First and Second Generation Impacts of the Biafran War

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Abstract

We analyze long-term impacts of the 1967-1970 Nigerian Civil War, providing the first evidence of intergenerational impacts. Women exposed to the war in their growing years exhibit reduced adult stature, increased likelihood of being overweight and obese, and earlier age at first birth. Exposure to a primary education program mitigates impacts of war exposure on education. War exposed men marry later and have fewer children. War exposure of mothers (but not fathers) has adverse impacts on next-generation child growth, survival, and education. Impacts vary with age of exposure. For mother and child health, the largest impacts stem from adolescent exposure.

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1 Introduction

"Starvation is a legitimate weapon of war,
and we have every intention of using it against the rebels."
Mr. Alison Ayida, Head of Nigerian Delegation, Niamey Peace Talks, July 1968

More than fifty years ago, the Nigerian Civil War captured the world's attention. The war raged in Biafra, the secessionist region in the southeast of Nigeria from 6 July 1967 to 15 January 1970. It was the first modern civil war in sub-Saharan Africa after independence and one of the bloodiest. About 1 to 3 million people died, mostly from starvation, sparking a massive humanitarian crisis. The levels of starvation in Biafra were three times higher than the starvation reported during World War II in Stalingrad and Holland (Alade, 1975; Mustell, 1977). War-induced food blockades led to severe protein shortages in the secessionist region and caused widespread malnutrition and devastation among adults and children (Miller, 1970). The Biafra war ranks as one of the great nutritional disasters of modern times.

As it was one of the first post-World War II tragedies, Biafra drew the widespread attention of global religious and political leaders and gained extensive coverage in the international media. It was the first African war to be televised, and millions around the globe witnessed the large-scale devastation that ensued in the war-torn region. As one Western observer noted: "This kind of tragedy was new to [television] viewers. Most hadn't seen a starving child in glorious Technicolor, looking like a matchstick, with a protruding stomach and the reddish-brown hair that signals a slow death from starvation" (Waters, 2005, p.697). The Biafran war triggered the rise of the international humanitarian movement, as several prominent relief organizations, including *Doctors without Borders*, were formed to deal with the war. However, relief efforts during the war were limited by the food blockades imposed by the country's military regime.

This paper extends the growing literature on the legacies of war by producing evidence of wars' long-term impacts and by producing the first evidence of war's intergenerational consequences. While negative impacts on the first generation could potentially translate into negative effects for the next generation, no studies have documented evidence of the intergenerational effects of conflict. Since the Biafra war was waged half a century ago, it also presents an opportunity to analyze whether development programs in the intervening years since the war have mitigated the short-run negative effects that are caused by wars. While immediate adverse human capital consequences of wars have been well documented, long-run impacts have received far less attention. Studies of the longer run economic effects of war have tended to conclude that economies are resilient, with capital stocks having been rebuilt (Waldinger, 2011). However, the available evidence suggests that human capital accumulation may be more permanently scarred following conflict, particularly in developing countries.[1] This is consistent with models of human capital formation in which initial endowments and early investments have persistent and self-reinforcing impacts on subsequent investments (Cunha and Heckman, 2008).

We examine impacts of the Biafran war on adult human capital (height, body mass index (BMI), and education). We then present estimates for second generation outcomes including child mortality (a marker of child health), anthropometrics, and education. We attempt to provide a relatively comprehensive picture by linking first and second generation impacts and presenting estimates for marriage, fertility, employment, and income for first generation war-exposed women and men.[2]

Our main identification strategy relies on evidence that ethnic groups indigenous to the

[1] Previous studies have documented long-run effects of exposure to stress (Camacho, 2008; Bozzoli and Quintana-Domeque, 2014; Quintana-Domeque and Ródenas-Serrano, 2014) and famine (Almond, 2006; Maccini and Yang, 2009; Meng and Qian, 2009; Gorgens et al., 2012), which are correlates of war exposure. Most existing research on war has focused on the human capital effects observed during or shortly after the conflict (Akresh and De Walque, 2008; Bundervoet et al., 2009; Shemyakina, 2011; Akresh et al., 2012b; Mansour and Rees, 2012; Akresh et al., 2014; Valente, 2014; Pivovarova and Swee, 2015). Several recent papers summarize the growing literature measuring the short-term impacts (Blattman and Miguel, 2010; Justino et al., 2013; Bauer et al., 2016; Brück et al., 2016). In contrast with those papers, a few recent studies explore the long-term impacts of conflict on human capital accumulation with some research finding no negative long-term effects (Miguel and Roland, 2011) and others finding significant long-term impacts (Akresh et al., 2012a; León, 2012; Justino et al., 2014; Akbulut-Yuksel, 2014; Palmer et al., 2016; Akbulut-Yuksel, 2017).

[2] All adult outcomes are, as in all related studies, conditional upon individuals having survived the war to adulthood. To the extent that survivors are positively selected, estimates of scarring are downward biased.

secessionist Biafra region (the Igbo being the most populous ethnic group) were the main victims of the war, which was fought entirely in Biafra. We interact an individual's ethnicity with the duration of exposure to the war.[3] Most studies documenting long-run consequences of childhood shocks restrict their analysis to *in utero* and early life exposure. We extend this strategy by adopting a flexible specification of exposure, allowing for age-specific effects across a wide range of ages, from the fetal year through adolescence.

Our main findings are as follows. Women exposed to the war carry the scars of exposure into adulthood, being shorter and more likely to be overweight than unexposed women. They start fertility at an earlier age, although we find no significant impacts on education, their age at marriage, or total fertility. We also find no significant impacts on employment or wealth. Their children are more likely to die in early childhood and, if their children survive, exhibit growth stunting. There is also some evidence that their children are less educated. We cannot comprehensively identify the mechanisms generating the second generation impacts, but we do find that war-exposed women are less likely to use prenatal care, skilled attendance at birth, and safe delivery facilities.

The damaging effects of girls' war exposure on their adult outcomes and on their children's outcomes are both increasing in age of exposure to the shock. Consistent with a growing literature, we do find harmful effects of war exposure *in utero* and early childhood. However, the adverse effects of war exposure are larger for women exposed during ages 13-16.[4] Adolescence is an age when a "second growth spurt" occurs (Case and Paxson, 2008b), and this makes it plausible that nutritional deficits at this age have large effects; further discussion of this is in section 3 where we also consider that the allocation of rationed food to children of different ages may be a function of the distribution of work across children of different

[3]Our results are also robust to using an identification strategy that is more typical in this literature and that focuses on geographical variation across regions that did or did not experience the fighting. The use of ethnic variation has the advantage of being less sensitive to selective migration than using geographical variation, as individuals will maintain their ethnicity if they migrate. In Section 5, we discuss in more detail the advantages of this identification strategy and in section 8 we show that results using geographical variation are comparable.

[4]The age profile of the second generation results lines up with that in the first generation results.

ages. Our findings cohere with recently increasing attention directed towards adolescence as a critical period in development (Caruso, 2017; León, 2012; Agüero and Deolalikar, 2012; Van den Berg et al., 2014).

The effects are large, even though measured so long after the end of the war. Women exposed to the war during adolescence for the average months of exposure (20.6 months) are on average 4.52 centimeters shorter, have a BMI that is 4.5 points higher, and are 18 percentage points more likely to be obese. Their children suffer increases of 1.5, 2.1, and 3.4 percentage points in neonatal, infant, and child mortality rates and if they survive are 14 percentage points more likely to be stunted and 22 percentage points more likely to be underweight.[5] We investigate whether exposure to the Universal Primary Education (UPE) Program initiated in 1976, six years after the end of the war, mitigated impacts of the war on education. We find that it did - by almost 70 percent in states where program resources were more intensively deployed.

We also examined long-run outcomes for war-exposed men. We do not have data on men's health, and we find no impact on their education, employment, or wealth. However men exposed to the war marry later, have their first birth later, and have fewer children overall. We find no deleterious impacts of father's exposure on child survival or indicators of child quality. This result is consistent with the condition of mothers being key in the intergenerational transmission of human capital.

This paper links two strands of the literature. The first strand is on the long-run impact of early life conditions on adult outcomes (Almond and Currie, 2011b,a) and critical periods for investments in children (Cunha and Heckman, 2007). A few recent studies in economics recognize that adolescence may be a critical period in height formation (Schultz, 2002; Case and Paxson, 2008b) but there is limited (causal) evidence of the consequences of nutritional

[5] Viewed alternatively, a one standard deviation increase in an adolescent's war exposure leads to a 1.5 percent decline in average adult height, a 10 percent increase in BMI, and a **100** percent increase in obesity rates. Their children have 12 percent higher neonatal, 8 percent higher infant, and 8 percent higher child mortality rates and if they survive are 17 percent more likely to be stunted and 37 percent more likely to be underweight.

deprivation at this age for other outcomes, and in particular, for next generation outcomes (Benny et al., 2017). The second strand is on the intergenerational transmission of human capital (Currie and Moretti, 2003, 2007; Almond et al., 2012; Bhalotra and Rawlings, 2013).

This paper makes the following contributions. First, it appears to be the first study that identifies next-generation impacts of war, providing somewhat unique evidence of the importance of maternal versus paternal war exposure, and some evidence of behavioral mechanisms. Second, it is one of the few papers to relax the common restriction that long-run human capital is most malleable in early childhood. This is important as it suggests a potential role for interventions in later childhood or that the scope for remediation is enlarged. Third, in providing evidence that expansion of a free primary schooling initiative mitigated the impacts of war exposure on education, it adds to still scarce evidence of how a positive intervention following a negative shock can (partially) offset its impacts; this is relevant to the double shocks literature (Almond and Mazumder, 2013; Adhvaryu et al., 2016; Rossin-Slater and Wüst, 2017) and also to a literature documenting pathways from health to educational attainment (e.g., Almond and Currie, 2011a). Finally, it presents estimates not only for measures of human capital but also for marriage and fertility that are relevant to next-generation impacts. However, we do not attempt to harness all results into a structural framework, and the array of results we present merits further analysis and integration.

The rest of this paper is organized as follows. Section 2 provides background on the Biafran war. Section 3 discusses the literature on early life shocks and the potential intergenerational transmission of those shocks. Section 4 presents the data. Section 5 presents the identification strategy. Section 6 presents results examining impacts of the war on a range of first and second generation outcomes. Section 7 shows results analyzing the effects of the mitigating effect of a free primary schooling program on first generation education outcomes. Section 8 shows results of a number of robustness checks. Section 9 concludes.

2 Biafran War

Sub-Saharan Africa has experienced a disproportionate share of conflicts, with nearly three-fourths of countries in this region having experienced armed conflict (Gleditsch et al., 2002). Nigeria is an important country in which to study the long term consequences of war. Termed the "Giant of Africa", Nigeria is the most populous and one of the largest economies of sub-Saharan Africa. The underlying causes of the Nigerian Civil War were complex (see Diamond, 1967; Sklar, 1967; Forsythe, 1969; Kirk-Greene, 1971; De St. Jorre, 1972; Nwankwo, 1972; Madiebo, 1980; Jacobs, 1987; Stremlau, 2015). Nigeria gained independence from Great Britain in 1960. Like many other African nations, at the time of independence Nigeria was composed of diverse religious, economic, cultural, and ethno-linguistic groups that were governed as semi-autonomous regions. Under colonial rule, Nigeria was a federation of three regions: northern, western, and eastern, each populated mainly by three distinct ethnic groups, Hausa, Yoruba, and Igbo respectively. The Northern region was predominantly Muslim while the Eastern region was mainly Christian; both religions were present in the Western region.

The fighting occurred principally in the Biafra region, located in southeastern Nigeria. On May 30, 1967, Biafra declared itself an independent state. The Federal Government declared this act of secession illegal. On July 6, 1967, which marked the onset of the war, federal troops invaded the eastern region. The Northern armies of the ruling power advanced into Biafra and pushed the Biafrans into a small enclave where food inflows were cut off. The result was extensive famine among the Igbos and other minority Biafran ethnicities (Miller, 1970). The Nigerian government did not initially allow international relief agencies to reach the secessionist region. In August 1968, the first international relief operations were launched but the amount of food proved insufficient to meet the demands of the affected groups (Aall et al., 1970). The war ended on January 15, 1970 with the defeat of Biafra. The secessionist region was characterized by a severe scarcity of proteins, as Biafra primarily cultivated yams and cassava, and vast numbers of children developed kwashiorkor.

Recovery interventions in the Eastern States began after the war ended, enabled by an oil boom that occurred in the 1970s, which provided an expansion in government resources. The Nigerian government and international agencies attempted to reintegrate the war-affected populations, repair the damage to physical infrastructure including health and educational facilities, and restore social services and public utilities to war-affected regions (Ukpong, 1975). In 1976 the federal government introduced the Universal Primary Education program (Osili and Long, 2008). As some of the war-exposed cohorts were also exposed to this schooling expansion, we investigate the extent to which the UPE directly mitigated any war impacts.

3 Mechanisms

A growing literature documents that investments made during critical periods of child development lead to larger returns or, conversely, that failure to invest can lead to irreversible damage (Cunha and Heckman, 2007). As the Biafran War was a major nutritional shock that occurred alongside reduced access to medical care and education, this paper is related to studies that have analyzed sharp disruptions in the mother's nutrition as a result of famine, for example the Dutch Hunger Winter (Stein et al., 1975) and the Great Chinese Famine (Almond et al., 2007; Chen and Zhou, 2007; Fung and Ha, 2010). However, these studies restrict their analysis to *in utero* and early life exposure.

The focus on the fetal environment stems from biomedical research suggesting that survival-motivated responses of the developing fetus to nutritional scarcity may get "coded" in an irreversible manner, with potential elevation of later life health risks (Barker and Robinson, 1992; Gluckman and Hanson, 2004). Economists have documented influences of fetal exposure on a range of socio-economic outcomes (Almond and Currie, 2011b). More recent work establishes that there remains sufficient developmental plasticity in the year after birth, with health shocks in infancy also being predictive of long-run socio-economic outcomes (Almond et al., 2017; Bhalotra and Venkataramani, 2013, 2015) and life expectancy (Bhalotra et al., forthcoming).

The reason that health and other investments in the early years matter is that physiological and neurological development is especially rapid in these years. There are two reasons for this. First, there is plasticity, the idea that shocks change the developmental path through altering tissue structure and metabolic and endocrine processes. Second, early childhood is a period of rapid growth and this makes young children particularly sensitive to nutritional deprivation.

In fact, individuals experience a second growth spurt in adolescence (Case and Paxson, 2008b). Growth tends to remain rapid from birth until age three, then stabilizes until adolescence at a rate of about 6 centimeters per year, after which it jumps to about 10 centimeters per year (Beard and Blaser, 2002).[6] Yet there is surprisingly little research investigating whether shocks in the adolescent years have impacts that persist through to adulthood. Although growth is rapid, there is less plasticity in some domains of development and so the question of whether shocks that strike in the adolescent growth spurt leave permanent scars is moot.

