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Are There Any Long-Lasting Human-Capital Effects from Exposure to the United States' Herbicide Bombings over Generations? Evidence from the Vietnam War

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Are there any Long-lasting Human-Capital Effects from Exposure to the United States' Herbicide Bombings over Generations? Evidence from the Vietnam War

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Abstract

This study analyses the long-lasting effects of the Vietnam War on the human capital of first and second generations after 50 years. Our focus is on Agent Orange, herbicide bombings used by the US military during the Vietnam War from 1962 to 1971. Although there is extensive research on the direct impacts of exposure to the war on education, health, and economic conditions, little is known about its outcomes on children born well after the war. Using the nationally representative household data in 2014, 2016, and 2018, combined with Agent Orange Data, this paper finds evidence that bombing exposure has long-lasting adverse effects not only on the affected generation but also on the children of those who experienced the conflicts. Overall, women tend to be more severely influenced by bombings than men, and the adverse effects on years of education are persistent in the second generation. In the first generation, there are also stronger effects on individuals exposed to the bombing after birth than those exposed in utero. Results based on 2SLS show that mothers' exposure to shocks during the prenatal period or after birth significantly affects the schooling level of their children, especially among the mother-daughter dyads.

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I. Introduction

Operational Ranch Hand - a well-known United States military operation adopting the herbicide bombings, called Agent Orange [2] during the Vietnam War - caused 400,000 deaths due to cancers and other illnesses over ten years between 1962 and 1971, making this operation alone one of the most damaging conflicts in Vietnamese history (Hynes, 2015). Its original claim to aerial defoliation at various strategic areas resulted in widespread malnutrition and devastation. Despite the large scale of the war and its significance in modern history, only a few studies have attempted to examine the long-term impacts of bombing in Vietnam (Miguel and Roland, 2011; Singhal, 2019; Le et al., 2022) with a focus on the impact of the first generation. At district and province levels, Miguel and Roland (2011) do not find statistically significant differences between bombed and non-bombed provinces during the Vietnam War in 1965-1975 in terms of poverty rate, consumption level or population density 30 years later. Focusing on health aspects, Le et al. (2022) found that exposed cohorts have a higher probability of suffering from heart disease, blood pressure disease and mobility disability even three decades later, while Singhal (2019) provides evidence of the severe effects of early life exposure on mental health in adulthood. [3] These

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[2] Herbicide mixtures were nicknamed by their colours, and Agent Orange was the most common mixture (see Stellman et al., 2003, for details).

[3] In a similar context, Guo (2020) and Yamada and Yamada (2021) examine the long-term first-generation effects of war in Laos. Using the distance from the Plain of Jars (POJ) in northern Laos or the Ho Chi Minh Trail (HCMT) in the south as an IV, Guo showed that the US's unexploded bombs on education, while Yamada and Yamada used the distance from HCMT as an

studies are part of the broader literature on the long-term effects of early-life exposure to wars or conflicts or large shocks, e.g., weather shocks or famines.

Drawing upon recent nationally representative household data and novel identification strategies following Miguel and Roland (2011) and Singhal (2019), this paper asks a question: Are there any long-lasting effects of Agent Orange during the Vietnam War on human capital - proxied by schooling years - of first and second generations after 50 years? We contribute to the emerging literature on the long-run effects of the Vietnam War in the following ways. First, we study the impact of Agent Orange on not only the first generation directly but also the second generation indirectly through their parents. This is important as it is challenging to identify the second-generation effects of wars due to the lack of data (Devakumar et al., 2014).[4] Second, we focus on its effect on educational attainment using nationally representative data. Third, unlike the previous works, we examine the heterogeneous impacts of herbicide bombings on educational outcomes at different stages of exposure during early life, including the fetal period, early childhood, preschool and primary

IV and showed that the effect of the US bombings on economic development of Laos, proxied by nighttime light emissions.

[4] A notable exception is Akresh et al. (2023) to be reviewed in Section 3.

school ages, in contrast to focusing on either before or after birth.[5][6] In doing so, we specifically focus on gender differences of the impact for both first and second generations. This is important because the gender inequality in education could be magnified if mothers were more vulnerable to herbicide bombings and their girls tended to be affected by the indirect effects. Finally, we identify the exposure to herbicide bombings by using the data of place of birth interacted with birth cohorts, rather than current place of birth as in the previous studies, to minimize the effect of migration.

While our study contributes to the specific literature on the impacts of the Vietnam War, from a broader perspective, it is placed in the growing literature on the legacy effects of conflicts in developing countries in several respects. Firstly, we examine the long-lasting consequences of the war at the individual level, while many studies in the literature used cross-country or cross-regional data to study the macroeconomic impacts in the long-term of

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[5] See the works documenting the concern that exposure to natural and economic shocks in the fetal period and early childhood not only affects individual health in the short run but also has long-lasting effects on educational and socioeconomic outcomes later in life (Strauss and Thomas, 2007; Carrillo, 2020), cognitive test scores, schooling attainment, employment, earnings and later. welfare outcomes (Almond, 2006; Rodrik and Rosenzweig, 2010; Almond and Currie, 2011; Leon 2012).

[6] There are relatively few studies comparing the effects of the shocks between these two important periods - in utero and early childhood. Evidence from the impacts of the Great Famine in China shows that exposure in utero reduces schooling by 0.58 years (Meng et al., 2015), while in Ethiopia, no evidence was found on the education of those exposed in utero except for those exposed after birth (Dercon and Porter, 2014).

the shocks (e.g., Blattman and Miguel, 2010; Miguel and Roland, 2011). Using the information of the year of birth and place of birth of individuals, we study the impacts of conflict at a more disaggregate level using household data, while other studies using the microdata assume that the current living place is the same as the birthplace.

Secondly, drawing upon notable contributions in the literature (Singhal, 2019; Le et al., 2022), our study adopts the quasi-experimental design to address the endogeneity by instrumenting parental exposure to shocks with the distance to the 17^{th} north latitude. We apply the Instrumental Variable (IV) estimation to the Difference-In-Difference (DID) specification utilizing the geographical distribution of herbicide bombings and age cohorts of the first generation. The IV would help to isolate the impacts of parents' experience of the bombing from the educational attainment of their children by controlling for unobserved characteristics correlated with both early life health and later outcomes.[7] The IV approach can also help to reduce the attenuation bias arisen from measurement errors in the US bombing data as noted in Stellman et al. (2003), up to 14% of herbicides were sprayed without information on coordinates so these data are not reported in the herbicide bombing data. Moreover, the herbicide use was largely concentrated in Central and South Vietnam from 1962 to 1971 while Northern areas did not have the same experience. In this study, we exploit two sources of variation, including geographical variation and birth cohort variation, to

[7] It is argued in the literature that there are unobserved characteristics correlated with both early life health and later outcomes (e.g., parents' preferences or behavior towards health). However, most of the existing studies do not control for unobserved factors that can result in misleading results (Chen and Zhou, 2007; Fung and Ha, 2010; Le´on, 2012; Dercon and Porter, 2014; Carrillo, 2020).

analyze the bombing effects on educational level of children whose parents were born or conceived or alive during the Operational Ranch Hand.

Thirdly, as noted above, we focus on not only the long-term effects on the first generation but the second generation, distinct from not only the literature on the effects of the Vietnam war but also the broad literature on the effects of wars or conflicts. Most of the works on the impact of wars have focused on the contemporaneous impacts, and studies on intergenerational transmission of the shocks are scarce due to data limitations (Devakumar et al., 2014). Our study contributes to the emerging literature on the long-term cross-generational effects of the war (Akresh et al., 2023). Another contribution is that we examine the heterogeneous impacts of 'shocks' on educational outcomes at different stages of exposure during early life, including the fetal period, early childhood preschool and primary school ages, in contrast to focusing on either before or after birth.

The remainder of the paper proceeds as follows. Section II describes the background of herbicide bombing during the Vietnam War. Section III then discusses possible mechanisms behind the cross-generational effects of wars on human capital through a selective review of theoretical and empirical works. Section IV describes the data sets used in this study, while Section V presents the empirical strategy. Section VI explains the results, and the last section offers concluding remarks and policy implications.

II. The Operation Ranch Hand during the Vietnam War

From July 1954, Vietnam was split into Northern and Southern areas as a result of the First Indochina War. The Communist-dominated organization *(Viet Minh)* controlled the northern part of Vietnam and later formed the Democratic Republic of Vietnam, while Southern Vietnam was ruled under President Ngo Dinh Diem's regimes chosen and supported by the US (Wiest, 2003). Under the agreement reached by the two parties in the Geneva Accords, a Demilitarized Zone (DMZ) was established at the 17^{th} parallel and remained open for 300 days with unrestricted movement to allow people to choose their political side. Initially, the Geneva Accords also set a general election in 1956 for peaceful reunification (*ibid*, 2003). However, with the desire to reunify Vietnam from both sides, the election was never held but turned into another raid called the Ranch Hands.

Operation Ranch Hand refers to a military code name for an Aerial Spray mission by the US Air Force in Southeast Asia from 1962 to 1971, with an estimated 19 million gallons of herbicides dropped on Vietnam (Buckingham, 1982). In November 1961, US President John F. Kennedy approved a request from President Diem of South Vietnam to conduct the aerial chemical attack, although the first major herbicide shipment did not arrive until January 1962. The scope of the program escalated rapidly over the years, and approximately more than 18 million gallons of heavily dioxin-based herbicides, mainly Agent Orange (see Figure 1) was sprayed on an estimated 20 per cent of South Vietnam's jungles (Stellman et al., 2003). The US only decided to officially cancel the program in January 1971 under increased political pressure from internal and international groups, although, due to the large stockpile of herbicides in the country, use around military bases and elsewhere continued (Frey, 2013).

[Figure 1 to be inserted around here]

The primary purpose of the bombings was to defoliate forests, clear perimeters of military installations and destroy crops to decrease the enemy's food supplies (Buckingham, 1982).

Initially, the use of herbicides required mission-by-mission approval as a limited experiment, then the restriction was gradually relaxed with higher frequency and larger coverage (*ibid.,* 1982). Thus, the cognitive effects of the bombing could be due to direct exposure to herbicides or indirect effects due to the lack of nutrition and its negative effects on health. The adverse shock is expected to affect educational attainment as well as return to education. These sources of variation in bombing intensity over the years and the geographical variation of birthplace help us identify the long-lasting effects of exposure on human capital. Historically, Vietnam experienced several adverse shocks from wars and famines. Among countries on the spectrum of natural disasters, several studies on Vietnam have found a positive association between the frequency of shocks and human loss (Noy and Vu, 2010). However, using the poverty trap model, Miguel and Roland (2011) found no evidence of robust long-run effects of U.S. bombing on poverty 25 years after the war in Vietnam. This somewhat surprising result is claimed to be because of relatively strong and centralized political institutions, post-war government interventions in regions that were more heavily bombed and inter-regional mobility (*ibid.,* 2011).

