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Intergenerational mobility of education in Vietnam

Evidence from the Vietnam War

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Abstract: Vietnam's education system has recently attracted international attention for exceptional learning outcomes and success in improving schooling outcomes over a short period, despite being a lower-middle-income country. One potential explanation is the substantial increase in parental schooling after the Vietnam War ended in 1975, which might have led to better educational outcomes for the next generation. This study examines the causal effect of parental schooling on children's educational attainment in Vietnam. We exploit variation in parental exposure to aerial bombing at an early age to identify the effect of parental schooling. Our instrumental variable estimates indicate that the father's schooling does not affect a child's educational outcomes. Furthermore, we find that although parental bombing exposure reduced their schooling, it did not affect children's educational outcomes or parental investment in their children. Taken together, these findings suggest that Vietnam's recent success in education is not caused by the rise in the schooling of the parents of today's children after the war ended in 1975.

Key words: intergenerational mobility of education, Vietnam, Vietnam War

JEL classification: I20, J62, C36

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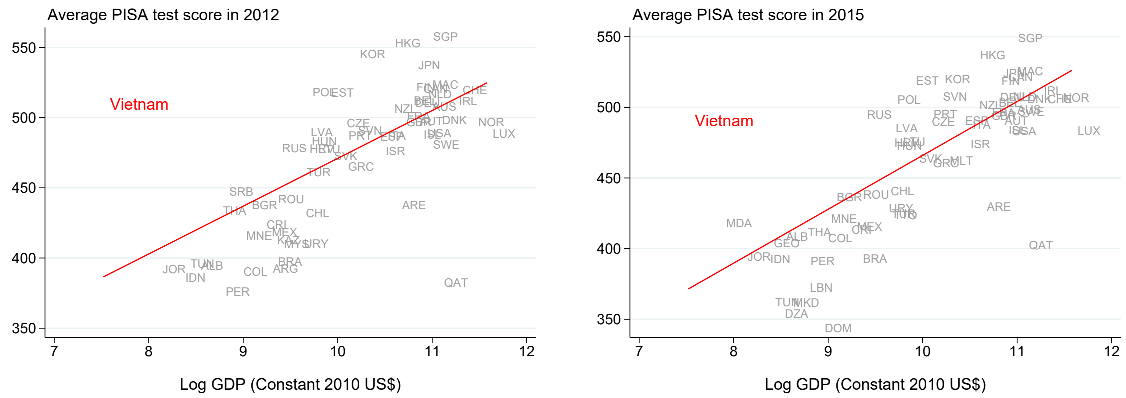
1 Introduction

Vietnam has recently been recognized for the exceptional performance of its educational system, despite being a lower-middle-income country.[1] For example, Vietnamese students performed very well on the Programme for International Student Assessment (PISA) in 2012 and 2015, with results comparable to those from wealthy countries like the USA and the UK (Figure 1). Singh (2019) also finds that Vietnamese children have better academic performance than children from India, Peru, or Ethiopia, based on Young Lives data. Vietnam has also been successful at raising school enrolment and educational attainment over a short period of time. Primary and lower secondary enrolment in Vietnam are close to universal, and upper secondary enrolment has almost tripled from 27 per cent in 1992–93 to 70 percent in 2014. The average completed years of schooling have also risen substantially over time and are higher than would be predicted by income (Dang and Glewwe 2018).

Figure 1: PISA scores and GDP per capita by countries



Source: PISA data are provided by the OECD. GDP data are drawn from the World Development Indicators database of the World Bank.

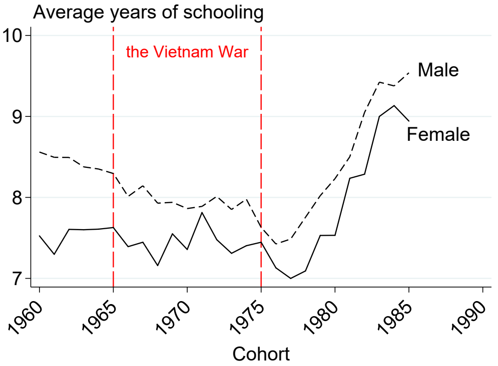
There is growing interest in understanding how Vietnam achieved these successes and, more importantly, whether other low-income countries can learn from Vietnam and improve their educational systems. For example, the Bill and Melinda Gates Foundation (2018) highlighted Vietnam as an exemplar 'of success at scale in low- and lower-middle income countries' for 'achieving system-wide improvements in learning'; the Center for Global Development, in an article titled 'Vietnam's Exceptional Learning Success: Can We Do That Too?', also questioned whether other developing countries could learn from Vietnam. Recent studies have focused on the potential role of systemic factors such as schooling productivity (Dang and Glewwe 2018; Singh 2019) and parental and school inputs (Dang et al. 2017; Dang and Glewwe 2018).

The existing literature, however, has overlooked Vietnam's unique historical context in relation to the comparatively recent end of the Vietnam War in 1975, and how it might have contributed to Vietnam's recent success in education. As shown in Figure 2, those who were born shortly after the war ended in 1975 tend to have much higher levels of schooling than those who were born during or before 1975. This is likely due to the fact that the cohorts born before or during the war were exposed to health risks such as poor nutrition or maternal stress (Akresh et al. 2017; Alderman et al. 2006), and other negative factors of schooling such as violence, instability, or use of child-soldiers (Blattman and Annan 2010). This increase in schooling due to the end of the war might also have affected the next generation's educational outcomes; for example, parents with improved education may have invested more in their children, allowing them to do better in school and during exams. In other word, the recent success in

[1] For example, see Business Insider (2016), Center for Global Development (2018), and the Gates Foundation (2018).

education in Vietnam that can be observed in recent cohorts of children might have been due to the increase in parental schooling linked to the end of the Vietnam War.

Figure 2: Average years of schooling by year of birth and gender



Source: authors' estimation based on 2006–16 VHLSS (Vietnam Household Living Standard Survey) data.

The causal link between parental schooling and a child's educational outcome, however, has been studied mainly in the context of developed countries.[2] These studies also use various empirical strategies and, hence, provide various estimates for the causal effects of parental schooling on children's education. Therefore, it is unclear whether and which of these findings can be applied to Vietnam.

In this study, we leverage a natural experiment from the Vietnam War to identify the causal effect of parental years of schooling on a child's educational outcome. During the war, a significant number of aerial bombs were dropped by the US Air Force, with the number dropped varying substantially across provinces between 1965 and 1972 (Miguel and Roland 2011). We subsequently exploit the variation in bombing intensity across parental birthplace, as well as variation in exposure to bombing across parental birth cohorts, as a source of exogenous variation in parental years of schooling. Assuming that the effects of bombing on parental education are not genetically inheritable, this design allows us to examine whether changes in parental education induced by bombing exposure affect children's educational outcomes.

Formally, we use parental exposure to bombing at an early age to instrument for parental years of schooling. This approach relies on two key assumptions. The first assumption is that parental bombing exposure is exogenous to parental years of schooling and a child's educational outcomes. The second assumption is that parental bombing exposure only affects a child's education through parental schooling.

We find that OLS (ordinary least squares) estimates are positive and statistically significant, indicating that there is a positive association between parental schooling and a child's educational attainment. However, our IV (instrumental variable) estimates suggest that the father's years of schooling does not have any effect on a child's educational attainment. Specifically, the IV point estimates are close to zero and statistically insignificant. The first-stage estimates reveal that the father's bombing exposure has a large and negative impact on his years of schooling and that it passes the weak instrument test. In contrast, our IV estimates for the mother's years of schooling is invalid because the mother's bombing exposure does not affect her years of schooling.

[2] See, *inter alia*, Oreopoulos et al. (2006), Black and Devereux (2010), Holmlund et al. (2011), and Majlesi et al. (2019). Two exceptions are Alesina et al. (2019) on 26 African countries and Asher et al. (2018) on India; both of these studies focus on documenting the geography of intergenerational mobility of education and what factors drive it, and do not measure the causal effect of parental schooling on children's education.

This study draws on several data sources. The main sample consists of individuals age seven years or older in the 2014 and 2016 Vietnam Household Living Standard Survey (VHLSS). We restrict the sample to those with parents born between 1965 and 1980. We merge the bombing intensity data at the province level provided by (Miguel and Roland 2011) with parental province of birth, and combine with parental dates of birth to construct our measure of parental exposure to bombing at an early age.

We supplement our analyses with other data sources to address various important concerns. First, we construct an extended VHLSS sample for the period 2006–16 to address the sample selection bias arising from the co-residence restriction (i.e. only individuals co-residing with parents are observed in the sample (Francesconi and Nicoletti 2006)). We also use the 2006–16 VHLSS data to validate our main findings by showing that parental bombing exposure does not affect investment in children's health and education. Second, we use the 2009 Vietnam Population and Housing Census (IPUMS 2018) to show that parental exposure does not affect parental disability status. This addresses a potential concern that low birth weight, which is often associated with war exposure in utero (Lee 2014b), may affect parental disability (Elder et al. 2019).

