the TWH should be tested in a multivariate framework so that one can examine the "simultaneous role" of various population stressors. They included not only economic and climatic covariates but also other variables such as mortality in their time-series models. In this article, we also add extra control variables to the baseline models. Importantly, one should be aware that the additional variables included in Helle et al. (2009) and in our study are likely to be the outcomes rather than the causes of economic and climatic changes so that these variables are likely to be "bad controls". We treat these multivariate models as additional robustness checks and report the results in Appendix A.

The first control variable is annual life expectancy at birth (e_0) available from the HMD, and in addition to total e_0, we also use female and male e_0 separately. e_0 summaries the overall mortality of the Swedish population in a given year and serves as a proxy for population-level stress with respect to health.

Two other control variables are both gathered from the Human Fertility Collection (Max Planck Institute for Demographic Research (Germany) and Vienna Institute of Demography (Austria), 2021). We use total fertility rate (TFR) as a proxy for birth order and mean age at childbearing (MACB) as a proxy for maternal age, as both have been shown to be negatively correlated with SRB (Chahnazarian, 1988). If there are two or more estimates of TFR or MACB for a given year, we average all estimates in that year to get a single measure.

1.2.6 Statistical Analyses

We perform three sets of analyses to examine the correlation between SRB and maternal stress in this paper: Analysis I covers the period 1749–1991; Analysis II covers the period 1749–1861; and Analysis III covers the period 1862–1991. The study period of Analysis I is based on the time span of available SRB data; the study period of Analysis II can be roughly seen as the pre-industrial era of Sweden (Edvinsson, 2017); and the study period of Analysis III is identical to that of Catalano and Bruckner (2005), which enables us to compare our results with theirs. Data availability of covariates varies across the three analyses, and accordingly Analysis I contains the following covariates: GDP per capita, GDP volume growth, and CPI; Analysis II contains: GDP per capita, GDP volume growth, CPI, real wage, rye price, and crop index; and Analysis III contains: GDP per capita, GDP volume growth, CPI, private consumption (new), private consumption (old), and temperature anomaly.

In the multivariate models as robustness checks, we add control variables to each of the baseline models in Analyses I, II, and III. e_0 is added to all three analyses, and we use total, female, and male e_0 in separate models to avoid multicollinearity problems. TFR and MACB are added only to Analysis III because they are available only from 1850. In addition, data on e_0 are available only from 1751 so Analyses I and II in robustness checks start from 1751 instead of 1749.

To test the foetal loss mechanism, we replicate Analysis III and replace the outcome variable by annual sex ratio of stillbirths. Because data on stillbirths are only available from 1861, we are unable to test this mechanism in the pre-industrial era.

For each of the analyses, we employ an ARIMA (autoregressive integrated moving average) approach to modelling the relationship between SRB and each covariate. Given the fact that foetuses conceived after April in one year are likely to be born in the next year, we also include a 1-year lag in covariates in our ARIMA models. We do not apply the lag term to temperature anomaly since its value in the present year has already taken into account the temperatures in the previous year.

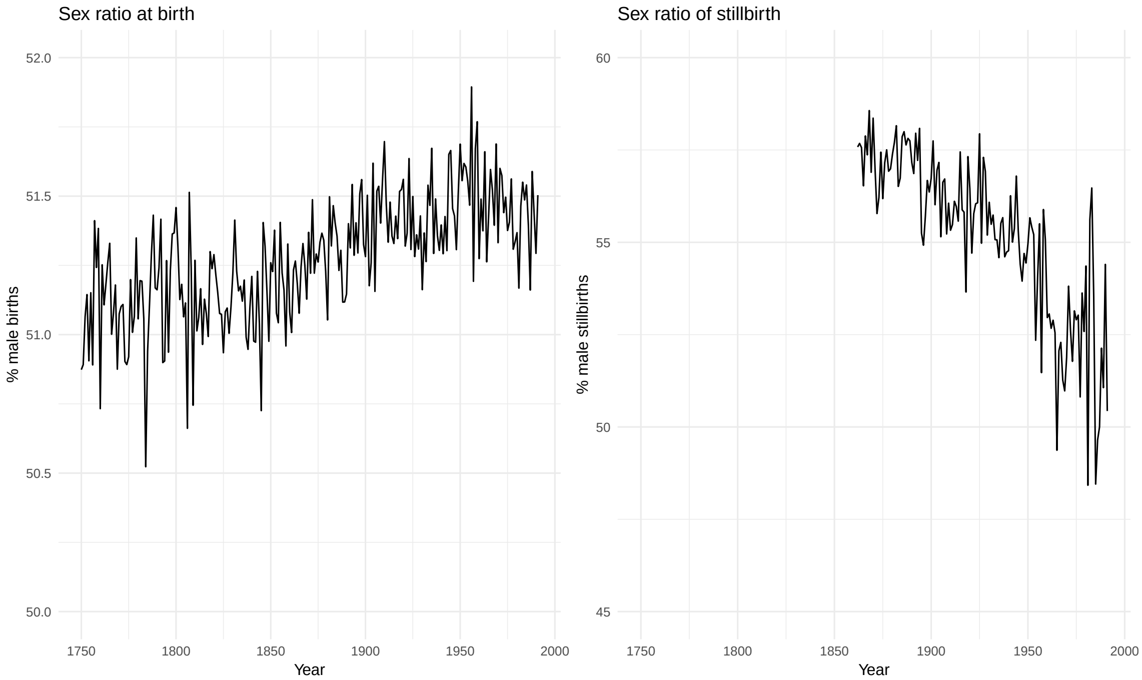
All statistical analyses are conducted using R version 3.4.4. To perform our analyses, we first test the stationarity assumption of all variables being modelled using the augmented Dickey–Fuller test, which tests the null hypothesis that a unit root is present in a time series process and therefore the process is non-stationary. Once we confirm that all covariates are stationary processes, we estimate the ARIMA model with optimal orders of autoregressive, differencing, and moving average chosen by the `auto.arima()` function in the R package `forecast` (Hyndman and Khandakar, 2008).

1.3 Results

1.3.1 Summary Statistics

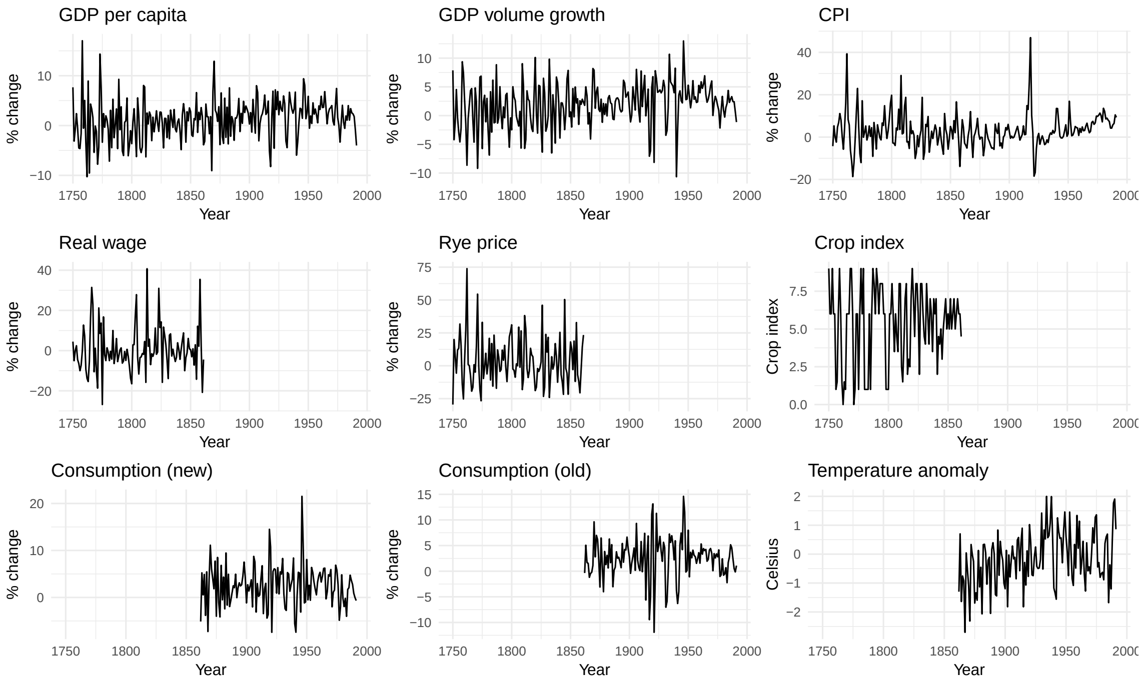
Figure 1.1 depicts temporal changes in Swedish SRB (left) and sex ratio of stillbirths (right). The values of SRB in 1749–1991 vary between 50.5% and 51.9% with a mean of 51.3 and a standard deviation of 0.22. A slightly increasing trend can be detected prior to 1950. The values of sex ratio of stillbirths in 1862–1991 fluctuate between 48.4% and 58.5% with a mean of 55.2 and a standard deviation of 2.25. The variation in sex ratio of stillbirths is appreciably larger than that in SRB, and a descending trend can be observed before 1950. Figure 1.2 shows changes in nine economic and climatic covariates in Sweden. Table 1.1 shows summary statistics of and results from the augmented Dickey–Fuller tests on outcome variables and covariates. By

Figure 1.1: Trends in sex ratio at birth and sex ratio of stillbirths in Sweden



comparing the augmented Dickey–Fuller test statistics with recommended critical value (-3.51 at 1% with a sample size of 100 observations), the null hypothesis is

Figure 1.2: Trends in economic and climatic covariates in Sweden



rejected in favour of the alternative hypothesis that all covariates are stationary and thus are suitable for ARIMA model. Correlation matrices of the covariates used in three analyses are shown in Tables A.1, A.2, and A.3; it can be seen that apart from a few covariates, the correlation between most covariates is below 0.7, indicating that these covariates do capture potentially different stressors.

1.3.2 Analysis I: 1749–1991

Results from Analysis I are shown in Table 1.2. We can see that none of the coefficients are statistically significant. The coefficients of GDP per capita and GDP volume growth are negative (except that of GDP volume growth with no lag time) while the coefficients of CPI are positive. The directions of these coefficients are opposite to the TWH's prediction that SRB should be positively associated with GDP and negatively associated with CPI because SRB should decline during an economic contraction when GDP growth decelerates and price level rises.

1.3.3 Analysis II: 1749–1861

Results from Analysis II are shown in Table 1.3. Again, most coefficients are not significant, and the signs of coefficients of GDP per capita, GDP volume growth, CPI, and crop index (at a 1-year lag) are in the opposite direction compared to that suggested by the TWH. Real wage and rye price at a 1-year lag are significant at 10% and 5%, respectively ($p = 0.058$ and 0.032). A 1% increase in real wage is associated with a 0.003% reduction in the proportion of male births in the following year, while a 1% increase in rye price is associated with a 0.002% increase in the proportion of male births in the subsequent year. These results are inconsistent with the TWH, according to which economic improvements, as proxies for reduced maternal stress, should be linked with increases in SRB.

1.3.4 Analysis III: 1862–1991

Results from Analysis III are shown in Table 1.4. The only significant covariate is consumption (old) ($p = 0.004$), the covariate used in Catalano and Bruckner (2005). A 1% increase in percentage change in private consumption is associated with a 0.008% increase in SRB in the current year, which is qualitatively identical to the findings of Catalano and Bruckner (2005) and in line with the TWH. It should also be noted that consumption (new), the alternative and updated measure of consumption, shows no statistically significant association with SRB, despite its high correlation

Table 1.1: Summary statistics and augmented Dickey–Fuller test statistics of outcome variables and covariates

	1749–1991			1749–1861			1862–1991		
	Mean	SD	ADF	Mean	SD	ADF	Mean	SD	ADF
Outcome variables									
SRB	51.27	0.22	−5.10	51.12	0.18	−5.07	51.40	0.16	−3.46
Sex ratio of stillbirths							55.20	2.25	−3.40
Covariates									
GDP per capita	1.29	4.06	−7.11	0.37	4.44	−5.88	2.08	3.53	−5.64
GDP volume growth	1.94	3.85	−7.59	1.06	4.21	−7.69	2.71	3.35	−5.82
CPI	2.89	7.88	−5.48	2.64	8.70	−5.50	3.11	7.11	−4.62
Real wage				0.63	11.19	−6.16			
Rye price				3.77	18.47	−6.84			
Crop index				5.29	2.52	−5.29			
Consumption (new)							2.51	4.45	−5.39
Consumption (old)							2.53	4.05	−6.28
Temperature anomaly							−0.16	0.95	−4.70

Notes: ADF = Augmented Dickey–Fuller test statistics; CPI = consumer price index; GDP = gross domestic product; SD = standard deviation; SRB = sex ratio at birth.

Table 1.2: Coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1749–1991

	Outcome variable: SRB, 1749–1991		
GDP per capita, t	−0.0016		
	(0.0026)		
GDP per capita, t−1	−0.0020		
	(0.0026)		
GDP volume growth, t		0.0007	
		(0.0028)	
GDP volume growth, t−1		−0.0014	
		(0.0028)	
CPI, t			0.0002
			(0.0015)
CPI, t−1			0.0024
			(0.0015)
ARIMA(p,d,q)	(1,1,2)	(1,1,2)	(1,1,2)
Ljung–Box Q test	7.51	7.47	8.41
AIC	−166.00	−165.48	−168.30

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.
Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; SRB = sex ratio at birth; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

with the old measure ($r = 0.79$, $p < 0.001$). Temperature anomaly is positively correlated with SRB, indicating that more sons should be born in warmer years, but we need to interpret this with extra caution as the coefficient is not statistically significant.

1.3.5 Multivariate Models as Robustness Checks

Trends in total, female, and male e_0 as well as TFR and MACB are illustrated in Figure A.1, from which we can visibly see upward trends in e_0 and downward trends in TFR and MACB, showing the demographic transition in Sweden. Clearly, all of the control variables are not stationary so we have to difference them first. The number of differences required to make these time series stationary is selected through the `ndiffs()` function in the R package `forecast`. MACB requires a second-order differencing, while all others need to be differenced just once.

Results from multivariate models for the period 1752–1991 controlling for total, female, and male e_0 are shown in Tables A.4, A.5, and A.6, respectively. We can

Table 1.3: Coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1749–1861

	Outcome variable: SRB, 1749–1861					
GDP per capita, t	−0.0063					
	(0.0039)					
GDP per capita, t−1	−0.0027					
	(0.0039)					
GDP volume growth, t		−0.0017				
		(0.0041)				
GDP volume growth, t−1		−0.0031				
		(0.0041)				
CPI, t			0.0010			
			(0.0020)			
CPI, t−1			0.0031			
			(0.0020)			
Real wage, t				−0.0016		
				(0.0015)		
Real wage, t−1				−0.0029†		
				(0.0015)		
Rye price, t					−0.0001	
					(0.0009)	
Rye price, t−1					0.0020*	
					(0.0009)	
Crop index, t						0.0016
						(0.0074)
Crop index, t−1						−0.0042
						(0.0073)
ARIMA(p,d,q)	(0,0,0)	(0,0,0)	(0,0,0)	(0,0,0)	(0,0,0)	(0,0,0)
Ljung–Box Q test	5.5	5.38	6.44	4.48	6.67	5.78
AIC	−56.78	−54.74	−57.17	−58.96	−58.58	−54.36

†$p < 0.1$; *$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; SRB = sex ratio at birth; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Table 1.4: Coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1862–1991

	Outcome variable: SRB, 1862–1991					
GDP per capita, t	0.0044					
	(0.0035)					
GDP per capita, t−1	−0.0018					
	(0.0035)					
GDP volume growth, t		0.0046				
		(0.0036)				
GDP volume growth, t−1		−0.0003				
		(0.0037)				
CPI, t			−0.0012			
			(0.0022)			
CPI, t−1			0.0019			
			(0.0022)			
Consumption (new), t				0.0032		
				(0.0028)		
Consumption (new), t−1				−0.0006		
				(0.0028)		
Consumption (old), t					0.0083**	
					(0.0029)	
Consumption (old), t−1					−0.0021	
					(0.0029)	
Temperature anomaly, t						0.0153
						(0.0136)
ARIMA(p,d,q)	(0,1,1)	(1,1,1)	(1,1,1)	(0,1,1)	(1,0,2)	(1,1,1)
Ljung–Box Q test	9.15	7.33	8.79	9.13	6.76	8.06
AIC	−124.43	−124.16	−123.28	−124.03	−127.59	−125.78

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.
Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; SRB = sex ratio at birth; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

see that all of the three economic covariates still show no significant correlation with SRB. Life expectancies are themselves not significant.

Results from multivariate models for the period 1752–1861 are presented in Tables A.7, A.8, and A.9. Again, life expectancies are not significant, and the only significant (at 10%) covariate is rye price at a 1-year lag, while the sign of its coefficient is inconsistent with the TWH's prediction.

Results from robustness checks on Analysis III (1862–1991) controlling for life expectancies, TFR, and MACB are reported in Tables A.10, A.11, and A.12. In all but one case, MACB as a proxy for maternal age is negatively and significantly correlated with SRB between 1862 and 1991, while all other control variables are not. The only exception where MACB is not statistically significant is the models using the old measure of private consumption, the same economic proxy used in Catalano and Bruckner (2005). Similar to Analysis III, consumption (old) is the only variable that is significantly associated with SRB, and all other economic and climatic proxies show no significant association with SRB.

1.3.6 Foetal Loss Mechanism

Table 1.5 presents results from analysis of the correlation between sex ratio of stillbirths and proxies for maternal stress for the period 1862–1991. None of the coefficients are statistically significant, suggesting no evidence that maternal stress reduces SRB by increasing male foetal loss.

1.4 Discussion

This study uses time series models to analyse the relationship between Swedish SRB and a number of economic and climatic proxies for maternal stress over 243 years between 1749 and 1991. The vast majority of covariates are not statistically significant in any of the three study periods, 1749–1991, 1749–1861, and 1862–1991, indicating that the relationship between maternal stress and SRB is extremely weak or even non-existing. Only three covariates are statistically significant: real wage and rye price in 1749–1861 and consumption (old) in 1862–1991. However, real wage and rye price are just significant at 10% and 5%, respectively, and the signs of the coefficients conflict with the prediction of the TWH, leaving consumption (old) as the only significant covariate in support of the TWH. Additional analysis finds no significant relationship between sex ratio of stillbirths and maternal stress in 1862–1991.

Table 1.5: Coefficients from regression models predicting Swedish sex ratio of stillbirths (calculated as proportion of male stillbirths), 1862–1991

	Outcome variable: Sex ratio of stillbirths, 1862–1991					
GDP per capita, t	0.0035					
	(0.0312)					
GDP per capita, t−1	−0.0191					
	(0.0313)					
GDP volume growth, t		0.0051				
		(0.0332)				
GDP volume growth, t−1		−0.0516				
		(0.0333)				
CPI, t			−0.0137			
			(0.0200)			
CPI, t−1			0.0165			
			(0.0200)			
Consumption (new), t				−0.0181		
				(0.0247)		
Consumption (new), t−1				−0.0260		
				(0.0247)		
Consumption (old), t					0.0013	
					(0.0272)	
Consumption (old), t−1					−0.0271	
					(0.0272)	
Temperature anomaly, t						0.1813
						(0.1265)
ARIMA(p,d,q)	(0,1,1)	(0,1,1)	(0,1,1)	(0,1,1)	(0,1,1)	(0,1,1)
Ljung–Box Q test	12.69	14.78	11.75	13.48	13.51	12.55
AIC	444.70	442.70	444.32	443.57	441.72	441.03

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Long-term time series data on SRB are available in several Nordic countries, but previous research is either entirely descriptive (e.g., only looking at the temporal trends and fluctuations in SRB without any explanatory variables) or limited to a specific group of the population that may not be representative (Fellman and Eriksson, 2011; Helle et al., 2007; Vartiainen et al., 1999). This article is the first to perform an exploratory analysis of SRB change and associated factors as well as the first to test the TWH over such a long duration. Analysis I examines the longest continuous time series of SRB from 1749 to 1991 and its association with three economic indicators as proxies for population stress. Our results suggest no evidence for the TWH as all of the coefficients in Analysis I are not significant. The results threaten the external validity of many existing studies with a much shorter period under investigation since the supportive evidence for the TWH reported in those articles may only hold within their study periods and cannot be generalised to other times (e.g., a pre-industrial era).