III. Mechanisms behind cross-generational effects of wars on human capital

In this section, we will provide a selective review of theoretical and empirical works to shed some light on possible mechanisms of how large aggregate shocks lead to lower human capital across generations. Direct impacts of a negative economic shock on households concerning the income aspect can be classified into the income and substitution effects. The income effect states that the adverse shocks to family income drive children out of school and make them more likely to be engaged in labor market activities as part of the household's risk-coping strategies. On the other hand, the substitution effect of the economic contraction makes the opportunity cost of continuing their education smaller, for instance, due to the lack of job opportunities or low wages during the recession, which would increase the demand for education. Given that the two effects offset each other, the decision to invest in education depends on three main factors: the severity and length of the crisis, the credit availability, and the existence of social safety nets (Torche, 2010). In Vietnam, there was virtually no form of social safety net or limited availability of credit as the crisis due to the US-Vietnam war became severe and prolonged. Thus, income effects were allegedly dominant and expected to cause a contraction in educational attainment.

In addition, the persistence of disadvantage across generations within families can also be derived from non-monetary factors through intergenerational transmission of health or education. Although the traditional neoclassical growth theory states that the destruction of war results in a loss of both physical and human capital, in the long run, the affected areas could catch up with the non-affected counterparts through capital accumulation due to public and private investment (Blattman and Miguel, 2010; Barro, 2015; Yamada and Yamada, 2021). However, the disadvantage of this model is that it does not elaborate on the pace of recovery of both affected and non-affected areas for convergence (León, 2012). The macro-level analyses do not consider the heterogeneity of the severity of the negative effects or the recovery at household or individual levels. Thus, understanding the extent to which the negative shocks of bombings could have long-run effects still depends on the country's context and requires further empirical studies based on household or individual data.

A number of studies have examined possible mechanisms linking parents' experiences with wars or conflicts and children's cognitive development. Existing studies show that the long-term effects of exposure to stress are associated with war (Camacho, 2008; Quintana-Domeque and Ro´denas-Serrano, 2014). For instance, using the panel data based on 781,000 birth records where mothers appeared more than once in 1998-2003 in Colombia, Camacho (2008) found that a baby experiencing conflicts in utero is on, average, 8.7 grams lighter, suggesting an adverse effect of the stress under conflicts of mothers on their children's nutritional status. Exposure to the war could directly impact children's education through school closure or indirectly impact health due to nutritional shocks or reduced access to medical care (Fung and Ha, 2010; Akresh et al., 2023). These early life shocks to health are then associated with decreased cognitive test scores and adult outcomes later in life (Rodrik and Rosenzweig, 2010).

However, few studies have tried to identify the impacts of parents' war exposure on the outcomes of children (Devakumar et al., 2014). A notable exception is Akresh et al. (2023). Using the Nigerian DHS data, Akresh et al. showed that childhood exposure to the Nigerian civil war negatively affected education outcomes in both first and second generations. This is important because natural science literature established several mechanisms whereby the nutritional or health conditions during early childhood negatively affect health and well-being in later life. A large aggregate shock, such as war or famine, cause malnutrition or ill health in utero or early childhood, which would negatively influence children's educational performances later on. If children were of school age when the large shock occurred, it would deprive them of educational opportunities through the destruction of schools. The lower human capital of the first generation directly influenced by the large shocks can be transmitted to the second generation, which we term the intergenerational effect (Cheema and Naseer, 2013; Azam and Bhatt, 2015; Emran and Shilpi, 2015; Torche, 2018; Emran et al., 2019).

Economists have modelled these mechanisms reflecting the broad science literature, epidemiology, epigenetics, or nutrition studies. The epidemiology and medical literature, for instance, has established the link between a mother's health/nutrition and her embryo or fetus, as well as the link between malnutrition and health in *utero* and early-life conditions, influence health and a variety of diseases in later life, for instance, because malnutrition in the perinatal period can significantly influence metabolism and hormone production later on (Gluckman et al., 2008). The early economics literature, however, did not pay much attention to this intergenerational transmission of health. For example, Grossman (1972), as discussed by Almond and Currie (2011), modelled the dynamic transmission of health stock over time (e.g., from childhood at t-1 to adult at t), assuming that the current health at t is influenced by the adult health investment at t, not the health investment at t-1. Almond and Currie (2011) and Currie and Almond (2011) emphasized the importance of considering a human capital investment in the life cycle and proposed a model where the health conditions in early childhood and investment will influence the health of adults. Cunha et al. (2006), Cunha and Heckman (2007), and Heckman (2007), on the other hand, suggested the cognitive and non-cognitive capabilities and health of children will be transmitted to those of adults in a complicated way, where investment in human capital at various stages is important and modelled the optimal life cycle investment considering the effect of the investment in early childhood on the formation of cognitive and non-cognitive capabilities. These studies have served as the theoretical foundations for empirical works on the intergenerational transmission of health and education (e.g., Ahlburg, 1998; Bhalotra and Rawlings, 2013; Dong et al., 2019). The studies provide broad backgrounds for our study examining whether the education of children of parents whose education or health was negatively impacted by bombing has also been negatively affected.

IV. Data and measurement

Household Data

The Vietnam Living Standards Measurement Survey (VHLSS) is a repeated cross-sectional survey conducted every two years by the General Statistics Office (GSO) with technical assistance from the World Bank. We have used three recent rounds of VHLSS in 2014, 2016 and 2018. Each survey has a sample size of 9,399 households across 3,133 communes and is representative at national, regional, urban/rural and provincial levels. In VHLSS, 50 per cent of communes are randomly selected and all households in these communes are retained in the following-up round. The advantage of the later rounds is that they contain data on both years of schooling and educational qualifications, as well as the professional occupation of all household members. The respondents in the survey are normally household heads. If the respondents have two or more children, we treat each child belonging to a corresponding household as a separate observation.

Following the literature studying the effects of shocks on human capital, we restrict our sample to young adults (i.e., the second generation) aged between 22 and 65 years old in all three rounds of surveys (i.e., 2014, 2016, 2018) as most of the individuals have completed their education before 22 years of age and years of schooling were relatively stable, which helps to reduce the right censoring of the education measure (Torche, 2018). To avoid including those who experienced the war directly, we further restrict the age of second generations to between 22 and 43. As a result, the sample consists of adults born between 1973 and 1996. This base sample is treated as 'children' and matched with their parents' data.

The final sample consists of 13,890 individuals (the base sample satisfying the age criteria described above) with 13,323 mothers and 10,963 fathers. The numbers of mothers and fathers differ as there are single families in the sample. To analyze the heterogeneous impacts associated with fathers' and mothers' exposure on children's education, we divide the sample into two subsamples for fathers and mothers.[8] Parental education is measured by the number of completed years of schooling.[9] Other demographic information such as gender, age, family status, regions, urban/rural areas, and educational level of grandparents are also included in the models.

Herbicide Bombings Data

The second dataset used in this study is constructed from the Herbicide Report System (HERBS) by the Department of Defense Military Assistance Command - Vietnam. HERBS compiled a logbook of primary source spray data, and the data are maintained by the Foundation for Worker, Veteran and Environmental Health, Inc., Columbia University. The Hamlet Evaluation System developed by the Advanced Research Project Agency (ARPA) is used to collect estimates of the population influenced by their exposures to herbicides during the Vietnam War. The main variables we are interested in are the detailed information on the intensity and types of herbicides

[8] There are six households in our sample that have two wives living within the same household, thus the maternal education is calculated as the average years of schooling of two mothers.

[9] Two main outcomes of education include years of schooling and the educational qualifications. As the questionnaire that provides this information only asks for twelve years of basic schooling, we combine information about their highest professional qualifications with 12-year education to construct the total years of schooling, assuming no grade repetition, as discussed in Hertz et al. (2008).

sprayed in Vietnam and used for their Military Herbicides project. We exploit the exact locations of the spray history from GPS addresses (i.e., the coordinates). Subsequently, the formatted address (i.e., province, district, commune, village, road) is obtained using the Google Map Geocoding Application Programming Interface. The province and district names are then labelled using a unified coding system with the VHLSS such that two data sets can be merged at the district level. Another advantage of data on herbicide bombing is that it comprises the exact timing of the bombing (i.e., date, month and year) that can be matched with the individual birth date. The key variables and the geographic coordinate data (i.e., longitude and latitude) are web-scrapped from The Agent Orange Data Warehouse.[10]

The data on bombing are then aggregated at the provincial level (58 provinces) for the period as a whole due to the unavailability of place of birth at the district level. The magnitudes of the bombing intensity are adjusted for the size of each province as measured by square kilometers. Figure A1 in the Online Appendix is the heat map that displays the geographical differences in bombing intensity across Vietnam. The darker shade corresponds to the higher total number of gallons of herbicides (e.g., Agent Orange) sprayed onto the areas during the whole period. Northern Vietnam (i.e., divided by the 17th parallel) was not sprayed during Operation Ranch Hand; thus, these areas have the lightest color.

One of the advantages of combining the VHLSS data with the HERBS data is that we can construct the duration (i.e., months) of their exposure to the war as VHLSS contains detailed information on the year and month of birth of each individual as well as the place of birth. This allows for heterogeneity in bombing exposure by age. Operation Ranch Hand lasted from February 1962 to December 1971. Thus, individuals in the sample who were born

[10] See http://www.workerveteranhealth.org/milherbs/new/ (accessed on 30/12/2023).

between February 1951 and August 1972 were exposed to the war as children. This accounts for those who were 11 years old or under when the war began including those who were exposed to the bombing *in utero*. To allow variation in the timing of exposure in early childhood, we divide the parents' sample into 4 cohorts, including the prenatal period, ages 0-3, 4-6 and 7-11..

However, the concern of using solely place of birth and the date of birth for identification may arise from the selection into the migration of individuals where people move to places with less bombing intensity. Unfortunately, the VHLSS data do not contain retrospective information on location during and after the war, and only information on place of birth and place of current residence are available. In our sample, approximately 7.3% of the total number of observations have their current locations different from their place of birth. Furthermore, the coefficient of correlation between individual migration and bombing intensity is only 0.09, and the estimates are robust after reapplying the analysis to the subsample without individual migrants (see Section 6). That indicates that migration does not have a significant influence on our results.

We also construct the variable based on the total number of months that individuals were potentially exposed to the bombing defined by (i) the month and year of birth of the individual and (ii) the period when Operation Ranch Hand lasted from February 1962 to August 1972. This measure allows us to distinguish further whether a child was *in utero* (i.e., 9 months prior to the month of birth) or early childhood during the war, revealing the total number of months exposed to the conflict before and after birth. It also allows us to identify the spill-over effect by including this measure (total months of exposure) and its interaction with the intensity of bombing.