As detailed below, for the health and second generation outcomes, we find larger impacts of war exposure in adolescence than in early childhood. Although both are intense periods of growth, the increased nutritional demands may be greater for adolescents than for younger children given their larger baseline size and this is moot given food shortages during the war. Moreover, the intra-household allocation of food may reflect the intra-household allocation of work. Although younger children often receive preferential treatment when resources are scarce because their health is more sensitive to inputs, older children are more likely to work and so may need more food (see Bhalotra and Attfield, 1998). In addition, individuals who are stunted (nutritionally deprived) in infancy may experience catch-up growth as they

[6] Consistent with this age profile of the growth velocity, height-for-age Z-scores for girls in Ghana and Cote d'Ivoire fall below a reference population of well-resourced children before the age of three and after the age of eleven but they track the reference population in the intervening age interval (Moradi, 2010).

approach adolescence (Deaton, 2007; Benny et al., 2017) and so we may expect to see higher nutritional demands in late childhood in resource-scarce environments than is generally the case. An alternative interpretation of the identified age profile of scarring coefficients that we shall attempt to investigate is that it reflects the age profile of selective mortality, with mortality rates and hence selection decreasing in age.

Consider height and BMI, which are among the adult outcomes we analyze. Height is widely used as a marker of the stock of health that correlates well with self-reported measures of the stock of health and with life expectancy (Crimmins and Finch, 2006). A number of previous studies show that adult height depends upon early childhood nutrition (Deaton, 2007; Bozzoli et al., 2009; Banerjee et al., 2010). However, there is limited evidence of whether height responds to health shocks that occur in adolescence. In a notable exception, in a paper emerging in parallel with our previous work on the Biafran war (Akresh et al., 2012a), Agüero and Deolalikar (2012) investigate impacts of the Rwandan genocide across the age distribution and find an age profile similar to ours.

BMI varies through the life course and so is often used as a marker of short-term health, capturing contemporary stresses (Dercon and Krishnan, 2000). However, recent research highlights that adult obesity (BMI$\geq$ 30) may originate from fetal exposure to under-nutrition. Thus, it is more recently recognized that BMI may also act as an indicator of the long-run persistent effects of early life health. The biomedical literature suggests that if the under-nourished baby experiences a diet high in fat and sugar later in life, then it is mal-adapted for this and hence at risk of obesity, diabetes, and cardio-vascular disease (Barker and Robinson, 1992; Fung, 2010; Gluckman and Hanson, 2006; Gluckman et al., 2011a,b). In fact there is evidence of this in medical research on Biafra. Hult et al. (2010) use a limited sample of 40 year old Nigerian women who were exposed to fetal and infant malnutrition during the Biafran war and document that they had elevated rates of hypertension, diabetes, and obesity. However, we are unaware of attempts to investigate whether health shocks in adolescence matter for BMI.

We also analyze impacts of war exposure on education, which may be influenced directly through school closures or indirectly through impacts on health. We then look at downstream outcomes that may depend on human capital, namely, marriage, fertility, employment, and household wealth. The health indicators are only available for women, while the other outcomes are available for men and women. The same exposure may have different impacts on men and women for biological reasons and/or because of socially-conditioned differences in responses to the shock (Akresh et al., 2011).

As regards mechanisms for impacts of war exposure of mothers on children, since we find impacts of growing up in the war on multiple dimensions of the mother's human capital and we have just one instrument (war exposure), we cannot identify the pathways through which the mother's exposure impacts her children. However, previous work shows that mother's height and BMI are associated with risks of low birth weight and infant mortality in a large sample of developing countries (Bhalotra and Rawlings, 2011), and there is evidence that maternal obesity involves epigenetic processes that are transmitted to children (Dudley et al., 2011). Since we find no impacts of war exposure on income, it is unlikely that income is a pathway. Women's education may matter for example because it influences health seeking behaviors (Currie and Moretti, 2003). We investigate such behavioral mechanisms by modelling investments in early childhood inputs, prenatal care, and safe delivery as a function of war exposure.

4 Data

The analysis uses the Nigeria Demographic Health Surveys (DHS) of 1999, 2003, and 2008. These are large nationally representative cross-sectional surveys designed to provide demographic and health-related information. They contain data on women aged 15 to 49 at the time of the survey. We observe them respectively 29, 33, and 38 years after the war ended in 1970. The surveys also contain data on men aged 15 to 59 at the time of the survey.[7]

[7] The Data Appendix provides more details about the surveys, the sample, and the variables analyzed.

We restrict the sample to women born between 1954 and 1974. Those born before October 1970 were potentially exposed to the war from the fetal year to adolescence (depending on their ethnicity), and the control cohorts are those born between November 1970 and 1974. For first generation women, but not men, the data contain height and weight as indicators of health measured by trained surveyors. We have information on schooling, fertility, marriage, employment, and wealth outcomes for both women and men.[8]

The DHS contain fertility histories, which allow us to extend the analysis to second generation outcomes. The histories provide the gender and dates of birth and death (if relevant) of all children. The second generation child-level sample includes all children of the mothers in the first generation analysis (i.e., mothers born between 1954 and 1974). We restrict the analysis to children born after 1970 to avoid the possibility that the children are directly exposed to the war.[9] We have anthropometric data for surviving children born in the five years before the 2003 and 2008 surveys and the three years before the 1999 survey, so for those analyses, the sample includes only children born between 1996 and 2008, again, to mothers born between 1954 and 1974. For surviving children up to age 18 at the time of the survey we have information on their education.[10]

We explore child mortality for the second-generation, which reflects the health environment at birth and in the early years of life as well as maternal health conditions (Bozzoli et al., 2009; Bhalotra and Rawlings, 2013). We are able to measure neonatal, infant, and under-five child mortality. We also analyze impacts of maternal war exposure on child nu-

[8]The employment variable measures whether an individual worked in the past 12 months. Although this is a more short-term indicator of labor supply, existing research using the same indicator shows that it compares well across countries and years with the World Development Indicators aggregate data (Bhalotra and Umana-Aponte, 2010). The wealth index is constructed as part of the DHS survey data and is based on data about the household's ownership of consumer goods, dwelling characteristics, type of drinking water sources, toilet facilities, and other characteristics related to the household's socio-economic status. This index has been extensively used and exhibits the expected correlation with many variables (Filmer and Pritchett, 2001).

[9]Only 0.2 percent of children in the sample were born before 1970.

[10]By definition, our sample only includes adult individuals who survived the war. This will tend to lead us to under-estimate the war's impact (Chay et al., 2009; Bozzoli et al., 2009). We provide a discussion of potential age-specific selective mortality in the robustness checks section, in relation to a discussion of the age-profile of war impacts.

tritional status. The height-for-age Z-score is an indicator of linear growth retardation and cumulative growth deficits and it reflects the effects of persistent malnutrition (NPC, 2009, p.164). Children whose height-for-age Z-score is below minus two standard deviations are considered stunted and chronically malnourished. We examine child weight-for-age Z-scores, which reflect short-term malnutrition. Children whose weight-for-age is below minus two standard deviations are defined as underweight.

To investigate impacts on the second generation's educational attainment, we can only include information from the 2003 and 2008 DHS surveys as information on children's education is not included in the DHS 1999 survey.[11] The sample is restricted to children aged 6 to 18 years old at the time of the survey to account for a school starting age of 6 and any potential selectivity of children remaining at home.

Table 1 reports the summary statistics of the main variables of interest for the sample analyzed. Adult women are on average 159 centimeters tall with a BMI of 24. Ten percent of women are underweight while 31 percent are overweight and 9 percent are obese. In the sample of first generation women, the average years of schooling is 4.5, more than 50 percent have completed at least primary school, and 27 percent have completed at least secondary school. On average, women get married before age 18 and have over 5 children. The children of these women (second generation) suffer extremely high child mortality with 18 percent dying before 60 months, 38 percent being stunted, and 28 percent being underweight.

5 Identification Strategy

Our identification strategy exploits the timing of the war jointly with the fact that the war was entirely fought in the southeastern region of Nigeria (Biafra) where the Igbo and other minority ethnic groups originated. We use a difference-in-differences specification, defining an individual's duration of exposure to the war (in months) at a given age as a function of

[11] We transform years of education into a Z-score to account for the fact that some children may still be in school. We calculate the deviation of a child's completed years of schooling from the mean years of schooling for their age, gender, and survey round and divide this by the standard deviation in that three-way cell.

her ethnicity and birth cohort. An important distinguishing feature of the specification is that it allows impacts of war exposure durations to vary by age at exposure.

5.1 First generation impacts

For the first generation analysis, we estimate the following equation:

$$Y_{imcesr} = \beta_a months\ war\ exposure_{mc} * war\ ethnicity_e + \delta_a months\ war\ exposure_{mc} + \alpha_c + \theta_e + \lambda_s + \mu_r + \gamma_e.t + u_{imcesr} \quad (1)$$

The subscripts *i, m, c, e, s,* and *r* index an individual *i*, born in month *m* and year *c*, of ethnicity *e*, resident in state *s*, whose outcome, *Y*, is measured in survey round *r*. The outcomes are indicators of health, education, marriage, fertility, employment, and income. The independent variable of interest, *months of war exposure * war ethnicity* is constructed as a vector with age-specific coefficients. The equation includes the main effect of months of war exposure, allowing for the war to influence the control group, and it includes fixed effects for birth year, α_{c}. Although there is a strong overlap of ethnicity and state, they are not identical, so we include both ethnicity and state fixed effects, θ_{e} and λ_{s}. We control for ethnicity-specific time trends to allow for any underlying divergence or convergence in outcomes across ethnic groups over time. We also include survey round fixed effects, μ_{r}, and u is a random, idiosyncratic error term.[12] The coefficients β_a and δ_a vary with the age at which the individual was exposed to the war. The coefficients β_a indicate the causal impact of war exposure under the standard identifying assumptions in a difference-in-differences model. The coefficients δ_a capture any potential spillovers and tell us whether the non-war ethnicity groups suffered from the war. The full impact of the war on war-exposed ethnicities for a given age cohort is $\beta_a + \delta_a$.

[12] To allow for correlation among the error terms within ethnic group and birth cohort, we cluster all standard errors at the ethnicity and birth year level (Moulton, 1986). The statistical significance of the results is similar if we instead cluster the standard errors at the ethnicity level.

We define war ethnicity as being Igbo or another minority ethnic group (Adoni, Adun, Annang, Efik, Ekoi, and Ibibio) from the Biafran region. Most previous research studying the impact of war uses geographical variation in conflict intensity, but that approach can be sensitive to selective migration. By using ethnic variation to measure war exposure, we minimize this problem as people maintain their ethnicity if they migrate. In robustness checks discussed in Section 8, results are generally similar when we use regional variation to measure war exposure.

We use information on the month and year of birth of each individual to construct the duration of war exposure in months, which ranges from 0 months if they were never exposed to 31 months if they lived through the entire war. All individuals in the sample born between 1954 (the earliest birth cohort in the sample) and October 1970 are war-exposed. The war ended in January 1970, but individuals born before October 1970 would have experienced *in utero* exposure to the war. Non-exposed individuals consist of post-war births from November 1970 (ten months after the end of the war to avoid *in utero* war exposure) until December 1974. We select a narrow window for the control group since the potential for confounding events increases with window size.[13]

Our differentiation of war exposure by age allows for the possibility that being exposed to the war for any given duration may be more harmful at a critical age than at another age. To reduce measurement error in age, gain statistical precision, and address any potential confounding due to age heaping, we group ages into five categories, *in utero*, ages 0-3, 4-6, 7-12, and 13-16. Age banding also allows for slight variation in the timing of the early childhood and puberty growth spurts that even in non-war conditions will arise from differences in baseline socioeconomic status across girls (Case and Paxson, 2008a). The age bands are constructed with reference to the relationship between age and height growth (Case and Paxson, 2008b) and map reasonably well into preschool and school age thresholds.

Since we have several health and education outcomes, we adjusted for multiple hypothesis

[13] We explore sensitivity of our results to this decision about the appropriate control group cohort window, and our findings in general hold with a longer post-war cohort window.

testing following the *Hochberg* step-up method (Hochberg, 1988), which we discuss in more detail in Section 8. We also investigated replacing ethnicity, which is time invariant, with an ethnicity-specific time-varying measure that attempts to captures variation in war intensity over time, which we describe in Section 8.

5.2 Second generation impacts

Sufficient time has elapsed since the Biafra war that we are able to analyze whether it has cast a shadow so long as to impact the children of individuals who were exposed to the war in their childhood. To measure second generation impacts, we estimate the following regression:

$$\begin{aligned} Y_{otimcesr} = &\beta_a mother\text{'}s\ months\ war\ exposure_{mc} * war\ ethnicity_e \\ &+ \delta_a mother\text{'}s\ months\ war\ exposure_{mc} + \alpha_t + \alpha_c + \theta_e \\ &+ \lambda_s + \mu_r + \gamma_e.t + u_{otimcesr} \end{aligned} \tag{2}$$

The subscripts *o, t, i, m, c, e, s,* and *r* index a child *o*, born in year *t*, to a woman *i*, who was born in month *m* and year *c*, of ethnicity *e*, and resident in state *s*. The child's outcome, Y, is measured in round *r*. The main variables of interest, *mother's months of war exposure * war ethnicity* are defined as in Equation 1. The equation also includes the main effect of mother's months of war exposure, as well as fixed effects for the child's birth year, α_t, and the mother's birth year, α_c. We include a binary variable indicating if the child is female. All other terms on the right hand side have the same meaning as in Equation 1 and refer to the child's mother. Similar to the first generation analysis, we also investigated the effects replacing ethnicity with ethnic mortality or with regional exposure. We also estimate similar equations describing child outcomes as a function of the father's exposure to the war.

6 Results

We present the first-generation results for women and men, followed by the second-generation results. We find that the profile of coefficients by age of exposure of the mother for the outcomes of her children is similar to that for her adult outcomes. This supports our interpretation of the second-generation coefficients as arising via impacts due to the mother's war exposure. In each table, we present the coefficients for the interactions of duration of war exposure with an indicator for the exposed ethnicity.

6.1 First generation analysis

6.1.1 Health

Table 2 presents estimates for women of the baseline specification (Equation 1) for height, BMI, and indicators for being underweight, overweight, or obese. These health indicators are not available for men. The overall picture is of a systematic tendency for war exposure to reduce adult stature and increase BMI. Both are consistent with the war constituting a nutritional shock in the growing years. The effects on exposed individuals are large. We interpret this as a compelling statement of the ruthlessness of this war.