A large body of literature has identified substantial demographic responses, such as fertility, mortality, and marriage, to economic variations, such as wages and prices, in the pre-industrial period (Bengtsson and Ohlsson, 1985; Eckstein et al., 1985; Edvinsson, 2017; Galloway, 1988; Larsen, 1988). The estimated relationship between demographic rates and economic conditions is usually more pronounced during the pre-industrial era than during industrialisation or the post-industrial time. Analysis II contributes to this vein of research by adding a key demographic outcome that has often been overlooked in this historical period, SRB. Unlike other demographic variables, we do not find evidence that trends in SRB respond to changes in a number of economic indicators, such as GDP, CPI, and crop yields. The results are surprising since in the absence of the welfare state and economic development that help women cope with economic stress, we would expect SRB to be more responsive to economic fluctuations in the pre-industrial period, based on the TWH. Although two economic variables, real wage and rye price, show a statistically significant association with SRB, the coefficients are in the opposite direction compared to the TWH's prediction.

The study period of Analysis III is 1862–1991, exactly the same as the study period of Catalano and Bruckner (2005). While they used only a single covariate, this study uses six variables covering different dimensions of maternal stress. Catalano and Bruckner (2005) showed that Swedish SRB is positively associated with the old measure of consumption with both no lag time and at a 1-year lag. This study agrees on their results when only considering consumption at no lag time, whereas all five other covariates, including an updated and preferred measure of consumption, are not significantly correlated with SRB, indicating that the results from Catalano and Bruckner (2005) are not robust to alternative measures of maternal stress. Given that almost all earlier research on the TWH relies on a single measure of maternal stress, their results are very likely to be speculative and require further robustness checks.

Analysis III also adds to the empirical literature on the relationship between SRB and temperature. In line with previous research (Catalano et al., 2008; Helle et al., 2007), we also find that warmer years are associated with more male births, but this association is not significant. One possible reason for the disagreement between the results regarding statistical significance is that compared with the indicators used in previous research, we rely on a more precise indicator, temperature anomaly, that covers broader areas (i.e., the entire Swedish territory) and better reflects the climatic shock to the population caused by abnormal temperature that deviates from time trends.

In the multivariate models, most of the explanatory variables, including the control variables, still show no significant association with SRB, indicating that our findings are robust to alternative specifications controlling for a number of confounders that may potentially affect SRB. Interestingly, female e_0 as a proxy for maternal stress resulting from mortality and health conditions also shows no significant correlation with SRB, providing further evidence against the TWH. According to the aforementioned demographic literature (e.g., Galloway, 1988), demographic rates and variables are often responsive to socioeconomic and climatic changes, so changes in the economic and climatic covariates would possibly cause changes in the control variables. Hence, these demographic variables are probably "bad controls" and the results from these multivariate models should be interpreted with caution.

Earlier literature has proposed that maternal stress reduces SRB through increasing male foetal loss (James and Grech, 2017), which has been verified using data on foetal deaths from the United States (Bruckner et al., 2010; Catalano et al., 2005a). While the two American studies only cover a study period of about a decade, we examine the relationship between stillbirth sex ratio and various proxies for maternal stress in Sweden over a century. We do not find a significant correlation between them, again questioning the external validity of existing research.

In addition to the impact of maternal stress on human SRB, one may also consider the influence of the father whose sperm contributes either an X chromosome that leads to a male offspring (XX-paired chromosomes) or a Y chromosome that leads to a female offspring (XY-paired chromosomes). The features of male-provided sperm are important in determining the sex ratio at conception, or the primary sex ratio

(PSR), and paternal stress may affect sperm and then alter PSR. Unfortunately, there is a lack of data on sperm quality and PSR in Sweden, so we cannot formally test this mechanism, however, we still believe that there is no evidence of the effect of paternal stress on offspring sex ratio for three reasons. First, it has generally been thought that X- and Y-bearing sperms are produced in equal number and have an equal possibility of insemination, and hence PSR should be 0.5 (James, 2012). Using detailed data from a variety of sources, a comprehensive analysis of the trajectory of the human sex ratio from conception to birth also reveals that PSR is unbiased in terms of sex (Orzack et al., 2015). Second, the economic and climatic covariates used in this study are at the national level so that they affect women and men simultaneously and can be seen as proxies for both maternal and paternal stress. Furthermore, the real wage modelled in Analysis II is based on the daily real wage of Swedish male labourers so that it is related directly to men. Given the lack of statistical significance in our findings, we think there is no evidence of the father's aspect of the TWH. Last, in the robustness checks, we show that male life expectancy at birth, a measure of male mortality and health conditions, is not significantly associated with SRB, indicating no evidence that the level of paternal stress resulting from poor health conditions is linked with trends in SRB.

This study encounters a few limitations and concerns about data quality. First, annual data are used in the analysis, and we cannot investigate the potential seasonality in SRB due to the lack of disaggregated monthly data. Another limitation is the ecological fallacy. This study uses national data and provides macro-level evidence that may not necessarily be true for all individuals. A related study using individual-level data from Swedish administrative registers available for a shorter duration from 1960 onward found no statistically significant association between offspring sex and maternal socioeconomic status (Kolk and Schnettler, 2016). A further investigation into historical period on this topic can be conducted given the enormous individual data available for pre-industrial Sweden, and the proxy for maternal stress can be socioeconomic status like occupations at the individual level, or local food price at the aggregate level. Furthermore, parents' preference for the offspring's sex may lead to sex-selective infanticide and abortion and thus cause inaccuracy in SRB records. Nevertheless, the literature has suggested that infanticide has been rare in Sweden since the early 18th century (Sandin, 2013), and prenatal sex selection is very unlikely in contemporary Sweden because of a small or non-existent son preference (Miranda et al., 2018) and the government's critical stance towards sex-selective abortion (Purewal and Eklund, 2018). As a result, it is reasonable to believe that Swedish SRB data are not subject to serious measurement errors due to sex-selective misreporting. Last, given the social and political stability of Sweden, our findings may not be generalised to the context of extreme maternal stress conditions, such as famine and war.

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Appendix A

Appendix to Chapter 1

Figure A.1: Trends in control variables in Sweden

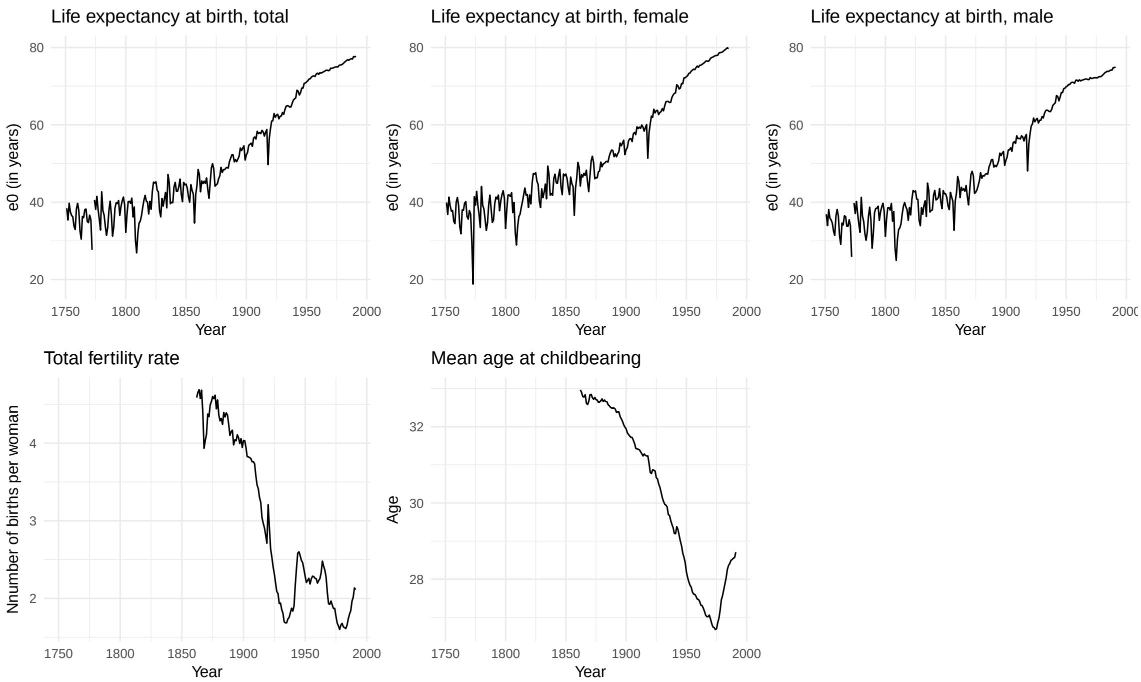

Table A.1: Correlation matrix of covariates used in Analysis I, 1749–1991

	GDP per capita	GDP volume growth	CPI
GDP per capita	1.00		
GDP volume growth	0.58***	1.00	
CPI	−0.30***	−0.46***	1.00

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Notes: CPI = consumer price index; GDP = gross domestic product.

Table A.2: Correlation matrix of covariates used in Analysis II, 1749–1861

	GDP per capita	GDP volume growth	CPI	Real wage	Rye price	Crop index
GDP per capita	1.00					
GDP volume growth	0.43***	1.00				
CPI	−0.37***	−0.63***	1.00			
Real wage	0.23*	0.48***	−0.65***	1.00		
Rye price	−0.39***	−0.77***	0.88***	−0.63***	1.00	
Crop index	0.26**	0.63***	−0.56***	0.38***	−0.60***	1.00

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Notes: CPI = consumer price index; GDP = gross domestic product.

Table A.3: Correlation matrix of covariates used in Analysis III, 1862–1991

	GDP per capita	GDP volume growth	CPI	Consumption (new)	Consumption (old)	Temperature anomaly
GDP per capita	1.00					
GDP volume growth	0.75***	1.00				
CPI	−0.24**	−0.28**	1.00			
Consumption (new)	0.81***	0.65***	−0.14	1.00		
Consumption (old)	0.69***	0.75***	−0.14	0.79***	1.00	
Temperature anomaly	$0.15^{\dagger}$	0.28**	−0.06	0.06	$0.15^{\dagger}$	1.00

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Notes: CPI = consumer price index; GDP = gross domestic product.

Table A.4: Robustness checks controlling for total life expectancy at birth: coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1752–1991

	Outcome variable: SRB, 1752–1991		
GDP per capita, t	−0.0005		
	(0.0052)		
GDP per capita, t−1	0.0000		
	(0.0052)		
GDP volume growth, t		0.0047	
		(0.0055)	
GDP volume growth, t−1		−0.0036	
		(0.0054)	
CPI, t			0.0018
			(0.0026)
CPI, t−1			−0.0015
			(0.0027)
Total life expectancy	0.0058	0.0051	0.0090
	(0.0075)	(0.0073)	(0.0067)
ARIMA(p,d,q)	(1,0,1)	(1,0,1)	(1,0,1)
Ljung–Box Q test	8.69	8.84	10.27
AIC	148.08	147.26	148.26

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.
Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; SRB = sex ratio at birth; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Table A.5: Robustness checks controlling for female life expectancy at birth: coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1752–1991

	Outcome variable: SRB, 1752–1991		
GDP per capita, t	−0.0005		
	(0.0052)		
GDP per capita, t−1	0.0001		
	(0.0052)		
GDP volume growth, t		0.0048	
		(0.0054)	
GDP volume growth, t−1		−0.0036	
		(0.0054)	
CPI, t			0.0017
			(0.0026)
CPI, t−1			−0.0014
			(0.0027)
Female life expectancy	0.0044	0.0041	0.0080
	(0.0075)	(0.0073)	(0.0065)
ARIMA(p,d,q)	(1,0,1)	(1,0,1)	(1,0,1)
Ljung–Box Q test	8.46	8.63	10.04
AIC	148.34	147.44	148.54

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.
Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; SRB = sex ratio at birth; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Table A.6: Robustness checks controlling for male life expectancy at birth: coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1752–1991

	Outcome variable: SRB, 1752–1991		
GDP per capita, t	−0.0005		
	(0.0052)		
GDP per capita, t−1	−0.0001		
	(0.0052)		
GDP volume growth, t		0.0037	
		(0.0055)	
GDP volume growth, t−1		−0.0042	
		(0.0054)	
CPI, t			0.0019
			(0.0026)
CPI, t−1			−0.0016
			(0.0027)
Male life expectancy	0.0077	0.0070	0.0100
	(0.0078)	(0.0074)	(0.0068)
ARIMA(p,d,q)	(1,0,1)	(1,0,1)	(1,0,1)
Ljung–Box Q test	8.74	9.01	10.50
AIC	149.58	147.24	147.95

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.
Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; SRB = sex ratio at birth; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Table A.7: Robustness checks controlling for total life expectancy at birth: coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1752–1861

	Outcome variable: SRB, 1752–1861					
GDP per capita, t	−0.0060					
	(0.0079)					
GDP per capita, t−1	0.0093					
	(0.0083)					
GDP volume growth, t		−0.0077				
		(0.0086)				
GDP volume growth, t−1		−0.0081				
		(0.0090)				
CPI, t			0.0043			
			(0.0039)			
CPI, t−1			0.0006			
			(0.0038)			
Real wage, t				−0.0047		
				(0.0031)		
Real wage, t−1				0.0016		
				(0.0031)		
Rye price, t					0.0017	
					(0.0018)	
Rye price, t−1					0.0033†	
					(0.0019)	
Crop index, t						−0.0076
						(0.0132)
Crop index, t−1						0.0075
						(0.0131)
Total life expectancy	0.0029	0.0047	0.0027	0.0093	0.0076	−0.0021
	(0.0104)	(0.0096)	(0.0091)	(0.0092)	(0.0092)	(0.0089)
ARIMA(p,d,q)	(0,0,1)	(3,0,0)	(3,0,0)	(0,0,1)	(3,0,0)	(3,0,0)
Ljung–Box Q test	7.05	15.05	12.10	8.83	18.22	10.84
AIC	95.85	114.81	114.41	92.88	110.26	117.42

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; SRB = sex ratio at birth; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Table A.8: Robustness checks controlling for female life expectancy at birth: coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1752–1861

	Outcome variable: SRB, 1752–1861					
GDP per capita, t	−0.0124					
	(0.0085)					
GDP per capita, t−1	0.0046					
	(0.0090)					
GDP volume growth, t		−0.0076				
		(0.0086)				
GDP volume growth, t−1		−0.0080				
		(0.0090)				
CPI, t			0.0043			
			(0.0039)			
CPI, t−1			0.0006			
			(0.0038)			
Real wage, t				−0.0048		
				(0.0031)		
Real wage, t−1				0.0016		
				(0.0031)		
Rye price, t					0.0017	
					(0.0018)	
Rye price, t−1					$0.0033^{\dagger}$	
					(0.0019)	
Crop index, t						−0.0076
						(0.0132)
Crop index, t−1						0.0075
						(0.0131)
Female life expectancy	0.0000	0.0045	0.0026	0.0099	0.0075	−0.0023
	(0.0100)	(0.0095)	(0.0090)	(0.0092)	(0.0091)	(0.0088)
ARIMA(p,d,q)	(3,0,0)	(3,0,0)	(3,0,0)	(0,0,1)	(3,0,0)	(3,0,0)
Ljung–Box Q test	10.82	14.91	12.01	8.88	18.05	10.85
AIC	115.64	114.83	114.42	92.76	110.26	117.42

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.
Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; SRB = sex ratio at birth; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Table A.9: Robustness checks controlling for male life expectancy at birth: coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1752–1861

	Outcome variable: SRB, 1752–1861					
GDP per capita, t	−0.0060					
	(0.0079)					
GDP per capita, t−1	0.0094					
	(0.0084)					
GDP volume growth, t		−0.0077				
		(0.0086)				
GDP volume growth, t−1		−0.0081				
		(0.0090)				
CPI, t			0.0043			
			(0.0039)			
CPI, t−1			0.0006			
			(0.0038)			
Real wage, t				−0.0047		
				(0.0031)		
Real wage, t−1				0.0015		
				(0.0031)		
Rye price, t					0.0017	
					(0.0018)	
Rye price, t−1					0.0033†	
					(0.0019)	
Crop index, t						−0.0076
						(0.0132)
Crop index, t−1						0.0074
						(0.0132)
Male life expectancy	0.0024	0.0048	0.0029	0.0088	0.0076	−0.0020
	(0.0103)	(0.0096)	(0.0091)	(0.0092)	(0.0092)	(0.0089)
ARIMA(p,d,q)	(0,0,1)	(3,0,0)	(3,0,0)	(0,0,1)	(3,0,0)	(3,0,0)
Ljung–Box Q test	6.98	15.17	12.18	8.76	18.32	10.86
AIC	95.88	114.80	114.40	92.99	110.26	117.43

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; SRB = sex ratio at birth; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Table A.10: Robustness checks controlling for total life expectancy at birth, total fertility rate, and mean age at childbearing: Coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1862–1991

	Outcome variable: SRB, 1862–1991					
GDP per capita, t	0.0020					
	(0.0037)					
GDP per capita, t−1	−0.0033					
	(0.0035)					
GDP volume growth, t		0.0036				
		(0.0036)				
GDP volume growth, t−1		−0.0024				
		(0.0037)				
CPI, t			−0.0016			
			(0.0023)			
CPI, t−1			0.0019			
			(0.0021)			
Consumption (new), t				0.0008		
				(0.0030)		
Consumption (new), t−1				−0.0017		
				(0.0028)		
Consumption (old), t					0.0063*	
					(0.0031)	
Consumption (old), t−1					−0.0022	
					(0.0029)	
Temperature anomaly, t						0.0161
						(0.0134)
Total life expectancy	0.0030	0.0029	0.0021	0.0032	0.0023	0.0047
	(0.0081)	(0.0079)	(0.0086)	(0.0081)	(0.0081)	(0.0079)
TFR	0.0134	0.0209	0.0146	0.0250	0.0034	0.0090
	(0.1095)	(0.1080)	(0.1065)	(0.1099)	(0.1093)	(0.1059)
MACB	−0.3176*	−0.3209*	−0.3293*	−0.3101*	−0.2345	−0.3142*
	(0.1415)	(0.1428)	(0.1432)	(0.1441)	(0.1454)	(0.1391)
ARIMA(p,d,q)	(0,1,1)	(1,1,1)	(1,1,1)	(0,1,1)	(2,0,1)	(1,1,1)
Ljung–Box Q test	7.95	5.82	6.19	8.21	7.70	5.67
AIC	−124.06	−123.98	−123.57	−123.28	−124.62	−126.20

† $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.
Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; MACB = mean age at childbearing; SRB = sex ratio at birth; TFR = total fertility rate; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Table A.11: Robustness checks controlling for female life expectancy at birth, total fertility rate, and mean age at childbearing: coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1862–1991

	Outcome variable: SRB, 1862–1991					
GDP per capita, t	0.0019					
	(0.0037)					
GDP per capita, t−1	−0.0034					
	(0.0035)					
GDP volume growth, t		0.0036				
		(0.0036)				
GDP volume growth, t−1		−0.0024				
		(0.0037)				
CPI, t			−0.0015			
			(0.0023)			
CPI, t−1			0.0018			
			(0.0021)			
Consumption (new), t				0.0007		
				(0.0030)		
Consumption (new), t−1				−0.0017		
				(0.0028)		
Consumption (old), t					0.0063*	
					(0.0031)	
Consumption (old), t−1					−0.0022	
					(0.0029)	
Temperature anomaly, t						0.0162
						(0.0134)
Female life expectancy	0.0042	0.0038	0.0031	0.0042	0.0032	0.0054
	(0.0081)	(0.0080)	(0.0086)	(0.0082)	(0.0083)	(0.0080)
TFR	0.0118	0.0187	0.0122	0.0232	0.0006	0.0070
	(0.1095)	(0.1081)	(0.1066)	(0.1100)	(0.1095)	(0.1060)
MACB	−0.3136*	−0.3171*	−0.3243*	−0.3066*	−0.2320	−0.3107*
	(0.1416)	(0.1430)	(0.1434)	(0.1442)	(0.1455)	(0.1394)
ARIMA(p,d,q)	(0,1,1)	(1,1,1)	(1,1,1)	(0,1,1)	(2,0,1)	(1,1,1)
Ljung–Box Q test	7.93	5.76	6.18	8.18	7.69	5.62
AIC	−124.18	−124.07	−123.64	−123.39	−124.69	−126.31

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.
Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; MACB = mean age at childbearing; SRB = sex ratio at birth; TFR = total fertility rate; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Table A.12: Robustness checks controlling for male life expectancy at birth, total fertility rate, and mean age at childbearing: coefficients from regression models predicting Swedish sex ratio at birth (calculated as proportion of male births), 1862–1991

	Outcome variable: SRB, 1862–1991					
GDP per capita, t	0.0021					
	(0.0037)					
GDP per capita, t−1	−0.0033					
	(0.0035)					
GDP volume growth, t		0.0036				
		(0.0036)				
GDP volume growth, t−1		−0.0024				
		(0.0037)				
CPI, t			−0.0017			
			(0.0023)			
CPI, t−1			0.0019			
			(0.0021)			
Consumption (new), t				0.0009		
				(0.0030)		
Consumption (new), t−1				−0.0017		
				(0.0028)		
Consumption (old), t					0.0063*	
					(0.0031)	
Consumption (old), t−1					−0.0023	
					(0.0029)	
Temperature anomaly, t						0.0161
						(0.0134)
Male life expectancy	0.0020	0.0021	0.0011	0.0022	0.0014	0.0039
	(0.0080)	(0.0078)	(0.0086)	(0.0080)	(0.0080)	(0.0078)
TFR	0.0149	0.0228	0.0167	0.0265	0.0058	0.0109
	(0.1094)	(0.1079)	(0.1063)	(0.1099)	(0.1091)	(0.1058)
MACB	−0.3210*	−0.3242*	−0.3341*	−0.3132*	−0.2365	−0.3174*
	(0.1413)	(0.1425)	(0.1430)	(0.1439)	(0.1452)	(0.1388)
ARIMA(p,d,q)	(0,1,1)	(1,1,1)	(1,1,1)	(0,1,1)	(2,0,1)	(1,1,1)
Ljung–Box Q test	7.97	5.86	6.20	8.24	7.72	5.71
AIC	−123.98	−123.91	−123.52	−123.20	−124.57	−126.11

$^{\dagger}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Notes: Standard errors in parentheses. AIC = Akaike information criterion; ARIMA = autoregressive integrated moving average; CPI = consumer price index; GDP = gross domestic product; MACB = mean age at childbearing; SRB = sex ratio at birth; TFR = total fertility rate; t = no lag in time between covariates; t−1 = 1-year lag between covariates.