V. Empirical Strategies

We divide our empirical analysis into three parts. First, we apply the Ordinary Least Squares (OLS) model for the DID specifications to investigate the direct effects of exposure to herbicide bombing on the human capital of the first generation (i.e., the parents' generation) and then further examine the heterogenous conflict experiences, focusing on age and gender differences when people were exposed to bombings in their early childhood. Second, we use nearly identical econometric specifications/models to examine the long-lasting effects of Operational Ranch Hand on the human capital of the second generation. Lastly, we employ an IV model for the DID specification to control for unobserved factors affecting our estimates.

First-generation Effects (DID based on OLS)

Our approach to estimating the effects of exposure to Agent Orange on the educational outcomes of the first generation (i.e., parents) relies on the natural experimental design. By making use of the geographical variation from the province of birth and the cohort variation from the year and month of birth (i.e., cohorts born between 1945 and 1981)[11], the two-way fixed effect specification is applied to estimate the average treatment effects (ATE) in the following equation:

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[11] It is ideal to use a finer geographical unit, such as districts (Singhal, 2019) or communes (Le et al., 2022), to match household data, but Le et al. (page 4) noted: "According to the 2016 VARHS, 77% of the sampled households had either the head or spouse born in the commune of current residence." So, 23% or more have migrated over the years, for instance, to mitigate any negative impact on health or education, which could cause some bias. We use the place of the birthplace, not the current birthplace, as the first attempt in the literature to mitigate any bias due to migrations. This approach is subject to the limitations that (i) the birthplace is available only at the province

$$y_{ihpym}^{P} = \delta_0 + \delta_1 (Young_{ihpym}^{P} * Exposed_p) + X_{ihpym}\delta_2 + \mu_p + \eta_y + \rho_m + u_{ihpym} \qquad (1)$$

where y_{ihpym}^{P} is an outcome variable, schooling years, of the individual *i* in household *h* born in province *p* in the month *m* in the year *y*.[12] $Young_{ihpym}^{P}$ is a dummy variable that indicates different age cohorts at which the individual first experienced the herbicide bombings[13] (i.e., whether the individual was exposed to the war *in utero* or not, whether the individual experienced the bombings before she reached the primary school ages or not; whether the individuals experienced the bombing during the primary school ages or not). Using the information on the year and month of birth, we divide our treated groups (i.e., those born

level and thus some people, e.g., living in less populous areas, might not have been affected by herbicide bombings and (ii) there was a possibility that people moved after the birth. However, a fairly large area in the province was bombed if the intensity was high, as suggested by the graphical analysis of Le et al. (Fig. 1 on page 7). Also, during the war, an opportunity for migration was deemed limited due to the higher direct and indirect costs of moving. Hence, despite the limitations, our use of the birthplace to identify the place of exposure to bombing will contribute to the literature.

[12] We have also used an educational qualification as an outcome variable and have obtained similar results. The quality of education, such as test scores, is unavailable.

[13] Since the early 1940s, the Viet Minh adopted the *ten great policies* from the Chinese party in 1937. One of those was to make primary school education compulsory to create a fully literate populace. The treated cohort is thus restricted to those who were at the compulsory age for primary school or below (i.e., below 12 years of age).

between 1951 and 1971) into four age ranges: the prenatal period, 0-3 years old, 4-6 years old, and 7-11 years old (see Figure 2). The control groups are the post-war cohorts born after 1972 (nine months after the cessation to avoid *in utero* exposure) up until December 1981 (to avoid inclusion of the second generation directly exposed to the war) and the pre-war cohorts born between 1945 and 1951, as shown in Figure 2.[14]

[Figure 2 to be inserted around here]

$Exposed_p$ is a binary variable to indicate whether the individual was born in the province that was exposed to the Operational Ranch Hand. X_{ihpym} indicates the vector representing individual and household characteristics, including age, polynomial terms of age, ethnicity (i.e., whether an individual belongs to ethnic majorities called Kinh or Hoa or other ethnic minorities), whether the household is living in the rural area, as well as indicators for regional trend and rural areas. η_y and ρ_m are year and the month fixed effect identifying the timing of birth. μ_p absorbs both observed and unobserved time-invariant effects at the provincial level on human capital.[15] In addition, information on the residence of parents at the beginning of the war (i.e., 1961) such as the population density, paddy yield, average precipitation (cm), average temperature (^0C) and the

[14] As a robustness check, we use the pre-war cohorts only for older cohorts (i.e., 4-6 and 7-11 years) and the post-war cohorts only for younger cohorts (i.e., the prenatal period and 0-3 years). We also tried the case where each period is halved so that it is adjacent to the treated period.

[15] This specification is equivalent to a basic DID specification with dummy variables for $Exposed_p$ and $Young_{ym}$ as well as their interaction.

proportion of land area at different altitudes are also included in the regressions to control for the location-specific measure of the shocks.[16]

Instead of assuming homogeneous effects of the shock across provinces below and above the 17^{the} parallel, we replace $Exposed_p$ in equation (1) with the variable $Intensity_p$ which measures the number of herbicide gallons dropped on individuals' province of birth p by the US military in the log form averaged over the years, as in equation (2).

$$y^P_{ihpym} = \delta_0 + \delta_1\left(Young^P_{ihpym} * Intensity_p\right) + X_{ihpym}\delta_2 + \mu_p + \eta_y + \rho_m + u_{ihpym} \qquad (2)$$

To analyze the heterogeneous effect for a more insightful understanding of the treatment effects, we also construct the exposure duration to the herbicide bombing measured by the total number of months individuals were alive or *in utero* during Operational Ranch Hand to derive more insightful impact estimates of the Operation Ranch Hand.

$$*y^P_{ihpym} = \delta_0 + \delta_1\left(Total\ months\ exposure^P_{ihpym} * Intensity_p\right) + \delta_2 Total\ months\ exposure^P_{ihpym} + X_{ihpym}\delta_3 + \mu_p + \eta_y + \rho_m + u_{ihpym} \qquad (3)$$

[16] Information from the pre-US war period is obtained from the 1959-1965 editions of the Statistical Yearbook of Vietnam and the Vietnam Agricultural Statistics published by the GSO Statistical Publishing House in Hanoi (1991) and included in the dataset of Miguel and Roland (2011). Other explanatory variables include a dummy for former South Vietnam and the north latitude (^0N).

Here the estimated coefficient of $Total\ months\ exposure_{ihpym} * Intensity_p$ captures the direct effect of exposure to bombings while that of $Total\ months\ exposure_{ihpm}$ shows its spillover effects. Furthermore, we disaggregate the total months of exposure into the period *in utero* and that after birth and form the two interaction terms as in equation (3)' to examine the relative size of the negative effect of exposure depending on the timing. This is further disaggregated into female and male samples.

$$y^{P}_{ihpym} = \delta'_0 + \delta'_1\left(Total\ months\ exposure^{P\ Utero}_{ihpym} * Intensity_p\right) + \delta'_2 Total\ months\ exposure^{P\ Utero}_{ihpym} + \delta'_3\left(Total\ months\ exposure^{P\ Post\ Birth}_{ihpym} * Intensity_p\right) + \delta_{\prime 2} Total\ months\ exposure^{P\ Post\ Birth}_{ihpym} + X_{ihpym}\delta_3 + \mu_p + \eta_y + \rho_m + u_{ihpym}$$

(3)'

Second-generation Effects (DID based on OLS)

This section focuses on the long-term effects of Agent Orange on human capital accumulation across generations and tests the hypotheses that chemical herbicides not only affect the educational levels of people directly exposed to the bombings but also influence their offspring's education.

We hypothesize that the bombings experienced by parents during the fetal period or in their childhood have a negative, long-lasting impact on the schooling years of their offspring. The interpretation of these effects relied on the identification assumption that the time and regional effects of 'bombing shocks' experienced by parents are independent of any other characteristics affecting the educational attainment of the children's generation. However, this assumption is potentially violated if there are unobserved factors common between a child and their mother or father. Therefore, we also present the specification that controls for the interactions between $Exposure^{P}_{ihpym}$, whether the individual's parent was exposed to the herbicide bombings, and the provincial intensity of the herbicide bombing.

$$y^{C}_{ihpym} = \gamma_0 + \gamma_1\left(Exposure^{P}_{ihpym} * Intensity_p\right) + X_{ihpym}\gamma_2 + \mu_p + \eta_y + \rho_m + v_{ihpym}$$

(4)

The fetal origins hypothesis suggested that exposure to adverse health shocks *in utero* has long-lasting effects on socioeconomic outcomes later in life (e.g., Almond, 2006; Almond and Currie, 2011; McCord et al., 2023). To test this hypothesis, we will test whether exposure to Agent Orange affects selective outcomes differently by the period that individuals first experience the shock. Here y^{C}_{ihpym} is the educational attainments of a child *i* born to parents in household *h* from the province *p* in the month *m* in the year *y*. It is expected that $\gamma_1 < 0$ shows that parents' exposure to Agent Orange negatively affects children's education.

Second-generation Effects (DID based on 2SLS)

As mentioned earlier, although we control for provincial fixed effects and include pre-war characteristics in the regressions, coefficient γ_1 could still be biased if the occurrence of bombings (*i.e.*, their timings and locations) is correlated with other unobserved time-variant factors. For instance, the US military may want to bomb the areas where perimeters are more likely to be located or agricultural production is relatively high if the US military puts more emphasis on the damage to the country's food security. To correct for endogeneity, we employ the IV method using the distance from the provincial centroid to the 17^{th} parallel following the literature (Miguel and Roland, 2011; Singhal, 2019).[17] The causal inference relies on the exogenous source of the variation in distance from each district to the DMZ. We will discuss this in detail in the next section.

Due to the space limitations, we will focus only on the following more comprehensive DID specification with two interactions, the total months of parental exposures to bombings either in utero or after birth, using the entire sample.[18] This will be first estimated by OLS and then by 2SLS. The OLS-DID model is written as:

$$y^{C}_{ihpym} = \beta_0 + \beta_1\left(Total\ months\ exposure^{P\ utero}_{ihpym} * Intensity_p\right) +$$
$$\beta_2\left(Total\ months\ exposure^{P\ post\ birth}_{ihpym} * Intensity_p\right) + X_{ihpym}\beta_3 + \delta_p + \eta_y + \rho_m + \epsilon_{ihpym}$$

(5)

We will then use the 2SLS model to estimate equation (5) by instrumenting the two interaction terms. These terms are considered endogenous due to one endogenous variable, $Intensity_p$, the bombing intensity, proxied by the average number of herbicide gallons in province *p.* This is interacted with the total months of bombing exposures during which the individual *in utero,* or after birth, during the war. The first-stage equations in 2SLS are estimated by equations (6) and (6)'.