The contribution of this study is threefold. First, we find that parental schooling has no causal effect on a child's educational attainment, which is consistent with the conclusion from most studies that also use IV (Black and Devereux 2010; Black et al. 2005; Holmlund et al. 2011; Majlesi et al. 2019).[3] Most of these studies exploit different compulsory schooling reforms in a developed country as an exogenous shock to parental schooling. The fact that we arrive at the same conclusion using a different instrument and different data supports the internal and external validity of these studies. Second, our findings confirm that war exposure in utero or at an early age can have severe effects on schooling, but the reduced-form estimates suggest that they do not affect the educational outcome of the subsequent generation. This contributes to a long list of studies on the impacts of war and conflict on human capital across different generations.[4]

More importantly, we contribute to a growing number of studies analysing Vietnam's recent success in education. Previous studies have focused on contemporary factors such as household income and parental background (Dang and Glewwe 2018), schooling productivity (Singh 2019), and other parental and schooling inputs (Dang et al. 2017). This is the first study to consider the potential role of the Vietnam War and the significant increase in parental schooling following the end of the war in 1975. While the Vietnam War had a detrimental impact on the schooling of those who were exposed to it in the early years of their lives, we find no evidence that the war affected the schooling outcomes of their offspring. This can be explained by the fact that there is no substantial difference in investment in children's education and health among the exposed and unexposed parents.

It is important to note that we only estimate the impacts of parental schooling on one dimension of children's education: educational attainment. It is possible that parental schooling and parental exposure to bombing do not affect educational attainment, but that they affect other important dimensions of a child's education, such as cognitive skills measured by test scores and non-cognitive skills. Unfortunately, the VHLSS data do not provide information on these educational outcomes, while other data with such information do not provide sufficient information on parental background to support our analysis.

The rest of the paper is organized as follows. In Section 2, we discuss the data and the empirical strategy. In Section 3, we present and discuss our results and robustness checks. In Section 4, we present our conclusion.

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[3] Two other IV studies with the opposite conclusion are Oreopoulos et al. (2006) and Carneiro et al. (2013), which find that parental schooling reduces the probability of grade repetition.

[4] For example, see Bundervoet et al. (2009), Akresh et al. (2012), Lee (2014a,b), and Quintana-Domeque and Ródenas-Serrano (2017).

2 Data and empirical strategy

2.1 Data

This study draws from a number of sources of data. The main sample is constructed from the VHLSS. This national household survey is conducted biannually by the General Statistic Office (GSO) with support from the World Bank. The main sample is constructed from two survey years: 2014 and 2016. Although data for earlier years are available, information about province of birth to construct parental exposure to bombing is only obtain through these two surveys.

We construct the main sample using the following procedure. First, we select all individuals listed as a child of the household head. Using information on gender and relationship with the household head, we identify father and mother to each individual in this sample. Second, we restrict the sample to individuals with a father or mother conceived between 1965 and 1978. The 1965 cutoff is chosen because we only want to include cohorts that were exposed from conception onward; those who were conceived before 1965 might have been affected differently. The 1978 cutoff is chosen because there was a series of national land reforms between 1980 and 1993 (Do and Iyer 2008); we do not want to include parental cohorts exposed in utero to these major policy changes. In the robustness check section, we show that our estimates are robust to various cutoffs for the parental cohorts. Because we study individuals' educational attainment, we further restrict our sample to individuals aged seven years or older (i.e. when individuals typically have already completed at least one year of schooling). Lastly, we flag individuals whose age difference with either parent is below 15 years or above 50 years as erroneous, and drop them from the sample. Descriptive statistics are provided in Table 1.

Table 1: Summary statistics

Variable	Mean	SD	Min	Median	Max
Father					
Years of schooling	7.45	3.86	0.00	8.00	16.00
Year of birth	1971.06	3.83	1965.00	1971.00	1978.00
Age	42.84	3.92	34.00	43.00	51.00
Mother					
Years of schooling	6.98	3.94	0.00	8.00	16.00
Year of birth	1971.57	3.90	1965.00	1972.00	1978.00
Age	42.32	4.01	34.00	42.00	56.00
Child					
Years of schooling	7.79	3.66	0.00	8.00	16.00
Grade-for-age	0.65	0.48	0.00	1.00	1.00
Female	0.47	0.50	0.00	0.00	1.00
Urban	0.28	0.45	0.00	0.00	1.00
Year of birth	1998.11	5.32	1981.00	1998.00	2009.00
Age	15.76	5.28	7.00	15.00	34.00

Source: authors' estimations based on data from the VHLSS 2014–16.

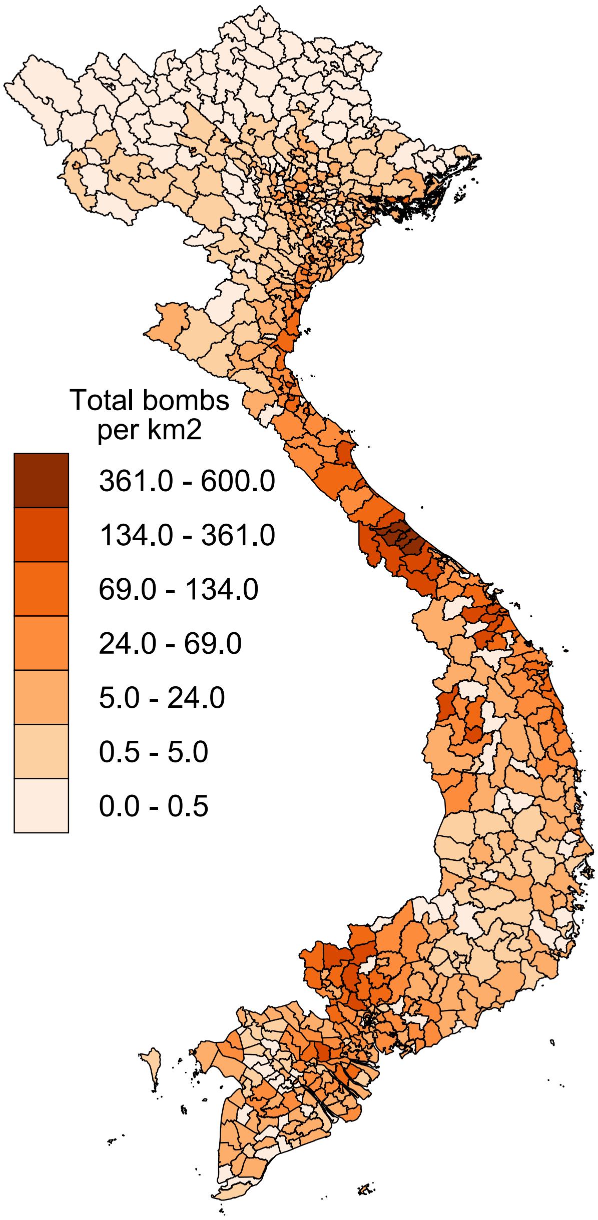
The main outcome variable is children's educational attainment. Following Oreopoulos et al. (2006) and Akresh et al. (2018), we measure educational attainment by a grade-for-age variable indicating whether an individual is on track compared to peers of the same age.[5] For parental educational attainment we use years of schooling, which are constructed from information on highest grade and highest degree completed.[6]

[5] For example, for an individual aged seven, this variable is 1 if the individual has completed one year of schooling, and 0 otherwise. For an individual aged eight, this variable is 1 if the individual has completed two years of schooling, and 0 otherwise. For individuals aged 18–21, this variable is 0 if the individual did not complete grade 12. For individuals aged 22, this variable is 0 if the individual did not complete a tertiary degree.

[6] In most cases, years of schooling is equal to highest grade completed. If an individual also completed a university degree, then we add four years of schooling. Similarly, we add 1–3 years of schooling for completion of a vocational degree, depending on the type of degree.

Next, we construct a continuous variable to measure bombing intensity at the province level using data provided by Miguel and Roland (2011).[7] Specifically, we merge the log total number of bombs per square kilometre at the province level with parental province of birth. The original data for bombing intensity, measured by number of bombs per square kilometre, are shown by district level in Figure 3.

Figure 3: Bombing intensity at the district level



Source: authors' creation based on bombing intensity data provided by Miguel and Roland (2011).

Lastly, we construct a binary variable indicating whether an individual was exposed to the bombing using information on year of birth. During the war, between 1965 to 1975, there were two major bombing periods. The first was 1965–68 and was associated with Operation Rolling Thunder. The second was 1972 and was associated with Operations Linebacker I and II. Using date of birth, we construct a variable indicating whether individuals were conceived during the 1965–72 bombing period or afterwards.

[7] According to the authors, these data are provided by the Vietnam Veterans of America Foundation with authorization from the Defense Security Cooperation Agency.

As explained later, the parental bombing exposure variable is constructed by interacting the parental province of birth's bombing intensity and the binary variable for whether the parent was born between 1965 and 1972.

2.2 Identification strategy

In this study, we exploit variation in parental exposure to the bombing period between 1965 and 1972 during the Vietnam War to identify the causal effect of parental years of schooling on children's education. The assumption here is that exposure to war and conflict in utero or at a very early age lead to worse educational outcomes through the cognitive development channel (Akresh et al. 2017; Alderman et al. 2006). This relationship, which has been tested extensively in the economic literature, relies on the fact that foetal responses to maternal nutritional shortages might compromise later-life health and cognitive conditions. Moreover, a large and growing number of biomedical and socioeconomic studies have documented the existence of a critical period in human life (namely the first three years) when brain development is most sensitive to shocks that result in poor nutrition.[8]

Because the effect of parental bombing exposure on parental education is not genetically inheritable by the child, using parental exposure as an instrument allows us to separate the parenting behaviours from the genetic correlation channel. Specifically, we specify our baseline model for individual i with parent born in province p and cohort c as follows:

$$\begin{aligned}\text{grade-for-age}_i = {} & \alpha.\text{schooling}_i^{parent} + \delta.\text{age}_i + \theta.\mathbf{X}_i^{parent} \\ & + \text{province}_p^{parent} + \text{cohort}_c^{parent} + {}_i\end{aligned} \tag{1}$$

$$\begin{aligned}\text{schooling}_i^{parent} = {} & \beta.(\text{bomb}_p^{parent} \times \text{exposed}_c^{parent}) + \eta.\text{age} + \mu.\mathbf{X}_i^{parent} \\ & + \text{province}_p^{parent} + \text{cohort}_c^{parent} + u_i\end{aligned} \tag{2}$$

where grade-for-age$_i$ denotes a child's educational attainment and schooling$_i^{parent}$ denotes a parent's years of schooling; bomb$_p^{parent}$ and exposed$_c^{parent}$ denote bombing intensity in province p and whether the parent was conceived between 1965 and 1972, respectively; age$_i$ denotes a child's age fixed effects; and province$_p^{parent}$ and cohort$_c^{parent}$ denote parental province of birth and parental cohort fixed effects. $\mathbf{X}_i^{parent}$ is a vector of controls for parental characteristics including parental age and ethnicity.