Chapter 2

The Effect of Maternal Education on Child Mortality in Bangladesh

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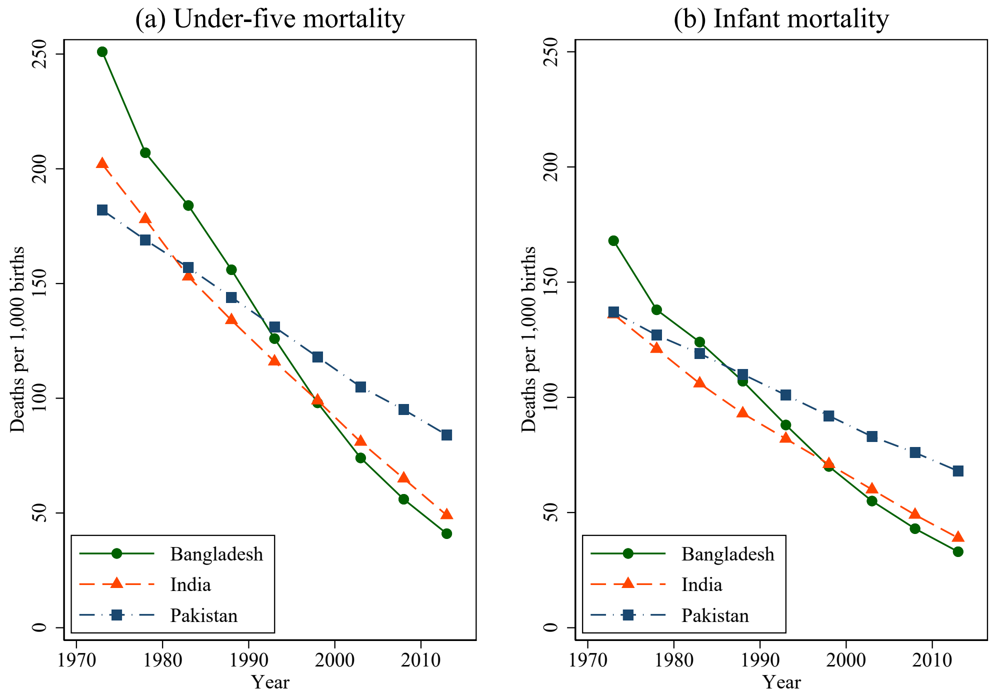
2.1 Introduction

The fourth Millennium Development Goal (MDG4) was to reduce the child mortality rate by two-thirds between 1990 and 2015 (United Nations, 2000). Despite a 53% decline in global under-five mortality during the challenge period, progress has not been made evenly across regions. The MDG4 target was in fact not achieved in most South Asian and sub-Saharan African countries where urgent actions are needed to lower child mortality (You et al., 2015). One action a country can take to reduce child mortality is to increase maternal education. Considerable literature has documented the negative relationship between maternal education and child mortality (Bicego and Boerma, 1993; Cleland and Van Ginneken, 1988; Hobcraft, 1993). A systematic analysis of census and survey data from 175 countries revealed that 51.2% of the declines in under-five mortality between 1970 and 2009 was associated with increased female educational attainment (Gakidou et al., 2010).

Since its independence from Pakistan in 1971, Bangladesh has seen dramatic improvements in child survival. Figure 2.1 depicts the trends in child mortality rates in Bangladesh as well as in other two South Asian countries, namely India and Pakistan, using the United Nations official estimates (United Nations Population Division, 2019). In 1970–1975, under-five and infant mortality rates in Bangladesh were as high as 251 and 168 deaths per 1,000 births, respectively, which were much higher than the rates in India and Pakistan. There was a marked child mortality decline in

Bangladesh in the next half-century. Both under-five and infant mortality rates in Bangladesh were halved by 1990–1995, falling below the Pakistani rates from then on and below the Indian rates from 1995–2000 onward. By 2010–2015, under-five and infant mortality rates in Bangladesh dropped respectively to 41 and 33 deaths per 1,000 births, which were below the average rates of all lower-middle-income countries (58 and 43). The impressive achievement in child survival made Bangladesh one of the 24 low-income and lower-middle-income countries that met the MDG4 target (You et al., 2015).

Figure 2.1: Trends in under-five and infant mortality rates (deaths per 1,000 births) in Bangladesh, India, and Pakistan, 1970–1975 to 2010–2015



Source: United Nations Population Division (2019).

Bangladesh has also made notable progress in increasing access to schooling through consistent government investments and policy interventions in the education sector. In 1990, the enrolment rate in Bangladesh was 56% in primary education and 28% in secondary education, and the adult literacy rate was 34%; while by 2009, these rates increased to 88%, 42%, and 55%, respectively (Sulaiman, 2012). A striking achievement in reducing the gender gap in schooling has also appeared since the mid-1990s (Asadullah and Chaudhury, 2009; Chowdhury et al., 2002).

The rapid demographic and socioeconomic changes in Bangladesh have motivated numerous investigations into the relationship between maternal education and child mortality (Abir et al., 2015; Chowdhury et al., 2017, 2020; Hossain et al., 2015; Huda et al., 2016; Maniruzzaman et al., 2018; Mohammad and Bari, 2017). These studies have mostly found a strong correlation between increased maternal education and decreased child mortality. Despite ample evidence, it is unclear whether these findings can be given a causal interpretation. Maternal education is likely to be endogenous to child mortality because of omitted-variable bias, that is, some unobserved variables, such as innate ability and family background, may confound both maternal schooling and child mortality—and therefore lead to biased estimates of regression coefficients (Desai and Alva, 1998; Hobcraft, 1993).

In 1994, the Bangladeshi government introduced a stipend project making secondary education free and providing extra stipends for girls of secondary-school age residing in rural areas. Its staggered implementation generated variations in the duration of exposure to the project for eligible girls. In this paper, we exploit such variations to identify the effect of mothers' education on their children's mortality. We also test a number of mechanisms through which maternal education affects child survival.

This study adds new empirical evidence to a growing body of literature on the causal link between maternal education and child mortality (reviewed in the next section). The existing research on this topic focused either on more developed regions, where child mortality has already declined to a relatively low level even before the start of the female educational expansion, or on sub-Saharan Africa, where child mortality still remained relatively high. In this paper, we focus on the case of Bangladesh, where child mortality fell from a very high number to a fairly low level in just 45 years. To our knowledge, this study is the first to estimate a causal impact of maternal education on child mortality in the South Asian context, where gender inequalities in education and child survival, son preference and gender discrimination, and maternal and child undernutrition have been notorious problems for decades. Given the fact that most of the South Asian countries have missed the MDG4 target, our findings can be seen as a guideline that may assist these countries in reducing child mortality, which continued being a target for the third Sustainable Development Goal (i.e., good health and well-being) succeeding the MDG4.

This study also contributes to the broader literature on the social determinants of health. In a groundbreaking paper, Caldwell (1979) analysed Nigerian data and found survivorship to be different for children born to mothers with different levels of education “in an otherwise similar socio-economic context and when there is equal access to the use of medical facilities”. His work complemented the classic literature on the drivers of mortality decline (e.g., economic development and advances in medical technology) and has made many researchers realise the importance of maternal education in child survival. Extensive research on this topic has been conducted since then, including one study done by Caldwell (1986) himself attributing the relatively low levels of mortality in places like Sri Lanka and China—where the levels of income were low at the time—to investment in education and health. Bangladesh is widely recognised as an exemplar of investing in the education sector, and our paper can be seen as an evaluation of the effectiveness of a government investment—a stipend project targeting rural girls' secondary education—with respect to the improvement of child survival.

Another vein of research focuses on the mechanisms that may explain the relationship between maternal education and child mortality (e.g., Cleland and Van Ginneken, 1988). Among the literature, a particularly influential article is an analytical framework for the study of child mortality proposed by Mosley and Chen (1984). They argued that socioeconomic factors such as maternal education tended to affect mortality indirectly through a set of proximate determinants, such as maternal factors and personal illness control. Drawing on such a framework and prior empirical studies, we examine the impact of maternal education on some proximate variables as mechanisms in this paper.

2.2 Review of Empirical Evidence

The health consequences of schooling have been subject to continuous debate, and the research agenda in recent years has moved from simply unveiling the correlation to exploring causation by taking advantage of quasi-experimental designs (Hamad et al., 2018; Mensch et al., 2019). A powerful way to uncover the causal impact of education is to use an instrumental variable (IV) approach, and conventional instruments for education rely on quasi-experiments like compulsory schooling laws (Hamad et al., 2018). Students who are eligible for these experiments are more likely to receive more schooling compared with those who are not eligible, and the eligibility often depends on plausibly exogenous factors. Our analysis builds on an IV estimation, so we start by reviewing empirical research employing similar IV strategies to look into the effect of parents' education on their children's mortality.

Two studies have tackled this question in developed regions. Chou et al. (2010) relied on the extension of compulsory education in Taiwan in 1968 as a natural experiment and found that increased parental schooling was significantly and causally associated with lower probability of neonatal, post-neonatal, and infant death. McCrary and Royer (2011) employed age-at-school-entry policies—which caused some women born one day before the school-entry cutoff date to attain more schooling than comparable women born one day later—to identify the effect of maternal education on infant death in the State of Texas. They found, however, no evidence for the causal link.

Research on developing countries was pioneered by Breierova and Duflo (2004) who made use of a massive school construction project in Indonesia during the 1970s to evaluate the effect of parental education on child mortality. They formulated their instrument based on the interactions between parents' birth cohorts with district-specific programme intensity measured by the number of newly constructed primary schools relative to the number of school-age children. They found that increased parental schooling significantly reduced child mortality.

A number of investigations into other parts of the developing world have been conducted, but some did not find supporting evidence. Dinçer et al. (2014) used exposure to the 1997 Education Law in Turkey that expanded compulsory schooling from five to eight years as an instrument. They reported a significant increase in exposed women's probability of acquiring at least eight years of education, but a weak impact of maternal education on neonatal, post-neonatal, infant, and under-five mortality. Zhang (2012) exploited exogenous variations in women's exposure to massive high school closures in rural China immediately after the end of the Cultural Revolution to examine the effect of maternal education on child health. Her results showed a 22.5% decline in high school completion for exposed women, but no effect of mothers' high school education on neonatal and infant mortality.

Several studies have examined this causal relationship in sub-Saharan Africa where child mortality remains high. Grépin and Bharadwaj (2015) used as an instrument age-specific exposure to the 1980 education reform in Zimbabwe which increased women's education at the secondary level. They found that an additional year of mothers' secondary schooling reduced child mortality by 21%. Makate and Makate (2016) found that the 1994 universal primary education policy in Malawi was associated with 34% and 36% reductions in infant and under-five mortality, respectively, but was uncorrelated with neonatal survival.

Two more studies both utilised the 1997 universal primary education programme in Uganda as a natural experiment but reached different conclusions. Based on a sample of first-born children, Keats (2018) found that increased maternal education led to better health of surviving children but had no impact on infant survival. With a similar IV strategy, Makate (2016) did not limit his study to the first-born sample and showed a maternal-schooling-led reduction in infant but not under-five mortality.

In a recent study, Andriano and Monden (2019) reconsidered the cases of Malawian and Ugandan universal primary education reforms. They estimated their second stage using survival models to account for right censoring. Their results indicated that an additional year of mothers' education reduced children's probability of dying before age five by 10% in Malawi and by 16.6% in Uganda. For both countries, the IV estimates were statistically significant in survival but not linear models.

2.3 The Female Secondary School Stipend Project in Bangladesh

The education system in Bangladesh involves five years of primary education (grades 1–5, ages 6–10), five years of secondary education (grades 6–10, ages 11–15), two years of higher secondary education (grades 11–12, ages 16–17), and tertiary education. Thanks to various incentive programmes targeting primary education (Behrman, 2015),[1] the enrolment rate in primary schools has reached a fairly high level. Unlike primary schools which are predominantly public, secondary schools in Bangladesh are largely nongovernmental or private and require students to pay tuition and other school fees (Hahn et al., 2018a), and the enrolment rate is therefore much lower in secondary schools. For example, the enrolment rate in primary schools was 75% for girls and 85% for boys in 1991, while the enrolment rate in secondary schools in the same year was only 14% for girls and 25% for boys (World Bank, 2003). In addition, gender and rural–urban gaps in access to secondary education remain wide in Bangladesh. The dropout rate in secondary education was more than 60% in the early 1990s (World Bank, 2002), and only 5% of rural girls were able to complete secondary education in 1991, compared to 12% of boys (Khandker and Samad, 1995).

[1]Primary education in Bangladesh was made free and compulsory in 1990; an extra Food for Education Programme was introduced in 1993 to encourage primary school attendance for vulnerable children by providing food to the children's households conditional on school attendance. the Food for Education Programme was replaced by the Primary Education Stipend Programme in 2002, providing cash stipends to households whose children have achieved school attendance of at least 85%.

Aiming to reduce gender and rural–urban inequalities in secondary education, the government of Bangladesh launched the Female Secondary School Stipend Project (FSSSP) in 1994. The project made tuition fees free for girls of secondary-school age residing in rural areas. A supplementary stipend was also paid to girls who maintained a minimum of 75% attendance in secondary school, achieved at least 45% test scores in the annual exams, and remained unmarried.

Since the introduction of the FSSSP, there has been a significant increase in female secondary education enrolment. In 2001, the secondary-school enrolment rate for girls reached 51.3%, higher than that for boys in the same year (47.5%; see Hahn et al., 2018b). The number of girls enrolled in secondary schools has overtaken the number of boys: the male-to-female secondary-school enrolment ratio was 66 to 34 in 1990, while in 2002 the ratio was 46 to 54 (Bangladesh Bureau of Educational Information and Statistics, 2013). The secondary-school completion rate also increased dramatically for girls from 36% in 1994 to 59.2% in 1998 (Khandker et al., 2003). The improvement in female secondary schooling was driven primarily by rural girls: the secondary-school attendance rate in rural Bangladesh increased from 49.8% in 1991 to 66.3% in 2001, whereas the change in urban areas over the same period was much smaller, only from 61.4% to 64.1% (Bangladesh Bureau of Statistics, 2012b).

In 1994, only girls enrolled in grades 6 and 9 were eligible for the stipends, while in 1995 girls enrolled in all grades except grade 8 were entitled to receive the stipends, and from 1996 onward girls enrolled in all secondary education grades (grades 6–10) qualified for the stipends (Khandker et al., 2003). The staggered introduction of the FSSSP generates variations in the duration of exposure to the project for eligible girls: rural girls born in 1980–1982 (aged 12–14 and enrolled in grades 7–9 in 1994) received two years of stipends, while those born in or after 1983 (aged 11 or younger and enrolled in grades 6 or lower) received five years of stipends, and those born in or before 1979 (aged 15 or older in 1994) received no stipends as they were enrolled in grade 10 or had already exceeded secondary-school ages in 1994 (Hahn et al., 2018b). Urban girls are not eligible for the stipends as the FSSSP only covers rural areas.[2]

[2]A few urban areas have also been covered by the FSSSP, but we do not know exactly what these areas are and thus have to treat all urban areas as being excluded from the project. We discuss later why we believe doing so would have only a limited impact on our findings.

2.4 Data and Methods

2.4.1 Data, Sample, and Measures

Pooled data from the 2007, 2011, and 2014 Bangladesh Demographic and Health Surveys (BDHS) are used in this paper (ICF, 2007–2014). The BDHS are nationally representative surveys that collect information on demographic and socioeconomic characteristics as well as health outcomes and related knowledge from ever-married women aged 15–49 in Bangladesh. The BDHS also provides information on women's husbands and children. The pooled sample consists of 41,953 women who had given birth to 120,143 children.

We limit our sample to mothers born between 1971 and 1988, that is, women aged from 5 or 6 to 23 in 1994 when the FSSSP was introduced (those aged from 18 or 19 to 36 in the 2007 survey, aged from 22 or 23 to 40 in the 2011 survey, and aged from 25 or 26 to 43 in the 2014 survey). This results in a sample of 25,816 women with 71,141 children. By restricting the sample to those who were at least 5 or 6 years old in 1994, we ensure that women in our analytical sample were at least 18 or 19 years old when the surveys were conducted. This is important because we want to consider only those who had completed secondary school and had given birth to their children by the time of the survey. We also set the upper age limit to 23 years old in 1994 because we are interested in those who just missed out on the stipend project. After excluding 41 women whose information on key variables is missing and 335 children whose birth or death information is incomplete or defective, our final sample contains 25,783 women with 70,765 children (6,165 women with 15,094 children in the 2007 survey, 10,032 women with 27,624 children in the 2011 survey, and 9,586 women with 28,047 children in the 2014 survey).

The BDHS also collects women's birth histories, providing basic information on all children born to women in the sample, such as sex, birth order, the date of birth of each child, and whether each child was still alive at the time of the survey. If the child died prior to the date of the survey, the age at death in months was also recorded. Using the BDHS birth history data, we construct two conventional measures of child mortality as outcome variables. The first is a dichotomous indicator of under-five mortality, taking the value 1 if the child died before reaching age 5 (i.e., died between 0 and 59 months) and 0 otherwise. The second is an indicator of infant mortality, taking the value 1 if the child died before reaching age 1 (i.e., died between 0 and 11 months). All child deaths are self-reported and hence subject to recall bias. Nevertheless, child death is a rare and vital event to mothers and families, and thus we expect that women would accurately recall and report the occurrence and age of child death.

2.4.2 Identification Strategy

To identify the impact of maternal education on child mortality, we follow an IV approach developed by Hahn et al. (2018b) who used women's eligibility for the FSSSP as an instrument for their years of education, and the eligibility depends on women's place of residence and birth cohorts. Since the FSSSP targets the rural population, women living in rural Bangladesh are regarded as the treatment group and women living in urban areas are regarded as the control group. Due to the staggered introduction of the FSSSP, the duration of exposure to the project varies between women in different birth cohorts. We regard women in the 1983–1988 cohorts as being fully exposed to the FSSSP since they were able to receive five years of stipends that covered the entire secondary-school period if they lived in rural areas. Women in the 1980–1982 cohorts were eligible for only two years of stipends if they lived in rural areas and are thus regarded as being partially exposed to the FSSSP. Women in the 1971–1977 cohorts had already passed secondary-school ages in 1994 so that they were not exposed to the stipend project even if they lived in rural areas.