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[17] Le et al. (2022) used the distance to North Vietnamese Army (NVA) bases as they use the commune as a unit of analysis, but this cannot be used in our study based on the province as a unit to identify the bombing exposure. We tried a number of NVA bases in each province as an alternative IV, but it was found to be a weak IV.

[18] Using only one interaction provides us with broadly similar results.

$$Total\ months\ exposure^{P\ utero}_{ihpym} * Intensity_p = \alpha_0 + \alpha_1 Total\ months\ exposure^{P\ utero}_{ihpym} * Distance_p + \alpha_2 X_{ihpym} + \delta_p + \eta_y + \rho_m + \eta_{ihpym} \quad (6)$$

$$Total\ months\ exposure^{P\ post\ birth}_{ihpym} * Intensity_p = \alpha'_0 + \alpha'_1 Total\ months\ exposure^{P\ post\ birth}_{ihpym} * Distance_p + \alpha'_2 X_{ihpym} + \delta_p + \eta_y + \rho_m + \eta'_{ihpym}$$

(6)'

where the variable $Distance_p$ is the north-south distance from a province to the 17^{th} parallel.[19] The second-stage equation is identical to equation (5) except that the two interaction terms are instrumented by equations (6) and (6)'. We assume $\epsilon_{ihpym} \sim iid(0, \sigma_\epsilon)$, $\eta_p \sim iid(0, \sigma_\eta)$ and $\eta'_p \sim iid(0, \sigma_,)$. The two interaction terms are instrumented by the distance from the 17^{th} parallel north latitude interacted with the months of exposure *in utero,* or after birth, as in Equations (6) and (6)'.[20]

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[19] Our data include the distance (north-south latitude) from each district centroid to the DMZ. The latitudes at the province level are derived as a simple average of the latitudes of districts within each province.

[20] While this is 2SLS with two endogenous variables, there is in essence, only one variable/source for endogeneity (i.e., $Intensity_p$) because this is the DID specification where the total months of exposure (which are assumed to be exogenous) are interacted with $Intensity_p$. Hence, we will need only one external instrument, $Distance_p$, to generate two instruments using a similar form of interaction to make the system 'exactly identified' (Chapter 9, Wooldridge, 2010).

The 2SLS estimator could be severely biased in the case of weak instruments (Andrews et al., 2019). In other words, to obtain a valid estimate, the instruments are required to be strongly correlated with the endogenous variables (i.e., relevance condition). Besides, the IV estimates also rely on the exclusion restriction. The results of specification tests are reported in the next section. We control for the province of birth and birth cohort fixed effects in all regressions. X_{ihpym} in equations (5), (6) and (6)' contain variables for the age of parents and the ages of children to control for the secular changes in educational attainment over time that may be unrelated to the war. X_{ihpym} also includes variables capturing geographic, demographic, economic and meteorological conditions of the location before the Vietnam War started (e.g., precipitation, agricultural production, population density, latitude, a dummy for former South Vietnam). The province fixed effects δ_p are also included to consider the possibility that the variation of the bombing intensity and the timing of the bombing across regions are correlated with parents' educational decisions. η_y and ρ_m jointly capture cohort fixed effects.

The treated groups are the children whose parents were born between 1951 and 1971, from the period inside a fetus through primary school ages (under 12 years old) during Operation Ranch Hand. We assume that the educational levels of children with parents from 281 districts where the herbicide bombings occurred are not systematically different from those whose parents were from the rest of the country without the bombing. One of the main challenges is that our instrument variable, $Distance_p$ needs to satisfy the exclusion restriction, where it only influences the schooling years of children indirectly through parents' exposure to the bombing. Here, we rule out the possibility that the distance from the 17^{th} parallel to the centroid of the parents' province of birth directly affects the educational outcome of their children. A reasonable justification for this assumption is that the demilitarized zone at the 17^{th} parallel was arbitrarily set according to the Geneva Accords in 1954 without consideration of socioeconomic conditions (Miguel and Roland, 2011). We further discuss the validity of the instrument in the next section.

VI. Results

Descriptive analyses

Table A1 in the Online Appendix provides descriptive statistics of the main variables used in the analysis. The primary sample for our study consists of the children generation aged between 22-43.[21] We have chosen the lower limit as 22 as a majority of individuals in Vietnam finish their college (about 16 years of education) at around this age. In our data, only 5.14% and 1.85% of individuals aged over 22 and 24 are still at school, respectively and have not completed their education.[22] On average, the young cohorts in our sample (i.e., second generation) are 28-30 years old while the father and mother cohorts (first generation) are aged 58 and 55 respectively. Fathers in our sample, on average, have 1.1 more years of schooling than mothers, while children have 2.4 and 3.9 more years of education than their father and mother, respectively. In contrast with the previous generation, girls, on average, are doing relatively better than boys. To minimize the effects of other confounding factors before Operational Ranch Hand, we also restrict the pre-war and post-war births of the first generation to between 1945 and 1981, respectively. We find that fathers were more likely to experience the bombing (11 per cent of

[21] The sampling weights, the inverses probability of being selected into the sample, are applied in all estimations.

[22] Those who were still at school at the time of the interview have been excluded from our sample. The upper age limit of 45 years is to exclude the second generation who were directly exposed to the bombings.

fathers compared to 7.8 per cent of mothers) as there were more males born before 1971 in our sample.

First-Generation Effects (DID based on OLS)

This sub-section examines the direct effects of exposure to Agent Orange in Vietnam from 1962 to 1971 on the first generation. It also presents the results for women and men separately, considering the gender heterogeneity. As discussed earlier, we distinguish the effects of bombing at different stages in their childhood by dividing our treated cohorts into four age groups *in utero*: 0-3, 4-6 and 7-11 years old. The age bands are driven by the concept of age-specific growth over the life course by Case and Paxson (2008) who documented that the association between environmental factors experienced *in utero* and early childhood can affect height as well as health and cognitive development in older ages and argued that intense growth happens from ages 0 to 3 and becomes relatively stable during early childhood and preschool age. This variation can be important for policy implications as the literature shows that environmental shocks can be more prominent at particular critical ages (*ibid.,* 2008). In addition to analyzing the variation of first exposure to Agent Orange, we also focus on the differences in duration of exposure to better capture the effects of the shock intensity.

Table 1 presents the baseline results of exposure to the bombing on parents' education corresponding to equations (1) and (2). We have applied the difference-in-differences model based on OLS to our dependent variable, 'years of education'. To control for specific characteristics of children born in the same area and in the same year, we include fixed effects for the province of birth and birth cohort in all models. The regressions have standard errors clustered at the province level to account for the correlation across districts within a province and for serial correlation at the province level. The estimated coefficients show consistent results between our two measures of shocks, namely, 1) war exposure (i.e., whether an individual was born in the affected provinces during or before the conflict period) and 2) war intensity (i.e., the average number of gallons of Agent Orange dropped into the area during Operational Ranch Hand interacted with a dummy for treated cohorts defined above). An important finding is that, although the herbicide bombings significantly affect both genders, it has stronger impacts on female individuals across all birth cohorts. Column (2) of Panel D of Table 1 implies that a woman who was exposed to the bombings *in utero* have 0.474 years[23] less in completed years of education compared to those in the control group. We also find that the effects after birth exposure are stronger than *in utero*, regardless of gender. Specifically, prenatal exposure shows statistically significant effects on mothers' schooling where an additional one percentage increase in the bombing intensity (gallons of herbicides) tends to reduce 0.146 years of mothers' education (-0.0359 (standardized coefficient) * 4.295 (SD)), Column (5) of Panel D). Consistent with the results for the binary treatment, exposure to more intensive herbicide bombing is the most prevalent at the age of 7-11 (Panel D) for both mothers and fathers. The schooling of men exposed to a one percentage increase in the bombing intensity during the primary schooling ages (i.e., 7-11 years old) is on average 0.253 years (-0.0652 * 3.876 (SD)) in schooling years lower than those not exposed to the bombings (Column (6) of Panel A). For women, each 1 per cent increase in the bombing intensity tends to reduce 0.45 years (-0.111 * 4.295 (SD)) in schooling years (Columns (5) of Panel A). On average, the later the fathers were exposed to the bombing, the larger the impact it had on fathers' schooling. The results show that the age for a child starting and finishing

[23] Years of education are standardized to see the relative magnitude of the effect of bombings in the entire distribution of schooling years. The estimated coefficient (-0.117) is in standard deviations (SD) and has been converted to years by using the SD of the dependent variable (4.053). The same conversion has been made in all the cases where we report size effects.

primary school is critical to the mother's highest schooling level. Overall, exposure to bombing has a negative impact across most of the age cohorts.[24]

[Table 1 to be inserted around here]

Using a dummy for being born in the affected areas does not capture the size effect; thus, we further explore the variation in the duration of exposure (equation (3)). The main explanatory variable is the interaction between the average number of gallons and total months of exposure. The OLS estimates in Table 2 are the Average Marginal Effects (AME) or the average of two different marginal effects for treated and control groups. Our estimates are also clustered at the provincial level. As one may be concerned about spillover effects, we also report the estimated coefficients of *Months Exposed to the Bombing* at different stages (Cases (4)-(6), corresponding to equation (3)'). The negative signs of these coefficients indicate that the control groups were also affected by 'being alive' in the provinces without any bombing when some part of the

[24] We have further investigated the heterogeneous effects of the bombings across different educational levels and obtained consistent results (available on request). The outcome variables are whether individuals have completed primary school (five years), lower-secondary school (9 years), and upper-secondary school (12 years) based on the logit model. The results show that the shocks have the strongest impact on the completion of lower-secondary school, followed by primary school. The effects on upper-secondary school are only negative for females, smaller in magnitude and no longer significant. Interestingly, exposure to the bombings in utero is more harmful in the long run than exposure at the age of 0 to 4, as the probability of completing lower-secondary school for girls and boys is reduced by almost 11% and 7.5%, respectively.

country was influenced by bombing (the second row of Table 2, Cases (1)-(3)). Spillover effects are significant to both cohorts born during the shock period but are stronger among cohorts exposed to bombing after birth (Cases (5) and (6)). Overall, the estimates confirm our previous results and show that mothers seem more affected at all ages than fathers. This finding confirms that girls are disproportionately affected by the shocks during the conflicts.