Critical Ages. A striking finding is that there are significant effects of war exposure right from birth (in the case of height) or the fetal period (in the case of BMI and obesity) through to age 16. For all outcomes, the effects on exposed individuals are increasing in age of exposure. Our expectation was that the coefficient profile would be U-shaped, with large effects at the start of life and in the teenage years, which are both growth spurts. Although growth is most rapid soon after birth (ages 0-3), an adolescent growth spurt starts around the age of 12 or 13 for girls (Case and Paxson, 2008a) and a few other studies find, as we do, that shocks at this age matter, (Agüero and Deolalikar, 2012; Van den Berg et al., 2014). In section 3, we discussed potential explanations of why scarring from exposure in adolescence may be greater than scarring from exposure in infancy.

Height. Girls who were exposed to the war between the ages of 0-3 for the average duration of exposure experienced by this cohort (17.5 months) suffered a reduction in adult height of 0.75 centimeters relative to unexposed girls of the same cohort.[14] A similar calculation for girls exposed to the war between the ages of 13-16 for the average duration (20.6 months) suggests a 4.52 centimeter deficit in adult height.[15] An alternative interpretation of the magnitude of the war's impact implies that a one standard deviation increase in an adolescent's war exposure leads to a 1.5 percent decline in average adult height.[16]

BMI. Results show that women exposed to the war in childhood (*in utero* through adolescence) exhibit a higher BMI and are more likely to be overweight (BMI above 25) as adults. Women exposed to the war are also more likely to be obese (BMI above and equal 30) as adults, in particular if they are exposed during adolescence. There is no effect of war exposure on being underweight (BMI below 18.5). These effects are largest for women exposed in adolescence, consistent with the results on height. Girls exposed to the war between ages 13-16 for the average duration have a BMI that is 4.5 points higher than non-war exposed girls, are 41 percentage points more likely to be overweight, and are 18 percentage points more likely to be obese. A one standard deviation increase in war exposure increased BMI by 2.4 points and the probability of being overweight or obese by 22 and 10 percentage points.

[14] All effects sizes presented here are calculated multiplying the average duration of exposure to war experienced by each cohort that belongs to the war ethnicity by its related β_a coefficient. Therefore, this measures the additional impact of war exposure for the war ethnicities (i.e., it measures the effect on exposed individuals belonging to the war ethnicity).

[15] The regressions also include the main effects of months of war exposure with age-specific coefficients, and in the height regressions, those coefficients are generally small and statistically insignificant.

[16] A common concern with difference-in-differences identification strategies is that there could be pre-trends in the outcomes by (exposed versus unexposed) ethnicity. Ideally, we could examine cohorts born before 1954 to show similar trends in height. However, the data does not have height information for cohorts born before 1954. The inclusion of ethnicity and state fixed effects and ethnicity specific time trends addresses the first-order concern. To assess this further, we also conducted the following placebo test. We estimate regressions using alternative placebo war dates (December 1983 to June 1986) assuming the war happened at a different time, which allows the same war duration (31 months) but 16 years later. We define the sample to include individuals born between November 1970 and 1990 so that there is no overlap in exposure with the real war. Table A.1 shows results for the first generation health outcomes. All coefficients in the height placebo regressions are close to zero and statistically insignificant. Some coefficients on BMI (column 2 and 4) are statistically significant, but negative, which is the opposite of the sign obtained when we use the actual war years.

As discussed in Section 3, the literature on fetal origins provides evidence that children exposed to nutritional shocks in early life may have smaller stature as adults and that the changes in early life metabolism generated by nutritional scarcity create the potential for maladaptation, leading to obesity in later life (Barker, 1998; Fung, 2010; Gluckman et al., 2009). However, in contrast to that literature we document impacts on height and obesity due to exposure beyond early childhood. In view of evidence that teenagers experience a second growth spurt, it may be that scarcity in this later growing period also has persistent effects. While the biomedical mechanisms for this are not well documented, there is related evidence in a few other studies (Caruso, 2017; Agüero and Deolalikar, 2012; Van den Berg et al., 2014).

6.1.2 Education

Table 3 shows estimates of the impact of war exposure on educational outcomes for women and men. Overall, the evidence indicates some adverse impacts of war exposure on women's education (columns 1 to 3), although the coefficients are mostly not significant.[17] We discuss below why we believe the coefficients for the youngest ages are likely to be underestimates of the impacts of war exposure. We see no evidence that the education of exposed men suffered (columns 4 to 6).[18]

Policy Remediation. Between 1976 and 1981, starting six years after the end of the war, Nigeria launched a nation-wide expansion in primary schooling known as the Univer-

[17]The average duration of exposure to war varies slightly across outcomes. We have reported these averages in the tables' notes.

[18]We are not able to conduct a similar placebo falsification test for education as we did for health because the cohorts assigned to the placebo groups could have been affected by the UPE program. Instead, we exploit the 1990 and 1999 DHS surveys, which contain information on the education outcomes of individuals born before 1950, to estimate an alternative placebo test. We define the sample to include women born between 1940 and 1950, with those women born 1945-1950 acting as the placebo war-exposed cohorts. Cohorts born after 1950 could have been exposed to the war at age 16 or younger and so are excluded from this placebo analysis. We regress education outcomes on the interaction between an indicator for women born from 1945-1950 and an indicator for women residing in the war region. Ethnicity information is not available in the DHS 1990 survey so we use regional variation to run this placebo test. We control for state fixed effects, year of birth fixed effects, and state time trends. The results in table A.2 show that the coefficients on the difference-in-differences terms are not significant in any specifications, which suggest that trends in education outcomes before the war were similar between war and non-war regions.

sal Primary Education program. The program gave all children six years of free primary education, facilitated by increased funding for school construction, hiring new teachers, and additional teacher training. This positive shock to education could have potentially mitigated some of the negative impacts of war exposure. Given the timing of when the UPE program began and when the war ended, only the youngest cohorts in the Table 3 regressions (*in utero*, ages 0-3, and ages 4-6) would have been of primary school age when the UPE began. These cohorts would have been 6-15 years old in 1976 at the start of the UPE program. It is possible that for these young cohorts who were subsequently exposed to the UPE program we under-estimate the impacts of war exposure on education. In contrast, the older cohorts (those ages 7-16 during the war) would have been 13-25 when the UPE program started in 1976 and hence would not have benefited from the UPE. In Section 7, we more formally examine the confluence of these two natural experiments (war exposure and UPE exposure) to analyze the extent to which the UPE mitigated war impacts on education outcomes.

6.1.3 Marriage, fertility, and socioeconomic status

The estimates in table 4 show results for marriage and fertility for women and men.

Marriage. For women, there is no consistent evidence that war exposure affected age at first marriage (column 1). However, men exposed to the war (at any age during childhood) marry at an older age (column 4), and the largest effects stem from exposure during adolescence (ages 13-16). Men experiencing the average duration of war exposure of 16.5 months at this age delay marriage by almost 6 years.[19]

The expected impacts on age at marriage for survivors of the war are theoretically ambiguous. Since more men than women died during the war, the relative scarcity of men could improve their marriage prospects. However, men are more likely than women to have developed a source of livelihood before they marry and achieving economic security may

[19] We also examined the effects of a woman's exposure to war on the age and education gaps between her and her husband. The results (available upon request) are generally not statistically significant.

have been more difficult for war-exposed men, contributing to delayed marriage.[20]

Fertility. War exposure influenced the age at which individuals had their first child for both women and men, but in opposite directions. Women exposed to the war during primary school ages for the average duration (22.7 months) marry at an earlier age (0.8 years earlier, although the coefficient is not statistically significant) and then have their first child 1.6 years earlier than non-exposed women. Among men, consistent with getting married at an older age, they have their first child at an older age (column 5). Depending on the age of war exposure, a man's first child is born 1.5 to 3.1 years later.

For both women and men, age at marriage and age at first birth tend to be positively associated with total fertility. However, we find no statistically significant impact of war exposure on the number of children ever born to women, and the estimated coefficients are small in magnitude (column 3). In contrast, war-exposed men have fewer children (column 6). Men exposed to the war between the ages of 7-12 for the average duration of 23 months get married 4 years later, have their first child 3 years later, and in total have 2 fewer children than unexposed men.

Socioeconomic Status. We also estimate Equation 1 to identify the effects of war exposure on employment and an index of household wealth (table 5). We find no statistically significant impacts for women or men. So, although women's health suffers from war exposure, there is no evidence that their employment or wealth is affected. Similarly, the lower fertility of men does not appear to follow from lower socio-economic status.

[20] "The huge human and material losses from the war made postwar adaptation difficult for older children. Some could not continue disrupted education because of abject poverty. In Abakaliki, boys in Enyinta's age group had to fend for themselves and their families as most bread winners were dead" (Uchendu, 2007, p.414).

6.2 Second generation analysis

6.2.1 Child health and education

Table 6 presents results estimating Equation 2 for the children of mothers growing up during the Biafran war.

Childhood Mortality. Maternal war exposure is associated with higher mortality rates of children (columns 1-3). These results are largest for exposure of the mothers during their adolescent years (ages 13-16). Mothers exposed in adolescence for the average duration (22.2 months) suffer increases of 1.5, 2.1, and 3.4 percentage points in neonatal, infant, and child mortality rates. These correspond to 28, 18, and 19 percent of the mean mortality rates. A one standard deviation increase in war exposure led to increases of 12.4, 8.0, and 8.3 percent in neonatal, infant and child mortality rates. Smaller effects for neonatal mortality rates are also observed for mother's war exposure at ages 4-12. In results (available on request) examining the mortality effects separately by child gender, the negative impact of mother's war exposure is 3 to 4 times larger for boys than girls, and the coefficients are only statistically significant for boys. This may be explained by the greater sensitivity of boys to the environment in early life. These results are striking, as they relate to second-generation children, all of whom were born after the war ended, in some cases decades later.

Growth Stunting. We also see the scars of maternal war exposure among second-generation children conditional on survival.[21] Columns 4 and 5 in Table 6 show that women exposed to the war during their adolescence have children who are more likely to be stunted and underweight in early childhood. War exposure at other ages has no persistent impacts on child health conditional on survival. Mother's war exposure has a much larger negative impact on the health of girls compared to boys (results not shown), consistent with parental investments favoring boys when resources are constrained.

Our finding that adolescence is a critical age for second generation outcomes reflects the

[21] This information is only available for children younger than 60 months at the time of the survey (younger than 36 months for data in the 1999 DHS survey).

age profile of coefficients for first generation outcomes. Women who had the average duration of exposure to the war in adolescence have children who are 14 percentage points more likely to be stunted and 22 percentage points more likely to be underweight.[22]

Education. Column 6 of Table 6 presents estimates of the impact of mother's war exposure on children's education. Children of mothers exposed to the war while they were *in utero* have a lower education Z-score. For mothers who experience the average duration of war exposure while *in utero* (7.3 months), education is reduced by 0.1 standard deviations. However, there are no statistically significant effects on children's education for mothers exposed to war during adolescence. This suggests that second generation child health and child education may be mediated by different factors. Impacts on education are similar for boys and girls.

6.2.2 Second-generation transmission mechanisms

Second-generation outcomes may be modified by war exposure of either the mother or father, working either through parental endowments (e.g., health, education) or parental investments (Cunha and Heckman, 2007). Other unobserved factors may also influence the intergenerational transmission process, including stress and genomic changes.

To identify potential mechanisms, we first exploit information available in the DHS sample, available only for recent births. We estimate reduced form regressions for prenatal care and delivery to investigate parental investments in children. This information is available for births in the five years preceding the survey in the 2003 and 2008 surveys and for births in the preceding three years in the 1999 survey.[23] We find that maternal exposure to the war

[22]We also estimate the same specification using height-for-age and weight-for-age Z-scores as continuous measures rather than using binary classifications of stunting and underweight. The results on the continuous height-for-age Z-score do not show any statistically significant effects of war exposure although the signs are negative. This may suggest that the intergenerational effect of the war had a large impact at the tails of the height distribution but for an average healthy child, there is no impact on height. The results on the continuous weight-for-age Z-score are consistent with the results using the binary variable and show that mother's exposure to war at ages 13-16 is correlated with their young children having a lower weight-for-age Z-score.

[23]This is the same sample used for the analysis of children's height-for-age and weight-for-age Z-scores. These cohorts born between 1996 and 2008 are more recent than the cohorts available for the child mortality

is negatively associated with the number of prenatal health clinic visits during pregnancy, receiving antenatal care from a skilled provider, receiving assistance during delivery from a skilled provider, and delivering in a health facility (Table 7). The effects sizes are large. Average exposure to the war at ages 7 to 12 decreases the number of prenatal visits by 1.42, the probability of receiving antenatal care by 8 percentage points, of receiving skilled assistance during delivery by 12 percentage points, and delivering in a health facility by 8 percentage points. These results are indicative but not conclusive that parental investments could be a potential channel as the age profile of impacts is not identical.

We have already shown that there were no significant impacts of war exposure on labor force participation or household wealth for women or men. To examine further the role of socio-economic constraints on second-generation child outcomes, we re-estimate Equation 2 for second generation health outcomes and include controls for the education of the husband and a wealth index. The estimates (results available upon request) are largely unchanged suggesting that socio-economic conditions do not seem to be the main mechanism linking first generation war exposure and second generation outcomes.

To explore whether exposure to the war by the mother or father has differential impacts on second generation outcomes, we construct a couples' sample and estimate separate regressions measuring the impact on second generation children of maternal war exposure and paternal war exposure.[24] Results are presented in Table A.3. Panel A shows the impacts of maternal war exposure while Panel B presents the impacts of paternal exposure to war. The results for maternal exposure in this reduced sample are broadly consistent with the main results in Table 6.

However, paternal exposure to war appears to have protective effects on children. Fathers exposed to the war *in utero* have children with lower mortality risk. We also find that fathers

analysis since mortality data are available from the complete retrospective fertility histories.

[24]We match each woman to her husband, so the analysis is restricted to currently married women, which partially explains why the sample size is smaller than in Table 6. In addition, only a sub-sample of households had both the husband and wife interviewed, which explains the further reduction in sample size in Panel B. Further details are provided in the Data Appendix.

exposed at age 4-12 have children with lower neonatal mortality risk and reduced chances their children are underweight. A potential rationalization of this finding is selective fetal survival, whereby surviving boys are selectively strong. It is plausible that we see this for paternal and not maternal exposure given evidence that boys are more vulnerable than girls in the fetal and infant years (Waldron, 1983; Almond et al., 2010; Eriksson et al., 2010; Hernández-Julián et al., 2014; Valente, 2015; Black et al., 2016). Alternatively these protective effects could stem from war-exposed men having fewer children overall, and from their education not being adversely affected by the war. Yet another possible explanation is that the men in the couples' sample are positively selected relative to the men in the full sample (indeed, we find they are more educated, marry later, have children later, have lower fertility, and are richer). In contrast, the means of the outcomes in the full sample of women are similar to the means for women in the couples' sample.[25]

Overall, the key finding here is that we find negative impacts of mother's, but not father's war exposure on their children. This suggests that transmission to the next generation is more likely through war-induced changes in health rather than in socio-economic conditions.[26]

7 Mitigating Effects of Universal Primary Education program

To examine whether the UPE program had a mitigating effect for those children exposed to the war, we estimate two regression specifications that attempt to isolate the separate effects

[25]We estimated Equation 1 for the education outcomes of men in the couples' sample to assess the role of sample selection in driving the first generation results for men (results not shown), but the results for education, marriage, and fertility of men are similar to the main results.