We expect the FSSSP to significantly improve the educational attainment of rural women who were fully exposed to the project. Partially exposed women living in rural areas also benefited from the FSSSP but to a lesser extent. We use two-stage least squares (2SLS) to compute IV estimates of the effect of maternal education on child mortality. The first stage is a difference-in-differences (DID) equation:

$$\begin{aligned} Edu_i = \gamma_0 + \gamma_1 Rural_i \times Full_i + \gamma_2 Rural_i \times Part_i \\ + \gamma_3 Rural_i + \gamma_4 Full_i + \gamma_5 Part_i + \mathbf{X}_i'\theta + \nu_i, \end{aligned} \tag{2.1}$$

where Edu_i is the child i's mother's years of schooling, which is endogenous to child mortality, $Rural_i$ is a dummy indicating the mother was residing in rural areas at the time of the survey, $Full_i$ denotes the mother who was fully exposed to the FSSSP (1983–1988 cohorts), and $Part_i$ denotes the mother who was partially exposed (1980–1982 cohorts); the unexposed cohorts (1971–1979 cohorts) are the reference category. Accordingly, the instrument for maternal education is based on two interaction terms: $Rural_i \times Full_i$, indicating the mother was eligible for five years of stipends (full treatment), and $Rural_i \times Part_i$, indicating the mother was eligible for two years of stipends (partial treatment). If the FSSSP was effective, the instrument's coefficients γ_1 and γ_2 would both have positive signs and be statistically significant, and γ_1 would be larger than γ_2. $\mathbf{X}_i$ is a vector of controls, including the child i's sex, birth order (first child vs. higher order), and birth year fixed effects, the child i's mother's age at survey fixed effects and religion (Islam vs. others), as well as survey year (2007, 2011, and 2014) and division of residence (Barisal, Chittagong, Dhaka, Khulna, Rajshahi, Rangpur, and Sylhet) fixed effects.

After the endogenous maternal education is instrumented by exposure to the FSSSP, we obtain the predicted values of years of maternal education, $\widehat{Edu}_i$, from the first stage and use it as the key explanatory variable in the second stage:

$$Mort_i = \beta_0 + \beta_1 \widehat{Edu}_i + \beta_2 Rural_i + \beta_3 Full_i + \beta_4 Part_i + \mathbf{X}_i'\theta + \varepsilon_i, \quad (2.2)$$

where $Mort_i$ is an indicator of whether child i died before reaching age 5 (under-five mortality) or reaching the first birthday (infant mortality), and all other variables are the same as those in Equation 2.1. The coefficient of interest, β_1, quantifies the impact of a one-year increase in maternal education on child mortality.

We also use the Cox proportional hazards model in an IV framework to account for right censoring and time-to-event structure of mortality data. Instead of replacing the endogenous variable with the first-stage predicted values in the second stage (as done in 2SLS estimation), we use a control function approach (also known as two-stage residual inclusion) in which the first-stage residuals are included as an additional regressor alongside the endogenous variable. The control function approach is an extension of the linear 2SLS estimator to the nonlinear case like the Cox proportional hazards model used in this paper and yields consistent estimates (Terza et al., 2008). The alternative second-stage equation is written as:

$$h(t) = h_0(t)\exp(\delta_1 Edu_i + \delta_2 \nu_i + \delta_3 Rural_i + \delta_4 Full_i + \delta_5 Part_t + \mathbf{X}_i'\theta), \quad (2.3)$$

where $h(t)$ refers to the hazard of death between 0 and 59 months (under-five mortality) or between 0 and 11 months (infant mortality), $h_0(t)$ is the baseline hazard, and t is the survival time in person-months censored at the child i's age at the time of the survey or 59 months/11 months, whichever comes first. The term ν_i represents the first-stage residuals, Edu_i is the endogenous maternal education, and the exponential of its coefficient, $\exp(\delta_1)$, is the hazard ratio caused by a one-year increase in maternal schooling. Accordingly, $\exp(\delta_1) - 1$ measures the impact an additional year of mother's education would have on her children's mortality risk. All other variables are the same as those in Equations 2.1 and 2.2.

In all regression models, standard errors are clustered at the level of treatment (i.e., rural dummy $\times$ mothers' birth cohorts dummies).

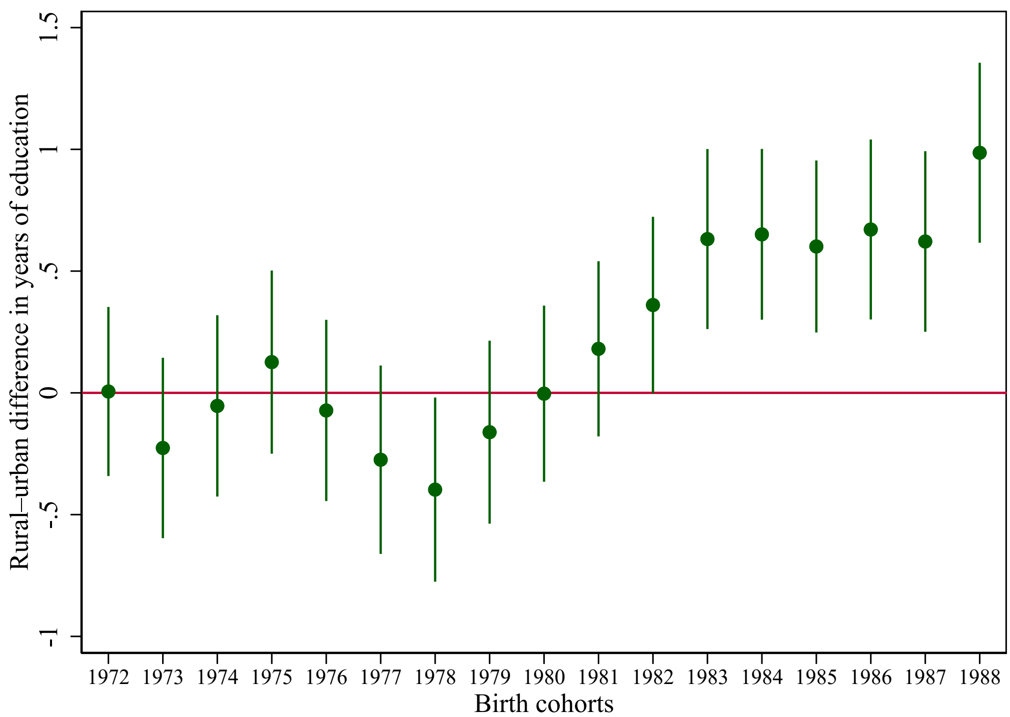
2.4.3 Threats to Identification

Rural women are eligible for the FSSSP, so the variable $Rural_i$ should ideally capture where the child i's mother lived in her school age. Unfortunately, the BDHS does not provide information on past migration and residence histories so we have to use whether the woman was living in rural or urban areas at the time of the survey as a proxy for her place of residence in her school age. This proxy seems to be valid given the low internal migration rate in Bangladesh: according to the 2011 census (Bangladesh Bureau of Statistics, 2012a), the rural-to-urban migration rate was 4.29% and the urban-to-rural rate was 0.36%. Furthermore, a small number of urban areas were indeed covered by the FSSSP (Schurmann, 2009; Shamsuddin, 2015), although the names of these areas have not been disclosed. The number of such urban areas is negligible compared to the number of rural areas (Hahn et al., 2018a), so we would expect that ignoring them would have a limited impact on our identification. Nevertheless, our first-stage results are likely to capture the lower bound of the effect of the project.

Grade repetition is another concern. If rural girls repeated grades of secondary school in order to receive the cash stipends, the exposure variables $Full_i$ and $Part_i$ would be subject to measurement errors. Grade repetition in Bangladesh is not frequent, and only less than 2% of all students across various birth cohorts repeated grades in secondary school (Shamsuddin, 2015). Besides, the opportunity cost of grade repetition is high compared to the cash stipends received, so we could expect only a small proportion of girls to remain extra years in secondary school in order to receive the stipends.

Our first stage is based on a DID specification, so it is important to assess the parallel trend assumption, that is, the difference between years of education among the treatment group (rural girls) and the control group (urban girls) would have been constant in the absence of the stipend project. Although it is not possible to firmly test the assumption, we follow Hahn et al. (2018a,b) and regress women's years of education on a full set of interactions between rural dummy and women' birth cohorts dummies, and the coefficients of these interaction terms are shown in Figure 2.2, reflecting the changes in the rural–urban gap in women's years of education across birth cohorts. For the reference cohort 1971, rural women on average received 1.90 fewer years of education than their urban counterparts. In unexposed cohorts (1972–1979), the changes in the rural–urban difference in education held largely constant as the 95% confidence intervals intersect zero for all but one cohort, suggesting that before the introduction of the FSSSP, the rural–urban gap in education did not change much. In partially exposed cohorts (1980–1982), the coefficients begin to cross and go beyond zero, despite the inclusion of zero within the 95% confidence intervals. Starting from the 1983 cohort (fully exposed), the rural–urban gap in education was significantly narrowing for those who were young enough in 1994 to be covered by the FSSSP. As a result, we would argue that the trends in years of education of rural and urban girls would have been parallel in the absence of the FSSSP, providing evidence in support of the parallel trend assumption.

Figure 2.2: Changes in the rural–urban gap in maternal education (in years) across mothers' birth cohorts



Notes: The figure shows the point estimates and 95% confidence intervals of the coefficients from an OLS model regressing women's years of education on a full set of interactions between rural dummy and women's birth cohorts dummies.
Sources: ICF, Bangladesh Demographic and Health Surveys 2007, 2011, and 2014.

The instrument also needs to satisfy the exclusion restriction that the FSSSP affects child mortality only through increasing maternal education. Although it is not directly testable, we believe that using eligibility for the FSSSP satisfies the exclusion restriction since the purpose of the project was to directly promote access to secondary education for girls from poor and rural families and was not to reduce child mortality or something else that might have had an impact on child mortality;

and the bulk of the FSSSP's funds was spent on affairs directly related to female education in rural Bangladesh (World Bank, 2002, 2003).

2.5 Results

2.5.1 Descriptive Statistics

Table 2.1 shows the sample statistics by exposure status. The mean years of education for all women in the sample is 4.11 years, almost one year below the normal duration required for completion of primary school. Women who were fully exposed to the FSSSP had an average of 5.21 years of schooling, that is, they received, on average, at least some secondary education, while women who were not exposed to the FSSSP received 1.78 years less education than fully exposed women and almost one year less than partially exposed women. The vast majority of participants were Muslims. The percentage of women being treated (those living in rural areas) were between 68% and 69% across cohorts with different exposure status.

From the child sample, we see that 7.94% of children died during the first five years of life, and 6.52% died in infancy. Under-five mortality risk of children born to fully exposed mothers was 2.56 percentage points less than those born to mothers who were not exposed to the FSSSP, and infant mortality risk was 1.65 percentage points lower.

2.5.2 First-Stage Results

Results from the first stage examining whether the FSSSP raised female education are presented in Table 2.2. We start with the baseline model without any controls (column a). For women living in rural Bangladesh, exposure to the FSSSP was associated with an increase of 0.77 years of schooling for those who were eligible for five years of stipends. The increase for women who were eligible for two years of stipends is smaller (0.27 years) and becomes significant once mother's characteristics are controlled for in column b. In column c, we include survey year and division fixed effects. There are no notable changes to the coefficients of the instrument, while the exposure variables become non-significant. This is reasonable because the exposure variables are constructed based on women's birth cohorts, which have already been taken into account when controlling for survey years and women's ages at survey (cohort = period − age).

Table 2.1: Sample statistics

	Total		Full exposure		Partial exposure		No exposure	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Mother sample								
Years of education	4.11	3.99	5.21	3.71	4.37	4.00	3.43	3.99
Rural	0.68	0.47	0.69	0.46	0.68	0.47	0.68	0.47
Islam	0.91	0.29	0.91	0.28	0.91	0.29	0.91	0.29
Age at survey time	32.05	5.57	26.09	2.99	30.05	2.92	35.89	3.89
Observations (mothers)	25,783		9,757		4,626		11,400	
Child sample								
Under-5 mortality	0.0794	0.270	0.0650	0.247	0.0690	0.253	0.0906	0.287
Infant mortality	0.0652	0.247	0.0558	0.230	0.0585	0.235	0.0723	0.259
Female	0.49	0.50	0.49	0.50	0.49	0.50	0.49	0.50
First child	0.36	0.48	0.48	0.50	0.37	0.48	0.30	0.46
Birth year	2000.81	5.99	2005.40	3.72	2002.38	4.33	1997.85	5.68
Observations (children)	70,765		20,325		12,392		38,048	

Notes: Full exposure = mothers born in 1983–1988; partial exposure = mothers born in 1980–1982; no exposure = mothers born in 1971–1979. SD = standard deviation.
Sources: ICF, Bangladesh Demographic and Health Surveys 2007, 2011, and 2014.

Table 2.2: Effect of exposure to the stipend project on maternal education (in years), OLS estimates from the first stage

	Outcome variable: Years of education			
	(a)	(b)	(c)	(d)
Rural × full exposure	0.77**	0.77***	0.79***	0.80***
	(0.21)	(0.14)	(0.09)	(0.08)
Rural × partial exposure	0.27	0.27**	0.25***	0.27***
	(0.18)	(0.07)	(0.05)	(0.05)
Rural	−1.99***	−1.99***	−1.99***	−1.82***
	(0.14)	(0.06)	(0.04)	(0.05)
Full exposure	1.25***	0.89***	0.11	0.08
	(0.00)	(0.12)	(0.17)	(0.17)
Partial exposure	0.75***	0.45***	0.04	0.07
	(0.00)	(0.07)	(0.09)	(0.08)
Constant	4.79***	5.79***	7.28***	1.96*
	(0.00)	(0.13)	(0.19)	(0.90)
Mother's characteristics	No	Yes	Yes	Yes
Survey year and division fixed effects	No	No	Yes	Yes
Child's characteristics	No	No	No	Yes
R^2	0.080	0.088	0.103	0.169
Observations (mothers)	25,783	25,783	25,783	25,783
Observations (children)	70,765	70,765	70,765	70,765

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
Notes: Standard errors, reported in parentheses, are clustered at the treatment level (rural dummy × mothers' birth cohorts dummies). Full exposure = mothers born in 1983–1988; partial exposure = mothers born in 1980–1982. Mother's characteristics include age at survey fixed effects and religion (Islam vs. others). Child's characteristics include sex, birth order (first child vs. higher order), and birth year fixed effects.
Sources: ICF, Bangladesh Demographic and Health Surveys 2007, 2011, and 2014.

Since the main analysis is at the child level, we also incorporate child's characteristics in our preferred first-stage specification (column d). The rural–urban gap in years of schooling is 1.82 years and has been significantly narrowed by the FSSSP. Full exposure to the project would improve rural girls' education by 0.80 years, nearly one-fifth of the average years of education of women in the sample. Even partial exposure to the FSSSP would improve rural women's education by 0.27 years.

2.5.3 Second-Stage Results

We report the 2SLS estimates of the effect of maternal education on under-five and infant mortality in Table 2.3. We also present the ordinary least squares (OLS) estimates for comparison. All regressions control for the same covariates in Table 2.2 column d, except the instrument.

Table 2.3: Effect of maternal education (in years) on under-five (0–59 months) and infant mortality (0–11 months), OLS and 2SLS estimates

	Outcome variable:			
	Under-five mortality		Infant mortality	
	(a) OLS	(b) 2SLS	(c) OLS	(d) 2SLS
Years of education	−0.0048***	−0.0171***	−0.0037***	−0.0145***
	(0.0003)	(0.0035)	(0.0003)	(0.0033)
Rural	0.0064***	−0.0126*	0.0049***	−0.0117*
	(0.0011)	(0.0056)	(0.0011)	(0.0050)
Full exposure	−0.0139**	−0.0060	−0.0134*	−0.0066
	(0.0045)	(0.0052)	(0.0047)	(0.0053)
Partial exposure	−0.0117***	−0.0085**	−0.0084**	−0.0055*
	(0.0023)	(0.0024)	(0.0021)	(0.0023)
Constant	1.1007***	1.1256***	1.0982***	1.1199***
	(0.0124)	(0.0215)	(0.0112)	(0.0189)
First-stage F statistic		57.39		57.39
Hansen J statistic		1.39		0.08
[p-value]		[0.2384]		[0.7788]
Observations (mothers)	25,783	25,783	25,783	25,783
Observations (children)	70,765	70,765	70,765	70,765

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
Notes: Standard errors, reported in parentheses, are clustered at the treatment level (rural dummy × mothers' birth cohorts dummies). Full exposure = mothers born in 1983–1988; partial exposure = mothers born in 1980–1982. All models control for survey year and division fixed effects, mother's age at survey fixed effects and religion (Islam vs. others), and child's sex, birth order (first child vs. higher order), and birth year fixed effects.
Sources: ICF, Bangladesh Demographic and Health Surveys 2007, 2011, and 2014.

The OLS estimates show a statistically significant and negative relationship between maternal education and child mortality. A one-year increase in maternal schooling was associated with a 0.48 percentage point lower under-five mortality (column a) and a 0.37 percentage point lower infant mortality (column c).

Once endogeneity is addressed using an IV approach, the 2SLS estimates manifest a stronger and still statistically significant effect of maternal education on child mortality. A one-year increase in maternal education led to a decline of 1.71 percentage points or 21.54% in under-five mortality (column b) and a decline of 1.45 percentage points or 22.24% in infant mortality (column d). The first-stage F statistic is as large as 57.39, indicating that our instrument is not weak. The Hansen J test statistics in both under-five and infant mortality models correspond to large p-values, suggesting a lack of evidence for rejecting the null hypothesis that the over-identification restrictions are valid. In other words, our models are unlikely to be over-identified when using two instrumental variables in the first stage, speaking in favour of the validity of our instrument.

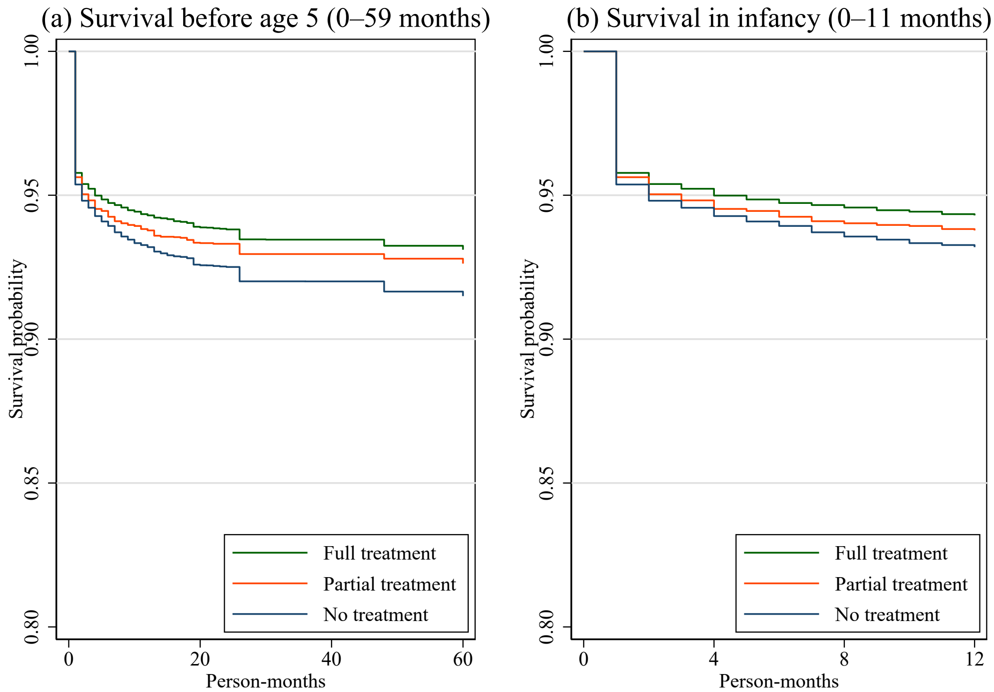
2.5.4 Survival Analysis Results

Figure 2.3 illustrates the Kaplan–Meier estimates of time to death in person-months for children born to mothers with different treatment status. Children of treated mothers had a higher probability of survival compared with children of untreated mothers. The log-rank test statistics indicate that the survival gap between children whose mothers were treated differentially by the FSSSP is statistically significant ($\chi^2 = 40.98$ and 22.78 for under-five and infant mortality, respectively, both $p < 0.001$).