Second-Generation Effects (DID based on OLS)

In this subsection, we will test the hypothesis on the intergenerational transmission of the shocks. The effects of herbicide bombing on the second generation (equations (4) and (5)) are estimated by OLS and reported in Tables 3 and 4. The estimates also eliminate the possibility of geographical sorting by controlling for provincial fixed effects and years of birth for the father and mother in all regressions. In contrast to significant impacts on both genders in the first generation, we have only found negative effects of mothers' exposure to bombing on their children. This result is reasonable as we also find females in the first generation are affected more severely by the shocks than their male counterparts. Overall, an additional year of mother's exposure to Agent Orange during the prenatal period results in 0.294 and 0.143 fewer years of education (-0.0057*4.295 (SD) *12 months and -0.00308 * 3.876 * 12) for their daughters and sons, respectively (Columns (8) and (9) of Table 4). We only find spillover effects on sons of the mothers who were exposed to the bombings during early childhood (Column (9)).

[Tables 3 and 4 to be inserted around here]

Second-Generation Effects (DID based on 2SLS)

The results of DID are deemed reasonably robust as the model accounts for the variation across the province and different birth cohorts as long as the US military determined the location and the intensity of bombing more or less randomly. However, if the bombing intensity was not random and was decided by the US military based on unobserved factors that we cannot control for, our estimates might be affected by both treatment and those unobserved heterogeneities. Hence, we will use the distance in kilometers from the 17^{th} parallel to the province centroid as an IV to address the possible endogeneity following the literature (Miguel and Roland, 2011; Singhal, 2019).

Table 5 reports the results from the second stage in equation (5) where we allow the instrument to vary by the time exposure to the bombings. The robust standard errors in all models are also clustered at the province level. The results based on the IV or 2SLS estimates in Table 5 are strikingly different from those in Table 4 in terms of the statistical significance of key coefficient estimates in terms of the sign of coefficient estimates. 2SLS results where the endogeneity of the bombing is corrected show that fathers' longer exposure to herbicide bombings after birth significantly reduces their daughter's years of education. A similar result is found also for mothers. On the other hand, sons' education is negatively and significantly affected when their mothers were exposed to bombings either *in utero* or after birth. The coefficient of mothers' exposure to bombings is negative and statistically significant at the 5% and 10% per cent levels across different regressions. For instance, for an additional year the mother was exposed to the herbicides after the birth, the education of female children in the second generation is decreased by 0.077 years (-0.00149 (standardized coefficient) * 4.295

(SD)* 12 months, Panel B, the third column of Table 5). This result is in line with existing literature that documents that the mothers' endowment (e.g., education, preference for investment in education) is more important than that of the fathers. The mothers' education is strongly associated with better childcare, improved child nutrition and the ability to mitigate adverse shocks (Thomas and Strauss, 1992). The coefficients of bombing intensity for fathers' exposure *in utero* are consistently negative for both boys and girls, which indicates a negative impact of Agent Orange on their offspring's schooling level. However, we do not find significant impacts on children of fathers' experience with bombing during the prenatal period for both boys and girls. Education of children in the second generation is decreased by 0.05 years for an additional year the father was exposed to the herbicide bombing after the birth (-0.00108 (standardized coefficient) * 3.876 (SD) *12 months) for an additional month, see the first column of Panel A in Table 5).[25]

The results in the first-stage estimation in Table 6 show that our instrument is statistically significant at a one per cent level, implying that it is a strong predictor for the endogenous variable (i.e., bombing intensity).[26] The inclusion of pre-war control variables in our regressions also helps to reduce the upward bias of the estimates. Due to the nature of first-stage heterogeneity in our setting, the standard Stock and Yogo's (2005) testing for a weak instrument is invalid; thus, the Sanderson-Windmeijer (SW) conditional F-statistics are reported instead

[25] The results – available upon request - are consistent when we use the binary treatment (i.e., whether fathers/mothers are exposed to the bombing).

[26] In all cases where we use two interaction terms as instruments in the first stage, both are highly significant, and the results (available upon request) are consistent with those in Table 6.

(Angrist and Pischke, 2009; Sanderson and Windmeijer, 2016; Andrews et al., 2019).[27] SW F-statistics in the first stage regressions are all above 10, which passes the conventional threshold levels to detect weak instruments in IV regressions, thus we reject the null hypothesis of weak instruments. In addition, the results from Durbin–Wu–Hausman tests for endogeneity consistently reject the null hypothesis of exogeneity for treatment variables, which justified the usage of instrumental variable strategy in our estimations (see Table 5).

Another concern is whether our IV satisfies the exclusion restriction. We argue that the instrumental variable used in our model only affects the educational outcomes of children through parents' exposure to bombing for several reasons. The demilitarized zone (DMZ) at the 17^{th} parallel north latitude is arbitrarily set according to the Geneva Accords in 1954. The border dividing North and South Vietnam was the result of negotiations between the US government and the Soviet Union. Areas that suffered the most from the bombing were provinces/districts near the 17^{th} parallel, such as Quang Tri. Self-selection or migration of the household into the areas according to the distance from the 17th parallel was unlikely because both governments decided it without any consideration of socioeconomic conditions, and thus, the distance itself is unlikely to influence the education of both generations. Miguel and Roland (2011, p.8) found, for instance, that conditional poverty rates were not much different in the areas closer to the 17th parallel and those far away from it, implying that the direct effect of the distance from the 17th parallel on education is likely to be weak.[28] It is noted that we include province fixed effects in

[27] Sanderson and Windmeijer (2016) developed the test for weak instruments in a model with multiple endogenous variables based on the proposal by Angrist and Pischke (2009). They adjust the first-stage F-statistics in order to get a correct asymptotic distribution.

[28] The issue on migration will be discussed later in this section.

all the specifications together with many variables capturing geographic, demographic (population density), economic and meteorological conditions before the Vietnam War started, including the latitude and a dummy for former South Vietnam. Hence, the IV captures only the physical closeness from the 17th parallel, which matters for pure military strategies, not others (e.g., agricultural production, climatic conditions, unobservable fixed effect for belonging to former South Vietnam, capturing differential institutional factors).[29] Thus, the random assignment of the DMZ allows us to use the north-south distance from each province centroid to the 17^{th} parallel as an instrumental variable for bombing intensity in a similar setting as a natural experiment. We further carry out a robustness test for this assumption by including the distance variable in the reduced form equation. We found that the coefficients of distance in all regressions are not statistically significant, which confirms our assumption.[30]

The identification of the effects of the bombings on education in our econometric models relies on the variation by birth cohort and region. A strict assumption of using the DID strategy is that there is no systematic difference in the trend of educational attainment in the absence of the

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[29] A possible limitation is that $Distance_p$ might capture unobservable *time-variant* factors associated with the distance from the 17^{th} parallel unaccounted for by the rich set of control variables (e.g., changes in education policies or in the social norms over time broadly associated with the distance from the 17th parallel). However, such changes were unlikely to occur in proportion to the distance from the 17^{th} parallel. In our view, controlling for the *time-invariant* effect at the province level is sufficient. After numerous trials for the available data, we consider $Distance_p$ the best instrument in our study context.

[30] The results will be available upon request.

war. In other words, both treatment and control groups follow the same time trend. Although we have already controlled for the province fixed effects and time trend effects, there may have been other factors that the inclusion of province and time fixed effects in the DID model cannot control for. We examine the parallel trend in parents' exposure to bombing by constructing sub-samples for placebo tests, including the pre-war sample (i.e., cohorts born between 1945 and 1949) and the post-war sample (i.e., cohorts born between 1972 and 1982). The idea is that we randomly assign the shock to alternative cohorts by assuming that the Operational Ranch Hand happened 17 years earlier (i.e., 1945-1947) or 11 years later (i.e., 1972-1976). The sub-samples are also restricted to avoid the overlap with the cohorts actually exposed to the war. The coefficients in all regressions with and without control variables from DID estimation in Table A2 are close to 0 and statistically insignificant, which confirms our assumption for the parallel trend. The estimated DID coefficients are statistically insignificant (Table A3 in the Online Appendix).

Another issue that may cause measurement error is internal migration induced by conflicts, making the results less convincing as previous studies suggest that economic shocks may induce migration (Dinkelman, 2011). If the adverse shock systematically drives the selection for migration, our results will be biased. For example, households victimized by the bombings may not be entirely random as wealthier households with more resources could predict where the bombing would be intense and be able to migrate temporarily or permanently to escape the bombings. Due to the data limitation, we do not know whether an individual's place of birth differs from where they accumulate their human capital. Using the information on the place of birth and current place of residence, we construct the variable for location variation.[31] This is a reasonable proxy as we only find that 7% of the total observations in our sample were migrants. As a robustness check, we restrict our sample to two subsets of individuals 1) those who still live in their birth province and 2) those who have moved at some point after the birth. Results from migrant and non-migrant sub-samples in Panel A and Panel B of Table 7 and the full sample in Table 2 are very similar. Effects of exposure to herbicide bombings on the non-migrant sample are just slightly higher compared to estimates from the migrant sample.

[Table 7 to be inserted around here]

Lastly, it is important to understand the mechanism whereby parents' exposure to shocks is transmitted to the second generation. Consistent with other studies about the intergenerational persistence of human capital in developing countries, we also found that parents' schooling levels significantly affect the educational attainment of their offspring. As parents directly exposed to the bombings result in fewer years of education and are less likely to complete formal educational qualifications, we expect that children whose parents belong to the affected cohorts would persistently have lower levels of education. The coefficients are consistently similar when comparing models with (Table 5) and without including parents' education (see Table A3 in the Online Appendix). These estimates indicate that parents' schooling levels do not eliminate the impacts of the bombings, although we observe a slight decrease in magnitudes across the

[31] VHLSSs are repeated cross-sectional data so information on residence over time is unavailable.

regressions. It suggests that the persistent impacts of the bombings are not likely to be driven through the intergenerational transmission of education.

VII. Conclusion

Operational Ranch Hand during 1962-1971, which dumped over two million herbicide gallons into Vietnam and aimed at destroying lives and tactics of the Vietnamese Communist, has caused devastating consequences. Since the US-Vietnam War ended 50 years ago, it allows us to examine its long-term consequences as well as the persistence carried across generations. Results from this paper contribute to the growing literature that estimates the welfare impacts of conflicts.

In this paper, we focus on the long-term impacts of the herbicide bombing from the Operational Ranch Hand during the US-Vietnam War on human capital for both cohorts directly exposed to the bombing and cohorts indirectly affected through their parents. Our main results find statistically significant lasting impacts of the war on educational outcomes for both genders, with stronger impacts on women's education. Furthermore, we compare the educational level of children exposed to the shocks at different stages in early life and for varying duration. There is evidence of heterogeneous impacts across birth cohorts. For instance, children exposed to the bombings after birth are found to be more severely affected in the long run than those who experienced them *in utero*. The most fragile period where we found the strongest effect of the bombing on the first generation is the primary schooling age. This can be explained by the disruption of school or income shocks, which prevented children from continuing their education at school.