[26]The mother's health matters much more because she carries the child in the womb. On the contrary, the socio-economic status is usually transmitted through the father. However, data limitations do not allow us to disentangle if the likely health effect explaining next generation impacts is driven through biological factors such as the mother's height and weight or psychological factors such as stress or depression due to war exposure.

of the war and the UPE and any interaction between them. We continue to use the 1999, 2003, and 2008 DHS surveys, and we extend our previous sample to now include women and men born between 1954 and 1981, where previously it only included the 1954 to 1974 cohorts. Figure 1 shows how first generation women and men could be exposed to the shocks. We define four cohort groups: i) cohorts exposed to the war only, born 1954-1963 (aged 4-16 during the war), ii) cohorts exposed to the war and UPE, born 1964-1970 (between the fetal year and age 6 during the war and age 6-12 during the UPE), iii) cohorts exposed to only the UPE, born 1971-1975, and iv) cohorts exposed to neither shock, born 1976-1981, who constitute the control cohort. Given the complexity of cohort exposure, which is unbalanced by age, we do not use exposure duration, but instead we use binary indicators of exposure. We first estimate the following difference-in-differences equation:

$$Y_{icesr} = \beta_a cohort_c * war\ ethnicity_e + \alpha_c + \theta_e + \lambda_s + \mu_r + \gamma_e.t + u_{icesr} \quad (3)$$

The subscripts i, c, e, s, and r index an individual woman or man i, of birth year c, of ethnicity e, resident in state s, whose outcome is measured in round r. The main variable of interest is the interaction term $cohort_c$ * $war\ ethnicity_e$, where $cohort_c$ is a set of dummies equal to one if an individual belongs to respectively the war cohort, the war and UPE cohort, the UPE only cohort, or the cohort exposed to neither shock (which is our excluded reference group). The variable $war\ ethnicity_e$ is a dummy equal to one if an individual belongs to the war exposed ethnicity. All other terms on the right hand side have the same meaning as in Equation 1. The coefficients of interest are β_a and represent the difference-in-differences estimators of the impact of the war for war-exposed ethnicities and cohorts exposed to the war only, to both shocks, or to the UPE only (relative to being exposed to neither shock).

The results in Table A.4 Panel A show that cohorts of women exposed to only the war have lower education relative to cohorts exposed to neither shock. A one standard deviation increase in war exposure for the older cohorts belonging to the war ethnicities results in

0.49 lower years of education. For cohorts of women exposed to both the war and the UPE program, the negative impacts of war exposure are greatly reduced. The coefficient for the years of education outcome, while still negative, is about one-third the magnitude of the coefficient for the war only cohorts and is no longer statistically significant. In addition, the F-test testing the null hypothesis of the equality of these two coefficients is rejected with a p-value of 0.003. These results are suggestive that the UPE significantly mitigated the adverse effect of war on cohorts exposed to both shocks.[27]

Our finding that women's education suffered while men's did not is consistent with lower weight being placed on girls' education. Recent research argues that factors that can tilt the gendered impacts one way or the other include the specifics of the conflict, pre-war differences in education levels by gender, and labor market and educational opportunities in the absence of war (Buvinić et al., 2014; Akresh, 2016).[28]

We estimate a second regression specification in which we exploit additional variation in the intensity of the UPE program across regions in Nigeria. Osili and Long (2008) note that because of differing historical educational attainment rates across regions in Nigeria, the UPE budget and educational investments varied across the country with the aim of achieving convergence. We construct a dummy variable indicating the states that received the highest level of per capita federal funding for classroom construction, and separately estimate the difference-in-differences regression in Equation 3 for high intensity and low intensity UPE

[27]Cohorts of men exposed to only the war and belonging to the war ethnicity have more education. Those male cohorts exposed to both the war and the UPE program show small and statistically insignificant effects on education. In the main results discussed earlier, the coefficients for men were generally positive but not statistically significant, consistent with the coefficients being a weighted average of the results for these two sub-samples of men. A possible reason that war exposed men had more education is that they had weaker labor market prospects and had to get more education to compete for jobs with unexposed men. We have no clean explanation for why UPE exposure will have dampened this effect.

[28]To highlight these factors, a study of the Nepal civil conflict from 1996 to 2006 illustrates just how much difference the context can make when it comes to a conflict's effect on education (Valente, 2014). In districts that saw more casualties from the conflict, girls' educational attainment increased. But in districts that saw more abductions by the Maoist insurgents, who often targeted school children, the opposite was true. Similarly, Justino et al. (2014) finds that the Indonesian occupation in Timor Leste, improved education for girls and reduced it for boys in the long term, one reason being that boys were put to work during the conflict.

states.[29]

Table A.4 shows that in the low intensity UPE states, women exposed to only the war suffer a reduction of 0.88 years of schooling (column 1) and the effect is only marginally smaller for cohorts who were also exposed to the UPE program (0.68). We cannot reject the null hypothesis that these coefficients are equal, so where UPE federal funding was more limited, mitigation of the effects of the war was also more limited. In contrast, in the high intensity UPE states (which happen to overlap more with the war region), we observe a large negative and statistically significant effect of the war on years of education (-1.27). But now mitigation is greater for cohorts exposed to both the war and the UPE, the coefficient is reduced to -0.39 and is no longer statistically significant. A back of the envelope calculation shows that in the high intensity UPE states, the UPE program reduced the negative impacts of war exposure on education by almost 70 percent.

8 Robustness Checks

In this section, we present a set of specification checks. As discussed in section 5, we accounted for **multiple hypothesis testing**. Following the *Hochberg* method, which is a step-up version of the Bonferroni test, we control the Family Wise Error Rate through a procedure that "rejects all hypotheses with smaller or equal p-values to that of any one found less than its critical value" (Hochberg, 1988). The estimates in Table A.5, are in general robust to this correction and the profile of our findings is maintained.

Our main results use an indicator for ethnicity to measure war exposure. We also re-estimate Equations 1 and 2 with two alternative measures of war exposure. First, we investigate replacing ethnicity, which is time invariant, with an **ethnicity-specific time-varying**

[29] Low intensity UPE states are those in the former Western region, while high intensity UPE states are the rest of the country. Since the war was fought in the South East, the war region is a high intensity UPE region. Currently, Nigeria has 36 states and one Federal Capital Territory. The number of states has changed over time to improve equity in the revenue-sharing system at federal level. We map each individual's region at the time of the survey to match the 19 states that existed when the UPE program began in 1976 (Osili and Long, 2008).

measure that captures variation in war intensity over time (labeled ethnic mortality in the tables). We create this variable using information from the 2008 DHS survey that asked women to report the dates of birth and death of each of their siblings. We define ethnic mortality as the percentage change at the ethnicity level in sibling mortality rates between the war years (1967-1970) and the post-war years (1973-1976).[30] The constructed ethnic mortality variable is interacted with months of war exposure in the same way that ethnicity is in the baseline specification. Second, we replace ethnicity with **region**. We define the war-exposed region as the Eastern region states that were part of the former Biafra, with states in the former Northern and Western regions being in the control group.[31] We control for state-specific linear trends and cluster standard errors at the state-year level where year refers to the mother's birth year.

The results using ethnic mortality (Panel A) or war region (Panel B), in Tables A.6, A.7, A.8 and A.9 are broadly similar to the main results. Since the regional exposure variable is potentially sensitive to migration, these results suggest that migration is not an important contaminator of our estimates. Likewise, since the ethnic mortality variable captures time-varying factors and results are consistent, it is unlikely that these factors are contaminating our estimates.

Table A.6 presents results for women's health. Results across height, BMI, and overweight and obese status show a broadly similar pattern to the main results.[32] The magnitudes of the effects are in general comparable, although slightly larger for some of the health outcomes

[30]More precisely, the mortality rate during the war years is calculated as the ratio of the number of siblings who died during the war and the number of siblings born up to 1970. The post-war mortality rate is calculated as the ratio of the number of siblings who died after the war (between 1973 and 1976) and the number of siblings born up to 1976. The Data Appendix presents in more detail the construction of this indicator.

[31]Under colonial rule, Nigeria was governed as three distinct regions: Northern, Western, and Eastern. The states in the Eastern region are: Akwa Ibom, Anambra, Cross River, Imo, Rivers, Abia, Enugu, Bayelsa and Ebonyi.

[32]In order to calculate the effect sizes in Panel B, we calculate the average difference between war exposed and non-war exposed ethnicities for mortality in war time versus peace time (i.e. the ethnic mortality term). We then multiply this difference (0.12) by the average months of war exposure and by the estimated coefficient. Doing this calculation delivers estimates that are very similar to the coefficients in the main estimates.

using exposure based on war region.

Table A.7 presents results for women's and men's education using these two alternative war exposure measures. Results for men are consistent with the main results, showing no significant impacts of war exposure on educational outcomes. Results for war exposure for women during adolescence are also consistent, showing negative but statistically insignificant impacts of war exposure.[33] In contrast with the earlier results, we do find statistically significant and negative coefficients for women's education when using war ethnicity for those women who were exposed in early childhood (ages 0-3) or during primary school (ages 7-12).

Women's and men's marriage and fertility outcomes are presented in Table A.8. Results for men using these alternative war exposure measures show similar patterns with later age at first marriage, age at first birth, and fewer children ever born. Results for women for age at first marriage and total fertility are consistent across the different specifications. For women's age at first birth, the results using ethnic mortality now show significant effects across all ages of childhood exposure, while using war region shows only significant effects for *in utero* exposure.

Focusing on second generation children whose mothers experienced war during adolescence shows consistent results when using these alternative specifications. Results for second generation mortality indicators (columns 1 to 3 of Table A.9) are consistent with the main results when using war ethnicity as the exposure measure, but when using war region, the results are only robust for child mortality and are no longer statistically significant for neonatal and infant mortality. On the other hand, results using war region show a consistent pattern for stunting and underweight indicators, while the ethnic mortality definition does not.

We do not have information on an individual's state of birth but only on their state of residence at the time of the survey. This is why in our main specification we used ethnicity to

[33] Our finding that war exposure at ages 13-16 has no impact on years of education possibly reflects the fact that fewer children in these cohorts complete secondary school (only 12 percent of women and 31 percent of men in these cohorts complete secondary education.)

define exposure, ethnicity being invariant to migration across states. To understand better the significance of **migration** we analyzed it directly. A significant percentage of individuals in our sample (62 percent of women and 45 percent of men) respond to a survey question indicating they have migrated at some point in their lives, but the information on the exact timing of migration is not available so it is not possible to identify if the migration took place at the time of the conflict or decades later. From other household survey data, we know that most individuals have migrated within a given region (NPC, 2010), highlighting that any potential bias in our estimates is likely to be small. Nevertheless, we examine whether the probability of ever having migrated is influenced by war exposure (Table A.10), and we find that it is not. The coefficients are close to zero and not statistically significant.

Another concern could be **selective mortality**. Although we find scarring effects of early childhood exposure to the war on most outcomes, we suspect that these may be attenuated by positive selection of survivors. While selection effects are often small relative to scarring effects (Bozzoli et al., 2009; Almond and Currie, 2011a), this is less so when mortality rates are high (Deaton, 2007). In an account of his experience of visiting the Biafra region, a Red Cross Committee officer documented seeing almost no young children: "In many hardest-hit areas, one hardly sees children aged between six months and five years. This is something which is immediately noticeable in a population in which normally up to 50 percent or more are children aged below 15 years" (Aall et al., 1970).

So as to investigate this, first, following Chay et al. (2009) and Meng and Qian (2009), we assume that those who died were the least healthy members of the population or that selection is greatest in the lower tail of the health distribution. Then, pooling (as in the main analysis) the data for individuals exposed to the war at different ages, in Table A.11 we estimate the probability that the height of surviving women is below 150 centimeters (column 1) or 152 centimeters (column 2), roughly the 15th percentile, as a result of exposure to the war. We chose height as it is a measure of health. We find no significant impact of the war on the lower tail of the height distribution, consistent with attenuation of effects on account of survival selection.

An important finding in this paper is that war exposure in adolescence has more harmful consequences for adult health and second generation outcomes than exposure *in utero* or at other childhood ages. We investigate the possibility that this age gradient in impacts stems from **age-selective mortality**. As the risk of mortality, in general, and hence also on account of war, is declining in age, there will tend to be greater selection at younger ages. The literature on long-run effects of exposure to early life shocks routinely estimates scarring effects net of selection (Almond, 2006; Bozzoli et al., 2009) and some studies estimate bounds to assess the influence of selective survival on the estimates of interest (Alderman et al., 2011; Bhalotra and Clarke, 2016). However, most previous studies do not model exposure beyond early childhood. We are in the somewhat unique position of being able to use data on the mortality of siblings of the adult women. We previously explained how we used sibling death reports to create a time and ethnicity varying measure of mortality that approximates population-level mortality. Importantly, since the data record the birth and death date of siblings (if they are dead), we can estimate age-specific mortality by ethnicity and year. We restrict the sample to deaths occurring between 1954 and 1970 (these dates being the war exposed birth cohorts in our first generation analysis) and model age-differences in mortality conditional on death. The dependent variable is a dummy indicating deaths at age 0-3 (or 0-6) versus deaths at older ages. The independent variable of interest is an interaction between indicators for war-exposed ethnicity and death during the war (1967-1970) rather than before the war (1954-1966). We control for the main effects. We find that the interaction term is not statistically significant (see columns 1 and 2 in table A.12).[34] Thus, we find no evidence that war-related deaths were disproportionately of young children. However, it is possible that war deaths are not accurately recorded (Almond, 2006) and it is often the case that child

[34]We re-estimated the model but re-defined the dependent variable to be 1 if there is a death at ages 0-3 (or 0-6), as before, but now it is 0 if death is at an older age or if the individual is still alive. This variable is regressed on the interaction between an indicator for the sibling being of a war exposed ethnicity and an indicator for whether the sibling was aged 0 to 3 during the war years (i.e., the sibling was born between August 1964 and January 1970) versus the sibling being older (i.e., born before August 1964). The coefficients on the interaction term are again not significant (see column 3 and 4 of table A.12)

deaths are less well recorded. Therefore, we cannot completely rule-out that age-selective mortality can partly explain the age profile of the estimated coefficients.