Results from Cox models are presented in Table 2.4. The naïve Cox models show that a one-year increase in maternal schooling was associated with a 6.94% lower under-five mortality risk (column a) and a 6.33% lower infant mortality risk (column c).

Once again when endogeneity is addressed using a control function, the effect on child mortality becomes even larger in magnitude compared with the naïve estimates. For each additional year of maternal education, children had an 18.51% lower probability of dying during the first five years of life (column b) and a 19.95% lower probability of dying in infancy (column d). The coefficient of the first-stage residuals has a positive sign (i.e., hazard ratio > 1), suggesting that the unobserved factors are likely to be positively correlated with both maternal education and child mortality, and the statistical significance of the first-stage residuals implies that the endogeneity problem is likely to arise in the naïve Cox models.

Figure 2.3: Kaplan–Meier estimates of under-five (0–59 months) and infant mortality (0–11 months) by treatment status



Notes: Full treatment = rural mothers born in 1983–1988; partial treatment = rural mothers born in 1980–1982; no treatment = rural mothers born in 1971–1977 and all urban mothers.
Sources: ICF, Bangladesh Demographic and Health Surveys 2007, 2011, and 2014.

2.5.5 Robustness Checks

We conduct some robustness checks and the results are presented in Table 2.5. First, we have demonstrated that internal migration between urban and rural areas may blur the lines of identification. In panel a, we exclude those living in the Dhaka division where the capital of Bangladesh is located and the urbanisation rate is faster than the rest of the country, probably as a result of rural-to-urban migration. Another concern is that some young women in our sample may not have finished their education yet, so the years of education measured at the time of the survey may not reflect their ultimate educational attainment. In panel b, we exclude those surveyed in 2007 during which time the youngest women in the survey might still be in school if they had access to tertiary education. Our analytical sample consists of mothers born between 1971 and 1988, corresponding to those aged 18 to 43 at the time of the

Table 2.4: Effect of maternal education (in years) on under-five (0–59 months) and infant mortality (0–11 months), hazard ratios from Cox proportional hazards models

	Outcome variable:			
	Under-five mortality		Infant mortality	
	(a)	(b)	(c)	(d)
	Cox PH	Cox IV	Cox PH	Cox IV
Years of education (in years)	0.9306***	0.8149***	0.9367***	0.8005***
	(0.0080)	(0.0379)	(0.0077)	(0.0397)
First-stage residuals		1.1421**		1.1702**
		(0.0549)		(0.0598)
Rural	1.0999***	0.8915	1.0903***	0.8520*
	(0.0179)	(0.0686)	(0.0203)	(0.0662)
Full exposure	0.8725*	0.9527	0.8597	0.9534
	(0.0576)	(0.0702)	(0.0740)	(0.0904)
Partial exposure	0.8645***	0.8967	0.8906**	0.9299
	(0.0257)	(0.0281)	(0.0313)	(0.0376)
Observations (mothers)	25,783	25,783	25,783	25,783
Observations (children)	70,765	70,765	70,765	70,765

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
Notes: Cox PH = Cox proportional hazards models; Cox IV = Cox proportional hazards models in an instrumental variable framework using control function approach. Standard errors, reported in parentheses, are clustered at the treatment level (rural dummy × mothers' birth cohorts dummies). Full exposure = mothers born in 1983–1988; partial exposure = mothers born in 1980–1982. All models control for survey year and division fixed effects, mother's age at survey fixed effects and religion (Islam vs. others), and child's sex, birth order (first child vs. higher order), and birth year fixed effects.
Sources: ICF, Bangladesh Demographic and Health Surveys 2007, 2011, and 2014.

survey; however, this age interval may be too wide to make a reasonable comparison. Thus, in panel c, we narrow the birth cohorts of women to 1973–1986. Comparing the 2SLS estimates reported in Table 2.3, we see that results from these robustness checks do not change much, indicating that our findings are robust to alternative model specifications.

In panels d and e, we estimate the impacts of maternal education on boys' and girls' mortality, respectively. Results indicate some degree of heterogeneous effect by children's sex, while the differences between the effects on girls' and boys' mortality are not substantial, agreeing with a previous study that did not find supporting evidence of a gender-differentiated relationship between maternal education and child mortality using demographic surveillance data from Matlab, Bangladesh (Muhuri and

Table 2.5: Robustness checks, 2SLS estimates from alternative model specifications

	Outcome variable:	
	Under-five mortality	Infant mortality
(a) Excluding Dhaka division		
Years of education	−0.0184***	−0.0173***
	(0.0044)	(0.0044)
First-stage F statistic	46.20	46.20
Observations (mothers)	21,088	21,088
Observations (children)	58,208	58,208
(b) Excluding 2007 survey		
Years of education	−0.0158***	−0.0127**
	(0.0041)	(0.0038)
First-stage F statistic	77.27	77.27
Observations (mothers)	19,618	19,618
Observations (children)	55,671	55,671
(c) Narrowed cohorts of mothers: 1973–1986		
Years of education	−0.0161***	−0.0117***
	(0.0030)	(0.0026)
First-stage F statistic	44.59	44.59
Observations (mothers)	20,103	20,103
Observations (children)	55,614	55,614
(d) Male children sample		
Years of education	−0.0160*	−0.0173**
	(0.0065)	(0.0058)
First-stage F statistic	34.83	34.83
Observations (mothers)	21,090	21,090
Observations (children)	36,239	36,239
(e) Female children sample		
Years of education	−0.0183***	−0.0123**
	(0.0041)	(0.0035)
First-stage F statistic	60.38	60.38
Observations (mothers)	20,000	20,000
Observations (children)	34,526	34,526

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

Notes: Standard errors, reported in parentheses, are clustered at the treatment level (rural dummy × mothers' birth cohorts dummies). All models control for rural dummy, full exposure dummy, partial exposure dummy, survey year and division fixed effects, mother's age at survey fixed effects and religion (Islam vs. others), and child's sex, birth order (first child vs. higher order), and birth year fixed effects, except in panels d and e where the sex of the child is excluded.

Sources: ICF, Bangladesh Demographic and Health Surveys 2007, 2011, and 2014.

Preston, 1991).

We conduct two sets of additional robustness checks in Appendix B. In Appendix B.1, we further deal with the issue of internal migration. The 2007 BDHS contains information on the number of years the respondent has lived in the current place, and in case she had moved, the type of place of previous residence (rural or urban). Using this information, we identify 2,386 children born to 927 mothers who have been living in the same place since before reaching secondary-school age (i.e., age 11) and 8,417 children born to 3,349 mothers who have moved but whose type of place of residence remained the same (i.e., they moved from rural to rural areas or from urban to urban areas), and thus the variable $Rural_i$ would likely represent the place where child i's mother lived during her secondary-school age. We replicate the analysis based on this subsample, and the results are shown in Table B.1. The 2SLS coefficients are statistically significant and have negative signs and are just slightly larger in magnitude than those estimated in the main analysis, indicating that our results are robust to subsample analysis taking account of internal migration.

Because the FSSSP specifically targets secondary education, in Appendix B.2, we use two dummy variables—whether a woman completed her secondary education (results shown in Table B.2) and whether a woman received some secondary education (results shown in Table B.3)—as alternative independent variables. The results are in line with those of the main analysis. Both completing and receiving secondary education significantly reduce under-five and child mortality, and the coefficients are much larger in magnitude than the coefficients when using years of maternal education as the independent variable. However, the 2SLS estimates presented in Tables B.2 and B.3 should be interpreted with caution as the small first-stage F statistics indicate that the instrument may be weak.

2.6 Potential Mechanisms

We test a number of pathways through which maternal education may exert its effect on child mortality. To do so, we identify a list of proximate variables representing possible mechanisms from the BDHS individual sample (i.e., women's sample). We then use the same IV approach as we do in the main analysis, but this time at the mother level, regressing these proximate variables on the predicted values of women's years of education from the first stage. The second-stage results are shown in Table 2.6.

Table 2.6: Analysis of potential mechanisms, 2SLS estimates

Outcome variable:	Coefficient of years of education	First-stage F statistic	Mean	Observations (women)
(a) Socioeconomic factors				
Poor household	−0.0023 (0.0051)	108.24	0.37	25,798
Rich household	0.0262*** (0.0038)	108.24	0.44	25,798
Woman is literate	0.0915*** (0.0045)	109.07	0.53	25,781
Woman is working	0.0095 (0.0069)	108.40	0.26	25,793
Woman in agricultural/domestic work	−0.0133 (0.0073)	98.77	0.45	7,357
(b) Assortative mating				
Husband's years of education	0.8290*** (0.0359)	107.29	5.25	25,777
Husband is skilled worker	0.0491*** (0.0067)	121.09	0.31	25,030
(c) Fertility and marriage				
Number of children ever born	−0.2541*** (0.0192)	108.24	2.76	25,798
Ideal number of children	−0.0519*** (0.0070)	119.97	2.23	25,461
Age at first birth	0.3713*** (0.0400)	108.24	17.98	25,798
Age at first marriage	0.4142*** (0.0216)	108.24	15.74	25,798
Age at sexual debut	0.3764*** (0.0235)	111.77	15.80	19,193
(d) Health-related knowledge				
Heard of AIDS	0.0763*** (0.0044)	108.41	0.72	25,797
AIDS knowledge index: protection	0.0443*** (0.0082)	122.82	0.00	18,411
AIDS knowledge index: transmission	0.0139* (0.0058)	122.82	0.00	18,411
(e) Health-related knowledge				
Prenatal care	0.0282*** (0.0067)	98.60	0.51	10,731
Delivery care	0.0554*** (0.0054)	99.19	0.31	10,739
Number of antenatal visits	0.3406*** (0.0384)	96.65	2.41	10,725
Delivery in a hospital/clinic	0.0416*** (0.0050)	98.89	0.27	10,740
(f) Female empowerment				
Empowerment index	0.0199* (0.0083)	133.71	0.00	24,800

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

Notes: Standard errors, reported in parentheses, are clustered at the treatment level (rural dummy × women' birth cohorts dummies). All models control for rural dummy, full exposure dummy, partial exposure dummy, survey year and division fixed effects, and women's age at survey fixed effects and religion (Islam vs. others).

Sources: ICF, Bangladesh Demographic and Health Surveys 2007, 2011, and 2014.

We first examine the effects of women's education on proximate variables relating to socioeconomic status (panel a). Research has shown that more-educated women are wealthier and can provide better nutrients for their children (Frost et al., 2005). Following Grépin and Bharadwaj (2015), we use the household wealth index from the BDHS to construct two indicators: "poor household" taking the value 1 if the woman's household is labelled "poorest" or "poorer", and "rich household" taking the value 1 if the woman's household is labelled "richer" or "richest". We find that better education significantly increased women's probability of living in a rich household, while the effect on the probability of living in a poor household is not statistically significant. Moreover, Smith-Greenaway (2013) showed a strong correlation between mother's reading skills and child mortality in Nigeria, but many women were still illiterate even with several years of formal schooling. In this study, we find that an extra year of education increased maternal literacy in Bangladesh by 9.15 percentage points, highlighting the important role of literacy in explaining the relationship between maternal education and child mortality. We do not find statistically significant evidence that better-educated women were more likely to work. Among women who were working, an additional year of education decreased the probability of engaging in agricultural or domestic work by 1.33 percentage points, but the coefficient is not statistically significant ($p = 0.068$).

Although education did not raise Bangladeshi women's probability of working, the economic and sociological literature has suggested that women's education is positively associated with their husbands' education and employment through assortative mating (Behrman and Rosenzweig, 2002; Mare, 1991). In panel b, we show that a one-year increase in a woman's education increased her husband's education by 0.83 years, and his probability of being a skilled worker (e.g., doctor, lawyer, teacher, accountant, and trader) by 4.91 percentage points. Compared to those who are less educated and working in agricultural and informal sectors, men with better education and employment normally earn more and their children's mortality is lower.

Next, we study the impacts of women's education on fertility, marriage, and sexual behaviours. In panel c, we show that increased female education led to lower actual and desired fertility as well as later ages at first birth, first marriage, and sexual debut, all of which are associated with lower child mortality (Hobcraft, 1993; Hobcraft et al., 1985).

Through education, women have an opportunity to acquire more health-related knowledge (Glewwe, 1999). In panel d, we focus on a series of questions in the BDHS inquiring about women's knowledge of AIDS, a major threat to maternal and child health. We show that an additional year of education increased women's awareness of AIDS by 7.63 percentage points. We also construct an AIDS knowledge index using factor analysis based on whether women who had ever heard of AIDS were able to correctly answer four questions: whether (1) using a condom during sex or (2) having just one sexual partner can reduce the risk of getting AIDS (the correct answer is yes); and if transmission of AIDS can happen (3) by being bitten by a mosquito or (4) by sharing food with a person who has AIDS (the correct answer is no). Two factors are retained, reflecting women's knowledge of AIDS regarding protection (first two questions) and transmission (last two questions), and a higher index implies greater knowledge. We find that schooling significantly increased women's knowledge of protective behaviours against AIDS and knowledge of HIV transmission.

Alongside the greater health-related knowledge, better-educated women are also more likely to visit health facilities, utilise health services, and seek help from qualified health professionals (i.e., personal illness control; see Mosley and Chen, 1984). In panel e, we look at four health-seeking behaviours when a woman had her most recent birth. We find that better-educated mothers were more likely to receive prenatal and delivery care from a qualified health professional (a doctor, nurse, midwife, or paramedic) and more likely to give birth in a hospital, clinic, or maternal and child welfare centre. An additional year of education also increased the number of antenatal visits by 0.34.

The last mechanism we consider is female empowerment, which has been linked with economic development and reductions in maternal and child mortality (Duflo, 2012). We use factor analysis to create an empowerment index based on whether a woman can make decisions alone or jointly with her husband on health care, large household purchases, and visits to family or relatives. The factor analysis yields only one factor with an eigenvalue greater than 1. In panel f, we show a positive and significant effect of women's years of education on female empowerment.

2.7 Discussion and Conclusion

In this study, we exploit the FSSSP as a quasi-experiment to identify the effect of maternal education on child mortality in Bangladesh, providing the first estimates of such a causal link in the South Asian context. We show a sizeable impact of the FSSSP on rural women's education. Using mothers' exposure to the FSSSP as an instrument, we find that an extra year of maternal schooling reduces under-five mortality by 21.54% and infant mortality by 22.24%. In line with findings of other quasi-experimental studies (Breierova and Duflo, 2004; Chou et al., 2010; Grépin and Bharadwaj, 2015; Makate, 2016; Makate and Makate, 2016), we also find that the coefficients are larger in magnitude in IV models than in linear models, indicating that the OLS estimates may be biased upward due to unobserved factors that influence both maternal education and child mortality. Another point to bear in mind is that the IV approach estimates a local average treatment effect, that is, the effect of maternal schooling on child mortality for compliers (mothers who would go to school for an extra year if they were covered by the FSSSP, but who would not if they were not covered).

In addition to linear probability models, we use Cox proportional hazard models and a control function approach to take into account right censoring of child mortality. Results indicate that one more year of maternal schooling reduces under-five mortality risk by 18.51% and infant mortality risk by 19.95%. The estimated effects are again larger in magnitude in Cox IV models than in naïve Cox models, which is consistent with Andriano and Monden (2019) using data from Malawi and Uganda. In their study, the linear specifications do not give significant results, and results are significant only in Cox models; while in this paper, the IV estimates in both linear specifications and survival analysis are statistically significant and qualitatively comparable, suggesting that our findings are robust to model specification controlling for right censoring.

Meta-analysis has shown that findings of previous research on the causal link between education and health have been inconsistent and suggested that more empirical research should be done to study this topic in various contexts and to examine other types of education policies besides compulsory schooling laws (Hamad et al., 2018; Mensch et al., 2019). This paper contributes to the empirical literature by studying a new setting (Bangladesh) and a new type of education reform (school stipend project). We show that the effect of maternal education on child mortality in Bangladesh is considerable, statistically significant, and robust. We also show a stronger effect in IV models, suggesting that existing correlational studies using linear/logistic models (Abir et al., 2015; Hossain et al., 2015; Huda et al., 2016; Maniruzzaman et al., 2018) or survival models (Chowdhury et al., 2017, 2020; Mohammad and Bari, 2017) tend to underestimate the role of maternal education in reducing child mortality in Bangladesh.

This study also sheds light on evidence-based policy evaluations. Bangladesh is known for the implementation of a series of ambitious education reforms aiming at increasing schooling (Behrman, 2015), and the effectiveness of these reforms has been systematically evaluated with regard to school enrolment and dropout, labour market outcomes, gender and socioeconomic inequalities, among others (Ahmed and Arends-Kuenning, 2006; Asadullah and Chaudhury, 2009; Behrman, 2015; Chowdhury et al., 2002; Hahn et al., 2018a,b; Meng and Ryan, 2010; Schurmann, 2009; Shamsuddin, 2015). This paper provides evidence that the FSSSP has been an effective policy to reduce child mortality in Bangladesh.

We acknowledge a limitation that this study focuses exclusively on the effect of maternal education and does not fully consider the impact of paternal education, the importance of which has been documented in previous research (Breierova and Duflo, 2004; Chou et al., 2010). However, we show that fathers' education is closely and positively correlated with mothers' through assortative mating, and hence we treat paternal education as a mechanism in lieu of an explanatory variable.

In addition to the assortative mating pathway, we suggest that other channels linking maternal education and child mortality include greater wealth and literacy, lower fertility, delayed marriage and childbearing, greater health-related knowledge, better health-seeking behaviours, and female empowerment. We do not find that women with better education were more likely to work in Bangladesh, which is a common situation in the Islamic world where female labour force participation remains low even when female education has improved dramatically (Lavy and Zablotsky, 2015). However, we do find that better-educated women were more likely to marry men with better jobs, again manifesting the assortative mating pathway. One thing that should be noted is that the proximate variables used in the analysis of potential mechanisms were measured contemporaneously to the BDHS survey times and may therefore be consequences rather than causes of improved child survival (Keats, 2018), and hence we should be cautious about interpreting the results.

To conclude, this study demonstrates that an education stipend project can foster maternal schooling and subsequently reduce child mortality. The findings have important policy implications for places where child mortality remains high: education reforms are needed to encourage women to go to school, which would contribute substantially to reductions in child mortality.

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Appendix B

Appendix to Chapter 2

B.1 Additional Robustness Checks: Internal Migration

Table B.1: Additional robustness checks, subsample of children born to non-migrant mothers from the 2007 Bangladesh Demographic and Health Survey

	Outcome variable:		
	Years of education	Under-five mortality	Infant mortality
	(a)	(b)	(c)
	First-stage OLS	2SLS	2SLS
Rural × full exposure	2.2084***		
	(0.2250)		
Rural × partial exposure	0.5614**		
	(0.1715)		
Years of education		−0.0211***	−0.0183***
		(0.0024)	(0.0024)
First-stage F statistic		50.88	50.88
Observations (mothers)	4,276	4,276	4,276
Observations (children)	10,803	10,803	10,803

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
Notes: Non-migrant mothers refer to those who have been living in the same place since before reaching secondary age (i.e., age 11) or who have moved but whose type of place of residence remained the same (i.e., they moved from rural to rural areas or from urban to urban areas). Standard errors, reported in parentheses, are clustered at the treatment level (rural dummy × mothers' birth cohorts dummies). Full exposure = mothers born in 1983–1988; partial exposure = mothers born in 1980–1982. All models control for rural dummy, full exposure dummy, partial exposure dummy, division fixed effects, mother's age at survey fixed effects and religion (Islam vs. others), and child's sex, birth order (first child vs. higher order), and birth year fixed effects.
Sources: ICF, Bangladesh Demographic and Health Survey 2007.

B.2 Additional Robustness Checks: Alternative Measures of Maternal Education

Table B.2: Additional robustness checks, completed secondary education as an alternative measure of maternal education

	Outcome variable:		
	Completed secondary school	Under-five mortality	Infant mortality
	(a)	(b)	(c)
	First-stage OLS	2SLS	2SLS
Rural × full exposure	0.0213***		
	(0.0036)		
Rural × partial exposure	0.0058		
	(0.0048)		
Completed secondary education		−0.6349***	−0.5444***
		(0.1330)	(0.1353)
First-stage F statistic		17.61	17.61
Observations (mothers)	25,783	25,783	25,783
Observations (children)	70,765	70,765	70,765

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
Notes: Standard errors, reported in parentheses, are clustered at the treatment level (rural dummy × mothers' birth cohorts dummies). Full exposure = mothers born in 1983–1988; partial exposure = mothers born in 1980–1982. All models control for rural dummy, full exposure dummy, partial exposure dummy, survey year and division fixed effects, mother's age at survey fixed effects and religion (Islam vs. others), and child's sex, birth order (first child vs. higher order), and birth year fixed effects.
Sources: ICF, Bangladesh Demographic and Health Surveys 2007, 2011, and 2014.