We estimate this baseline model using two-stage least square with Equation 2 as our first stage and Equation 1 as our second stage. In this setup, α in Equation 1 is the parameter of interest as it represents the effect of parental years of schooling on a child's educational outcome. The interaction term, bomb$_p^{parent} \times$ exposed$_c^{parent}$, in Equation 2 is the IV for the endogenous variable parental years of schooling and β represents the effect of parental exposure to bombing on parental years of schooling.

The first-stage equation is a difference-in-differences model capturing the effect of parental exposure to bombing on parental years of schooling. The second stage estimates the effect of changes in parental years of schooling induced by parental bombing exposure on a child's educational outcome. This model relies on two important assumptions: (1) the instrument is exogenous on parental years of schooling and child's education, conditional on parental province of birth and cohort fixed effects; and (2) parental exposure only affects a child's educational outcome via parental years of schooling (IV excludability assumption).

[8] See, *inter alia*, Dobbing (1976), Glewwe and King (2001), Bhalotra and Venkataramani (2013), Almond et al. (2018), and Lo Bue (2019).

2.3 Instrument validity

The IV exogeneity assumption implies that the parallel trends assumption has to hold for the first stage—that is, in the absence of bombing parental years of schooling would have followed the same cohort trends across provinces. Furthermore, the same assumption also has to hold for the reduced-form model which regresses a child's educational outcome directly against parental exposure to bombing. Formally, the reduced-form model takes the following form:

$$\begin{aligned}\text{grade-for-age}_i = \beta.(\text{bomb}_p^{parent} \times \text{exposed}_c^{parent}) + \eta.\text{age} + \mu.\mathbf{X}_i^{parent} \\ + \text{province}_p^{parent} + \text{cohort}_c^{parent} + u_i \qquad (3)\end{aligned}$$

To check whether controlling for province fixed effects is sufficient, we conduct two standard statistical tests to examine whether the parallel trends assumptions are likely violated. First, we estimate another specification in which we additionally control for province–cohort trends in the baseline model; that is, we interact each province indicator with a cohort trend variable as controls in the baseline model. This allows outcome variables to follow different cohort trends across provinces of different bombing intensity levels. These controls will absorb any unobserved heterogeneity that is cohort-varying across provinces.

Second, we conduct event study analysis for both the first-stage model and the reduced-form model. These estimates allow us to observe the effects of bombing intensity on the educational outcomes of each parental cohort and their child. Because the parental cohorts conceived after 1972 were not exposed to the bombing, there should not be any correlation between the bombing intensity and the educational outcomes of these unexposed cohorts conditional on the controls listed. In other word, if there is any notable correlation then the exogeneity assumptions are likely violated. While these statistical checks are useful to detect endogeneity, it is not sufficient to show that the IV exogeneity assumption would hold.

For instance, a potential concern of using the geographic variation in bombing intensity as exposure or instrument is that bombing intensity is unlikely to be random. Given that aerial bombing is a common tactic of counterinsurgency warfare, the amount of bombing in each province might have been selected strategically to reduce insurgent control of the population in that area (Kocher et al. 2011). In our model, we account for unobserved factors that might have been correlated with bombing intensity and parental years of schooling by controlling for parental province of birth fixed effects.[9]

A different approach from previous studies using these bombing data is instrumenting for the bombing intensity using provincial distance to the 17th Parallel, the demilitarized zone that separated the Democratic Republic of Vietnam in the north and the Republic of Vietnam in the south (Groce et al. 2015; Miguel and Roland 2011; Singhal 2018). We find that the two approaches provide similar results, once we control for province of birth fixed effects. Specifically, we re-estimate Equations 2 and 3 separately

[9] An underlying assumption that we make is that parental province of birth is the same as province of conception and exposure to bombing. One potential concern is that a mother residing in a province with greater bombing intensity might have migrated during pregnancy and given birth in a province with lower bombing intensity, so the exposure intensity would be mismeasured. We use the Vietnam Living Standard Survey (VLSS) 1997–98 to document the extent to which individuals in the parental cohort were born in a province that is different from their province of conception. For this purpose, we construct a sample of individuals born between 1965 and 1980 who are children of the household head. While there are no data on place of conception, we compare an individual's place of birth with their eldest sibling's place of birth, assuming that these individuals were conceived in the same province that their elder sibling was born. We further limit the sample to those who are not the eldest child in the household. We find that out of 2,314 individuals in the sample, only 196 individuals were born in a different province compared to their eldest sibling. Therefore, the assumption that parental province of birth is the same as province of conception is reasonable.

but we use province distance from the 17th Parallel as the instrument for bombing intensity:

$$\begin{aligned}\text{outcome}_i = \beta^{IV}.(\text{bomb}_p^{parent} \times \text{exposed}_c^{parent}) + \eta.\text{age} + \mu.\mathbf{X}_i^{parent} \\ + \text{province}_p^{parent} + \text{cohort}_c^{parent} + u_i\end{aligned} \tag{4}$$

$$\begin{aligned}\text{bomb}_p^{parent} \times \text{exposed}_c^{parent} = \gamma.(\text{distance}_p^{parent} \times \text{exposed}_c^{parent}) + \varkappa_1.\text{age} + \varkappa_2.\mathbf{X}_i^{parent} \\ + \text{province}_p^{parent} + \text{cohort}_c^{parent} + u_i\end{aligned} \tag{5}$$

where $\text{distance}_p^{parent}$ denotes distance from parental province of birth to the 17th Parallel. For the first-stage equation, outcome_i is parental schooling, and for the reduced-form equation, outcome_i is child's educational outcome.

We present the results of this analysis in Table A1 in the Appendix. In panel A we report the results from estimating the first-stage and the reduced-form equations from the main specification. In panel B we report the results from estimating using the instrumented bombing exposure—that is Equations 4 and 5. The results from using bombing exposure and instrumented bombing exposure are very similar for the first-stage and reduced-form equations for both parents. Under the assumption that the distance to the 17th Parallel is a valid IV, these results strongly indicate that bombing exposure is exogenous to parental schooling and child's educational outcome when we control for parental province of birth.

The IV excludability assumption is another crucial assumption for this identification strategy. Specifically, if the parental bombing exposure also affects a child's educational outcome through another causal pathway, then our estimates are biased. Previous studies have found that war and conflict exposure in utero or at an early age can affect not only educational outcomes, but also labour market performance and adult health, especially disabilities and mental health (Lee 2014a; Singhal 2018; Weldeegzie 2017). If parental bombing exposure affects non-educational outcomes directly, and not through the years of schooling channel, then the instrument is no longer valid. While this assumption is not directly testable, we proceed to check whether parental bombing exposure affects these non-educational outcomes among parents. Specifically, we use the difference-in-differences model in the first-stage equation, but replace parental years of schooling with other variables, such as their labour market outcomes, health, or wealth. We also control for province–cohort trends as a robustness check.

As shown in Table A2, we do not find any statistically significant relationship between parents' exposure and their occupational sector. Moreover, among wage-earning parents, exposure, especially among men, tends to not be associated with changes in wage.

When examining the parental exposure–adult health channel,[10] we find that the point estimates for the effects on parental disability are extremely small, and the signs are not consistent across different types of disability. Overall, we find no robust evidence that exposure is associated with a higher chance of disability (see Table A3).

Furthermore, we also measure parental exposure's impacts on a household's socioeconomic status. Specifically, we consider log household income per capita, household income quintile, household poverty status in the previous year, and household five-year cumulative poverty (5/5 if in poverty for five years, 4/5 if in poverty for four years, and so on). The results are presented in Table A4. We find that the father's exposure does not have any effect on household socioeconomic status, while the mother's exposure has some statistically significant effects across different outcomes, but the magnitudes of these effects are small and not robust.

[10]For this check we use data from the 2009 Population and Housing Census (IPUMS 2018), which provides detailed information about disabilities.

Lastly, we check whether parental exposure affects spousal characteristics. For instance, Akresh et al. (2018) find that parents exposed to school construction in Indonesia are associated with higher spousal education. The results are presented in Table A5. We find that exposed fathers tend to marry a spouse with less schooling; this is unsurprising, given that exposed fathers tend to have less schooling themselves. Furthermore, we also find that exposed fathers tend to marry a spouse with an unpaid job. The coefficient estimates for other variables are small and not robust to province-specific cohort trend controls. While it appears that the father's bombing exposure also affects spousal schooling and work, these effects are likely through the father's schooling channel instead of through a separate causal pathway. That is, the father's exposure his schooling which, in turn, leads to getting married to a spouse with less schooling (and working in an unpaid job). Therefore, we conclude that this is not a violation of the exclusion restriction.