Another concern is **endogenous fertility**, specifically whether total fertility was different during the war between the war-exposed and unexposed ethnicities. Using the sibling mortality data described earlier, we restrict the sample to siblings born between 1954 and 1970 for consistency with the sample used for the first generation analysis. We plausibly assume that respondent women have the same ethnicity as their siblings. We regress the total number of siblings born on an interaction between an indicator of war-exposed ethnicity and an indicator of birth during the war. Column 1 in Table A.13 reports the results while Column 2 reports estimates of the same specification but using the logarithm of the number of siblings born as the dependent variable. Columns 3 and 4 show results of specifications that control for the age of the oldest sibling, acting as a proxy for the mother's age and controlling for age and parity effects on fertility, and for state fixed effects. In all four specifications, the interaction term is small and never significant indicating there is no significant difference in fertility for the war-exposed ethnicity during the war years. This increases our confidence in our interpretation of the main results.

A closely related concern is **endogenous heterogeneity in fertility** or that certain types of women may have acted to defer fertility. If, for example, high-risk (less healthy) women were more likely to defer fertility during the war, then the composition of births during the war will be selectively lower risk. This would lead us to under-estimate the effects of the war. Similarly, if it is selectively low-risk women that deferred birth during the war, then we will tend to over-estimate impacts.[35] However, women require access to contraceptives in order to exert complete control over the timing of their births. According to the 1990 DHS survey (the oldest available DHS survey), only 9 percent of Nigerian women report using modern contraception. We can reasonably assume that usage was lower during the Biafran War. On this basis, we may expect that any bias deriving from the selective

[35]Bhalotra (2010) show that high-risk women are more likely to defer fertility during recessions.

timing of births is small. In addition, this potential bias would only be relevant for the youngest cohorts in the regressions, as the older cohorts were born before the war began, so estimates of war exposure during adolescence will not be affected.

9 Conclusions

The Nigerian Civil War triggered a massive humanitarian crisis in the southeast of Nigeria. It led to famine-like conditions and, in particular, severe shortages of proteins and calories. We find that exposure to the war during childhood had adverse consequences for the health and education of women. The shock of the war was followed fairly closely by the introduction of a massive primary education expansion. Analysis of the overlap of the two events shows that the education program mitigated the negative impacts of the war on women's education by almost 70 percent. We find no impacts on age at marriage or total fertility among women, but we find that they start fertility at a younger age, consistent with fewer years of completed education. Despite the adverse health and education effects, we find no evidence of effects on their employment or household wealth. We identify negative impacts on adult outcomes from war exposure between ages 0 to 16, with the largest marginal impacts tending to stem from exposure during adolescence.

We cannot estimate effects of war exposure on the health of men as the survey does not contain that data. However, we do have education, marriage, and fertility for men. We find no impacts of war exposure on men's education, but we find that war-exposed men marry later, have their first child later, and have fewer children in total. As for women, we find no impacts of war exposure on the employment or wealth of men.

Ours is the first study that shows that the effects of war exposure are transmitted to the next generation. We find that mothers exposed to the war have children who suffer from higher mortality rates, and conditional on surviving, then suffer worse health and education outcomes. The age profile of the effects of war exposure in the first generation lines up exactly with the age profile for the transmission of those effects to the second generation. We cannot directly identify mechanisms explaining the intergenerational transmission of war exposure, but the mother's health and possibly education are candidates as we document that they are lower for war-exposed women. We also find that war-exposed women make lower investments in prenatal care and safe delivery.

Although the Biafran war was fought half a century ago, our findings remain topical given the continuing prevalence of conflict around the world, and the finding that even when wars are over, their negative impacts are transmitted to the next generation. New child protection policies need to be developed to address both the intergenerational transmission of exposure to wars and the fact that war exposure outside of the first 1000 days of life has significant negative impacts.

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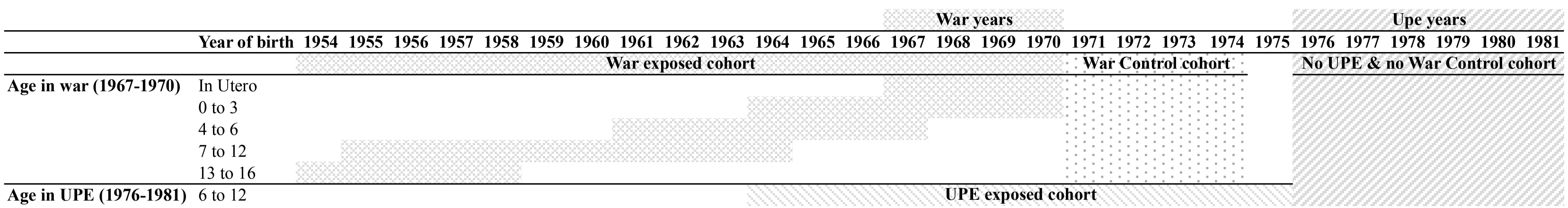
Figure 1: War and UPE Cohort Exposure



Tables

Table 1: Summary Statistics

	Obs.	Mean	St.dev.
Panel A: First generation - Women sample			
Height	13407	158.702	7.062
BMI	12385	23.637	4.960
Underweight	12385	0.098	0.297
Overweight	12385	0.308	0.462
Obesity	12385	0.096	0.294
Years of education	18284	4.489	5.269
Primary or more	18304	0.509	0.500
Secondary or more	18304	0.272	0.445
Age at first marriage	17715	17.897	5.079
Total number of children ever born	18304	5.429	3.026
Age at first birth	17214	19.685	4.819
Wealth index	13733	-0.040	1.032
Work participation	18283	0.752	0.432
Panel B: First generation - Men sample			
Years of education	7292	7.206	5.659
Primary or more	7312	0.723	0.447
Secondary or more	7312	0.459	0.498
Age at first marriage	6756	25.179	5.818
Total number of children ever born	7312	5.156	4.235
Age at first birth	5463	27.001	5.506
Wealth index	6023	0.044	1.062
Work participation	7308	0.811	0.391
Panel C: Second generation - Children sample			
Neonatal Mortality (<=1 Months)	99181	0.053	0.224
Infant Mortality (<=12 Months)	99181	0.114	0.317
Under 5 Mortality (<=60 Months)	99181	0.182	0.386
Stunted (Height-for-age Z-score<-2SD)	8622	0.379	0.485
Underweight (Weight-for-age Z-score<-2SD)	8622	0.277	0.447
Education Z score	26209	0.030	1.009

Notes: Summary statistics related to the health outcomes of the first generation include a sample of women born between 1954 and 1974 from the 2003 and 2008 DHS Surveys. The other summary statistics for first generation outcomes are related to a sample of women and men born between 1954 and 1974 from the 1999, 2003, and 2008 DHS Surveys. Summary statistics related to health outcomes for second generation children use the 1999, 2003, and 2008 DHS surveys and include all children born after 1970 to mothers who were born between 1954 and 1974 and who had at least one child. The sample for the height and weight-for-age Z-scores includes children born between 1996 and 2008 to mothers born between 1954 and 1974. The second-generation education outcomes use the 2003 and 2008 DHS surveys and include all children age 6-18 born to mothers who were born between 1954 and 1974 and who had at least one child.

Table 2: First Generation: Women's Health

	(1) Height	(2) BMI	(3) Underweight	(4) Overweight	(5) Obese
Duration of exposure to war x exposed ethnicity					
Months exposure in utero*War ethnicity	-0.047	0.066*	0.001	0.005*	0.000
	(0.038)	(0.034)	(0.002)	(0.003)	(0.002)
Months exposure at ages 0-3*War ethnicity	-0.043**	0.041**	-0.000	0.006***	0.002
	(0.021)	(0.020)	(0.001)	(0.002)	(0.001)
Months exposure at ages 4-6*War ethnicity	-0.061*	0.084***	0.001	0.007**	0.004*
	(0.034)	(0.032)	(0.001)	(0.003)	(0.002)
Months exposure at ages 7-12*War ethnicity	-0.094*	0.115**	0.001	0.011***	0.005
	(0.050)	(0.047)	(0.002)	(0.004)	(0.003)
Months exposure at ages 13-16*War ethnicity	-0.220***	0.219***	0.000	0.020***	0.009**
	(0.076)	(0.066)	(0.003)	(0.006)	(0.004)
Obs.	13407	12385	12385	12385	12385

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parentheses, clustered at the year*ethnicity level. The sample includes adult women born between 1954 and 1974 and surveyed in the 2003 and 2008 DHS surveys. War ethnicity is a dummy defined as 1 if an individual belongs to the Igbo or another minority ethnic group from the Biafran region. The variables for months of exposure *in utero*, at ages 0-3, 4-6, 7-12, and 13-16 are defined as the duration of exposure to the war in months during those age ranges. For the five age categories the average months of exposure for exposed women belonging to the war ethnicity is 7.3, 17.5, 17.9, 23, 20.6 respectively. In addition to the war exposure interaction term shown in the table, all regressions also include the level effect for months of war exposure at different age ranges, DHS survey dummies, year, state, and ethnicity fixed effects, and ethnicity specific time trends. Height is measured in centimeters. BMI is defined as an individual's weight in kilograms divided by their height in meters squared. Underweight indicates if BMI is below 18.5. Overweight indicates if BMI is above 25. Obese indicates if BMI is above and equal 30.

Table 3: First Generation: Women's and Men's Education

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
	Years education	Primary or more	Secondary or more	Years education	Primary or more	Secondary or more
Duration of exposure to war x exposed ethnicity						
Months exposure in utero*War ethnicity	0.008	0.000	0.000	0.042	0.006	0.008*
	(0.030)	(0.002)	(0.003)	(0.047)	(0.004)	(0.004)
Months exposure at ages 0-3*War ethnicity	-0.018	-0.001	-0.003*	0.007	0.001	0.001
	(0.018)	(0.001)	(0.002)	(0.021)	(0.002)	(0.002)
Months exposure at ages 4-6*War ethnicity	-0.005	-0.001	-0.002	0.003	0.004	0.001
	(0.027)	(0.002)	(0.003)	(0.031)	(0.003)	(0.003)
Months exposure at ages 7-12*War ethnicity	-0.051	-0.003	-0.008*	0.049	0.008**	0.007
	(0.040)	(0.003)	(0.004)	(0.046)	(0.003)	(0.004)
Months exposure at ages 13-16*War ethnicity	-0.031	-0.001	-0.007	-0.023	0.008	0.004
	(0.061)	(0.005)	(0.006)	(0.067)	(0.005)	(0.006)
Obs.	18284	18304	18304	7292	7312	7312

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors in parentheses, clustered at the year*ethnicity level. The sample includes adult women and men born between 1954 and 1974 and surveyed in the 1999, 2003, and 2008 DHS surveys. All of the control variables are as defined in Table 2. For the five age categories the average months of exposure for exposed women belonging to the war ethnicity is 7.3, 17.3, 18, 22.6, 22.3 respectively. For men, these averages are 7.5, 17.1, 18.6, 23.1, and 16.6, respectively. Years of education is defined as the number of years of completed schooling. Primary or more indicates if an individual completed at least primary school. Secondary or more indicates if an individual completed at least secondary school.

Table 4: First Generation: Women's and Men's Marriage and Fertility

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
	Age at first marriage	Age at first birth	Children ever born	Age at first marriage	Age at first birth	Children ever born
Duration of exposure to war x exposed ethnicity						
Months exposure in utero*War ethnicity	-0.036	-0.093**	0.020	0.117**	0.020	-0.044
	(0.039)	(0.036)	(0.019)	(0.051)	(0.056)	(0.031)
Months exposure at ages 0-3*War ethnicity	-0.034	-0.025	-0.004	0.115***	0.088***	-0.061***
	(0.022)	(0.019)	(0.011)	(0.032)	(0.033)	(0.014)
Months exposure at ages 4-6*War ethnicity	0.002	-0.035	-0.009	0.130***	0.107**	-0.056**
	(0.031)	(0.026)	(0.015)	(0.043)	(0.053)	(0.025)
Months exposure at ages 7-12*War ethnicity	-0.034	-0.072*	-0.006	0.184***	0.134*	-0.089***
	(0.050)	(0.041)	(0.022)	(0.066)	(0.073)	(0.033)
Months exposure at ages 13-16*War ethnicity	-0.013	-0.069	0.001	0.357***	0.199	-0.139***
	(0.071)	(0.059)	(0.032)	(0.103)	(0.149)	(0.047)
Obs.	17715	17214	18304	6756	5463	7312

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parentheses, clustered at the year*ethnicity level. The sample includes adult women and men born between 1954 and 1974 and surveyed in the 1999, 2003, and 2008 DHS surveys. War ethnicity is a dummy defined as 1 if an individual belongs to the Igbo or another minority ethnic group from the Biafran region. All of the control variables are as defined in Table 2. For the five age categories the average months of exposure for exposed women belonging to the war ethnicity is 7.4, 17.3, 17.9, 22.7, 22.3 respectively. For men, these averages are 7.5, 17.0, 18.9, 23.1, and 16.5, respectively. Children ever born indicates the number of children ever born alive.

Table 5: First Generation: Women's and Men's Work and Wealth

	Women		Men	
	(1)	(2)	(3)	(4)
	Work participation	Wealth Index	Work participation	Wealth Index
Duration of exposure to war x exposed ethnicity				
Months exposure in utero*War ethnicity	0.002	0.000	-0.001	-0.008
	(0.003)	(0.009)	(0.001)	(0.011)
Months exposure at ages 0-3*War ethnicity	-0.000	-0.004	-0.000	0.009*
	(0.001)	(0.004)	(0.001)	(0.005)
Months exposure at ages 4-6*War ethnicity	0.002	-0.003	-0.001	0.002
	(0.001)	(0.007)	(0.001)	(0.007)
Months exposure at ages 7-12*War ethnicity	0.002	-0.005	-0.002	0.007
	(0.002)	(0.010)	(0.002)	(0.011)
Months exposure at ages 13-16*War ethnicity	0.004	-0.005	-0.003	-0.015
	(0.003)	(0.014)	(0.002)	(0.018)
Obs.	18283	13733	7308	6023

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parentheses, clustered at the year*ethnicity level. The sample includes adult women and men born between 1954 and 1974 and surveyed in the 1999, 2003, and 2008 DHS surveys. War ethnicity is a dummy defined as 1 if an individual belongs to the Igbo or another minority ethnic group from the Biafran region. All of the control variables are as defined in Table 2. Work participation indicates whether the respondent worked in the past 12 months. The wealth index is constructed as part of the DHS survey and is based on data about the household's ownership of consumer goods, dwelling characteristics, type of drinking water, toilet facilities, and other characteristics related to the household's socio-economic status.