Results from the analysis on intergenerational transmission of the herbicide bombings indicate that children whose parents were exposed to the herbicides have worse educational outcomes compared to their non-exposed counterparts. Results from the two-stage least squares model show that mothers' exposure to shocks during the prenatal period significantly affects the schooling level of their sons, while their exposure to shocks after birth negatively influences the education of both boys and daughters. On the contrary, fathers' exposure to shocks significantly reduced only their daughters' schooling years. It is challenging to determine the exact mechanism of this transfer across generations. One of the key concerns in our identification strategy is the non-random nature of the bombing, where the US military may target provinces/districts with better economic aspects or with more important strategies for their military tactics, making our estimates potentially biased. Since we do not have long panel data to look at the characteristics of children over time, we combine DID, whereby we explored the timing of birth and the location/intensity of herbicide bombing, with the instrumental variable to mitigate the impacts of unobserved heterogeneity on spraying intensity.

After the cessation of the war, there have been a number of compensation programs from the US government trying to support family of those who were affected by the Operational Ranch Hand. However, the target has always been individuals with tangible physical disabilities, and the victims without any physical disabilities or their children are rarely covered. Our study has provided evidence of the long-term consequences of herbicide bombings and has suggested the importance of supporting households exposed to herbicide bombings.

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Table 1: OLS Regressions, Impacts of bombing on educational attainment

Dependent variable: Years of education

	Dummy treatment			**Continuous treatment**		
	(1) **All**	**(2)** **Female**	**(3)** **Male**	**(4)** **All**	**(5)** **Female**	**(6)** **Male**
Panel A: Exposed at age 7-11						
Affected cohort*Affected province	-0.283***	-0.332***	-0.209**	-0.0897***	-0.111***	-0.0652***
	(0.0773)	(0.0922)	(0.0873)	(0.0135)	(0.0147)	(0.0183)
Observations	9748	5322	4426	9748	5322	4426
Adjusted R2	0.304	0.367	0.256	0.306	0.371	0.257
Panel B: Exposed at age 4-6						
Affected cohort*Affected province	-0.147**	-0.247***	-0.0688	-0.0397***	-0.0762***	-0.00955
	(0.0618)	(0.0874)	(0.0652)	(0.0144)	(0.0178)	(0.0148)
Observations	9977	5450	4527	9977	5450	4527
Adjusted R2	0.296	0.356	0.257	0.297	0.358	0.257
Panel C: Exposed at age 0-3						
Affected cohort*Affected province	-0.0995*	-0.147**	-0.0482	-0.0321***	-0.0395***	-0.0244*
	(0.0502)	(0.0576)	(0.0637)	(0.0102)	(0.0115)	(0.0134)
Observations	11339	6105	5234	11339	6105	5234
Adjusted R2	0.300	0.359	0.267	0.305	0.360	0.267
Panel D: Exposed in utero						
Affected cohort*Affected province	-0.0501	-0.117***	-0.0202	-0.0192**	-0.0359***	-0.0139
	(0.0312)	(0.0389)	(0.0420)	(0.00779)	(0.00959)	(0.0105)
Observations	14763	7849	6914	14763	8675	7606
Adjusted R2	0.296	0.377	0.277	0.299	0.354	0.258
Panel E: Exposed all						
Affected cohort*Affected province	-0.118***	-0.177***	-0.0613	-0.0347***	-0.0499***	-0.0200**
	(0.0347)	(0.0412)	(0.0409)	(0.00664)	(0.00686)	(0.00913)
Observations	19090	9993	9097	19090	9993	9097
Adjusted R2	0.300	0.359	0.262	0.301	0.361	0.262
Treatment FE and Time FE	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y

Notes: Authors' calculation from VHLSS 2014, VHLSS 2016 and VHLSS 2018. Robust standard errors in parentheses are clustered at the provincial level. Control variables include provincial fixed effects, year of birth and month of birth fixed effects, and cohort fixed effects. Other explanatory variables comprise age and age polynomials, ethnicity (i.e. Kinh/Hoa), whether the households live in rural areas, household size, years of education for grandparents, whether the household is female-headed and an indicator for a single household.
* p<0.10 ** p< 0.05 *** p<0.01.

Table 2: OLS Regressions, Duration of bombing exposure and education

Dependent Variable: Yeas of Education (Standardised)	(1) **All**	(2) **Female**	(3) **Male**	(4) **All**	(5) **Female**	(6) **Male**
Direct Effect	-0.000406***	-0.000523***	-0.000261***			
Intensity x Months Exposed to bombing	(0.0000789)	(0.0000856)	(0.0000813)			
Spillover Effect	-0.00190***	-0.00164**	-0.00230***			
Months exposed to bombing	(0.000698)	(0.000735)	(0.000794)			
Direct Effect (in utero)				-0.00200*	-0.00251**	-0.00154
Intensity × Months exposed to bombing in Utero				(0.00106)	(0.00106)	(0.00129)
Direct Effect (after birth)				-0.000409***	-0.000522***	-0.000268***
Intensity × Months exposed to bombing after Birth				(0.0000791)	(0.0000856)	(0.0000826)
Spillover Effect (in utero)				-0.0137**	-0.00588	-0.0204**
Months exposed to bombing in utero				(0.00672)	(0.00999)	(0.00865)
Direct Effect of Bombing (*after birth*)				-0.00203***	-0.00172**	-0.00244***
Months exposed to bombing after birth				(0.000722)	(0.000757)	(0.000809)
Observations	21830	11445	10385	21830	11445	10385
Adjusted R2	0.317	0.361	0.269	0.317	0.361	0.270
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Authors' calculation from VHLSS 2014, VHLSS 2016 and VHLSS 2018. Provincial fixed effects and cohort fixed effects are included in all regressions. Bombing intensity is specified as an interaction between the number of gallons in the logarithm and the total months exposed to bombing. Robust standard errors in parentheses are clustered at the provincial level. * p<0.10 ** p< 0.05 *** p<0.01.

Table 3: OLS Regressions, Impacts of bombing on the educational attainment of the second generation (Continuous treatment) - All sample

Dependent variable: Years of education (Standardised)	Mother			Father		
	All	**Female**	**Male**	**All**	**Female**	**Male**
Panel A: Bombing intensity at age 7-11						
Affected cohort*Affected province	0.0201	0.0297	0.00577	0.0303	0.00678	0.0459*
	(0.0186)	(0.0325)	(0.0179)	(0.0200)	(0.0350)	(0.0233)
Observations	2226	915	1311	1937	789	1148
Adjusted R2	0.308	0.363	0.293	0.326	0.409	0.275
Panel B: Bombing intensity at age 4-6						
Affected cohort*Affected province	-0.0350*	-0.0237	-0.0487**	0.0499***	0.0771***	0.0295
	(0.0188)	(0.0291)	(0.0241)	(0.0185)	(0.0278)	(0.0217)
Observations	2354	962	1392	2199	899	1300
Adjusted R2	0.339	0.388	0.316	0.366	0.460	0.323
Panel C: Bombing intensity at age 0-3						
Affected cohort*Affected province	-0.0299*	-0.0342	-0.0241	0.0146	-0.0140	0.0393**
	(0.0155)	(0.0229)	(0.0172)	(0.0140)	(0.0159)	(0.0177)
Observations	3456	1381	2075	3199	1273	1926
Adjusted R2	0.327	0.360	0.325	0.369	0.422	0.348
Panel D: Bombing intensity in utero						
Affected cohort*Affected province	-0.0240*	-0.0354*	-0.0194	0.0227	0.0242	0.0276*
	(0.0136)	(0.0188)	(0.0171)	(0.0136)	(0.0208)	(0.0157)
Observations	4064	1571	2493	4578	1785	2793
Adjusted R2	0.302	0.335	0.290	0.339	0.383	0.326
Panel E: Gallons all						
Affected cohort*Affected province	-0.00917	-0.00259	-0.0128	0.0233*	0.00915	0.0371**
	(-0.64)	(-0.14)	(-0.76)	(1.91)	(0.54)	(2.57)
Observations	8252	3254	4998	8234	3241	4993

Notes: Authors' calculation from VHLSS 2014, VHLSS 2016 and VHLSS 2018. Provincial fixed effects and cohort fixed effects are included in all regressions. Robust standard errors in parentheses are clustered at the provincial level. * p<0.10 ** p< 0.05 *** p<0.01.

Table 4: Duration of bombing exposure and education - Second Generation (Bombing exposure)

Dependent variable: Years of education (Standardised)	Father						Mother					
	(1) All	**(2) Female**	**(3) Male**	**(4) All**	**(5) Female**	**(6) Male**	**(7) All**	**(8) Female**	**(9) Male**	**(10) All**	**(11) Female**	**(12) Male**
Direct Effect	-0.00106	-0.00146	-0.00137				-0.00384*	-0.00570**	-0.00308			
Intensity x Months Exposed to bombing	(0.00207)	(0.00312)	(0.00282)				(0.00194)	(0.00241)	(0.00226)			
Spillover Effect	0.00208	0.000774	0.00282				-0.00219	-0.00214	-0.00258*			
Months exposed to bombing	(0.00162)	(0.00213)	(0.00200)				(0.00146)	(0.00188)	(0.00153)			
Direct Effect (in utero)				-0.00835	-0.0351	0.00404				0.0178	0.0267	0.0126
Intensity × Months exposed to bombing in Utero				(0.0287)	(0.0448)	(0.0322)				(0.0257)	(0.0401)	(0.0283)
Direct Effect (after birth)				-0.00162	-0.00363	-0.00115				-0.00266	-0.00413	-0.00208
Intensity × Months exposed to bombing after Birth				(0.00235)	(0.00370)	(0.00327)				(0.00236)	(0.00278)	(0.00285)
Spillover Effect (in utero)				-0.00776	-0.00150	-0.00810				0.00136	-0.0140	0.00837
Months exposed to bombing in utero				(0.0199)	(0.0337)	(0.0213)				(0.0201)	(0.0312)	(0.0226)
Direct Effect of Bombing (*after birth*)				0.00144	0.000406	0.00220				-0.00198	-0.00257	-0.00212
Months exposed to bombing after birth				(0.00181)	(0.00247)	(0.00228)				(0.00189)	(0.00227)	(0.00207)
Observations	8873	3555	5318	8873	3555	5318	11029	4452	6577	11029	4452	6577
Adjusted R2	0.193	0.239	0.168	0.193	0.239	0.167	0.203	0.245	0.180	0.203	0.245	0.180
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Authors' calculation from VHLSS 2014, VHLSS 2016 and VHLSS 2018. Provincial fixed effects and cohort fixed effects are included in all regressions. Bombing intensity is specified as an interaction between the number of gallons in the logarithm and the total months exposed to bombing. Robust standard errors in parentheses are clustered at the provincial level. * p<0.10 ** p< 0.05 *** p<0.01.