3 Results

3.1 Main results

We report the main results in Table 2. We consider two specifications. In panel A, we report the results from estimating the baseline model. The baseline model includes controls for parental province of birth and parental cohort fixed effects, child's gender and age fixed effects, and household head's ethnicity. In panel B, we add controls for province–cohort trends in the baseline model. In all models, standard errors are clustered at the parental province of birth level and VHLSS sampling weights are applied to account for the survey design. Given the non-homoscedastic setting, we report the effective first-stage F-statistic for the IV estimates (Andrews et al. 2019; Olea and Pflueger 2013). Following a recommendation by Andrews et al. (2019), we also present the Anderson–Rubin statistics, which are efficient regardless of the instrument's strength.

Table 2: Estimates of the impact of parental years of schooling on child's educational attainment

	Father's education				Mother's education			
	OLS	First stage	IV	Reduced form	OLS	First stage	IV	Reduced form
Educational outcome of	Child (1)	Father (2)	Child (3)	Child (4)	Child (5)	Mother (6)	Child (7)	Child (8)
Panel A: baseline model								
Parental education	0.025***		–0.010		0.030***		–0.053	
	(0.002)		(0.025)		(0.002)		(0.062)	
Parental exposure		–0.300***		0.003		–0.164		0.009
		(0.082)		(0.007)		(0.107)		(0.008)
Effective first-stage F-stat			13.4				2.4	
Weak IV p-value			0.69				0.25	
Panel B: controls for province–cohort trends								
Parental education	0.025***		–0.008		0.030***		–0.008	
	(0.002)		(0.040)		(0.002)		(0.022)	
Parental exposure		–0.345**		0.004		–0.017		0.028*
		(0.134)		(0.014)		(0.143)		(0.014)
Effective first-stage F-stat			6.5				0.0	
Weak IV p-value			0.78				0.06	
Observations	9,813		9,813	9,813	10,254		10,254	10,254
Dep. var. mean	0.650	7.452	7.452	7.452	0.650	6.985	6.985	6.985

Notes: all models control for parental age fixed effects, parental cohort and province of birth fixed effects, and child's gender and age fixed effects. Parental bombing exposure is constructed by interacting bombing intensity of parental province of birth and an indicator of whether parents were conceived before 1973. Standard errors are clustered at the parental province of birth level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on VHLSS 2014–16 data.

Columns 1 and 5 report the OLS estimates for the relationship between a father's (mother's) years of schooling and a child's educational outcome. These estimates are positive and statistically significant, suggesting that an additional parental year of schooling is associated with an increase in the likelihood of the child staying on track compared to his or her peers.

Columns 2 and 3 report the results from the first and second stages of our IV estimation for the child's education against the father's years of schooling. The first-stage estimates are negative, significant, and robust to controls for province–cohort trends, indicating a strong and negative relationship between exposure to bombing during the war and the father's years of schooling. IV estimates for the child's education against the father's years of schooling are small and statistically insignificant. Specifically, the point estimates are –0.010 in the baseline model and –0.014 when controlling for province–cohort trends.

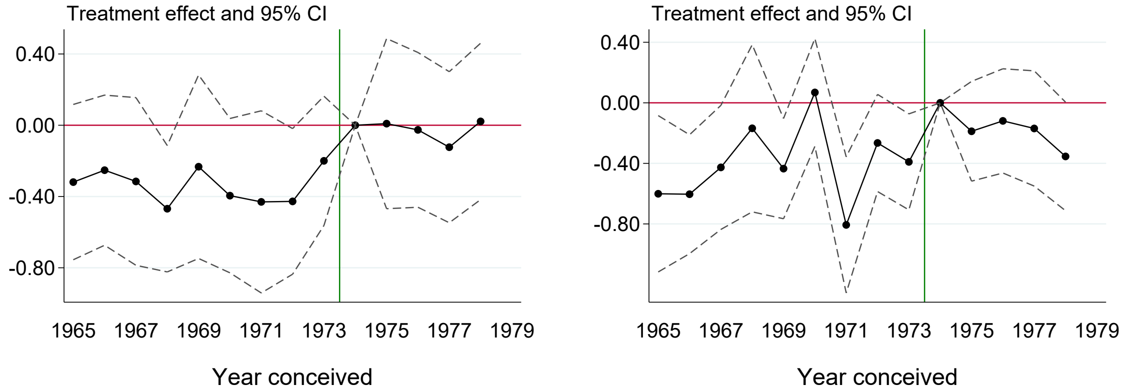
The reduced-form estimates in Column 4 indicate that the father's exposure to bombing does not affect the child's educational attainment. This is consistent with the fact that IV estimates are close to zero and insignificant. These results, along with the fact that the effect of the father's bombing exposure is not genetically inherited by the child, indicate that the positive correlation between the father's years of schooling and the child's education is related to genetics.

Similarly, Columns 6 and 7 report the results from IV estimates for the child's education against the mother's years of schooling. Interestingly, the effect of bombing exposure on the mother's education is also negative but much smaller, and not robust to controlling for province–cohort trends. It follows that IV estimates are not reliable because the instrument is not relevant—that is, women exposed to the war in their early years of life did not have significantly different education outcomes to non-exposed individuals. A similar gender bias in exposure to the war in childhood on subsequent education outcomes, particularly disfavouring boys, has been found by Justino et al. (2013) for Timor-Leste, Akresh and De Walque (2008) for Rwanda, and Verwimp and Van Bavel (2013) for Burundi. Nonetheless, the reduced-form estimates in Column 8 also suggest that the mother's bombing exposure does not affect the child's education.

To visualize the first-stage results, we present the event study analysis for the effects of parental bombing exposure on parental years of schooling in Figure 4. In panel A we find that exposure to bombing is associated with a reduction in years of schooling among father cohorts conceived between 1965 and 1972 (i.e. the exposed cohorts). In contrast, we find no association between exposure and years of schooling among the cohorts conceived between 1973 and 1980 (i.e. the unexposed cohorts). In panel B, we find no association between the mother's exposure and the mother's years of schooling among the exposed and unexposed cohorts. These results are consistent with the estimates in Table 2. Similarly, we present the event study estimates for the effects of parental bombing exposure on a child's educational outcome in Figure 5. We find no evidence that parental exposure affected children's outcomes. The fact that our results are very similar when controlling for province–cohort trends suggest that the IV exogeneity assumption likely holds, especially for fathers' bombing exposure.

We also apply the same analysis separately to educational attainment of sons and daughters and report the results in Table 3. First, we find that OLS estimates are similar across parental and child gender. Second, we find that fathers' schooling does not affect either sons' or daughters' education. Given that mothers' exposure is not a relevant instrument in both cases, our IV estimates are also not valid.

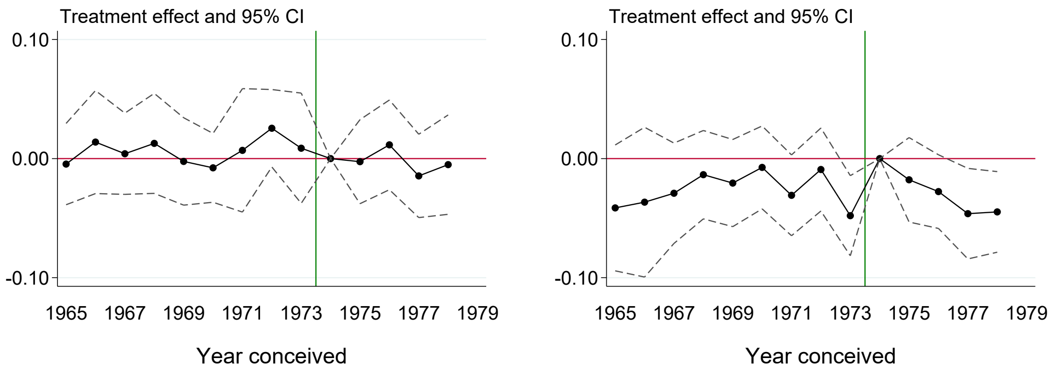
Figure 4: Effects of parental bombing exposure on parental years of schooling (first stage): (a) father; (b) mother



Note: dynamic difference-in-differences estimates for the first-stage equation. The outcome variables are father's and mother's years of schooling. Each point is an estimate for the interaction term of the bombing intensity and year of the parent's conception. The model controls for parental province of birth, cohort, and age fixed effects, as well as child's age and gender. Standard errors are clustered at the parental province of birth.

Source: authors' estimation based on data from the VHLSS 2014–16.

Figure 5: Effects of parental bombing exposure on child's education (reduced form): (a) father; (b) mother



Note: dynamic difference-in-differences estimates for the reduced-form equation. The outcome variables are child's educational outcome. Each point is an estimate for the interaction term of the bombing intensity and year of the parent's conception. The model controls for parental province of birth, cohort, and age fixed effects, as well as child's age and gender. Standard errors are clustered at the parental province of birth.

Source: authors' estimation based on data from the VHLSS 2014–16.