Table 6: Second Generation: Children's Health and Education

	(1) Under-1 month mortality	(2) Under-12 months mortality	(3) Under-60 months mortality	(4) Height for age below -2sd	(5) Weight for age below -2sd	(6) Education Z score
Duration of exposure to war x exposed ethnicity						
Months exposure of mother in utero*War ethnicity	0.409	0.376	0.544	0.001	-0.003	-0.013**
	(0.509)	(0.994)	(0.942)	(0.004)	(0.003)	(0.005)
Months exposure of mother at ages 0-3*War ethnicity	-0.166	-0.217	-0.082	0.001	0.000	0.003
	(0.196)	(0.339)	(0.359)	(0.001)	(0.001)	(0.002)
Months exposure of mother at ages 4-6*War ethnicity	0.375**	0.532	0.605	0.001	0.001	0.000
	(0.177)	(0.330)	(0.378)	(0.001)	(0.001)	(0.002)
Months exposure of mother at ages 7-12*War ethnicity	0.456**	0.373	0.638**	-0.000	-0.001	-0.000
	(0.217)	(0.267)	(0.314)	(0.001)	(0.001)	(0.002)
Months exposure of mother at ages 13-16*War ethnicity	0.671**	0.925**	1.531**	0.006**	0.010**	-0.000
	(0.296)	(0.418)	(0.620)	(0.003)	(0.005)	(0.004)
Obs.	99181	99181	99181	8622	8622	26209

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors in parentheses, clustered at the year*ethnicity level. Dependent variables in columns 1-3 measure under-1 month, under-12 months, and under-60 months mortality. They are dummies equal to 1 x 1000 if a child died respectively before one month, 12 months, or 60 months, and 0 otherwise. Column 4 defines stunting as height-for-age Z-scores below -2 standard deviations and column 5 defines underweight as weight-for-age Z-scores below -2 standard deviations. Column 6 defines an education Z-score as the deviation of the child's completed years of school from the mean years of school for children their age, gender, and survey round. In addition to the mother's war exposure interaction term, all regressions also include the level effect for the mother's months of war exposure at different age ranges, DHS survey dummies, year (mother and child), state, and ethnicity fixed effects, ethnicity specific time trends and a dummy for whether the child is a girl. For the five age categories, the average months of exposure in columns 1-3 for exposed women belonging to the war ethnicity is 7.4, 17.7, 18.0, 22.8, and 22.2, respectively. In columns 4-5, the averages are 7.1, 18.0, 17.4, 20.1, and 22.2, while in column 6, the averages are 7.3, 17.8, 17.8, 22.6, and 19.2. Columns 1 to 3 use the 1999, 2003, and 2008 DHS surveys and include all children born after 1970 to mothers who were born between 1954 and 1974. Columns 4 and 5 include children born in the five years preceding the 2003 and 2008 DHS surveys (and due to data limitations the three years preceding the 1999 DHS survey) to mothers born between 1954 and 1974. Column 6 uses the 2003 and 2008 DHS surveys and includes all children age 6-18 born to mothers who were born between 1954 and 1974.

Table 7: Second Generation: Potential Mechanisms

	(1) Number of visits during pregnancy	(2) Received antenatal care from skilled provider	(3) Received assistance during delivery from skilled provider	(4) Delivery in health facility
Duration of exposure to war x exposed ethnicity				
Months exposure of mother in utero*War ethnicity	-0.033	0.001	0.001	0.003
	(0.042)	(0.003)	(0.003)	(0.004)
Months exposure of mother at ages 0-3*War ethnicity	-0.027**	-0.003**	-0.003**	-0.003**
	(0.013)	(0.001)	(0.001)	(0.001)
Months exposure of mother at ages 4-6*War ethnicity	-0.014	0.001	-0.001	-0.002
	(0.024)	(0.001)	(0.001)	(0.001)
Months exposure of mother at ages 7-12*War ethnicity	-0.070***	-0.004***	-0.006***	-0.004***
	(0.015)	(0.001)	(0.001)	(0.001)
Months exposure of mother at ages 13-16*War ethnicity	-0.009	-0.000	0.004	-0.006*
	(0.034)	(0.003)	(0.003)	(0.003)
Obs.	8839	9556	13322	13236

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors in parentheses, clustered at the year*ethnicity level. The dependent variables measure the number of prenatal health clinic visits during pregnancy (column 1), whether the mother received antenatal care from a skilled provider (column 2), whether the mother received assistance during delivery from a skilled provider (column 3), and whether the mother delivered in a health facility (column 4). All of the control variables are defined as in Table 6. All regressions include children born in the five years preceding the 2003 and 2008 DHS surveys (and due to data limitations the three years preceding the 1999 DHS survey) to mothers born between 1954 and 1974.

A Online Appendix Tables

Table A.1: Robustness Check: First Generation Health - Placebo Tests

	(1) Height	(2) BMI	(3) Underweight	(4) Overweight	(5) Obese
Placebo duration of exposure to war x exposed ethnicity					
Placebo months exposure in utero*War ethnicity	-0.043	-0.043**	0.001	-0.002	-0.002***
	(0.035)	(0.019)	(0.002)	(0.002)	(0.001)
Placebo months exposure at ages 0-3*War ethnicity	0.004	-0.023*	-0.000	-0.002	-0.001
	(0.016)	(0.012)	(0.001)	(0.002)	(0.001)
Placebo months exposure at ages 4-6*War ethnicity	-0.000	-0.020	-0.000	-0.002	-0.001
	(0.024)	(0.019)	(0.001)	(0.002)	(0.001)
Placebo months exposure at ages 7-12*War ethnicity	-0.003	-0.049	-0.000	-0.004	-0.002
	(0.036)	(0.029)	(0.002)	(0.004)	(0.002)
Placebo months exposure at ages 13-16*War ethnicity	0.001	-0.097**	0.001	-0.010**	-0.004
	(0.054)	(0.039)	(0.003)	(0.005)	(0.003)
Obs.	26485	22823	22823	22823	22823

Notes:* p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parenthesis, clustered at the year * ethnicity level. The sample includes adult women born between 1970 and 1990 (1970-1985 for DHS 2003 and 1970-1990 for DHS 2008). Placebo exposure defined as exposure to placebo war years (December 1983 to June 1986) for women born between 1970 and March 1987. War ethnicity is a dummy defined as 1 if an individual belongs to the Igbo or another minority ethnic group from the Biafran region. The variables for months of exposure *in utero*, at ages 0-3, 4-6, 7-12, and 13-16 are defined as the duration of exposure to the war in months during those age ranges. In addition to the placebo war exposure interaction term shown in the table, all regressions also include the level effect for months of war exposure at different age ranges, DHS survey dummies, year, state, and ethnicity fixed effects, and ethnicity specific time trends. Height is measured in centimeters. BMI is defined as an individual's weight in kilograms divided by their height in meters squared. Underweight indicates if BMI is below 18.5. Overweight indicates if BMI is above 25. Obese indicates if BMI is above and equal 30.

Table A.2: Robustness Check: First Generation Education - Placebo Tests

	(1) Years of education	(2) Primary or more	(3) Secondary or more
Panel A: All			
Born in 1945-1950 * War region	-0.221	0.028	-0.052
	(0.615)	(0.076)	(0.043)
Obs.	1969	1974	1974
	Years of education	Primary or more	Secondary or more
Panel B: Women			
Born in 1945-1950 * War region	-0.014	0.024	-0.027
	(0.577)	(0.096)	(0.033)
Obs.	1611	1611	1611
	Years of education	Primary or more	Secondary or more
Panel C: Men			
Born in 1945-1950 * War region	-0.386	0.079	-0.160
	(2.553)	(0.162)	(0.221)
Obs.	358	363	363

Notes:* p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parenthesis, clustered at the year * state level. The sample in Panel A includes all individuals born between 1940 and 1950 surveyed in DHS 1990 and DHS 1999. In Panel B the sample includes all women born between 1940 and 1950 surveyed in DHS 1990 and DHS 1999. The sample in Panel C includes all men born between 1940 and 1950 surveyed in DHS 1999 (no information is available in DHS 1990 on men). Born in 1945-1950 * War region is the interaction between a dummy defined as 1 if an individual was born between 1945-1950 and 0 if born between 1940-1944. War region is defined as 1 if an individual resides in the war region and 0 otherwise. In addition to the war exposure interaction term shown in the table, all regressions also include year, state, and state specific time trends. Years of education is defined as the number of years of completed schooling. Primary or more indicates if an individual completed at least primary school. Secondary or more indicates if an individual completed at least secondary school.

Table A.3: Second Generation: Children's Health and Education - Mother and Father Exposure

	(1) Under-1 month mortality	(2) Under-12 months mortality	(3) Under-60 months mortality	(4) Height for age below -2sd	(5) Weight for age below -2sd	(6) Education Z score
Panel A: Duration of mother exposure to war x exposed ethnicity						
Months exposure of mother in utero*War ethnicity	0.626	1.901	2.187	0.001	-0.002	-0.019*
	(0.992)	(1.575)	(2.166)	(0.007)	(0.005)	(0.012)
Months exposure of mother at ages 0-3*War ethnicity	-0.554	-0.649	-0.168	0.002	-0.002	0.008**
	(0.429)	(0.628)	(0.894)	(0.003)	(0.002)	(0.004)
Months exposure of mother at ages 4-6*War ethnicity	-0.001	0.173	0.496	0.000	0.001	0.003
	(0.559)	(0.619)	(0.719)	(0.003)	(0.002)	(0.004)
Months exposure of mother at ages 7-12*War ethnicity	0.321	1.144*	1.624**	-0.000	0.002	-0.003
	(0.637)	(0.629)	(0.734)	(0.003)	(0.002)	(0.005)
Months exposure of mother at ages 13-16*War ethnicity	1.549**	2.041**	2.900**	0.017	0.007*	0.018
	(0.742)	(0.828)	(1.215)	(0.011)	(0.004)	(0.014)
Obs.	26390	26390	26390	2524	2524	7502
	Under-1 month mortality	Under-12 months mortality	Under-60 months mortality	Height for age below -2sd	Weight for age below -2sd	Education Z score
Panel B: Duration of father exposure to war x exposed ethnicity						
Months exposure in utero*War ethnicity	-6.807***	-6.808**	-7.364**	0.004	-0.007	0.021
	(1.855)	(2.786)	(3.519)	(0.008)	(0.011)	(0.022)
Months exposure of father at ages 0-3*War ethnicity	-1.115	-0.103	-0.297	-0.005	-0.007**	0.009
	(0.693)	(0.804)	(1.142)	(0.004)	(0.003)	(0.009)
Months exposure of father at ages 4-6*War ethnicity	-1.521**	-1.104	-1.629	-0.003	-0.005*	0.012
	(0.719)	(0.927)	(1.353)	(0.004)	(0.003)	(0.008)
Months exposure of father at ages 7-12*War ethnicity	-1.450**	-1.256	-1.532	-0.002	-0.005*	0.010
	(0.694)	(0.788)	(1.141)	(0.003)	(0.003)	(0.007)
Months exposure of father at ages 13-16*War ethnicity	-0.578	-0.042	0.498	0.005	0.001	-0.016**
	(1.269)	(1.314)	(1.548)	(0.006)	(0.004)	(0.008)
Obs.	16024	16024	16024	1911	1911	5121

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors in parentheses, clustered at the year*ethnicity level. Columns 1 to 3 use the 1999, 2003, and 2008 DHS surveys and include all children born after 1970 to mothers who were born between 1954 and 1974. Columns 4 and 5 include children born in the five years preceding the 2003 and 2008 DHS surveys (and due to data limitations the three years preceding the 1999 DHS survey) to mothers born between 1954 and 1974. Column 6 uses the 2003 and 2008 DHS surveys and includes all children age 6-18 born to mothers who were born between 1954 and 1974. The sample in Panel A is further restricted to children of mothers that could be matched to a husband (i.e. couple sample). The sample in Panel B, is further restricted to children of fathers born between 1954 and 1974. The dependent variables in columns 1-6 are defined as in Table 6. In Panel A (Panel B), in addition to the mother's (father's) war exposure interaction term, all regressions also include the level effect for the mother's (father's) months of war exposure at different age ranges, DHS survey dummies, child's year fixed effects, mother's (father's) year, state, and ethnicity fixed effects, ethnicity specific time trends and a dummy for whether the child is a girl.

Table A.4: First Generation: Women's and Men's Education - Accounting for UPE

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
	Years education	Primary or more	Secondary or more	Years education	Primary or more	Secondary or more
Panel A: All States						
War cohort * War ethnicity	-1.239**	-0.073	-0.112**	1.560*	0.131**	0.155**
	(0.580)	(0.050)	(0.056)	(0.870)	(0.060)	(0.076)
War and UPE cohort * War ethnicity	-0.385	-0.045	-0.044	0.299	0.006	0.033
	(0.359)	(0.033)	(0.034)	(0.608)	(0.046)	(0.050)
UPE cohort * War ethnicity	-0.323	-0.030	-0.052**	0.036	-0.003	0.010
	(0.209)	(0.019)	(0.024)	(0.421)	(0.029)	(0.033)
Obs.	29560	29596	29596	11365	11389	11389
	Women			Men		
	Years education	Primary or more	Secondary or more	Years education	Primary or more	Secondary or more
Panel B: UPE Low Intensity States						
War cohort * War ethnicity	-0.879	-0.051	0.122	2.831	0.159	0.160
	(2.310)	(0.148)	(0.204)	(2.389)	(0.158)	(0.244)
War and UPE cohort * War ethnicity	-0.676	-0.067	0.125	1.523	0.009	0.049
	(1.416)	(0.086)	(0.128)	(1.512)	(0.102)	(0.163)
UPE cohort * War ethnicity	-0.340	-0.020	0.025	1.837*	0.063	0.147
	(0.837)	(0.048)	(0.076)	(1.020)	(0.061)	(0.103)
Obs.	4786	4789	4789	1995	2001	2001
	Women			Men		
	Years education	Primary or more	Secondary or more	Years education	Primary or more	Secondary or more
Panel C: UPE High Intensity States						
War cohort * War ethnicity	-1.274**	-0.064	-0.128**	1.515	0.165**	0.156**
	(0.637)	(0.054)	(0.056)	(0.944)	(0.068)	(0.075)
War and UPE cohort * War ethnicity	-0.387	-0.042	-0.049	0.239	0.024	0.037
	(0.389)	(0.036)	(0.034)	(0.687)	(0.053)	(0.051)
UPE cohort * War ethnicity	-0.366	-0.029	-0.057**	-0.151	-0.002	-0.007
	(0.236)	(0.022)	(0.025)	(0.446)	(0.033)	(0.033)
Obs.	24774	24807	24807	9370	9388	9388

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parentheses, clustered at the year*ethnicity level. The sample includes adult women and men born between 1954 and 1981 surveyed in the 1999, 2003, and 2008 DHS surveys. Panel A includes the full sample, Panel B includes women and men living in UPE low intensity states, and Panel C includes women and men living in UPE high intensity states. War ethnicity is a dummy defined as 1 if an individual belongs to the Igbo or another minority ethnic group from the Biafran region. War cohort, war and UPE cohort, and UPE cohort are defined respectively as 1 if an individual was born between 1954 and 1963 (exposed to war only), between 1964 and 1970 (exposed to war and UPE), and between 1971 and 1975 (exposed to UPE only). Cohorts exposed to neither the war nor the UPE are the omitted category (born between 1976 and 1981). In addition to the war exposure interaction term, all regressions include DHS survey dummies, year, state, ethnicity fixed effects and ethnicity specific time trends. Dependent variables are defined as in Table 3.