Table B.3: Additional robustness checks, received some secondary education as an alternative measure of maternal education

	Outcome variable:		
	Received some secondary school	Under-five mortality	Infant mortality
	(a)	(b)	(c)
	First-stage OLS	2SLS	2SLS
Rural × full exposure	0.0744***		
	(0.0108)		
Rural × partial exposure	0.0347***		
	(0.0069)		
Received some secondary education		−0.1879***	−0.1542***
		(0.0435)	(0.0408)
First-stage F statistic		27.09	27.09
Observations (mothers)	25,783	25,783	25,783
Observations (children)	70,765	70,765	70,765

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
Notes: Standard errors, reported in parentheses, are clustered at the treatment level (rural dummy × mothers' birth cohorts dummies). Full exposure = mothers born in 1983–1988; partial exposure = mothers born in 1980–1982. All models control for rural dummy, full exposure dummy, partial exposure dummy, survey year and division fixed effects, mother's age at survey fixed effects and religion (Islam vs. others), and child's sex, birth order (first child vs. higher order), and birth year fixed effects.
Sources: ICF, Bangladesh Demographic and Health Surveys 2007, 2011, and 2014.

Chapter 3

Does Exposure to Armed Conflict Affect Women's Attitudes towards Intimate Partner Violence? Evidence from Vietnam

3.1 Introduction

Recent research indicates that more than one in four ever-partnered women worldwide have experienced intimate partner violence (IPV)[1] in their lifetime (Sardinha et al., 2022). A large body of literature has revealed various negative impacts of IPV on women's physical, mental, sexual, and reproductive health (Campbell, 2002; Devries et al., 2013a; Ellsberg et al., 2008; Grose et al., 2021; Stöckl et al., 2013). Because of its high prevalence and severe consequences, violence against women is a global public health, policy, and human rights concern (Devries et al., 2013b), and the United Nations has called for its elimination in the Sustainable Development Goals target 5.2 (SDG5.2; United Nations, 2017).

Researchers have suggested a number of risk factors for IPV (Abramsky et al., 2011; Heise, 1998; Heise and Kotsadam, 2015), and many found that attitudes towards IPV—as measured by whether a person agrees that a husband is justified in beating his wife under some hypothetical circumstances—tend to be a strong predictor of IPV occurrences. In other words, women are at higher risk of experiencing domestic violence if they or their spouses think that such violence is acceptable (Flood and

[1] We use intimate partner violence, domestic violence, and violence against women interchangeably throughout this paper. All of these terms refer to violence against women committed by their current or former spouses or partners in intimate relationships.

Pease, 2009). Thus, understanding the patterns and correlates of how people view violence against women is crucial for achieving the SDG5.2.

An emerging strand of research has related IPV to another type of violence, armed conflict. Empirical investigations indicated that domestic violence is more likely to happen in households in which members were exposed to armed conflicts (e.g., Ekhator-Mobayode et al., 2022; La Mattina and Shemyakina, 2017; Østby et al., 2019; Svallfors, 2023; Torrisi, 2023). Findings on the association of conflict exposure with attitudes to IPV are, however, mixed. Some scholars found that women and men who were exposed to armed conflicts are more likely to believe that violence against women is acceptable (Gutierrez and Gallegos, 2016; La Mattina and Shemyakina, 2017), while others failed to report statistically significant results (Svallfors, 2023; Torrisi, 2023).

Despite growing evidence, considerable knowledge gaps are yet to be filled. First, prior work overlooks Southeast Asia, where 21% of women reported that they have experienced IPV (Sardinha et al., 2022). Moreover, earlier research focuses on either the concurrent role of ongoing conflict or the short/medium-term aftermath (i.e., 5–15 years after the end of the recent armed conflict), but there is a lack of empirical investigation into the long-term influence, which has been emphasised in the literature on the role of early-life shocks, such as armed conflicts and economic recessions, in individual attitude formation (e.g., Adhvaryu and Fenske, 2023; Giuliano and Spilimbergo, 2014). Last and most importantly, previous research that often uses the number of conflict-related events or deaths in an area as a proxy for conflict exposure for people living in that area does not fully tackle the endogeneity problem arising from measurement error in and non-random nature of conflict intensity. As a result, a correlation between conflict exposure and acceptance of IPV does not necessarily imply a causal link.

This paper contributes to the current literature on armed conflict and domestic violence by identifying the impact of early-life war exposure on women's attitudes towards IPV more than three and a half decades after the end of the conflict in Vietnam, a Southeast Asian country where 25% of ever-partnered women of reproductive age reported that they have experienced physical and/or sexual IPV in their lifetime (Sardinha et al., 2022). Specifically, we look at the Vietnam War, an interstate conflict between North Vietnam and South Vietnam that also involved both the Eastern and Western Blocs of the Cold War and caused millions of casualties (Hirschman et al., 1995). During the war, Vietnamese people were exposed to large-scale and indiscriminate aerial bombardment conducted by the United States and its allies. In this paper, we leverage the amount of ordnance dropped in a province to represent the intensity of war exposure for Vietnamese people living in that province.

To deal with the endogeneity issue, we exploit the distance to the border between North Vietnam and South Vietnam as an instrument for bombing intensity. The logic is that most of the intense fighting took place near the North-South border, so a place close to the border received heavier bombing compared to a place located far away. Besides, the border was arbitrarily drawn at the 17th parallel as a compromise between the superpowers of the Cold War. Accordingly, the border provides a source of exogenous variation in bombing intensity. To the best of our knowledge, this paper provides the first causal estimate of the effect of armed conflict on domestic violence.

3.2 Related Literature

3.2.1 Theoretical Perspectives

Why do we expect a link between early-life exposure to armed conflict and women's acceptance of IPV? A commonly referred explanation is the normalisation of and desensitisation to violence. Children who grow up in a violent environment such as armed conflict are more likely to witness, be victimised by, and even participate in violent events. Because of social learning, they may normalise violence as a conventional approach to dispute resolution and desensitise themselves to future violence in adulthood, making them accept IPV as a form of violence in the private sphere (Cappell and Heiner, 1990; Dodge et al., 1990; Pollak, 2004). The psychological literature suggested that this explanation may be particularly profound when experiencing violence in child developmental ages because attitude and belief formation processes are likely to occur in childhood and adolescence (Fowler et al., 2009; Guerra et al., 2003). Our main sample consists of Vietnamese women born in 1960–1975, indicating that they all experienced the Vietnam War in their developmental ages (ages 0–15), so we expect that the war will have a significant impact on their later-life attitudes.

Another explanation is the amplification of traditional gender, patriarchal, and social norms in conflict-affected areas (Svallfors, 2023), which is particularly relevant in the Vietnamese context. Drawing on qualitative data from fieldwork in a rural community, Rydstrøm (2003) posited three cultural factors that made Vietnamese women endure domestic violence. First, the traditional gender norm in Vietnam assumes a "cool" female character and a "hot" male character. In this case, men flying into a rage and using violence are considered to be normal, and women are expected to adjust themselves to maintain household harmony. Second, the patrilineal tradition in

Vietnam gives men superiority over women, which worsens women's position within a household. As a result, women may "encounter the common Vietnamese idea that it is a husband's right to punish his wife" (Rydstrøm, 2003, p. 684). Last, the prevailing Confucian virtues in Vietnam regulate married women's behaviours, so if they violate these social codes, their husbands are, from a Vietnamese point of view, justified in beating them. Rydstrøm (2003) also argued that these cultural factors were intensified by people's traumatic war experiences. Using quantitative data, Palmer et al. (2019) and Singhal (2019) found that intense bombing during the Vietnam War caused more individuals living with disabilities and mental health problems in postwar Vietnam, pointing to noteworthy physical and psychological trauma as a consequence of the war.

Education is another possible channel linking armed conflict and IPV attitudes. Women's education may increase their intra-household bargaining power because better-educated women normally have more options outside marriage and hence are more likely to feel confident to reject domestic violence (Friedman et al., 2016; Lundberg and Pollak, 1996). Furthermore, Pierotti (2013) argued that schools may serve as a venue for the diffusion of new ideas such as the rejection of IPV, which may challenge girls' existing perceptions of domestic violence. Therefore, women who attend schools are more likely to be exposed to the new ideas and think that IPV is unacceptable (Cannonier and Mocan, 2018; Chicoine, 2021; Friedman et al., 2016; Pierotti, 2013). Numerous studies have shown a disruption to education induced by violent conflict across different contexts (Bertoni et al., 2019; Brown and Velásquez, 2017; Chamarbagwala and Morán, 2011; Diwakar, 2015; Islam et al., 2016; Justino et al., 2014; Lai and Thyne, 2007; Leon, 2012; Shemyakina, 2011; Swee, 2015; Verwimp and Van Bavel, 2014), so we test the education mechanism in this paper.

Women's marital characteristics may also affect their attitudes to IPV. For instance, previous research has shown that early marriage is associated with a number of adverse outcomes for women, such as low educational attainment, little decision-making power, and poverty (Chari et al., 2017; Dahl, 2010; Field and Ambrus, 2008; Sunder, 2019), which are all known factors associated with a tolerant view about IPV. A few studies even found that child marriage is correlated with higher incidence of spousal violence against women (Nasrullah et al., 2014; Rahman et al., 2014). Findings on the association between armed conflict and age at marriage are mixed (Jayaraman et al., 2009; Saing and Kazianga, 2020; Shemyakina, 2013; Torrisi, 2022), and we explore such correlation in the Vietnamese case.

In a recent paper, Behrman and Frye (2021) highlighted the importance of considering intra-couple contexts when studying attitudes towards IPV, a perspective that has also been supported by the current research. For example, Bertrand et al. (2015) found that when a wife earns more than her husband, the couple is less satisfied with their marriage and is more likely to divorce. Their finding has an important implication for our study because marital satisfaction has been shown to closely related to domestic violence in Vietnam (Rydstrøm, 2003). Moreover, Cools and Kotsadam (2017) and Weitzman (2014) showed that a resource mismatch between wife and husband is associated with more IPV against women in Africa and India. In this paper, we assess if the Vietnam War affects spousal age difference—a key discrepancy in marital characteristics that may make women more tolerant of IPV.

3.2.2 Prior Empirical Evidence

Gender-based violence in armed conflict such as wartime sexual abuse of women has been of longstanding interest to scholars of gender studies, public health, and peace and conflict research (Cohen, 2013; Krüger and Nordås, 2020; Nordås and Cohen, 2021). In recent years, researchers have started to look into violence against women perpetrated by their intimate partners in conflict-prone and post-conflict settings, a topic that receives much less attention compared to violence committed by armed actors (Stark and Ager, 2011). A handful of surveys were conducted purposefully in conflict-affected places to examine the correlation between conflict exposure and IPV against women. For example, a survey of people in the occupied Palestinian territory showed that women whose husbands were exposed to political violence and whose households were financially affected by the Israeli occupation were more likely to experience physical and sexual IPV in the past year (Clark et al., 2010). Two other surveys reported a high prevalence of IPV against women in Northern Uganda, where people suffer from the ongoing Lord's Resistance Army insurgency, and highlighted the role of male partners' alcohol misuse (Mootz et al., 2018; Saile et al., 2013).

The sample size of these special surveys was usually small because of financial and technical constraints, and many studies have therefore utilised secondary data from the Demographic and Health Surveys (DHS), which contain a series of standardised questions regarding IPV in the domestic violence module. Not like those special surveys, DHS does not directly ask respondents about their involvement in armed conflicts, so a proximate measure of conflict exposure has to be employed. Using the number of survivors and accused perpetrators of the Rwandan genocide as a proxy for local-level conflict intensity, La Mattina (2017) found that women who married after the 1994 genocide were more likely to be victims of physical IPV relative to women who married before, and such correlation was stronger for women living in communes with higher genocide intensity. Focusing on the internal conflict in Peru between the government and the communist guerrilla group Shining Path, Østby et al. (2019) found that women living in areas with a higher number of reported incidents of sexual violence perpetrated by an armed group had a greater risk of suffering IPV. A number of studies linked geo-referenced data on conflict-related fatalities and events from the Armed Conflict Location & Event Data Project or the Uppsala Conflict Data Program with DHS data on IPV. They showed that exposure to greater civil conflicts was associated with higher risk of IPV against women in Liberia (Kelly et al., 2018), Nigeria (Ekhator-Mobayode et al., 2022), multiple countries in sub-Saharan Africa (La Mattina and Shemyakina, 2017; Østby, 2016), Columbia (Svallfors, 2023), and ex-Soviet states (Torrisi, 2023). In addition to conflict intensity, a few studies emphasised the age at conflict exposure from a life course perspective and suggested that women who were exposed to conflict violence in early childhood had an elevated likelihood of being exposed to IPV later in life compared with women who were exposed at other ages and who were never exposed (Gutierrez and Gallegos, 2016; La Mattina and Shemyakina, 2017; Torrisi, 2023).

Although the work cited above has all indicated a significant correlation between armed conflict exposure and domestic violence presence, the results are inconsistent when it comes to IPV attitudes. Using data from 20 sub-Saharan African countries, La Mattina and Shemyakina (2017) found that exposure to armed conflicts between ages 6 and 10 increased women's acceptance of domestic violence by 2.7% and men's by 3.6%. Similarly, Gutierrez and Gallegos (2016) showed that Peruvian women who were exposed to more intense civil conflict in their late childhood and early adolescence (ages 9–16) were more likely to say that domestic violence is acceptable. In contrast, Svallfors (2023) did not find statistically significant evidence that conflict exposure was associated with greater tolerance of IPV among Colombian women, but the null results may be attributable to low statistical power. In another paper exploring the link between conflict and domestic violence in four former Soviet Union countries, namely Armenia, Azerbaijan, Moldova, and Tajikistan, Torrisi (2023) found no association between early-life conflict exposure and women's opinions about IPV, but an increase in men's acceptance of IPV was discovered if they underwent conflicts between ages 16 and 19, which contrasts with the finding of La Mattina and Shemyakina (2017) that exposure to conflict violence between ages 16 and 20 significantly reduced African men's acceptance of IPV.

3.3 Empirical Context: The Vietnam War

The origin of the Vietnam War can date back to the First Indochina War (1946–1954), during which the Viet Minh, a communist-led national independence coalition, initiated an insurgency against French colonial rule. Following French defeat and military withdrawal in 1954, the Viet Minh, France, the United States, the Soviet Union, the United Kingdom, and China participated in the Geneva Conference to discuss the restoration of peace in Indochina. The conference delegates agreed to temporarily separate the country into two parts but debated on the exact location of the partition. The Viet Minh called for the line of division to be at the 13th parallel while the French called for partition at the 18th parallel (Logevall, 2012). After three months of negotiations, the Geneva Accords were issued on 21 July 1954, setting out a "provisional military demarcation line" running through the 17th parallel until 1956, when a general election would be held to build a unified Vietnamese state. As a consequence, the country was divided into two along the 17th parallel, a communist state in the north supported by the Soviet Union and China and a pro-Western regime in the south supported by the United States.

The election planned for 1956 had never been run as South Vietnam refused to participate in it, leading to the Vietnam War. There was frequent insurgency in South Vietnam throughout the second half of the 1950s. In December 1960, the National Liberation Front, commonly known as the Viet Cong, was established under the direction of North Vietnam to start a guerrilla war in South Vietnam, marking a build-up of military activity. Initially, the United States provided limited advisory support to South Vietnam, but its military involvement escalated after the Gulf of Tonkin incident in 1964 as the US Congress allowed the president to use conventional military force in Southeast Asia.

Apart from the deployment of strong ground strike forces, the United States also conducted massive strategic bombing campaign against North Vietnam, which is one of the most intense bombardment campaigns in history. The US Armed Forces dropped more than 7.5 million tons of bombs and other ordnance (Clodfelter, 1995), which is three times the weight of bombs dropped during World War II, fifteen times the tonnage in the Korean War, and about 100 times the impact of the atomic bombings of Hiroshima and Nagasaki (Miguel and Roland, 2011). The aerial bombardment of Vietnam was indiscriminate—it targeted communities where individuals lived rather than specific individuals (Kocher et al., 2011).

As illustrated in Figure 3.1, the bombing of Vietnam was geographically concentrated. The most heavily bombed areas were close to the North-South border. The most intense bombing happened in Quang Tri, the northernmost province of South Vietnam located right on the 17th parallel, and provinces just north and south of Quang Tri were also subject to heavy bombing. In North Vietnam, regions along the coastline and around the capital Hanoi received massive bombing, and so did areas near the South Vietnamese capital Saigon that were close to the Cambodian border.[2]

3.4 Data and Methods

3.4.1 Individual-Level Data and Variables

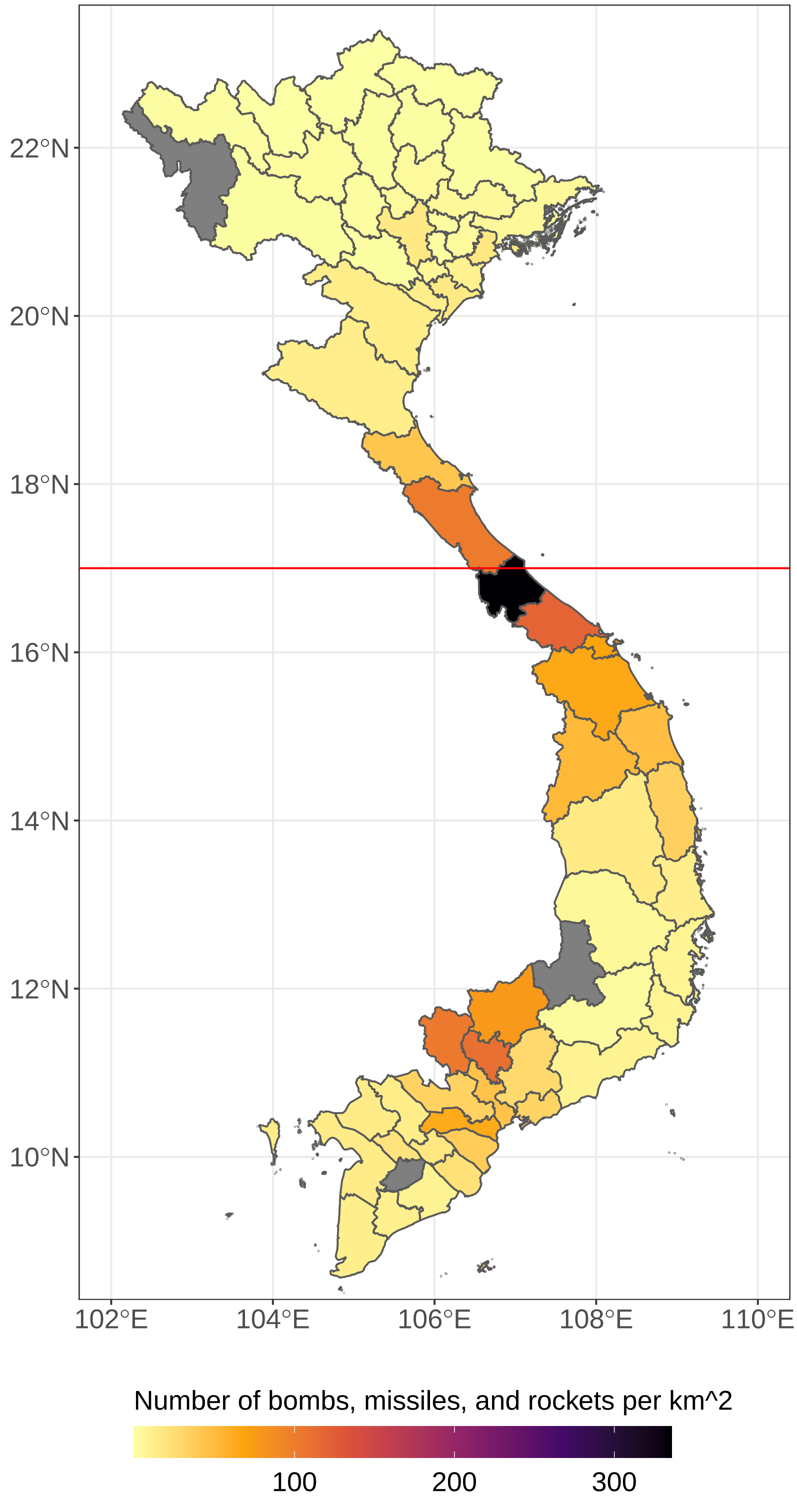
The main data used in this paper come from the Vietnam Multiple Indicator Cluster Survey round 4 (VMICS-4), which was carried out between November 2010 and January 2011 in all 63 provinces of Vietnam (General Statistics Office of Vietnam and UNICEF, 2011). A nationally representative sample of 11,663 Vietnamese women aged 15–49 was drawn using a two-stage sampling frame stratified by region (Red River Delta, Northern Midland and Mountain area, North Central and Central Coastal area, Central Highlands, South East, and Mekong River Delta) and area (rural/urban). The rural and urban areas within each region were set as the main sampling strata. In each stratum, a number of enumeration areas were selected using probability proportional to size sampling, and in each sampled enumeration area, 20 households were surveyed.