Table 3: Estimates of the impact of parental years of schooling on children's educational attainment by child's gender

	Father's education				Mother's education			
	OLS	First stage	IV	Reduced form	OLS	First stage	IV	Reduced form
Educational outcome of	Child (1)	Father (2)	Child (3)	Child (4)	Child (5)	Mother (6)	Child (7)	Child (8)
Panel A: child is male								
Parental education	0.029***		0.002		0.030***		–0.036	
	(0.002)		(0.027)		(0.002)		(0.065)	
Parental exposure		–0.329***		–0.001		–0.197*		0.007
		(0.080)		(0.009)		(0.107)		(0.012)
Effective first-stage F-stat			16.9				3.4	
Weak IV *p*-value			0.95				0.54	
Observations	5,166		5,166	5,166	5,457		5,457	5,457
Dep. var. mean	0.594	7.386	7.386	7.386	0.594	6.886	6.886	6.886
Panel B: child is female								
Parental education	0.021***		–0.033		0.028***		–0.101	
	(0.002)		(0.048)		(0.003)		(0.152)	
Parental exposure		–0.247**		0.008		–0.107		0.011
		(0.101)		(0.011)		(0.126)		(0.010)
Effective first-stage F-stat			6.0				0.7	
Weak IV *p*-value			0.44				0.26	
Observations	4647		4647	4647	4797		4797	4797
Dep. var. mean	0.713	7.526	7.526	7.526	0.713	7.098	7.098	7.098

Notes: all models control for parental age, parental cohort, and province of birth fixed effects, and child's gender and age fixed effects. Parental bombing exposure is constructed by interacting bombing intensity of parental province of birth and an indicator of whether parents were conceived before 1973. Standard errors are clustered at the parental province of birth level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the VHLSS 2014–16.

3.2 Robustness checks

Sample selection

Because we only observe individuals in the same household as their parents and omit those who already left, there is a concern that this omission is non-random, which leads to sample selection bias (Akresh et al. 2018; Francesconi and Nicoletti 2006). One approach is to only consider individuals when they are not likely to move out (Akresh et al. 2018). Using the VHLSS for 2010–16, we estimate the average move-out age to be roughly 22. Subsequently, we re-estimate the models on a subsample of individuals aged 7–18, and present the results in Table A9. The estimates for this age-restricted sample are only slightly different from the main findings, suggesting that sample selection is not an issue.

We also re-estimate the model on an alternative sample which is constructed from the 2006–16 VHLSS. Because this alternative sample spans a longer period, we are able to capture the same parental cohorts at earlier ages during which their children are more likely to still stay in their households. An important caveat of this approach is that the 2006–12 VHLSS data do not provide information about province of birth, so we have to use parental province of current residence to merge with the bombing intensity data for this alternative sample. In other words, we have to assume that the bombing intensity of parental province of current residence is not systematically different from bombing intensity of parental province of birth.

We check whether this assumption is reasonable using the main sample from the 2014–16 data. First, we construct a bombing intensity variable using parental province of current residence instead of parental province of birth. We find that they are strongly correlated ($\beta = 0.934$, p-value $= 0.00$), although β is less than 1. This suggests that parental exposure constructed from province of current residence is likely different from that constructed from province of birth. Second, we re-estimate the results on the main sample using this alternative measure of bombing intensity, and compare the results, which are presented in Table A6, with those in Table 2. The estimates for the first-stage equation in Table A6 are smaller than those in the main results. The effect of the father's exposure on his years of schooling is smaller and not robust to province-specific cohort trends controls. In other word, using parental exposure constructed from parental province of current residence can lead to substantial difference in the results (especially for the first-stage estimates).

Therefore, comparing the results from estimations using the 2006–16 sample, presented in Table A7, with the main findings in Table 2 is not useful because the differences in results can be driven either by sample selection or by the difference in the measure of parental exposure. Instead, we compare with the results in Table A6, where we estimate the model on the main sample using a similar measure of parental exposure (using parental province of current residence). The results are strikingly similar in both magnitude and sign across different equations and models. Given that these results rely on the same measure of parental exposure but different samples, the similarity of the results strongly indicates that sample selection is not a major concern.

Other robustness checks

We examine whether our results are robust to different measures of children's educational attainment. Specifically, we consider binary variables indicating whether individuals completed primary education, lower secondary education, and upper secondary education. We also consider years of schooling as an additional outcome. The results are presented in Table A8. Across four different educational outcomes, we find no evidence that instrumented father's schooling and bombing exposure affect a child's education.

Next, we test whether our results are sensitive to different definitions of the parental cohorts. Given that the main sample includes parents conceived between 1965 and 1978, we vary the lower limit between 1960 and 1965 and the upper limit between 1974 and 1980. In other word, we re-estimate the baseline model on 42 different samples (e.g., 1960–74, 1960–75, ..., 1960–80, 1961–74, 1961–75, ...,1961–80, and so on). We report the point estimates and the 95 per cent confidence interval (CI) for the first-stage and IV estimations in Figure A1. The first-stage estimates for fathers range between –0.2 and –0.3 across all 42 samples, and all estimates are statistically significant. On the other hand, the IV estimates for fathers' schooling and children's education are very close to zero and insignificant across all samples. Mothers' first-stage estimates range between –0.2 and –0.15, although the results are only marginally significant. Similarly, the IV estimates for mothers are also small and insignificant. These results indicate that our main findings are robust to different sample definitions.

3.3 Parental bombing exposure and investment for children

The main findings indicate that parental exposure to bombing and parental schooling do not affect children's educational attainment. This implies that parental exposure and parental schooling do not affect parenting behaviours such as investment in their child, assuming that parental exposure does not correlate with genetic factors of parents and children. We test whether this implication is true by measuring the effect of parental exposure on children's educational attainment.

We use the 2006–16 VHLSS to conduct these tests. We consider three investment outcomes: total spending on education, non-tuition spending on education, and spending on preventive care for the child. We consider non-tuition educational spending to account for the fact that tuition is mandatory so parents may not be able to choose whether to spend or not. Furthermore, according to the data, spending on tuition is minimal compared to non-tuition spending.

To capture the parental decision to invest in their child (instead of the child's own decision), we restrict our sample to ages 7–14, when most children are still in school. Given that parental exposure does not appear to affect household income, we control for household income quintile fixed effects in the model. The results are presented in Table 4. The point estimates are close to zero across all outcomes, and are not robust to controls for province–cohort trends. These results strongly indicate that parental bombing exposure does not affect parental investment in children, although exposure reduces parental schooling, as indicated in the previous section. This conclusion is consistent with our main finding that parental schooling does not affect children's educational outcomes.

Table 4: Estimates of the impact of parental exposure on parental investment in child's education and health

	Father		Mother	
Spending outcomes	Baseline model	Robustness check	Baseline model	Robustness check
Spending on education				
Parental exposure	0.1026**	0.0346	0.1453**	–0.0177
	(0.0413)	(0.0620)	(0.0602)	(0.1040)
Mean dep. var.	2.7918	2.7918	2.7918	2.7918
Observations	21,495	21,495	23,852	23,852
Spending on education (non-tuition)				
Parental exposure	0.0020	0.0067	0.0120	0.0167
	(0.0336)	(0.0417)	(0.0308)	(0.0694)
Mean dep. var.	2.0690	2.0690	2.0690	2.0690
Observations	21,495	21,495	23,852	23,852
Spending on preventive care				
Parental exposure	0.0028	0.0057**	–0.0021	–0.0000
	(0.0023)	(0.0026)	(0.0019)	(0.0024)
Mean dep. var.	0.0174	0.0174	0.0174	0.0174
Observations	26,608	26,608	30,656	30,656

Notes: the sample includes individuals aged 7–14. All spending outcomes are in thousand VND. The baseline model controls for individuals' age fixed effects and gender, parental age, province fixed effects, and cohort fixed effects, household income quintile, and survey year fixed effects. The model for the robustness check also controls for province–cohort trends. Standard errors are clustered at the commune-survey year level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the VHLSS 2006–16.

4 Conclusion

The causal link of parental schooling has not been considered in the growing literature on Vietnam's recent success in education. Given that there was a significant rise in schooling for the post-war cohorts, this might have led to better learning and schooling outcomes of the next generations, given the positive correlation between parental schooling and children's educational outcomes. Indeed, we document a positive and statistically significant correlation between parental schooling and children's educational attainment in Vietnam. However, such a positive relationship is also driven by the fact that parental schooling and children's educational attainment are affected by unobserved personal traits and abilities of parents and children, which are genetically correlated.

To address this issue of endogeneity, we use parental exposure to aerial bombing at an early age, during the Vietnam War, as an instrument for parental schooling; assuming that such exposure is not correlated with unobserved traits and abilities, it allows us to identify the causal impact of parental schooling on children's educational attainment. Parental exposure is constructed using variation in parental birth cohorts and variation in bombing intensity across provinces.

After accounting for the endogeneity issue, we find no evidence that parental schooling affects children's education. This finding is only true for the relationship between father and child because of the strong first-stage relationship between the father's exposure and the father's schooling. For the relationship between mother and child, IV estimates are not valid because the mother's schooling does not appear to be affected by the mother's exposure to bombing. These findings are robust when controlling for province–cohort trends, and when estimating using different data or different sample definitions.

The identification strategy of this study relies on two important assumptions: parental bombing exposure is exogenous and parental exposure only affects children's education through parental schooling. We provide several pieces of evidence to support these assumptions. First, we argue that parental exposure is exogenous conditional on parental province of birth and cohort fixed effects. Our event-study analysis indicates that provincial bombing intensity has no effect on parental cohorts born after 1972, when the last bombing campaign ended. We further show that our first-stage and reduced-form estimates are similar when we instrument the provincial bombing intensity with provincial distance to the 17th Parallel—a common approach that other studies use to account for endogeneity in bombing intensity (Groce et al. 2015; Miguel and Roland 2011; Singhal 2018).