Table 5: The causal effects of exposure to bombing exposure on the education of the second generation

Dependent Variasecond stage): Yeas of Education (Standardised)	Whole Sample		Daughter		Son	
	2SLS	**First stage**	**2SLS**	**First stage**	**2SLS**	**First stage**
Panel A: Father's bombing exposure						
Intensity × Months exposed to bombing in utero	-0.00141		-0.00119		-0.00143	
	(0.00281)		(0.00369)		(0.00331)	
Intensity × Months exposed to bombing after birth	-0.00108**		-0.00187**		-0.000685	
	(0.000487)		(0.000754)		(0.000484)	
Distance		-1.662***		-1.807***		-1.578***
		(0.400)		(0.432)		(0.385)
Observations	8252	8252	3254	3254	4998	4998
Adjusted R2	0.280		0.292		0.274	
Sanderson-Windmeijer F-stats	37.04		56.47		26.80	
Durbin-Wu-Hausman	25.261		16.566		27.654	
Panel B: Mother's bombing exposure						
Intensity × Months exposed to bombing in utero	-0.00422*		-0.00402		-0.00485*	
	(0.00249)		(0.00357)		(0.00275)	
Intensity × Months exposed to bombing after birth	-0.00120**		-0.00149**		-0.00112*	
	(0.000595)		(0.000640)		(0.000679)	
Distance		-1.709***		-1.830***		-1.646***
		(0.420)		(0.477)		(0.392)
Observations	10210	10210	4056	4056	6154	6154
Adjusted R2	0.274		0.303		0.253	
Sanderson-Windmeijer F-stats	51.00		73.07		29.81	
Durbin-Wu-Hausman	29.154		23.501		29.370	
Cohort FE	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y

Notes: Author's calculation from VHLSS 2014, 2016 and 2018. The main explanatory variable is the interaction between the average number of herbicide gallons sprayed in the province of birth and the total number of months exposed to the bombings. Fixed effects and control variables are included in all regressions. Robust standard errors in parentheses are clustered at the provincial level. * p<0.10 ** p<0.05 *** p<0.01.

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Table 6: Distance to the province centroid and bombing, First stage

Dependent variable: Log total gallons of herbicide bombing				
	(1)	(2)	(3)	(4)
Distance from 17th parallel	-2.625***	-2.636***	-0.721***	-0.752***
	(0.0498)	(0.0509)	(0.120)	(0.130)
Observations	3624	3624	3295	3295
Adjusted R2	0.347	0.356	0.781	0.785
Cohort FE	No	Yes	Yes	Yes
Pre-war province characteristics	No	No	Yes	Yes
Individual & household characteristics	No	No	No	Yes

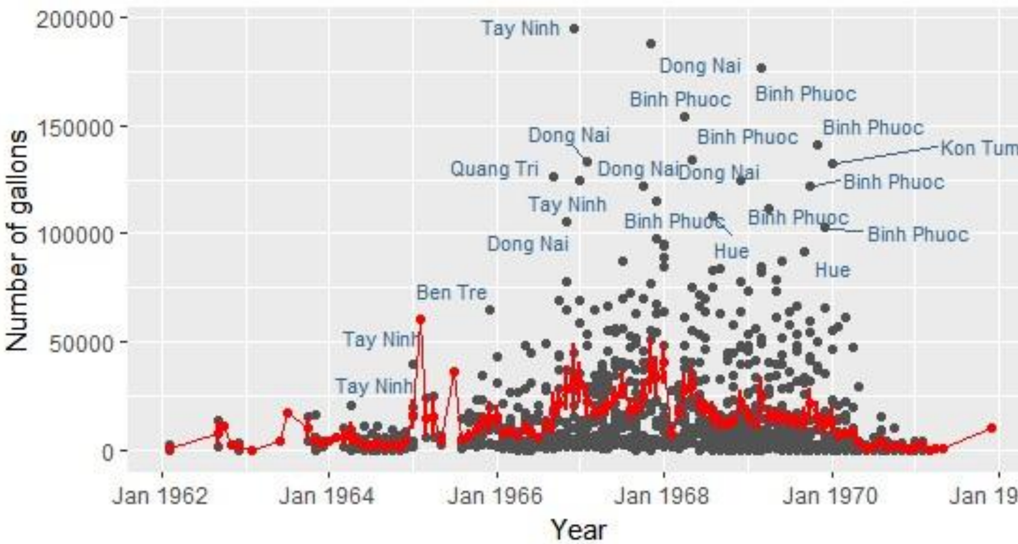
Notes: Authors' calculation from VHLSS 2014, VHLSS 2016 and VHLSS 2018. Robust standard errors in parentheses. Pre-war characteristics include the population density in 1960-1961, total paddy yield in 1960-1961, average precipitation (cm), average temperature (^{0}C), North latitude (^{0}N) and proportion of land areas at different high altitudes.

Table 7: Duration of bombing exposure and education - Robustness test for sample selection

	(1) All	(2) Female	(3) Male	(4) All	(5) Female	(6) Male
Panel A: Sample of migrants						
Bombing intensity	-0.000572***	-0.000748***	-0.000372*			
	(0.000157)	(0.000173)	(0.000195)			
Total months exposed to bombing	-0.00423***	-0.00444***	-0.00380**			
	(0.00124)	(0.00154)	(0.00182)			
Bombing intensity in utero				-0.000285	0.000174	-0.000645
				(0.00157)	(0.00153)	(0.00268)
Bombing intensity after birth				-0.000580***	-0.000761***	-0.000380*
				(0.000158)	(0.000177)	(0.000195)
Months exposed to bombing in utero				-0.0122	-0.0121	-0.0174
				(0.0139)	(0.0233)	(0.0180)
Months exposed to bombing after birth				-0.00428***	-0.00452***	-0.00378**
				(0.00124)	(0.00156)	(0.00181)
Observations	3295	1719	1576	3295	1719	1576
Adjusted R2	0.272	0.312	0.233	0.271	0.312	0.232
Panel B: Sample without migrants						
Bombing intensity	-0.000479***	-0.000625***	-0.000307***			
	(0.0000782)	(0.0000758)	(0.000100)			
Total months exposed to bombing	-0.000793	-0.000281	-0.00155*			
	(0.000626)	(0.000589)	(0.000878)			
Bombing intensity in utero				-0.00121	-0.00258**	0.0000526
				(0.000892)	(0.00101)	(0.00101)
Bombing intensity after birth				-0.000481***	-0.000620***	-0.000312***
				(0.0000800)	(0.0000778)	(0.000101)
Months exposed to bombing in utero				-0.00914	-0.00140	-0.0185*
				(0.00775)	(0.0127)	(0.00953)
Months exposed to bombing after birth				-0.000871	-0.000314	-0.00166*
				(0.000636)	(0.000585)	(0.000895)
Observations	16383	8589	7794	16383	8589	7794
Adjusted R2	0.316	0.375	0.253	0.317	0.375	0.254

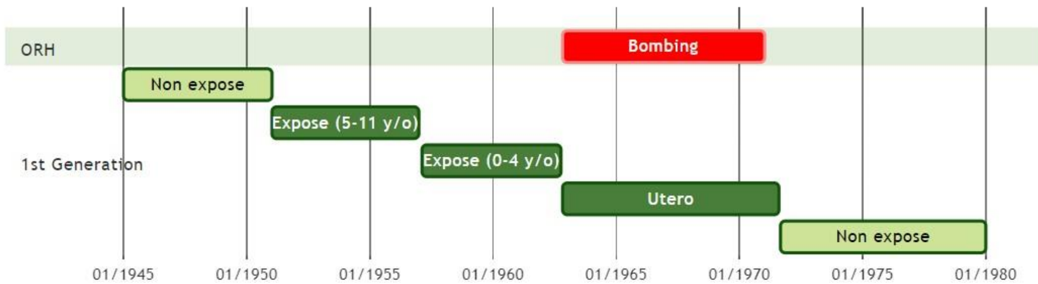
Notes: Authors' calculation from VHLSS 2014, 2016 and 2018. Fixed effects and control variables are included in all regressions. Robust standard errors in parentheses are clustered at the provincial level. * p<0.10 ** p<0.05 *** p<0.01.

Figure 1: Changes in the Intensity of Herbicide Bombings sprayed by the US Military Airforce in Vietnam from 1962 to 1971



Notes: Authors' calculation from HERBS. The y-axis denotes the total number of dropped herbicide spraying aggregated by month within a province, each grey dot represents one province. The magnitudes of intensity vary largely across provinces and were escalating between 1965 and 1968 with around 20,000 gallons each month.

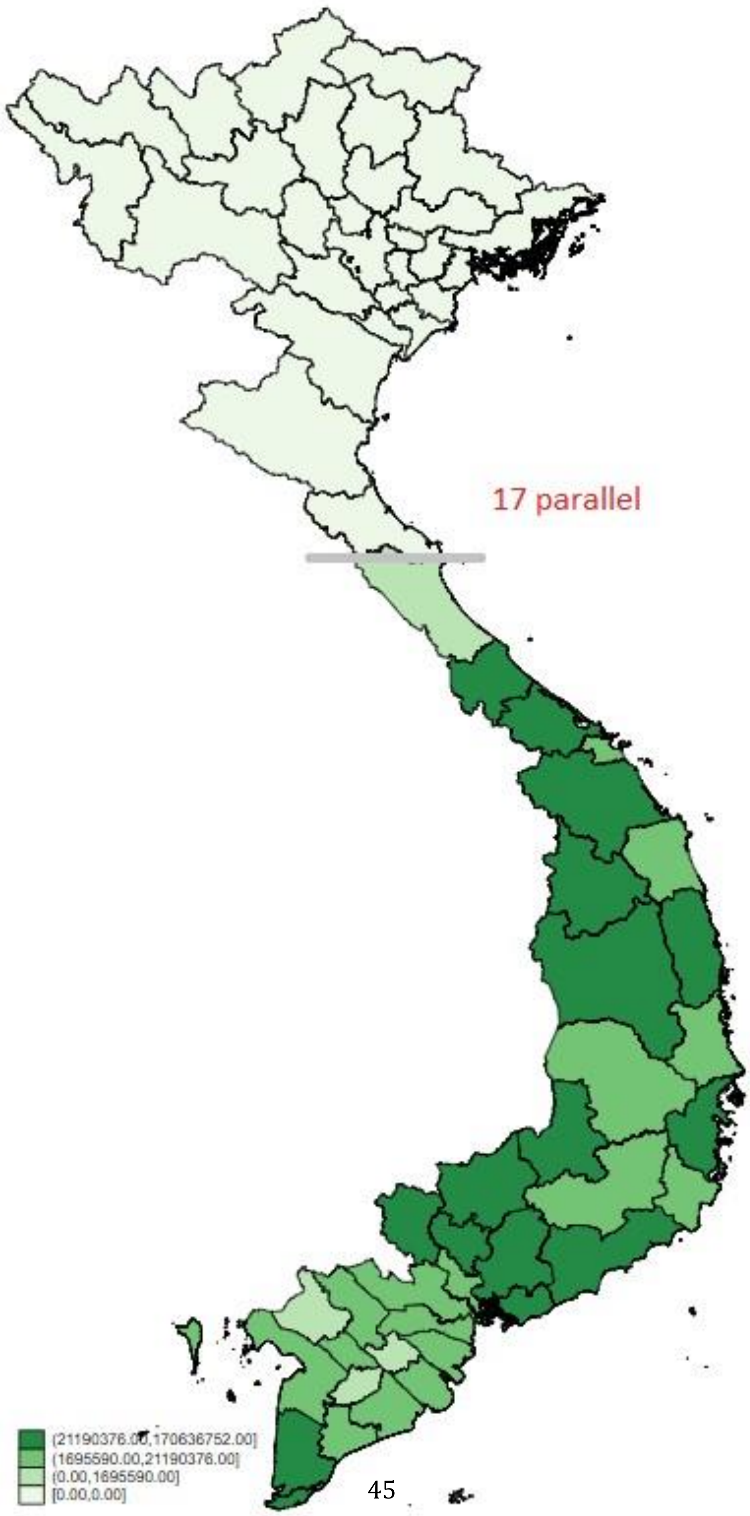
Figure 2: Timeline of cohorts exposed to chemical herbicides during the period January 1962 - January 1971



Notes: Authors' illustration.