The VMICS-4 asked women about their attitudes towards IPV through five questions: "In your opinion, is a husband justified in hitting or beating his wife in the following situation: (1) if she goes out without telling him, (2) if she neglects the children, (3) if she argues with him, (4) if she refuses to have sex with him, and (5) if she burns the food?" Based on these questions, we create a dummy variable taking the value 1 if a woman accepted wife beating in at least one of the five situations and 0 if a woman rejected wife beating to measure IPV acceptability.[3]

[2] The United States and its allies bombed these areas in an effort to suppress support to North Vietnamese and Viet Cong troops arriving in South Vietnam through the so-called Ho Chi Minh trail.

[3] Following Behrman and Frye (2021) and Pierotti (2013), we code women who responded that they "don't know" if wife beating is justified in a scenario also as 1. Therefore, the 1 category technically indicates that a woman failed to reject wife beating, but for simplicity, we interpret this dummy variable as accepting vs. rejecting IPV throughout the paper. Only between 2% and 6% of the respondents reported "don't know" across the five questions about IPV acceptability, and the regression results (available upon request) are robust to coding these women as 0.

Figure 3.1: Amount of ordnance dropped in Vietnamese provinces, 1965–1975



Notes: The red line represents the 17th parallel—the border between North Vietnam and South Vietnam. Grey areas indicate no bombing data available.
Source: Miguel and Roland (2011).

A list of individual-level control variables, as suggested by Trinh et al. (2016), are extracted from the VMICS-4, including respondent's age, marital status (currently, formerly, or never married), household head's ethnicity (Kinh vs. non-Kinh), rural/urban residence, region of residence (six regions listed above), and a household wealth index constructed based on household assets (five quintiles from poorest to richest).

3.4.2 Province-Level Data and Variables

We use province-level data on bombing of Vietnam assembled by Miguel and Roland (2011) to construct a measure of war exposure (hereafter the MR). Based on military records from different sources, the MR contains the amount of various types of ordnance dropped by US and its allies in Vietnam between 1965 and 1975. In this paper, the intensity of bombing is measured by the number of bombs, missiles, and rockets dropped in a province per square kilometre.[4]

We then match bombing intensity to individual data from the VMICS-4 based on women's current provinces of residence, and we are able to match 11,302 women living in 60 out of 63 Vietnamese provinces (96.9% of the total sample).[5] Provinces in Vietnam are relatively small subdivisions,[6] so bombing intensity at the provincial level serves as a good proxy for war exposure for individuals living in the province.

Because of lack of information about women's residence and migration histories in the VMICS-4,[7] we have to use the province where a woman was living at the time of the survey as a proxy for her wartime location of residence, which may cause some problems because people living in heavily bombed provinces might be more likely to flee from their homes. Although we cannot directly test it, we refer to some indirect evidence that migration may only have a limited impact on our results. Because war exposure is measured at the provincial level, only migration across provinces

[4] Miguel and Roland (2011) showed substantial correlations between the number of bombs, missiles, and rockets and other types of ordnance and hence argued that it is a good proxy for overall bombing intensity.

[5] We exclude 361 women who were residing in three provinces that were not covered in the MR: (1) Dak Nong, located in the southern part of the Central Highlands, bordering Cambodia, (2) Dien Bien, located in the Northwest region of Vietnam, bordering China and Laos, and (3) Hau Giang, located in the Mekong Delta region in the southern part of Vietnam.

[6] Barceló (2021) drew an analogy between Vietnam and American states: Vietnam's surface area is similar to that of New Mexico, and the average area of Vietnamese provinces is smaller than that of counties in New Mexico.

[7] Unfortunately, not like the DHS, the VMICS-4 does not have information on how long a woman had been living in the current place, so we cannot do a subsample analysis of women who had not moved.

would threaten our identification. The 1989, 1999, and 2009 censuses revealed a relatively low level—only between 2.7% and 3.1%—of inter-provincial migration stock in Vietnam (Minnesota Population Center, 2020). Using recently collected survey data on migration histories of older Vietnamese people who lived through the Vietnam War period, Young et al. (2021) found that 71% of their overall sample did not migrate from one province to another during the war, and more than 90% of the civilian sample reported no wartime inter-provincial movements. They also pointed out that most of those who had moved in the war have eventually returned to their provinces of birth. Similarly, Barceló (2021) found that only 11% of the respondents in his sample reported a province of current residence that was different from where they lived during the war years. Furthermore, analyses of Vietnamese census and survey data indicated no statistically significant effects of bombing intensity on postwar population density, growth, and cohort size at the provincial and district levels, and an interpretation of these findings is that the bombing did not lead to large postwar population movements, or those who displaced by the war had returned to their homes shortly after the end of the conflict (Miguel and Roland, 2011; Singhal, 2019).

Vietnam's political and societal contexts may also help us to alleviate some concerns about internal migration. First, similar to the Chinese "hukou", there is a household registration system, the "ho khau", in Vietnam limiting internal migration. People who intend to move from one province to another have to meet certain requirements in order to register their households in the destination province, and very few people could move without transfer of their household registration. Moreover, past research suggested that inter-provincial migration in Vietnam was largely driven by economic reasons, and more developed provinces attracted more immigrants, while less developed provinces saw more emigrants (Dang et al., 1997; Phan and Coxhead, 2010). However, Miguel and Roland (2011) has argued that the Vietnam War did not have long-run impact on Vietnamese economic development. Therefore, the war arguably had no long-run impact on internal migration either. Nevertheless, one should bear in mind that inter-provincial migration remains a threat to identification, and we perform additional analysis to allay this concern.

In our models, we control for a series of provincial characteristics that are available from the MR. First, we control for population density (log) of a province in 1960 as a proxy for the prewar condition. We also add a North-South dummy indicating that whether a province was part of North Vietnam or South Vietnam during the war. Additionally, we include some geographic and climatic characteristics—including latitude,[8] wartime average precipitation (in centimetre) and temperature (in Celsius), and elevation[9]—to control for factors associated with military and bombing strategy.

3.4.3 Empirical Strategy

In the baseline analysis, we restrict the sample to those who were born in or before 1975 and hence directly exposed to the bombing, resulting in 4,614 women born in 1960–1975. We then fit the following regressions model using ordinary least squares (OLS):

$$IPV_{i,j} = \alpha + \beta Bombing_j + \mathbf{X}_i'\gamma + \mathbf{W}_j'\delta + \varepsilon_{i,j}, \tag{3.1}$$

where $IPV_{i,j}$ is the measure of acceptability of IPV for woman i living in province j, $\mathbf{X}_i$ and $\mathbf{W}_j$ are sets of controls at the individual and provincial levels, respectively, and $\varepsilon_{i,j}$ is an error term clustered at the provincial level. Individual war exposure is measured by $Bombing_j$, the log of the amount of bombs, missiles, and rockets dropped in province j over the period 1965–1975 per square kilometre. Thus, a greater value of bombing intensity indicates greater exposure to the Vietnam War for women living in the province, and a positive sign of the coefficient β implies that women with greater exposure to the war were more likely to have an acceptive opinion on IPV.

The geographical distribution of bombing across provinces is likely to be endogenous (Barceló, 2021). For example, although the scale of bombing tends to be a reasonably good measure of conflict intensity, it does not capture some aspects of war violence such as the damage caused by ground operations and the North Vietnamese Army. Hence, the variable $Bombing_j$ is subject to measurement error. Additionally, endogeneity concerns could arise because of omitted-variable bias or reverse causality. For instance, areas with more aggressive and combative individuals who were more willing to defend themselves and their families against attackers—and hence were more likely to normalise and be desensitised to the use of violence in future intimate relationships—may be more likely to be targeted by the US bombardment. These issues may bias the OLS estimate of β and impede our attempt to establish a causal link between war exposure and IPV attitudes.

Building on an instrumental variable (IV) approach developed by Miguel and Roland (2011), we utilise $Distance_j$, the distance from province j to the border

[8] Provincial latitude is calculated as the average of latitudes of all districts in a province, weighted by district land area.

[9] Provincial elevation is measured by the proportion of a province's land area at high altitude (250–500, 500-1000, and over 1000 metres).

between North Vietnam and South Vietnam, as an instrument for the endogenous bombing intensity and estimate the following first-stage equation:

$$Bombing_j = \zeta + \eta Distance_j + \mathbf{X}_i'\theta + \mathbf{W}_j'\lambda + \nu_{i,j}, \tag{3.2}$$

where $Distance_j$ is calculated as the absolute difference between provincial latitude and the 17th parallel north latitude. For being a valid instrument, $Distance_j$ has to have a strong first stage. As shown in Figure 3.1, most of the intense fighting occurred near the border, so bombing and war exposure grew markedly as a place came closer to the border. Thus, we expect the coefficient η to be negative.

The instrument has to satisfy the exclusion restriction as well. In other words, it should not suffer from the same endogeneity problem as the endogenous explanatory variable. As mentioned before, the division of Vietnam was a result of negotiations in the context of the Cold War. At the Geneva Conference, the Soviet Union and China wanted to push the partition line further south because doing so would enlarge the territory under the control of North Vietnam. On the other hand, United States and its allies sought to draw the line further north as it would expand the anti-communist power in the south. At the end, the border was arbitrarily determined at the 17th parallel without taking account of the Vietnamese people's opinions and thoughts. Throughout the history of Vietnam, the 17th parallel did not have any political and economic significance other than being the North-South border during the Vietnam War. For instance, Dell et al. (2018) found that the seventeenth-century boundary that divided Vietnam into two historical states with different institutions had persistent impacts on economic outcomes over the past 150 years. However, this historical boundary is unlikely to be relevant to the Vietnam War because it is far away from the 17th parallel and conflict-related measures were similar across the boundary (Dell et al., 2018). In general, this evidence suggests that the North-South border is exogenous and our instrument does not seem to violate the exclusion restriction.

3.5 Results

3.5.1 Main Results

Summary statistics are presented in Tables C.1 (for individual-level variables) and C.2 (for province-level variables). More than one-third of women in our main sample (38.8%) thought that IPV was acceptable in at least one situation. The most "IPV-justified situation" is if the woman neglects the children (27.7%), followed by arguing with (23.8%), going out without telling (16.7%), and refusing to have sex with the husband (10.8%). There are also 4.8% women justifying IPV if they burn the food. Of the 60 provinces, the average amount of bombs, missiles, and rockets dropped in 1965–1975 per square kilometre is 28.98 with a standard deviation of 50.09. The (logged) bombing intensity measure used in regression analysis has a mean of 2.19 and a standard deviation of 1.96.

Results of the baseline analysis are shown in Table 3.1. The OLS result indicates a positive association between bombing intensity and acceptability of IPV (column b). However, the OLS estimate is likely to be biased because of endogeneity, and we address this problem by instrumenting bombing intensity with provincial distance to the 17th parallel, the border between North Vietnam and South Vietnam. The first stage shows a negative correlation between the instrument and the endogenous independent variable (column a), indicating that provinces located closer to the border received heavier wartime bombing than provinces located further away, which accords with our expectation. The large first-stage F statistic (32.40) suggests that our instrument is not weak.

Table 3.1: Effect of bombing intensity on IPV acceptability

	Outcome variable:			
	Bombing intensity	IPV acceptability		
	(a) First-stage OLS	(b) OLS	(c) 2SLS	(d) Reduced form
Bombing intensity (log)		0.0286*	0.0481*	
		(0.0124)	(0.0189)	
Distance to the 17th parallel	−0.9577***			−0.0461**
	(0.1683)			(0.0152)
First-stage F statistic	32.40			
N (women)	4,614	4,614	4,614	4,614
N (provinces)	60	60	60	60

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
Notes: Standard errors, reported in parentheses, are clustered at the provincial level. All models control for respondent's age, marital status, household head's ethnicity, rural/urban residence, region of residence, household wealth index as well as pre-war population density (log), South Vietnam dummy, latitude, average precipitation, average temperature, and elevation at the provincial level.

The IV estimate using two-stage least squares (2SLS) is reported in Table 3.1 column c. In line with the OLS result, the regression coefficient is positive and significant at conventional levels ($p = 0.011$). The effect is also substantial. A 1% increase in the amount of ordnance dropped in a province per square kilometre led to an increase of 4.81 percentage points in the possibility that a woman accepted IPV in any of the five situations, which is equivalent to 12.4% of the sample mean. The 2SLS estimate of the coefficient is 1.68 times the OLS estimate, suggesting that the OLS estimate may be biased downward because of measurement error. The reduced-form result presented in Table 3.1 column d indicates a negative correlation between the distance to the 17th parallel and women's likelihood of accepting IPV.

Next, we consider two alternative measures of IPV acceptability. First, we construct a justification index using factor analysis based on the five questions about attitudes towards IPV. A greater index indicates greater tolerance of IPV. The factor analysis yields one factor with an eigenvalue greater than one, which we use as an extra outcome variable. The OLS and 2SLS results are shown in Table 3.2 columns a and b, respectively. The 2SLS estimate of the effect of bombing intensity on the index is once again positive, statistically significant ($p = 0.009$), and considerably larger in size than the OLS estimate.

Table 3.2: Effect of bombing intensity on alternative measures of IPV acceptability

	Outcome variable:			
	Justification index		Number of situations	
	(a)	(b)	(c)	(d)
	OLS	2SLS	OLS	2SLS
Bombing intensity (log)	0.0593*	0.1273**	0.0861*	0.1872*
	(0.0270)	(0.0489)	(0.0400)	(0.0728)
N (women)	4,614	4,614	4,614	4,614
N (provinces)	60	60	60	60

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
Notes: Standard errors, reported in parentheses, are clustered at the provincial level. The instrument for bombing intensity (log) is the distance to the 17th parallel. All models control for respondent's age, marital status, household head's ethnicity, rural/urban residence, region of residence, household wealth index as well as pre-war population density (log), South Vietnam dummy, latitude, average precipitation, average temperature, and elevation at the provincial level.

A second alternative outcome variable is the number of situations in which a woman justified IPV. Results presented in Table 3.2 columns c and d confirm the previous findings—the positive impact of bombing is sizeable and significant ($p = 0.010$), and the coefficient is bigger in 2SLS than in OLS models.

In Table 3.3, the situations in which women would tolerate IPV are analysed separately. We find consistently positive effects of bombing intensity on IPV acceptability across all five situations, although the coefficients estimated in three of the five models are not significant at 5%. The effects are particularly pronounced for the situations in which a woman neglects the children as well as if she goes out without telling her husband.

Table 3.3: Effect of bombing intensity on IPV acceptability by situation

	Outcome variable: Woman accepted IPV in situation				
	(1)	(2)	(3)	(4)	(5)
	2SLS	2SLS	2SLS	2SLS	2SLS
Bombing intensity (log)	0.0429*	0.0598***	0.0332	0.0254	0.0259
	(0.0178)	(0.0163)	(0.0190)	(0.0152)	(0.0144)
N (women)	4,614	4,614	4,614	4,614	4,614
N (provinces)	60	60	60	60	60

$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.
Notes: Standard errors, reported in parentheses, are clustered at the provincial level. Situation (1) = if she goes out without telling her husband; Situation (2) = if she neglects the children; Situation (3) = if she argues with her husband; Situation (4) = if she refuses to have sex with her husband; Situation (5) = if she burns the food. The instrument for bombing intensity (log) is the distance to the 17th parallel. All models control for respondent's age, marital status, household head's ethnicity, rural/urban residence, region of residence, household wealth index as well as pre-war population density (log), South Vietnam dummy, latitude, average precipitation, average temperature, and elevation at the provincial level.

3.5.2 Robustness and Heterogeneity

We conduct additional analyses to check if our findings are robust. First, the province of Quang Tri, which is located right on the 17th parallel, received far more bombing than any other provinces. In Table 3.4 panel a, we exclude respondents from this province, and the result remains unchanged.

As mentioned previously, internal migration is a major concern in our analysis. Although we have argued that migration between provinces is limited in Vietnam, we take a step forward by excluding respondents living in Hanoi and Ho Chi Minh City, two largest municipalities that attracted large immigrants from other provinces (Phan and Coxhead, 2010). We see from Table 3.4 panel b that our findings are robust.

Yet we have looked at women who were born in 1960–1975 and hence were directly exposed to the Vietnam War. As a placebo test, we examine the impact of bombing on IPV acceptability for those who were born after 1975 and were at least 18 years old at the time of the survey (5,529 women born in 1976–1993). These women were born after the end of the Vietnam War so they did not experience wartime bombing.[10] Descriptive statistics shown in Table C.1 reveal a similar pattern of attitudes towards IPV and of most individual-level characteristics between women in the two groups, but women in the placebo sample were younger than those in the main sample, and there were more unmarried women in the placebo sample at the time of the survey. We re-estimate the 2SLS coefficient using the placebo sample and still find a positive effect even though these women did not see the massive bombing themselves (Table 3.4 panel c). We see this as an indirect effect of the Vietnam War on IPV justification. These women were likely to grow up in violent homes in which household members such as their parents were exposed to the war and therefore likely to engage in domestic violence as either victims or perpetrators. As a consequence, these women might have been desensitised to the use of violence at home and adopted an acceptive attitude to IPV through the intergenerational transmission of domestic violence (Pollak, 2004). This explanation seems plausible given the literature showing that witnessing IPV in childhood was associated with adulthood perpetration and enduring views on IPV (Flood and Pease, 2009; Roberts et al., 2010; Waltermaurer, 2012). However, one should note that he 2SLS coefficient estimated based on the placebo sample is non-significant ($p = 0.100$) and smaller in magnitude relative to the estimate based on the main sample, indicating that the indirect effect of the Vietnam War on women's attitudes towards IPV is weaker than the direct effect.

The majority of existing research is based on ever-married samples and does not involve women who were single at the time of the survey. Given that armed conflict may alter marriage market conditions (e.g., changes in local marriage market sex ratios resulting from male-biased casualties, see La Mattina, 2017), which may affect women's chance of getting married and reduce their bargaining power, it is interesting to examine potential heterogeneity in the effects of bombing on IPV acceptability across women with different marital status. We find that the effect size for formerly and never-married women is much larger than that for currently married women (Table 3.4 panel d), but the coefficients of the former are not significant at

[10] Although these women did not experience the Vietnam War, some of them were exposed to other armed conflicts afterwards, such as the Cambodian–Vietnamese War (1978–1989) and the Sino-Vietnamese War (1979), which were much less intense compared with the Vietnam war.