Table A.5: Multiple Hypothesis Testing

Panel A: First generation women's health and education outcomes

	(1) Height	(2) BMI	(3) Underweight	(4) Overweight	(5) Obese	(6) Years of education	(7) Primary or more	(8) Secondary or more
FWER p-value (Months exposure in utero * War Ethnicity)	0.675	0.195	0.881	0.195	0.881	0.957	0.957	0.957
FWER p-value (Months exposure at ages 0-3 * War Ethnicity)	0.122	0.122	0.825	0.025	0.225	0.346	0.346	0.227
FWER p-value (Months exposure at ages 4-6 * War Ethnicity)	0.142	0.037	0.636	0.037	0.142	0.864	0.864	0.864
FWER p-value (Months exposure at ages 7-12 * War Ethnicity)	0.183	0.055	0.573	0.037	0.245	0.310	0.310	0.205
FWER p-value (Months exposure at ages 13-16 * War Ethnicity)	0.011	0.004	0.960	0.003	0.073	0.844	0.844	0.727
Observations	13407	12385	12385	12385	12385	18284	18304	18304

Panel B: Second generation health outcomes

	(1) Under-1 month mortality	(2) Under-12 months mortality	(3) Under-60 months mortality	(4) Height for age below -2sd	(5) Weight for age below -2sd
FWER p-value (Months exposure in utero * War Ethnicity)	0.705	0.705	0.705	0.698	0.674
FWER p-value (Months exposure at ages 0-3 * War Ethnicity)	0.819	0.819	0.819	0.504	0.641
FWER p-value (Months exposure at ages 4-6 * War Ethnicity)	0.102	0.109	0.109	0.491	0.491
FWER p-value (Months exposure at ages 7-12 * War Ethnicity)	0.085	0.163	0.085	0.988	0.988
FWER p-value (Months exposure at ages 13-16 * War Ethnicity)	0.027	0.027	0.027	0.031	0.031
Observations	99181	99181	99181	8622	8622

Notes: The table reports Family Wise Error Rate (FWER) p-values from a multiple hypothesis testing following the *Hochberg* step-up method. Panel A reports results on first generation women's health and education outcomes. The sample used and estimated specifications are the same as those reported in Table 2 and 3. Panel B reports results on second generation health outcomes. The sample used and estimated specifications are the same as those reported in Table 6.

Table A.6: Robustness Check: First Generation Women's Health, Alternative Measures of War Exposure

	(1)	(2)	(3)	(4)	(5)
	Height	BMI	Underweight	Overweight	Obese
Panel A: Duration of exposure to war x ethnic mortality					
Months exposure in utero*Ethnic mortality	-0.373	0.573**	0.007	0.040**	0.014
	(0.257)	(0.270)	(0.016)	(0.018)	(0.021)
Months exposure at ages 0-3*Ethnic mortality	-0.339***	0.164	0.003	0.018	0.011
	(0.127)	(0.156)	(0.007)	(0.012)	(0.012)
Months exposure at ages 4-6*Ethnic mortality	-0.522**	0.573**	0.007	0.039**	0.033*
	(0.217)	(0.259)	(0.011)	(0.019)	(0.020)
Months exposure at ages 7-12*Ethnic mortality	-0.866***	0.673*	0.010	0.046*	0.034
	(0.294)	(0.384)	(0.014)	(0.027)	(0.029)
Months exposure at ages 13-16*Ethnic mortality	-1.607***	1.469***	0.006	0.112***	0.078*
	(0.531)	(0.541)	(0.022)	(0.040)	(0.040)
Obs.	13407	12385	12385	12385	12385
	Height	BMI	Underweight	Overweight	Obese
Panel B: Duration of exposure to war x exposed region					
Months exposure in utero*War region	-0.059	0.035	0.000	0.003	-0.001
	(0.066)	(0.045)	(0.002)	(0.004)	(0.003)
Months exposure at ages 0-3*War region	-0.074**	0.030	0.000	0.004**	0.003
	(0.034)	(0.026)	(0.001)	(0.002)	(0.002)
Months exposure at ages 4-6*War region	-0.103**	0.080**	0.001	0.008**	0.005**
	(0.051)	(0.040)	(0.001)	(0.003)	(0.003)
Months exposure at ages 7-12*War region	-0.158**	0.110*	0.002	0.012**	0.007*
	(0.075)	(0.058)	(0.002)	(0.005)	(0.004)
Months exposure at ages 13-16*war region	-0.291***	0.228***	0.001	0.023***	0.012**
	(0.110)	(0.085)	(0.003)	(0.008)	(0.005)
Obs.	13407	12385	12385	12385	12385

Notes:* p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parentheses, clustered at the year*ethnicity (state) level in Panel A (Panel B). The sample includes adult women born between 1954 and 1974 and surveyed in the 2003 and 2008 DHS surveys. Ethnic mortality is defined as the percentage change at the ethnicity level in mortality rates during the war (1967-70) relative to a post-war period (1973-76). War region is a dummy equal to 1 if a woman resided in the war region and 0 otherwise. The variables for months of exposure *in utero*, at ages 0-3, 4-6, 7-12, and 13-16 are defined as the duration of exposure to the war in months during those age ranges. In addition to the war exposure interaction term, all regressions also include the level effect for months of war exposure at different age ranges, DHS survey dummies, year, state, and ethnicity fixed effects, and ehtnicity (state) specific time trends in Panel A (Panel B). Dependent variables are defined as in Table 2.

Table A.7: Robustness Check: First Generation Education, Alternative Measures of War Exposure

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
	Years education	Primary or more	Secondary or more	Years education	Primary or more	Secondary or more
Panel A: Duration of exposure to war x ethnic mortality						
Months exposure in utero*Ethnic mortality	0.016	-0.010	-0.005	0.082	0.025	0.040*
	(0.195)	(0.011)	(0.018)	(0.297)	(0.024)	(0.024)
Months exposure at ages 0-3*Ethnic mortality	-0.275***	-0.019**	-0.034***	0.014	0.003	0.001
	(0.093)	(0.008)	(0.009)	(0.168)	(0.012)	(0.014)
Months exposure at ages 4-6*Ethnic mortality	-0.193	-0.024*	-0.025*	-0.039	0.013	0.006
	(0.148)	(0.012)	(0.014)	(0.239)	(0.016)	(0.020)
Months exposure at ages 7-12*Ethnic mortality	-0.519**	-0.044**	-0.064***	0.127	0.031	0.035
	(0.221)	(0.019)	(0.022)	(0.343)	(0.022)	(0.028)
Months exposure at ages 13-16*Ethnic mortality	-0.354	-0.024	-0.062*	-0.194	0.026	0.039
	(0.362)	(0.030)	(0.036)	(0.540)	(0.033)	(0.046)
Obs.	18284	18304	18304	7292	7312	7312

	Women			Men		
	Years education	Primary or more	Secondary or more	Years education	Primary or more	Secondary or more
Panel B: Duration of exposure to war x exposed region						
Months exposure in utero*War region	-0.012	-0.002	0.002	0.009	0.005	0.009**
	(0.033)	(0.002)	(0.003)	(0.049)	(0.004)	(0.004)
Months exposure at ages 0-3*War region	-0.033	-0.002	-0.003	-0.001	0.001	-0.001
	(0.021)	(0.002)	(0.002)	(0.032)	(0.002)	(0.003)
Months exposure at ages 4-6*War region	-0.025	-0.002	-0.001	-0.019	0.004	-0.000
	(0.031)	(0.003)	(0.003)	(0.044)	(0.003)	(0.004)
Months exposure at ages 7-12*War region	-0.081*	-0.005	-0.006	0.034	0.009**	0.005
	(0.045)	(0.004)	(0.005)	(0.066)	(0.004)	(0.006)
Months exposure at ages 13-16*war region	-0.078	-0.004	-0.006	-0.070	0.007	-0.000
	(0.068)	(0.006)	(0.007)	(0.095)	(0.006)	(0.008)
Obs.	18284	18304	18304	7292	7312	7312

Notes:* p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parentheses, clustered at the year*ethnicity (state) level in Panel A (Panel B). The samples in Panel A and B include adult women and men born between 1954 and 1974 and surveyed in the 1999, 2003, and 2008 DHS surveys. Ethnic mortality is defined as the percentage change at the ethnicity level in mortality rates during the war (1967-70) relative to a post-war period (1973-76). War region is a dummy equal to 1 if a woman resided in the war region and 0 otherwise. All of the control variables are as defined in Table A.6. Dependent variables are defined as in Table 3.

Table A.8: Robustness Check: First Generation Marriage and Fertility, Alternative Measures of War Exposure

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
	Age at first marriage	Age at first birth	Children ever born	Age at first marriage	Age at first birth	Children ever born
Panel A: Duration of exposure to war x ethnic mortality						
Months exposure in utero*Ethnic mortality	-0.142	-0.388*	0.087	0.631*	0.128	-0.356*
	(0.232)	(0.224)	(0.122)	(0.337)	(0.363)	(0.195)
Months exposure at ages 0-3*Ethnic mortality	-0.396***	-0.319***	0.061	0.695***	0.542***	-0.335***
	(0.147)	(0.115)	(0.064)	(0.232)	(0.160)	(0.089)
Months exposure at ages 4-6*Ethnic mortality	-0.085	-0.319*	0.028	0.712**	0.669**	-0.436**
	(0.220)	(0.170)	(0.108)	(0.304)	(0.289)	(0.174)
Months exposure at ages 7-12*Ethnic mortality	-0.372	-0.543**	0.050	1.133**	0.937**	-0.591***
	(0.364)	(0.258)	(0.150)	(0.466)	(0.370)	(0.206)
Months exposure at ages 13-16*Ethnic mortality	-0.349	-0.640*	0.154	2.125***	1.335	-0.929***
	(0.505)	(0.374)	(0.213)	(0.722)	(0.883)	(0.318)
Obs.	17715	17214	18304	6756	5463	7312

	Women			Men		
	Age at first marriage	Age at first birth	Children ever born	Age at first marriage	Age at first birth	Children ever born
Panel B: Duration of exposure to war x exposed region						
Months exposure in utero*War region	-0.043	-0.096***	0.027	0.103	-0.025	-0.039
	(0.039)	(0.036)	(0.021)	(0.076)	(0.080)	(0.036)
Months exposure at ages 0-3*War region	-0.043*	-0.031	0.004	0.105***	0.085*	-0.046***
	(0.023)	(0.021)	(0.012)	(0.038)	(0.043)	(0.017)
Months exposure at ages 4-6*War region	-0.022	-0.036	0.001	0.119**	0.086	-0.042
	(0.032)	(0.031)	(0.016)	(0.047)	(0.065)	(0.032)
Months exposure at ages 7-12*War region	-0.051	-0.071	0.004	0.156**	0.083	-0.059
	(0.050)	(0.047)	(0.024)	(0.076)	(0.099)	(0.042)
Months exposure at ages 13-16*war region	-0.047	-0.079	0.018	0.304***	0.195	-0.109*
	(0.074)	(0.070)	(0.035)	(0.105)	(0.148)	(0.060)
Obs.	17715	17214	18304	6756	5463	7312

Notes:* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors in parentheses, clustered at the year*ethnicity (state) level in Panel A (Panel B). The samples in Panel A and B include adult women and men born between 1954 and 1974 and surveyed in the 1999, 2003, and 2008 DHS surveys. Ethnic mortality is defined as the percentage change at the ethnicity level in mortality rates during the war (1967-70) relative to a post-war period (1973-76). War region is a dummy equal to 1 if a woman resided in the war region and 0 otherwise. All of the control variables are as defined in Table A.6. Dependent variables are defined as in Table 4.

Table A.9: Robustness Check: Second Generation Health and Education, Alternative Measures of War Exposure

	(1) Under-1 month mortality	(2) Under-12 months mortality	(3) Under-60 months mortality	(4) Height for age below -2sd	(5) Weight for age below -2sd	(6) Education Z score
Panel A: Duration of exposure to war x ethnic mortality						
Months exposure of mother in utero*Ethnic mortality	2.935	6.191	5.732	0.018	-0.008	-0.116***
	(3.858)	(7.001)	(6.794)	(0.029)	(0.023)	(0.033)
Months exposure of mother at ages 0-3*Ethnic mortality	-1.083	-2.096	-1.178	0.013	0.005	0.019*
	(1.404)	(2.304)	(2.728)	(0.010)	(0.008)	(0.011)
Months exposure of mother at ages 4-6*Ethnic mortality	2.868**	5.633***	6.394***	0.008	0.006	-0.020*
	(1.263)	(1.882)	(2.112)	(0.010)	(0.008)	(0.012)
Months exposure of mother at ages 7-12*Ethnic mortality	2.492*	1.034	2.502	0.001	0.002	-0.017
	(1.497)	(1.993)	(2.264)	(0.009)	(0.008)	(0.014)
Months exposure of mother at ages 13-16*Ethnic mortality	4.971***	6.443**	8.915**	0.033	0.060	0.010
	(1.794)	(2.728)	(4.157)	(0.031)	(0.037)	(0.032)
Obs.	99181	99181	99181	8622	8622	26209
	Under-1 month mortality	Under-12 months mortality	Under-60 months mortality	Height for age below -2sd	Weight for age below -2sd	Education Z score
Panel B: Duration of exposure to war x exposed region						
Months exposure of mother in utero*War region	-0.497	-0.754	-0.340	0.000	-0.004	-0.015**
	(0.532)	(1.004)	(1.152)	(0.004)	(0.003)	(0.006)
Months exposure of mother at ages 0-3*War region	-0.013	0.084	0.186	0.001	0.001	0.002
	(0.195)	(0.316)	(0.396)	(0.001)	(0.001)	(0.002)
Months exposure of mother at ages 4-6*War region	0.430**	0.398	0.364	0.001	0.000	0.000
	(0.205)	(0.373)	(0.462)	(0.001)	(0.001)	(0.002)
Months exposure of mother at ages 7-12*War region	0.512***	0.479*	0.782**	0.002	-0.001	0.000
	(0.194)	(0.289)	(0.333)	(0.001)	(0.001)	(0.002)
Months exposure of mother at ages 13-16*war region	0.418	0.643	1.163*	0.007**	0.011**	0.003
	(0.398)	(0.538)	(0.667)	(0.003)	(0.005)	(0.004)
Obs.	99181	99181	99181	8622	8622	26209

Notes:* p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parentheses, clustered at the year*ethnicity (state) level in Panel A (Panel B). Ethnic mortality is defined as the percentage change at the ethnicity level in mortality rates during the war (1967-70) relative to a post-war period (1973-76). War region is a dummy equal to 1 if a woman resided in the war region and 0 otherwise. The variables for months of exposure *in utero*, at ages 0-3, 4-6, 7-12, and 13-16 are defined as the duration of exposure to the war in months during those age ranges. In addition to the mother's war exposure interaction term, all regressions also include the level effect for the mother's months of war exposure at different age ranges, DHS survey dummies, year (mother and child), state, and ethnicity fixed effects, and ethnicity (state) specific time trends in Panel A (Panel B), a dummy for whether the child is a girl. Columns 1 to 3 use the 1999, 2003, and 2008 DHS surveys and include all children born after 1970 to mothers who were born between 1954 and 1974. Columns 4 and 5 include children born in the five years preceding the 2003 and 2008 DHS surveys (and due to data limitations the three years preceding the 1999 DHS survey) to mothers born between 1954 and 1974. Column 6 uses the 2003 and 2008 DHS surveys and includes all children age 6-18 born to mothers who were born between 1954 and 1974. Dependent variables are defined as in Table 6.