To check the exclusion restriction, we first estimate the reduced-form relationship between parental exposure and children's educational outcomes. We find no relationship between the two variables, which supports the exclusion restriction assumption. More importantly, we consider whether parental exposure affects any parental adult outcome that might have been separated from the schooling channel. Specifically, we find that parental exposure does not appear to have an effect on parental labour market outcomes, despite having a negative impact on their schooling. Furthermore, we also find no effect on parental health and socioeconomic status. Lastly, we find evidence that exposed fathers tend to marry spouses with less education and an unpaid job or lower wage; this is likely due to assortative mating. Therefore, we argue that the exclusion restriction likely holds for our study design.

We further validate our results by considering whether parental exposure affects parental investment in a child. Using a subsample of children aged 7–14, we find no evidence that parental exposure is associated with changes in spending on a child's education and health, despite the negative effect on parental schooling. This is consistent with the null findings from estimating the reduced-form equation that parental exposure has no effect on children's education.

The results are consistent with previous studies' finding that parental schooling does not affect children's education once we account for the genetic correlation channel (Black and Devereux 2010; Black et al. 2005; Majlesi et al. 2019). Since the existing literature mostly relies on data from developed countries and on compulsory schooling reform as the instrument for parental schooling, these results show that this conclusion also holds in a developing country setting and when using a different instrument.

Furthermore, this study contributes to the ongoing debate about what is behind Vietnam's success in learning and schooling outcomes by showing that while aerial bombing had a detrimental effect on parental schooling, it does not affect the next generation's educational outcomes. This suggests that the sharp increase in parental schooling after the end of the Vietnam War, shown in Figure 2, was not a factor behind the recent success of Vietnam's educational achievements. The fact that the substantial rise in parental schooling due to the end of the Vietnam War does not have a causal effect on children's educational attainment highlights the importance of contemporaneous factors such as schooling productivity and parental and school inputs.

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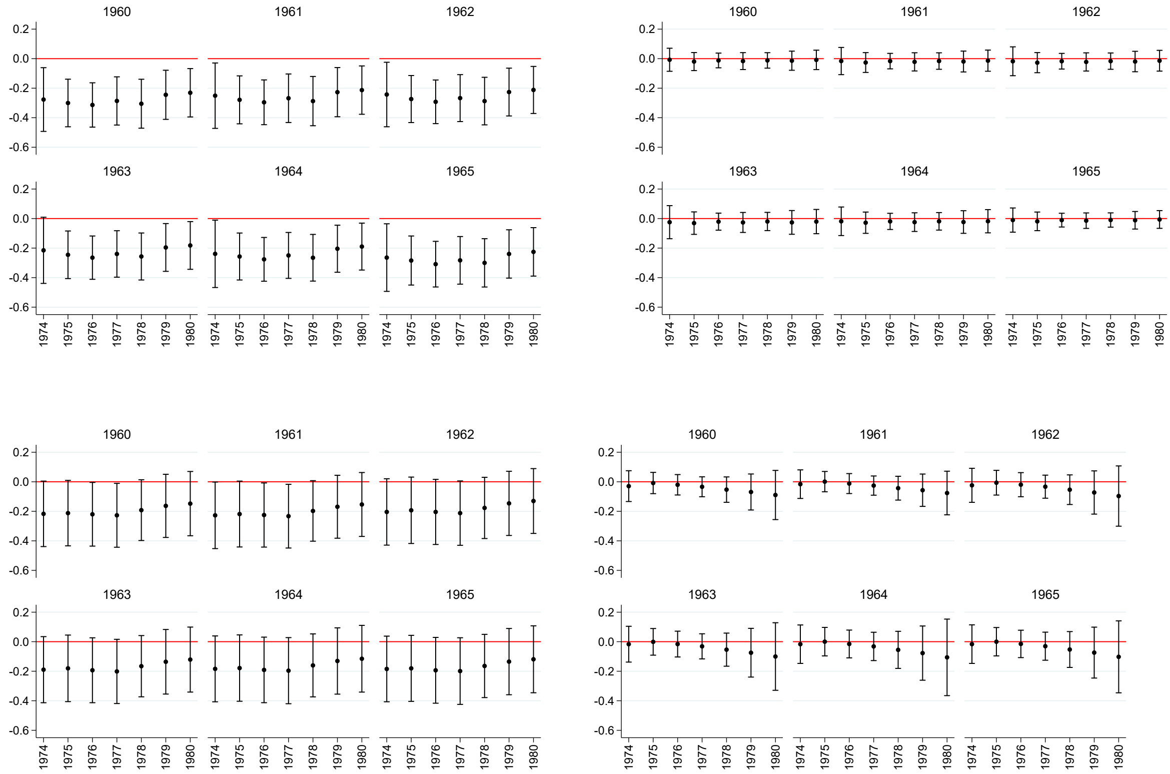
Appendix A

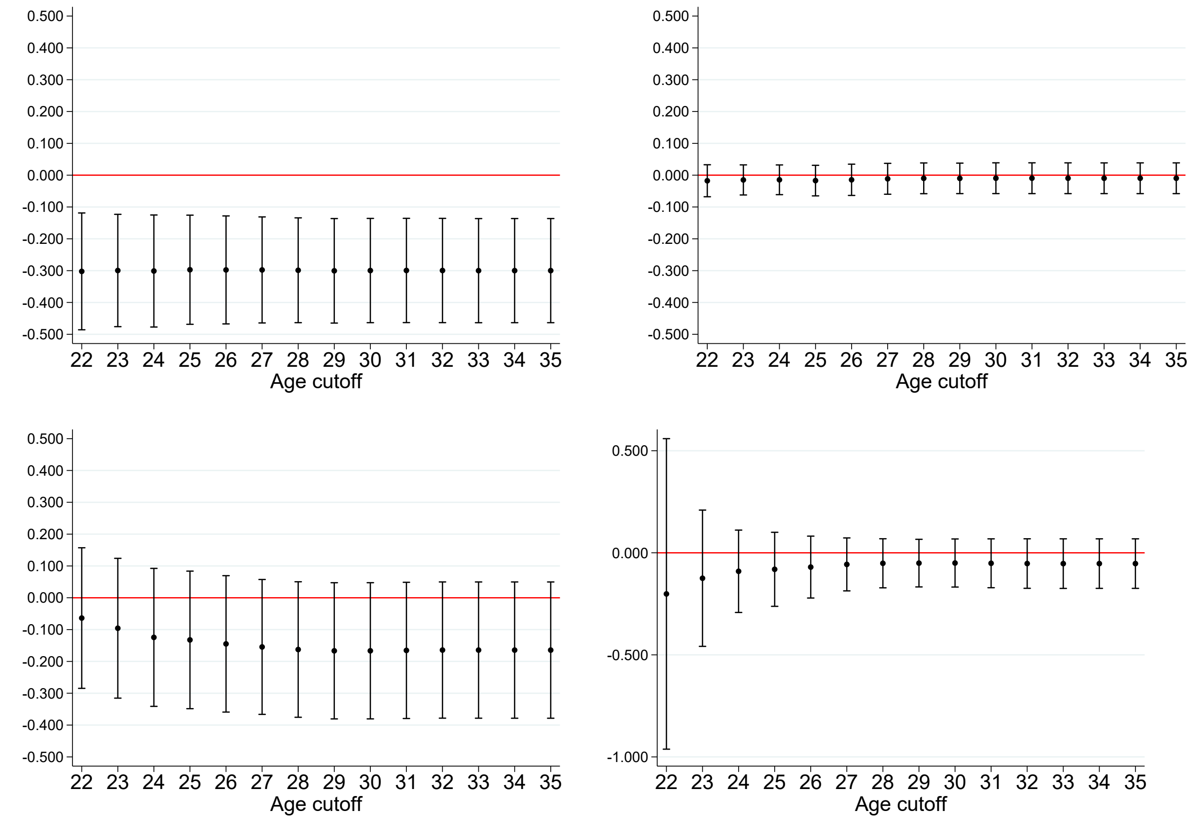
Figure A1: Robustness check: alternative samples



Note: the baseline model is estimated for 42 samples constructed from different parental cohorts. Each point represents the point estimate and the 95 per cent CI of the coefficient of interest for each sample. Each sample includes parents conceived between A and B, where $A = \{1960, 1965\}$ and $B = \{1974, 1980\}$. The top of each panel shows the value of A, and the x-axis shows the values of B. The model controls for parental province of birth, cohort, and age fixed effects, as well as child's age and gender. Standard errors are clustered at the parental province of birth.

Source: authors' estimations based on data from the VHLSS 2014–16.

Figure A2: Robustness check: alternative age restriction



Note: the baseline model is estimated for 14 samples constructed from different age restrictions for the child. Each point represents the point estimate and the 95 per cent CI of the coefficient of interest for each sample. Each sample includes individuals age 7 to X, where $X = \{22, 35\}$ reported in the x-axis. The model controls for parental province of birth, cohort, and age fixed effects, as well as child's age and gender. Standard errors are clustered at the parental province of birth.

Source: authors' estimations based on data from the VHLSS 2014–16.

Table A1: Robustness check: estimates of the first-stage and reduced-form equations using instrumented bombing intensity

	Father's education		Mother's education	
	First-stage	Reduced-form	First-stage	Reduced-form
	(1)	(2)	(3)	(4)
Panel A: main specification				
Parental exposure	–0.300***	0.003	–0.164	0.009
	(0.082)	(0.007)	(0.107)	(0.008)
Panel B: instrumented bombing				
Parental exposure	–0.254**	0.004	–0.210	0.001
	(0.123)	(0.009)	(0.150)	(0.010)
F-stat	75.7	75.7	82.3	82.3
Observations	9,813	9,813	10,254	10,254
Dep. var. mean	7.452	7.452	6.985	6.985

Notes: all models control for parental age fixed effects, parental cohort fixed effects, and child's gender and age fixed effects. Parental bombing exposure is constructed by interacting bombing intensity of parental province of birth and an indicator of whether parents were conceived before 1973. Panel A report the main results. In Panel B, provincial bombing intensity is instrumented by province distance to the 17th Parallel (see text for details). Standard errors are clustered at the parental province of birth level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the VHLSS 2014–16.