Table 3.4: Effect of bombing intensity on IPV acceptability, robustness and heterogeneity

	Outcome variable: IPV acceptability			
	2SLS coefficient	First-stage F statistic	N (women)	N (provinces)
(a) Excluding Quang Tri province				
Bombing intensity (log)	0.0428*	28.88	4,580	59
	(0.0189)			
(b) Excluding municipalities				
Bombing intensity (log)	0.0461*	33.65	4,391	59
[Excluding Hanoi]	(0.0182)			
Bombing intensity (log)	0.0420*	28.88	4,178	59
[Excluding Ho Chi Minh City]	(0.0187)			
Bombing intensity (log)	0.0361*	33.41	3,955	58
[Excluding both]	(0.0167)			
(c) Placebo test (women born after the Vietnam War)				
Bombing intensity (log)	0.0331	29.44	5,529	60
	(0.0201)			
(d) Heterogeneous effects by marital status				
Bombing intensity (log)	0.0460*	32.58	4,094	60
[Currently married]	(0.0193)			
Bombing intensity (log)	0.0888	27.15	347	57
[Formerly married]	(0.0489)			
Bombing intensity (log)	0.1766	14.69	173	44
[Never married]	(0.1080)			
(e) Heterogeneous effects by rural/urban residence				
Bombing intensity (log)	0.0352	32.16	2,504	59
[Rural]	(0.0188)			
Bombing intensity (log)	0.0729**	28.94	2,110	60
[Urban]	(0.0221)			
(f) Heterogeneous effects by household wealth				
Bombing intensity (log)	0.0394*	34.32	2,416	60
[Poorest, poorer, or middle]	(0.0193)			
Bombing intensity (log)	0.0699**	26.10	2,198	59
[Richer or richest]	(0.0237)			

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

Notes: Standard errors, reported in parentheses, are clustered at the provincial level. The instrument for bombing intensity (log) is the distance to the 17th parallel. All models control for respondent's age, marital status (not in panel d), household head's ethnicity, rural/urban residence (not in panel e), region of residence, household wealth index (not in panel f) as well as pre-war population density (log), South Vietnam dummy, latitude, average precipitation, average temperature, and elevation at the provincial level.

conventional levels ($p = 0.070$ and 0.102, respectively). It should also be noted that formerly married and single women only constitute a small fraction of the total sample, so the estimates are based on a small number of respondents and may likely be subject to low statistical power.

We then assess whether wartime bombing had different impacts on women in rural and urban areas. We find that exposure to more intense bombing during the Vietnam War made both rural and urban women more likely to tolerate IPV (Table 3.4 panel e), but the effect is significant only for women living in urban areas ($p = 0.001$). Moreover, we find that the effect on urban women is more than double the effect on rural women, providing suggestive evidence that the causal relationship between war exposure and women's opinions about IPV is driven by those residing in urban areas.

Krause et al. (2016) and Trinh et al. (2016) found a strong negative correlation between justification of IPV among Vietnamese women and household wealth, and we explore the heterogeneous effects by women's household wealth in Table 3.4 panel f. We find that greater war exposure significantly increased tolerance of IPV among women regardless of household wealth, but the effect size for women living in richer or richest households is almost twice as big as that for women whose household wealth is classified as poorest, poorer, or middle.

3.5.3 Mechanisms

We rely on three proximate variables to explore possible mechanisms. We first consider female education and create a dummy variable taking the value 1 if a woman's educational attainment was above primary education (i.e., secondary or tertiary) and 0 if the woman received primary or no education. The other two variables are about women's marital characteristics: her age at first marriage and age difference between her and her husband.[11] We replicate the main analysis with these proximate variables as new outcomes. As expected, 1% increase in bombing intensity decreased the chance that a woman would get schooling above the primary level by 4.1 percentage points (Table 3.5 column a), but this effect is not statistically significant ($p = 0.115$). Similarly, we find that 1% more ordnance dropped in the Vietnam War lowered women's age at first marriage by 2.76 months and spousal age difference by less than a month (Table 3.5 columns b and c), and the effects are both non-significant ($p = 0.115$ and 0.374, respectively).

[11] The former applies to both currently and formerly married women, but information on the latter was collected only from currently married women.

Table 3.5: Effect of bombing intensity on women's education, age at first marriage, and spousal age difference

	Outcome variable:		
	Above primary education	Age at first marriage	Spousal age difference
	(a)	(b)	(c)
	2SLS	2SLS	2SLS
Bombing intensity (log)	−0.0410	−0.23	−0.06
	(0.0260)	(0.14)	(0.07)
First-stage F statistic	32.40	32.67	32.76
Mean of outcome variable	0.74	21.59	3.36
N (women)	4,614	4,441	4,057
N (provinces)	60	60	60

$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.
Notes: Standard errors, reported in parentheses, are clustered at the provincial level. The instrument for bombing intensity (log) is the distance to the 17th parallel. All models control for respondent's age, marital status, household head's ethnicity, rural/urban residence, region of residence, household wealth index as well as pre-war population density (log), South Vietnam dummy, latitude, average precipitation, average temperature, and elevation at the provincial level.

Given a lack of evidence that bombing has significant influences on Vietnamese women's education and age at marriage as well as age difference between her and her husband, we follow La Mattina and Shemyakina (2017) and re-estimate the baseline model controlling for the three variables (Table 3.6). We find that adding the new covariates only slightly attenuates the effect of bombing intensity on women's IPV acceptability, and the estimated coefficients are still statistically significant. In addition, women with above primary education were less likely to accept IPV compared to women with primary or no education ($p < 0.0001$, Table 3.6 column a). Women's age at first marriage is also negatively and significantly associated with IPV acceptability ($p = 0.014$, Table 3.6 column b).

3.6 Discussion

An increasing body of literature has discovered a correlation between exposure to armed conflict and individual experience of IPV. However, past scholarship often got conflicting findings on the association between conflict exposure and attitudes towards IPV. Furthermore, none of the existing studies have exhaustively dealt with

Table 3.6: Effect of bombing intensity on IPV acceptability, controlling for women's education, age at first marriage, and spousal age difference

	Outcome variable: IPV acceptability		
	(a) 2SLS	(b) 2SLS	(c) 2SLS
Bombing intensity (log)	0.0441*	0.0475*	0.0450*
	(0.0179)	(0.0188)	(0.0191)
Above primary education	−0.0989***		
	(0.0255)		
Age at first marriage		−0.0040*	
		(0.0016)	
Spousal age difference			−0.0013
			(0.0025)
First-stage F statistic	32.07	32.69	32.75
N (women)	4,614	4,441	4,057
N (provinces)	60	60	60

$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.
Notes: Standard errors, reported in parentheses, are clustered at the provincial level. The instrument for bombing intensity (log) is the distance to the 17th parallel. All models control for respondent's age, marital status, household head's ethnicity, rural/urban residence, region of residence, household wealth index as well as pre-war population density (log), South Vietnam dummy, latitude, average precipitation, average temperature, and elevation at the provincial level.

the endogeneity in the conflict intensity measures so that their findings lack a causal interpretation. This paper makes an attempt for the first time to estimate a causal effect of armed conflict on adult women's viewpoints about IPV in Vietnam, a country that has seen both fierce battles and widespread violence against women throughout its recent history. We examine whether exposure to massive bombing in the Vietnam War early in life had lasting impact on Vietnamese women's attitudes to IPV thirty-five years after the end of the war. To address the endogeneity issue, we exploit the distance to the arbitrarily drawn border between North Vietnam and South Vietnam (the 17th parallel) as a source of exogenous variation in the geographical distribution of wartime bombing. We find that women living in a province that was heavily bombed during the Vietnam War were more likely to accept domestic violence against them relative to women living in a province that received less bombing, which probably reflects the normalisation of and desensitisation to violence in the private sphere and interpersonal relationships. The estimated effect is sizeable, statistically significant, and robust to alternative specifications. We also find that the effect is particularly notable for those living in urban areas and in wealthy households.

In all of our main analyses using various measures of IPV acceptability, the IV estimates are greater in magnitude than the OLS estimates (Tables 3.1 and 3.2). A plausible interpretation of this is that the downward bias stemming from measurement error predominates the source of endogeneity compared with the upward bias caused by omitted variable or reverse causality. Measurement error in bombing intensity measure may come from the omission of the violence instigated by the North Vietnamese Army and ground operations. Besides, the measure of bombing intensity we use counts the amount of ordnance dropped in a province and does not capture the entire damage the bombardment campaign caused to the province. The vast majority of prior studies on the demographic consequences of armed conflict—including IPV (Ekhator-Mobayode et al., 2022; Gutierrez and Gallegos, 2016; La Mattina and Shemyakina, 2017; Østby, 2016; Østby et al., 2019; Svallfors, 2023; Torrisi, 2023), fertility (Castro Torres and Urdinola, 2019; Islam et al., 2016; Svallfors, 2022b; Thiede et al., 2020; Torrisi, 2020), family formation (Saing and Kazianga, 2020; Torrisi, 2022), and reproductive health (Svallfors, 2022a; Svallfors and Billingsley, 2019)—relied on the number of conflict events as a proxy for individual conflict exposure, which is likely to be measured with error and therefore be endogenous. In fact, concerns over measurement error in conflict exposure have already been raised in earlier scholarship (Ekhator-Mobayode et al., 2022; Gutierrez and Gallegos, 2016; Leon, 2012). Because of the endogeneity problem arising from measurement error, previous correlational studies tend to underestimate armed conflict's impacts.

Findings of our work add to the literature on the lasting influences of the Vietnam War. Miguel and Roland (2011) found no evidence that the aerial bombardment in wartime Vietnam had negative impacts on later poverty rates, consumption levels, literacy rates, and population density at the provincial and district levels. Adopting a similar analytic approach but using microdata from representative surveys, recent studies showed that the Vietnam War had adverse influences on household agricultural productivity (Appau et al., 2021) and health (Palmer et al., 2019; Singhal, 2019), whereas a few studies argued that exposure to the war early in life had positive impacts on Vietnamese people's civic engagement (Barceló, 2021) and entrepreneurial behaviours in adulthood (Churchill et al., 2021). In this paper, we provide new empirical evidence that the Vietnam War had another unfavourable consequence—it caused Vietnamese women to be more tolerant of domestic violence.

This study also deepens our understanding of domestic violence in Vietnam. Past research on this topic has considered sociodemographic factors associated with women's experiences and views on IPV (Trinh et al., 2016; Vung et al., 2008), men's perceptions of violence against wives (James-Hawkins et al., 2021; Krause et al., 2016; Yount et al., 2014), how female empowerment may backfire on women and lead to more domestic violence against them (Bulte and Lensink, 2019), and the difficulties female victims of IPV had in making decisions about divorce (Vu et al., 2014). Our paper complements this literature by demonstrating how domestic violence in contemporary Vietnam can be linked to the country's violent history of warfare, a question that has been raised before in a qualitative study conducted by Rydstrøm (2003), who postulated that traditional gender, patrilineal, and Confucian norms in Vietnam were magnified by individual war experience, which explains why Vietnamese women may condone domestic violence. Our results provide some quantitative evidence in support of her qualitative findings.

Apart from recognising the causal link between exposure to bombing and women's attitudes to IPV, we also explore three potential mechanisms—women's education and age at first marriage as well as spousal age difference. Similar to Torrisi (2023), our analysis of potential mechanisms does not seem to give sufficient supportive evidence that wartime bombing influenced how women in Vietnam perceived domestic violence through these proposed channels. In addition, there is a lack of evidence that these proximate variables confound the relationship between war exposure and IPV justification, which is inconsistent with La Mattina and Shemyakina (2017) who found that controlling for female education appreciably attenuates the coefficient of conflict exposure estimated from a model predicting women's tolerant viewpoints about domestic violence in sub-Saharan Africa. Instead, we find that women's education and age at marriage are negatively correlated with IPV acceptability, and such correlations are independent of the influence of the Vietnam War.

The IV strategy identifies the local average treatment effect (LATE), that is, the average effect of exposure to bombing on IPV acceptability among compliers, that is, women living in provinces that saw heavy bombing if they are close to the North-South border but little bombing if they are far away from the border. Thus, one may wonder how different the LATE would be from the average treatment effect (ATE) for the entire population that comprises both compliers and non-compliers. To assess this, we regress bombing intensity (log) on the distance to the 17th parallel and provincial-level controls included in the main analysis and look at the residuals. We find that Ha Giang[12] is the only province with an absolute value of residual greater than 2 ($=-2.64$). An interpretation of this is that Ha Giang is the only province that received more bombing during the Vietnam War than expected given its distance to the border. In other words, this province tends to be the always taker that would always receive more bombing even if it is located far away from the border. According to the demographic statistics provided by the General Statistics Office of Vietnam,[13] the 2011 population of Ha Giang was 746,600, which only accounts 0.85% of the total population. As a result, we believe that more than 99% of Vietnamese people lived in provinces that can be characterised as compliers and that the LATE would be comparable to ATE.

This paper has some limitations that need to be addressed in future work. First, due to lack of data, we are unable to examine the impact of war exposure on women's IPV experiences. What is more, the questions on IPV attitudes used in our study only concern physical violence (i.e., wife beating) but do not cover psychological and sexual harm to women, which may be more salient in the modern world. Thus, later large-scale household surveys on Vietnamese people should gather not only attitudes to but also actual experiences of domestic violence against women in order to provide a comprehensive understanding of this topic.

Second, individual migration history was not available in the data used in this paper as well as in the recent waves of the MICS, which prevents us from thoroughly examining the interplay between warfare and conflict-induced displacement. Given the pivotal role the Vietnam War plays in the history of Vietnam, population-based surveys, such as the Vietnam Health and Aging Study (Young et al., 2021) and the Vietnam segment of the World Value Survey (Barceló, 2021), have recently started to collect information on wartime and postwar migration trajectories and wartime place of residence, and household surveys focusing on sociodemographic characteristics should also include relevant questions in their questionnaires.

Last, our study only considers women's attitudes but does not examine men's due to data limitation. Because "IPV is inherently a relational event shaped by couple-level factors", Behrman and Frye (2021) recommended a dyadic approach to analysing couples' IPV attitudes and their correlates in conjunction. Pollak (2004) also suggested that women who grew up in violent homes (e.g., early-life war exposure) tend to marry men who also had similar exposure to violence. In subsequent studies, schol-

[12] Ha Giang is the far north province of Vietnam.

[13] Available at https://www.gso.gov.vn/en/population/.

ars should strive to examine whether joint husband-wife exposure to armed conflict affects couple-level concordance in IPV attitudes when such data become available.

Violence against women remains a serious concern in the present world, and the United Nations (2017) has called for the elimination of “all forms of violence against all females in the public and private spheres including trafficking, sexual and other types of exploitation” in the SDG5.2. Findings of this paper have important policy implications for Vietnam as we identify early-life exposure to armed conflict as a causal factor increasing Vietnamese women’s acceptance of IPV, which may help the Vietnamese government to design better interventions targeting people who suffered from Vietnam War experience and hence are more vulnerable to domestic violence. Our study also sheds light on another SDG target—goal 16 to “promote peaceful and inclusive societies for sustainable development, provide access to justice for all and build effective, accountable and inclusive institutions at all levels” (United Nations, 2017) by enhancing our understanding of the long-term impact of conflict violence on family and gender dynamics.

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Appendix C

Appendix to Chapter 3

Table C.1: Summary statistics of individual-level variables

	Main sample			Placebo sample		
	Mean	SD	%	Mean	SD	%
Attitudes towards IPV						
Accept IPV in at least one situation	0.389	0.488		0.385	0.487	
Accept IPV if:						
Going out without telling husband	0.167	0.373		0.145	0.353	
Neglecting children	0.277	0.448		0.273	0.446	
Arguing with husband	0.238	0.426		0.230	0.421	
Refusing to have sex with husband	0.108	0.311		0.103	0.304	
Burning food	0.048	0.213		0.050	0.219	
Individual-level control variables						
Age	41.7	4.2		26.2	4.8	
Marital status:						
Currently married			89			69
Formerly married			8			3
Never married			4			29
Household head's ethnicity (Kinh)	0.87	0.33		0.84	0.37	
Area of residence (rural)	0.54	0.50		0.55	0.50	
Region of residence:						
Red River Delta			16			15
Northern Midlands and Mountain area			16			17
North Central and Central Coastal area			17			15
Central Highlands			17			16
South East			18			20
Mekong River Delta			16			17
Household wealth:						
Poorest			16			19
Poorer			17			15
Middle			19			19
Richer			21			23
Richest			26			24

Notes: Main sample = women born in 1960–1975 (N = 4,614); Placebo sample = women born in 1976–1993 (N = 5,529). SD = standard deviation.

Table C.2: Summary statistics of province-level variables

	Mean	SD
Bombing intensity (log)	2.19	1.96
Bombing intensity (number)	28.98	50.09
Distance to the 17th parallel	5.05	2.03
Province-level control variables		
Pre-war population density (log)	4.62	1.25
South Vietnam dummy	0.50	0.50
Provincial latitude	17.87	5.42
Average precipitation	156.59	31.14
Average temperature	24.29	1.92
Provincial elevation:		
Proportion of land area 250–500 metres	0.13	0.14
Proportion of land area 500–1000 metres	0.14	0.17
Proportion of land area over 1000 metres	0.05	0.10

Notes: N (provinces) = 60. SD = standard deviation.

Conclusion

In this concluding chapter, I discuss how my work contributes to the literature.

Demography is the statistical study of populations and used to be focused primarily on describing and analysing macro-level population dynamics. Thanks to expanded availability of and access to detailed survey data as well as advances in quantitative methods and computer performance, we are now able to examine individual demographic behaviours and their determinants, such as education and income. Despite a promising future of micro-demographic approaches, some demographers, such as Ronald Lee, were worried that the "less macro, more micro" tendency in population studies made the discipline face the risk of "losing its core".[1] They also called for more research on macro-demography and micro-macro integration. In this thesis, I respond to their call in two ways. In Chapter 1, I analyse human sex ratio at birth over a long time span using time series modelling, which is a classic macro-demographic approach. In Chapters 2 and 3, I reveal how individual demographic and attitudinal outcomes—child mortality and attitudes towards family violence—can be linked to macro social contexts, such as educational expansion and state violence.

Demography is an old discipline, the history and thoughts of which can date back to antiquity. Over the course of its evolution as an academic field, many theories have been proposed and tested, such as the Trivers–Willard hypothesis I revisit in Chapter 1. I show that the established hypothesis may no longer hold using refined measurements and more comprehensive data. My findings add some non-significant evidence to the demographic and sociological literature, which is known for being less likely to publish statistically insignificant results. Moreover, this chapter also highlights the need for re-evaluation of existing evidence in support of old theories, arguments, and hypotheses because these previous results may lack robustness checks.

Demographers are good at depicting demographic trends and their associations with social changes. However, we learn from the introductory statistics course that

[1] https://u.demog.berkeley.edu/~rlee/papers/FormalDemog.pdf.

"correlation does not imply causation". Because of measurement error, omitted variable, and/or simultaneity, endogeneity appears in many social science questions, leading to biased estimates. In Chapters 2 and 3, I seek to identify the causal determinants of demographic and attitudinal outcomes using quasi-experimental designs, which rely on some unique social, political, and historical contexts across different countries. I find that in both chapters, the ordinary least squares estimates, which is the most commonly used method in sociological and demographic studies, tend to be biased upward due to omitted variable (Chapter 2) or be biased downward because of measurement error (Chapter 3). My findings emphasise the need for more careful treatment of endogeneity in demographic and sociological research.

The role of mother in child development has been highlighted in the demographic, sociological, and psychological literature. Chapters 1 and 2 add new empirical evidence to this line of research by demonstrating whether maternal stress affects children's biological sex and how maternal education improves children's survival.

Chapters 2 and 3 contribute to the life course literature. I show how early-life experiences, such as exposure to beneficial or disruptive events, have long-lasting influences on later-life outcomes. These two chapters can also be seen as empirical investigations into the consequences of historical events, such as policy and warfare.

In addition to academic interests, my thesis also sheds light on global sustainable development. At the beginning of the twenty-first century, the United Nations established eight Millennium Development Goals (MDGs) to facilitate global development. The MDGs have been achieved in many parts of the world by the target year 2015, but progress has not been made evenly across regions, and places like South Asia and Africa still lacked substantial advancements in many aspects of the MDGs. In 2015, the Sustainable Development Goals (SDGs) were formulated as a future global development framework to succeed the MDGs. Chapters 2 and 3 tackle various topics in global sustainable development, such as reducing child mortality, increasing access to education, achieving gender equality, and promoting peace. My findings unveil the complex interplay between different dimensions of the SDGs and suggest that these goals need to be jointly pursued.

When writing the thesis, I encountered two main challenges that need to be addressed in future studies. First, my thesis mainly considers women and maternal conditions, but the men and paternal side has largely been overlooked. Future surveys should collect data on men and examine men's influences, such as the impacts of husband on woman and the effects of father on children.

Another challenge is related to migration. Many well-known surveys, such as the Demographic and Health Surveys and the Multiple Indicator Cluster Surveys, do not collect respondent's migration history. Due to the lack of data on past residence, I have to use the current location of residence as a proxy when constructing georeferenced treatment variables. Future surveys should collect information on migration and residence, which enables us to more accurate treatment status for individuals.