Table A.10: Robustness Check: Probability of Migration

	(1) Health sample	(2) Education sample
Months exposure in utero*War ethnicity	-0.003	-0.002
	(0.004)	(0.003)
Months exposure at ages 0-3*War ethnicity	-0.000	-0.002
	(0.002)	(0.002)
Months exposure at ages 4-6*War ethnicity	-0.004	-0.003
	(0.004)	(0.003)
Months exposure at ages 7-12*War ethnicity	-0.005	-0.006
	(0.005)	(0.005)
Months exposure at ages 13-16*War ethnicity	0.003	-0.001
	(0.007)	(0.007)
Obs.	13261	18048

Marginal effects

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parenthesis, clustered at the year*ethnicity level. The sample in column 1 includes adult women born between 1954 and 1974 and surveyed in the 2003 and 2008 DHS surveys. The sample in column 2 includes adult women born between 1954 and 1974 surveyed in the 1999, 2003, and 2008 DHS surveys. War ethnicity is a dummy defined as 1 if an individual belongs to the Igbo or another minority ethnic group from the Biafran region. The variables for months of exposure *in utero*, at ages 0-3, 4-6, 7-12, and 13-16 are defined as the duration of exposure to the war in months during those age ranges. The dependent variable is a dummy equal to 1 if a woman ever migrated and 0 otherwise. In addition to the war exposure interaction term, all regressions also include the level effect for months of war exposure at different age ranges, DHS survey dummies, year, state, and ethnicity fixed effects, and ethnicity specific time trends. Estimates show marginal effects from probit regressions.

Table A.11: Robustness Check: First Generation - Women's Health

	(1) Height below 150cm	(2) Height below 152cm
Panel A: Duration of exposure to war x exposed ethnicity		
Months exposure in utero*War ethnicity	-0.001	-0.002
	(0.002)	(0.002)
Months exposure at ages 0-3*War ethnicity	-0.000	0.000
	(0.001)	(0.001)
Months exposure at ages 4-6*War ethnicity	-0.002	-0.001
	(0.002)	(0.002)
Months exposure at ages 7-12*War ethnicity	-0.001	0.000
	(0.002)	(0.003)
Months exposure at ages 13-16*War ethnicity	-0.000	0.001
	(0.004)	(0.004)
Obs.	13407	13407

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors in parentheses, clustered at the year*ethnicity level. The sample includes adult women born between 1954 and 1974 and surveyed in the 2003 and 2008 DHS surveys. War ethnicity is a dummy defined as 1 if an individual belongs to the Igbo or another minority ethnic group from the Biafran region. The variables for months of exposure *in utero*, at ages 0-3, 4-6, 7-12, and 13-16 are defined as the duration of exposure to the war in months during those age ranges. In addition to the war exposure interaction term shown in the table, all regressions also include the level effect for months of war exposure at different age ranges, DHS survey dummies, year, state, and ethnicity fixed effects, and ethnicity specific time trends.

Table A.12: Robustness Check: Selective Mortality

	(1) Sibling died at ages 0-3	(2) Sibling died at ages 0-6	(3) Sibling died at ages 0-3	(4) Sibling died at ages 0-6
War ethnicity*Sibling died during war years	-0.032	-0.009		
	(0.031)	(0.024)		
War ethnicity*Sibling aged 0-3 during war years			-0.002	
			(0.009)	
War ethnicity*Sibling aged 0-6 during war years				-0.001
				(0.010)
Sibling died in war years	Yes	Yes	No	No
Sibling aged 0-3 during war years	No	No	Yes	No
Sibling aged 0-6 during war years	No	No	No	Yes
War ethnicity	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
N	3981	3981	33552	33552

Robust standard errors in parentheses, clustered at the woman level. The sample includes sibling level data, which are siblings of surveyed women in the DHS 2008 survey. The sample in columns 1 and 2 includes siblings who died between 1954 and 1970. The dependent variable in column 1 (column 2) is a dummy equal to 1 if a sibling died between the ages of 0-3 (0-6) and 0 otherwise. War ethnicity*Sibling died during war years is defined as the interaction between a dummy equal to 1 if a sibling belongs to the war ethnicity and a dummy equal to 1 if a sibling died during the war years (1967-1970) and equal to 0 if she died before (1954-1966). The sample in columns 3 and 4 includes siblings born between 1954 and 1970 (which includes both survivors and those who died). The dependent variable in column 3 (column 4) is defined as a dummy equal to 1 if a sibling died at ages 0-3 (0-6) and 0 if a sibling died at an older age or if the sibling is still alive. War ethnicity * Sibling aged 0-3 during war years (sibling aged 0-6 during war years) is defined as the interaction between a dummy equal to 1 if the sibling belongs to the war ethnicity and a dummy equal to 1 if a sibling was 0 to 3 (0 to 6) years old during the war years.

Table A.13: Robustness Check: Endogenous Fertility

	(1) Number of sibling births	(2) Log (number sibling births)	(3) Number of sibling births	(4) Log (number sibling births)
War ethnicity*Sibling born in war	0.029	0.006	0.025	0.006
	(0.069)	(0.012)	(0.067)	(0.012)
War ethnicity dummy	Yes	Yes	Yes	Yes
Sibling born in war dummy	Yes	Yes	Yes	Yes
Year birth oldest sibling fixed effects	No	No	Yes	Yes
State fixed effects	No	No	Yes	Yes
N	33803	33803	33803	33803

Notes:* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors in parentheses, clustered at the woman level. The sample includes sibling level data, which are siblings of surveyed women in the DHS 2008 survey. It includes siblings born between 1954 and 1970. The dependent variable in columns 1 and 3 is defined as the total number of siblings born between 1954 to 1970 (columns 2 and 4 is the log of this variable). The term war ethnicity * sibling born in war is defined as the interaction between a dummy equal to 1 if the sibling belongs to the war ethnicity and 0 otherwise and a dummy equal to 1 if the sibling was born during the war years (1967-1970) and 0 otherwise. All specifications include also the non-interacted terms. Estimates in column 3 and 4 include fixed effects for the year of birth of the oldest sibling and state fixed effects.

B Data appendix

B.1 Data and samples

DHS survey sample

In the DHS 2008 survey, all women age 15-49 who were either permanent residents of the households in the DHS survey sample or visitors present in the households on the night before the survey were eligible to be interviewed. In a sub-sample of half of the households, all men age 15-59 who were either permanent residents of the households in the 2008 sample or visitors present in the households on the night before the survey were eligible to be interviewed (NPC, 2009). Similarly, in the DHS 2003 (DHS 1999) survey, a probability sample of households was selected and all women age 15-49 (10-49 in the DHS 1999 survey) identified in the household were eligible to be interviewed. In a sub-sample of one-third of the households selected for the survey, all men 15-59 (15-64 in the DHS 1999 survey) were eligible to be interviewed (NPC, 2000, 2004).

First generation sample

Health outcomes: For the first generation analysis on health outcomes we use the sample of women in the 2003 and 2008 DHS as health indicators are only collected for the sample of women. We do not use the DHS 1999 survey for the analysis of first generation health outcomes because the nutritional status and height of women is collected only for women who had a birth in the 3 years prior to the survey year, which would result in a non-representative sub-sample of interviewed women. The DHS 2003 includes women born since 1954 and the DHS 2008 those born since 1958. We choose the 1974 birth cohort as the upper bound in our sample in order to reduce possible confounding effects. Moreover, we need to include adult individuals that have completed their growth process.

Education, fertility, and marriage outcomes: For the analysis of education, fertility, and marriage outcomes, we use the sample of women and men in the three DHS surveys from 1999, 2003, and 2008. We include both women and men born between 1954 (1958 for the 2008 DHS survey) and 1974. As for the health outcomes we choose the 1974 birth cohort as the upper bound in our sample in order to reduce possible confounding effects. Moreover, we need to include adult individuals that have completed their education processes.

Second generation sample

We use the information on the fertility histories of mothers in the three DHS surveys from 1999, 2003, and 2008 to construct a child level sample for the analysis of second generation children. The 1999 DHS survey, while not containing information on health outcomes of all women, includes information on the year of birth and the state of residence of all women 15-49 years old. This additional dataset allows including mothers and children born in earlier years and hence covering additional cohorts. The sample we use for the second generation analysis includes all children born to mothers who had at least one child. The child-level sample is based on children of mothers born between 1954 and 1974 (i.e., this is consistent with the sample used for the first generation analysis). We further restrict the sample to children born after 1970 to avoid any confounding effect of the war on second generation children.

There is a likelihood of under-reporting of births and deaths of children not alive at the time of the survey because the mother may be reluctant to talk about deceased children. Children' deaths in early infancy may be those most likely under reported. However, as the proportion of neonatal deaths in our surveys is substantial, this may suggest that the under-reporting is not severe (NPC, 2000, 2004, 2009).

Second generation sample for education outcome

Children's educational attainment can be only retrieved in the 2003 and 2008 DHS surveys. The 1999 DHS survey does not contain the identifier that allows one to match the household level dataset (which contain information on education) with the child level dataset that we use for the analysis. Only children that are listed in the household level dataset and that are alive can be matched to the child level dataset. We further restrict the sample to children that are aged 6-18 at the time of the survey to account for potential selection of children that stay or leave home once they turn 18.

Couple sample

The couple sample is obtained by matching each woman in the women's sample to her husband in the male sample. We are only able to match men and women that live in the same household and that are currently married. The size of the couple sample is smaller than the full sample because, as explained above, men were only surveyed in a sub-sample of half of the households included in the women's sample. Men can have polygamous unions. We include all women that could be married to the same man.

For the second generation outcomes, we match women to their children, hence restricting the sample to women that had at least one child. For consistency with our main analysis we further restrict the sample to women that were born between 1954 and 1974. The father sample is further restricted to only men that were born between 1954 and 1974.

B.2 Variables definitions

BMI, underweight, overweight and obese: We look at BMI as a continuous measure and at overweight and underweight indicators. Pregnant women at the time of the survey are excluded from this sample. The underweight variable is a dummy defined as equal to 1 if the BMI of an adult woman is below 18.5 and 0 otherwise. The overweight indicator is defined as equal to 1 if the BMI is above 25 and 0 otherwise. The obese indicator is defined as equal to 1 if the BMI is above and equal 30 and 0 otherwise.

Education: We look at the years of completed education and at primary and secondary school completion. Primary school completion is a dummy defined as 1 if an individual has completed at least primary school and 0 otherwise. Similarly, secondary school completion is a dummy defined as 1 if an individual has completed at least secondary school and 0 otherwise. We use information on women and men. We choose for both women and men the same sample used for the health outcomes.

Ethnic mortality exposure: The information on siblings is reported only in the 1999 and 2008 DHS surveys but not in the 2003 DHS survey. In the 2008 survey, each woman was asked to report information on her siblings and their related date of birth and death. We use these data to construct mortality rates of siblings at different time periods. The sibling mortality rate at the ethnicity level during the war is calculated as the ratio of the number of siblings who died during the war (1967-70) and the total number of siblings born before 1970. Similarly, the mortality rate after the war is calculated as the ratio of the number of siblings who died after the war between 1973 and 1976 and the number of siblings born before 1976. We obtain a measure of ethnic mortality as the percentage change in siblings' mortality rates between the war years and post-war years as follows: *ethnic mortality*$_e$*=(mortality rate in war*$_e$ *- mortality rate after war*$_e$*) / mortality rate after war*$_e$.

Child mortality: We construct binary variables equal to 1 x 1000 if the child died within 1 month, 12 months, and five years after birth and 0 if the child is still alive or died afterwards. Since this sample includes children born between 1966 and 2008, we restrict the sample to children born after 1970 to avoid including confounding effects from the war years.

Height-for-age Z-score: This nutritional status indicator is expressed in standard deviation (SD) units from the median of the reference population as calculated in the NDHS/CDC/WHO Child Growth Standards (NPC, 2000, 2004, 2009). We define the height-for-age indicator as a binary variable equal to 1 if the height-for-age Z-score is below minus 2 standard deviations from the reference population median. Children below this threshold are considered stunted and so chronically malnourished. We define the weight-for-age indicator as a binary variable equal to 1 if the weight-for-age Z-score is below minus 2 standard deviations from the reference population median. Children below this threshold are considered underweight. Anthropometric data on height are only collected on children born in the five years prior to the 2008 and 2003 DHS surveys and in the three years prior to the 1999 DHS survey. Therefore, the sample includes children born between 1996 and 2008 from mothers born between 1954 and 1974.

Education Z-score: The outcome we analyze is the education Z-score that is calculated as the deviation of a child's completed years of education from the mean years of education and divided by its standard deviation calculated on a sample of children aged 6-18 years old. The average and standard deviation of the completed years of education are calculated at the DHS survey year, child age, and state level. The sample includes only children that are aged 6-18 years old.