Table A2: Estimates of the impact of parental exposure on parental work

	Father		Mother	
Outcomes	Baseline model	Robustness check	Baseline model	Robustness check
Agiculture work				
Parental exposure	–0.0077	–0.0259	0.0007	–0.0024
	(0.0085)	(0.0190)	(0.0098)	(0.0201)
Mean dep. var.	0.6327	0.6327	0.6248	0.6248
Observations	9,813	9,813	10,254	10,254
Production work				
Parental exposure	0.0049	0.0106	0.0083	0.0112
	(0.0091)	(0.0176)	(0.0110)	(0.0192)
Mean dep. var.	0.2351	0.2351	0.3146	0.3146
Observations	9,813	9,813	10,254	10,254
Unpaid work				
Parental exposure	–0.0114	–0.0230	0.0133	0.0134
	(0.0127)	(0.0228)	(0.0100)	(0.0179)
Mean dep. var.	0.5777	0.5777	0.6896	0.6896
Observations	9,813	9,813	10,254	10,254
Log wage				
Parental exposure	0.0134	0.0462	–0.0377	0.0358
	(0.0208)	(0.0404)	(0.0346)	(0.0665)
Mean dep. var.	8.3585	8.3585	8.1704	8.1704
Observations	3,995	3,995	2,628	2,628

Notes: all models control for parental age fixed effects, parental cohort and province of birth fixed effects, and child's gender and age fixed effects. Parental bombing exposure is constructed by interacting bombing intensity of parental province of birth and an indicator for whether parents were conceived before 1973. Standard errors are clustered at the parental province of birth level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the VHLSS 2014–16.

Table A3: Estimates of the impact of parental exposure on parental health

	Father		Mother	
Outcomes	Baseline model	Robustness check	Baseline model	Robustness check
Disabled				
Parental exposure	–0.0002	–0.0001	–0.0001	0.0000
	(0.0001)	(0.0002)	(0.0001)	(0.0001)
Mean dep. var.	0.0055	0.0055	0.0019	0.0019
Observations	1537410	1537410	1824867	1824867
Blind				
Parental exposure	0.0009*	–0.0006	0.0008**	–0.0003
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Mean dep. var.	0.0164	0.0164	0.0115	0.0115
Observations	1537410	1537410	1824867	1824867
Deaf				
Parental exposure	–0.0006*	–0.0009**	–0.0003**	–0.0000
	(0.0003)	(0.0004)	(0.0002)	(0.0003)
Mean dep. var.	0.0083	0.0083	0.0052	0.0052
Observations	1537410	1537410	1824867	1824867
Mentally disabled				
Parental exposure	–0.0002	0.0001	0.0001	0.0002
	(0.0002)	(0.0003)	(0.0001)	(0.0002)
Mean dep. var.	0.0104	0.0104	0.0069	0.0069
Observations	1537410	1537410	1824867	1824867
Disabilities affecting lower extremities				
Parental exposure	–0.0002	0.0006*	–0.0000	0.0001
	(0.0003)	(0.0004)	(0.0002)	(0.0004)
Mean dep. var.	0.0120	0.0120	0.0066	0.0066
Observations	1537410	1537410	1824867	1824867

Notes: standard errors are clustered at the commune-survey year level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the 2009 Vietnam Population and Housing Census.

Table A4: Estimates of the impact of parental exposure on parental socioeconomic status

	Father		Mother	
Outcomes	Baseline model	Robustness check	Baseline model	Robustness check
Log household income per capita				
Parental exposure	0.0159	0.0290	–0.0030	–0.0452
	(0.0151)	(0.0269)	(0.0143)	(0.0286)
Mean dep. var.	10.0368	10.0368	10.0368	10.0368
Observations	9812	9812	10254	10254
Household income per capita quintiles				
Parental exposure	0.0170	0.0147	0.0039	–0.0854
	(0.0282)	(0.0516)	(0.0273)	(0.0530)
Mean dep. var.	2.9418	2.9418	2.9418	2.9418
Observations	9812	9812	10254	10254
Household poverty status				
Parental exposure	0.0128*	0.0003	0.0186***	0.0113
	(0.0073)	(0.0125)	(0.0068)	(0.0124)
Mean dep. var.	0.1051	0.1051	0.1051	0.1051
Observations	9813	9813	10254	10254
Household five-year poverty				
Parental exposure	0.0089	–0.0053	0.0147**	0.0006
	(0.0067)	(0.0118)	(0.0061)	(0.0116)
Mean dep. var.	0.1128	0.1128	0.1128	0.1128
Observations	9813	9813	10254	10254

Notes: all models control for parental age fixed effects, parental cohort and province of birth fixed effects, and child's gender and age fixed effects. Parental bombing exposure is constructed by interacting bombing intensity of parental province of birth and an indicator for whether parents were conceived before 1973. Standard errors are clustered at the parental province of birth level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the VHLSS 2014–16.

Table A5: Estimates of the impact of parental bombing exposure on spouse characteristics

	Father		Mother	
Spouse outcomes	Baseline model	Robustness check	Baseline model	Robustness check
Years of schooling				
Parental exposure	–0.0609	0.2227*	–0.0581	0.0881
	(0.0744)	(0.1146)	(0.0713)	(0.1068)
Mean dep. var.	6.9240	6.9240	7.6508	7.6508
Observations	9,813	9,813	10,254	10,254
Work				
Parental exposure	0.0074	0.0068	–0.0008	0.0029
	(0.0052)	(0.0086)	(0.0041)	(0.0090)
Mean dep. var.	0.9481	0.9481	0.9790	0.9790
Observations	9,813	9,813	10,254	10,254
Agriculture work				
Parental exposure	0.0161*	–0.0116	–0.0039	–0.0030
	(0.0089)	(0.0141)	(0.0093)	(0.0167)
Mean dep. var.	0.6239	0.6239	0.6207	0.6207
Observations	9,813	9,813	10,254	10,254
Production work				
Parental exposure	0.0073	0.0365***	0.0239***	0.0094
	(0.0090)	(0.0140)	(0.0092)	(0.0156)
Mean dep. var.	0.3109	0.3109	0.2370	0.2370
Observations	9,813	9,813	10,254	10,254
Unpaid work				
Parental exposure	0.0196**	0.0172	–0.0025	0.0043
	(0.0091)	(0.0144)	(0.0101)	(0.0172)
Mean dep. var.	0.6865	0.6865	0.5803	0.5803
Observations	9,813	9,813	10,254	10,254
Wage				
Parental exposure	–0.0016	–0.0013	–0.0008	0.0007
	(0.0013)	(0.0020)	(0.0021)	(0.0033)
Mean dep. var.	0.0349	0.0349	0.0720	0.0720
Observations	9,812	9,812	10,254	10,254

Notes: standard errors are clustered at the commune-survey year level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the VHLSS 2014–16.

Table A6: Estimates of the impact of parental years of schooling on child's educational attainment using parental current province's bombing intensity

	Father's education				Mother's education			
	OLS	First stage	IV	Reduced form	OLS	First stage	IV	Reduced form
Educational outcome of	Child (1)	Father (2)	Child (3)	Child (4)	Child (5)	Mother (6)	Child (7)	Child (8)
Panel A: baseline model								
Parental education	0.027***		–0.029		0.032***		–0.231	
	(0.002)		(0.044)		(0.002)		(0.320)	
Parental exposure		–0.135**		0.004		–0.058		0.013**
		(0.063)		(0.005)		(0.074)		(0.006)
Effective first-stage F-stat			4.7				0.6	
Weak IV *p*-value			0.47				0.04	
Panel B: controls for province–cohort trends								
Parental education	0.027***		–0.030		0.033***		0.237	
	(0.002)		(0.091)		(0.002)		(0.225)	
Parental exposure		–0.051		0.005		0.081		0.022***
		(0.070)		(0.008)		(0.091)		(0.008)
Effective first-stage F-stat			0.5				0.8	
Weak IV *p*-value			0.50				0.01	
Observations	9,813		9,813	9,813	10,254		10,254	10,254
Dep. var. mean	0.650	7.452	7.452	7.452	0.650	6.985	6.985	6.985

Notes: all models control for parental age fixed effects, parental cohort and province of current residence fixed effects, and child's gender and age fixed effects. Parental bombing exposure is constructed by interacting bombing intensity of parental province of current residence and an indicator for whether parents were conceived before 1973. Standard errors are clustered at the parental province of current residence level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the VHLSS 2014–16.