Online Appendix

Figure A1: Intensity of herbicidal bombing across Vietnam during 1962-1971



Notes: Authors' calculation from HERBS.

Table A1: Summary statistics by treatment groups

	2014			2016			2018		
	Full	**Treat**	**Non-**	**Full**	**Treat**	**Non-**	**Full**	**Treat**	**Non-**
Children									
Age	27.88	27.21	27.66	28.23	27.88	27.82	28.92	29.04	28.29
	(5.686)	(4.291)	(5.754)	(5.521)	(4.501)	(5.713)	(5.861)	(5.053)	(5.651)
Years of education	10.62	10.40	10.17	10.69	10.53	10.09	10.97	10.88	10.71
	(4.027)	(4.058)	(3.852)	(4.012)	(4.004)	(3.869)	(3.946)	(4.025)	(3.689)
Female=1	0.405	0.395	0.390	0.414	0.403	0.403	0.413	0.413	0.404
	(0.491)	(0.489)	(0.488)	(0.493)	(0.491)	(0.491)	(0.493)	(0.493)	(0.491)
Father									
Father educational level	7.964	7.492	7.676	7.968	7.744	7.332	8.092	7.877	7.822
	(4.014)	(4.036)	(3.422)	(4.026)	(4.176)	(3.621)	(3.936)	(4.091)	(3.479)
Father's age	57.81	56.76	57.59	58.18	57.67	57.46	58.36	59.06	57.18
	(9.137)	(6.049)	(10.56)	(8.986)	(5.578)	(10.74)	(8.646)	(5.690)	(9.768)
Father in farm	0.552	0.541	0.713	0.539	0.508	0.713	0.533	0.503	0.681
	(0.497)	(0.498)	(0.453)	(0.499)	(0.500)	(0.453)	(0.499)	(0.500)	(0.466)
Mother									
Mother's education	6.765	6.157	6.592	6.911	6.439	6.471	6.938	6.461	6.888
	(4.030)	(3.843)	(3.758)	(4.060)	(3.961)	(3.843)	(3.990)	(3.898)	(3.715)
Mother's age	55.10	53.87	55.26	55.64	54.98	55.36	55.96	56.31	55.11
	(8.253)	(4.995)	(9.775)	(8.380)	(5.079)	(10.20)	(8.293)	(5.402)	(9.479)
Mother in farm	0.557	0.457	0.824	0.520	0.422	0.761	0.519	0.394	0.771
	(0.497)	(0.498)	(0.381)	(0.500)	(0.494)	(0.427)	(0.500)	(0.489)	(0.421)
Household characteristics									
Female headed household	0.110	0.121	0.0442	0.120	0.146	0.0560	0.119	0.148	0.0612
	(0.313)	(0.327)	(0.206)	(0.325)	(0.353)	(0.230)	(0.323)	(0.355)	(0.240)
Ethnicity	0.847	0.931	0.744	0.835	0.923	0.702	0.825	0.916	0.733
	(0.360)	(0.253)	(0.437)	(0.371)	(0.267)	(0.458)	(0.380)	(0.278)	(0.442)
Expenditure pc	21578.4	21693.8	17253.7	25045.1	26203.0	19610.8	23967.1	25122.5	19897.1
	(17324.0)	(16722.1)	(10022.3)	(21004.5)	(21746.2)	(15214.6)	(18858.8)	(17567.3)	(12825.7)
Total income pc	31905.3	34496.1	24879.1	41242.2	45222.4	30759.8	43801.7	46608.6	36430.8
	(18065.8)	(37884.9)	(46006.0)	(23642.8)	(35173.8)	(32468.2)	(31016.2)	(24347.3)	(26875.2)
Grandparent's education	0.162	0.106	0.280	0.159	0.0686	0.259	0.149	0.0803	0.218
	(1.078)	(0.739)	(1.561)	(1.000)	(0.501)	(1.309)	(0.900)	(0.559)	(1.132)
Second job	0.485	0.440	0.638	0.387	0.435	0.358	0.449	0.348	0.639
	(0.500)	(0.497)	(0.481)	(0.487)	(0.496)	(0.479)	(0.497)	(0.476)	(0.481)
Household size	5.396	5.396	5.438	5.292	5.240	5.364	5.331	5.351	5.290
	(1.596)	(1.650)	(1.539)	(1.583)	(1.555)	(1.637)	(1.615)	(1.578)	(1.508)
Commune characteristics									
Traditional village			0.801			0.794			0.838
			(0.399)			(0.405)			(0.369)
Car road			0.930			0.958			0.967
			(0.256)			(0.200)			(0.180)
Cultural house			0.584			0.638			0.744
			(0.493)			(0.481)			(0.436)
Having primary schools			0.989			0.986			0.963
			(0.106)			(0.119)			(0.189)
Having secondary schools			0.932			0.940			0.912
			(0.251)			(0.237)			(0.283)
Having high schools			0.157			0.173			0.160
			(0.364)			(0.378)			(0.367)

Source: Authors' calculation from VHLSS 2014, VHLSS 2016 and VHLSS 2018. Standard deviations in parentheses.

Table A2: Robustness check for the parallel trend

Dependent variable: Years of education								
	Dummy treatment				**Continuous treatment**			
	Older (1945-1949)		**Younger (1972-1981)**		**Older (1945-1949)**		**Younger (1972-1981)**	
Panel A: All sample								
Placebo cohorts	-0.114	0.00570						
	(0.110)	(0.138)						
Placebo cohorts			0.0255	-0.0219				
			(0.0382)	(0.0451)				
Placebo cohorts					-0.0356	-0.0272		
					(0.0248)	(0.0266)		
Placebo cohorts							0.00990	-0.00411
							(0.00968)	(0.0120)
Observations	687	654	7009	6909	687	654	7009	6909
Adjusted R2	0.222	0.365	0.167	0.324	0.224	0.366	0.167	0.324
Panel B: Female								
Placebo cohorts	-0.0816	0.0779						
	(0.107)	(0.171)						
Placebo cohorts			-0.0158	-0.0522				
			(0.0546)	(0.0541)				
Placebo cohorts					-0.0230	0.00553		
					(0.0214)	(0.0315)		
Placebo cohorts							0.00242	-0.00763
							(0.0137)	(0.0136)
Observations	365	340	3747	3697	365	340	3747	3697
Adjusted R2	0.221	0.418	0.189	0.372	0.222	0.417	0.189	0.372
Panel C: Male								
Placebo cohorts	-0.0574	0.0539						
	(0.201)	(0.243)						
Placebo cohorts			0.0727	0.0134				
			(0.0532)	(0.0692)				
Placebo cohorts					-0.0334	-0.0281		
					(0.0500)	(0.0490)		
Placebo cohorts							0.0188	0.000644
							(0.0128)	(0.0161)
Observations	322	314	3262	3212	322	314	3262	3212
Adjusted R2	0.240	0.383	0.144	0.280	0.243	0.384	0.144	0.280
Fixed effects	N	Y	N	Y	N	Y	N	Y

Notes: Authors' calculation from VHLSS 2014, VHLSS 2016 and VHLSS 2018. The older cohorts (born between 1940-1949) are divided into two groups according to the birth year (i.e., 1945-1947 vs 1948-1949) while the younger cohorts (born between 1972-1981) are divided into two groups (i.e. 1972-1976 vs 1977-1981). Robust standard errors in parentheses are clustered at provincial level.

Table A3: The causal effects of exposure to bombing exposure on education of second generation (Excluding parents' education)

Dependent variable: Years of education

	Whole sample		Daughter		Son	
	2SLS	First stage	2SLS	First stage	2SLS	First stage
Panel A: Father's bombing exposure						
Intensity × Months exposed to bombing in utero	-0.00141		-0.00214		-0.000542	
	(0.00281)		(0.00355)		(0.00267)	
Intensity × Months exposed to bombing after birth	-0.00108**		-0.000284		0.000377	
	(0.000487)		(0.000369)		(0.000310)	
Distance		-1.662***		-1.785***		-1.536***
		(0.400)		(0.439)		(0.388)
Observations	8252	8252	3254	3254	4998	4998
Adjusted R2	0.280		0.193		0.120	
Sanderson-Windmeijer F-stats	37.04		47.99		41.97	
Panel B: Mother's bombing exposure						
Intensity × Months exposed to bombing in utero	0.000412		-0.000137		0.000235	
	(0.00195)		(0.00306)		(0.00227)	
Intensity × Months exposed to bombing after birth	-0.000373*		-0.000640**		-0.000378	
	(0.000221)		(0.000292)		(0.000270)	
Distance		-1.640***		-1.779***		-1.562***
		(0.422)		(0.478)		(0.394)
Observations	10210	10210	4056	4056	6154	6154
Adjusted R2	0.151		0.197		0.123	
Sanderson-Windmeijer F-stats	46.46		47.46		44.24	
Cohort FE	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y

Notes: Authors' calculation from VHLSS 2014, 2016 and 2018. The main explanatory variable is the interaction between the average number of herbicide gallons sprayed in the province of birth and the total number of months exposed to the bombings. Fixed effects and control variables are included in all regressions. Robust standard errors in parentheses are clustered at the provincial level. * p<0.10 ** p< 0.05 *** p<0.01.