Table A7: Estimates of the impact of parental years of schooling on child's educational attainment using the 2006–16 VHLSS samples

	Father's education				Mother's education			
	OLS	First stage	IV	Reduced form	OLS	First stage	IV	Reduced form
Educational outcome of	Child (1)	Father (2)	Child (3)	Child (4)	Child (5)	Mother (6)	Child (7)	Child (8)
Panel A: baseline model								
Parental education	0.027***		–0.005		0.032***		–0.047	
	(0.002)		(0.026)		(0.002)		(0.051)	
Parental exposure		–0.183***		0.001		–0.110		0.005
		(0.068)		(0.005)		(0.082)		(0.003)
Effective first-stage F-stat			7.2				1.8	
Weak IV *p*-value			0.83				0.13	
Panel B: controls for province–cohort trends								
Parental education	0.028***		–0.031		0.032***		–0.055	
	(0.002)		(0.113)		(0.002)		(0.261)	
Parental exposure		–0.074		0.002		0.040		–0.003
		(0.085)		(0.007)		(0.099)		(0.008)
Effective first-stage F-stat			0.8				0.2	
Weak IV *p*-value			0.74				0.76	
Observations	26,609		26,609	26,609	30,660		30,660	30,660
Dep. var. mean	0.685	7.246	7.246	7.246	0.685	6.838	6.838	6.838

Notes: all models control for parental age fixed effects, parental cohort and province of current residence fixed effects, and child's gender and age fixed effects. Parental bombing exposure is constructed by interacting bombing intensity of parental province of current residence and an indicator for whether parents were conceived before 1973. Standard errors are clustered at the parental province of birth level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the VHLSS 2014–16.

Table A8: Robustness checks for different measures of child's educational attainment

	Father's education				Mother's education			
	OLS	First stage	IV	Reduced form	OLS	First stage	IV	Reduced form
Educational outcome of	Child (1)	Father (2)	Child (3)	Child (4)	Child (5)	Mother (6)	Child (7)	Child (8)
Complete primary								
Parental education	0.005***		0.005		0.006***		0.007	
	(0.001)		(0.010)		(0.001)		(0.016)	
Parental exposure		–0.300***		–0.002		–0.164		–0.001
		(0.082)		(0.003)		(0.107)		(0.003)
Effective first-stage F-stat			15.0				2.6	
Weak IV *p*-value			0.60				0.67	
Dep. var. mean	0.793	7.452			0.793	6.985		
Complete lower secondary								
Parental education	0.013***		0.031*		0.017***		–0.010	
	(0.001)		(0.017)		(0.002)		(0.030)	
Parental exposure		–0.300***		–0.010*		–0.164		0.002
		(0.082)		(0.005)		(0.107)		(0.005)
Effective first-stage F-stat			15.0				2.6	
Weak IV *p*-value			0.08				0.74	
Dep. var. mean	0.476	7.452			0.476	6.985		
Complete upper secondary								
Parental education	0.014***		–0.012		0.020***		–0.063	
	(0.001)		(0.019)		(0.001)		(0.058)	
Parental exposure		–0.300***		0.004		–0.164		0.011*
		(0.082)		(0.006)		(0.107)		(0.006)
Effective first-stage F-stat			15.0				2.6	
Weak IV *p*-value			0.52				0.11	
Dep. var. mean	0.222	7.452			0.222	6.985		
Years of schooling								
Parental education	0.119***		–0.065		0.150***		–0.585	
	(0.010)		(0.160)		(0.012)		(0.510)	
Parental exposure		–0.300***		0.020		–0.164		0.096**
		(0.082)		(0.047)		(0.107)		(0.046)
Effective first-stage F-stat			13.4				2.4	
Weak IV *p*-value			0.68				0.05	
Dep. var. mean	7.790	7.452			7.790	6.985		
Observations	9,813		9,813	9,813	10,254		10,254	10,254

Notes: all models control for parental age fixed effects, parental cohort and province of birth fixed effects, parental province–cohort trends, and child's gender and age fixed effects. Parental bombing exposure is constructed by interacting bombing intensity of parental province of birth and an indicator for whether parents were conceived before 1973. Standard errors are clustered at the parental province of birth level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the VHLSS 2014–16.

Table A9: Estimates of the impact of parental years of schooling on children's educational attainment on sample of individuals aged 7–18.

	Father's education				Mother's education			
	OLS	First stage	IV	Reduced form	OLS	First stage	IV	Reduced form
Educational outcome of	Child (1)	Father (2)	Child (3)	Child (4)	Child (5)	Mother (6)	Child (7)	Child (8)
Panel A: baseline model								
Parental education	0.022***		–0.017		0.024***		–0.775	
	(0.002)		(0.025)		(0.002)		(10.645)	
Parental exposure		–0.324***		0.006		–0.008		0.006
		(0.095)		(0.007)		(0.105)		(0.007)
Effective first-stage F-stat			11.6				0.0	
Weak IV *p*-value			0.46				0.37	
Panel B: controls for province–cohort trends								
Parental education	0.021***		–0.055		0.024***		–0.188	
	(0.002)		(0.050)		(0.002)		(0.539)	
Parental exposure		–0.354**		0.019		–0.090		0.014
		(0.139)		(0.013)		(0.152)		(0.014)
Effective first-stage F-stat			6.3				0.4	
Weak IV *p*-value			0.17				0.32	
Observations	7,283		7,283	7,283	6,702		6,702	6,702
Dep. var. mean	0.749	7.471	7.471	7.471	0.749	6.972	6.972	6.972

Notes: the sample is drawn from the 2014–16 VHLSS (see text for the sample definition). All models control for parental age, parental cohort, and province of birth fixed effects, and child's gender and age fixed effects. Parental bombing exposure is constructed by interacting bombing intensity of parental province of birth and an indicator for whether parents were conceived before 1973. Standard errors are clustered at the parental province of birth level. Survey sampling weight is applied. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: authors' estimations based on data from the VHLSS 2010–16.

Appendix B

In this Appendix, we consider whether the effects of parental exposure to bombing on parental schooling varies by their age of exposure. Specifically, we ask whether parents who were only exposed after they were born, those who were exposed only in utero, and those who were exposed both in utero and at an early age, were affected differently.

To do this, we re-estimate the effect of bombing exposure on parental schooling using the parental sample conceived between 1960 and 1978. We split the sample into five groups. Group 0 are those who were conceived in 1973 or later; similar to the main analysis, this group was never exposed to the bombing, so it serves as a control group. Group 1 are those who were conceived in 1972 (during the bombing) but born in 1973; this group was only exposed in utero to bombing, but not exposed after birth because the bombing was over in 1973. Group 2 are those who were conceived between 1965 and 1971; this group was exposed both in utero and after birth. Group 3 are those who were conceived between 1963 and 1964; these individuals were already born and aged 0–2 years when the bombing started in 1965. Group 4 includes those who were conceived between 1960 and 1962 and aged 3–5 years in 1965. Group 1 to 4 are the treatment groups.

Using these group definitions, we estimate the following model:

$$\text{schooling}_i^{parent} = \sum_{g=1}^{4} \beta_g.(\text{bomb}_p^{parent} \times \text{group}_g^{parent}) + \eta.\text{age} + \mu.\mathbf{X}_i^{parent} \tag{6}$$

$$+ \text{province}_p^{parent} + \text{cohort}_c^{parent} + u_i \tag{7}$$

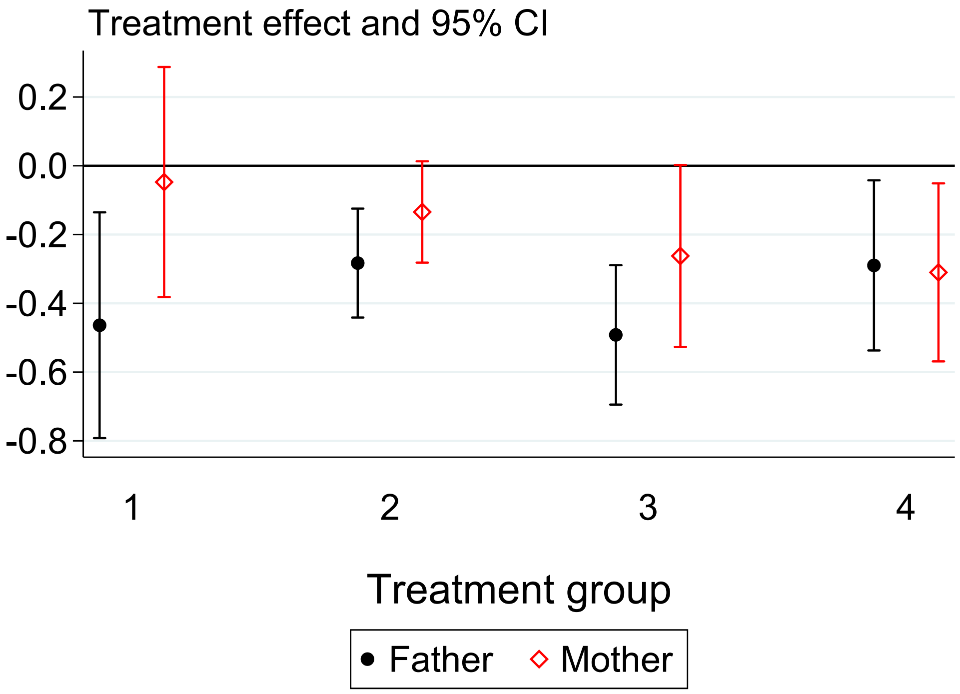
where β_g represents the treatment effects on group g. We estimate this model for father and mother separately.

We report the point estimates and their 95 per cent confidence intervals for β_1 to β_4 in Figure B1. First, we observe that the effects of bombing exposure on schooling for fathers are all negative, statistically significant, but not substantially different across different treatment groups. The impacts are larger for those who were only exposed in utero (group 1) and those who were exposed at age 0–2 (group 3).

For mothers, the effects are small and insignificant for those who were only exposed in utero (group 1) and marginally significant for those who were exposed in utero and at an early age (group 2). For those who were exposed at age 0–2 (group 3) and 3–5 (group 4), the effects are larger and statistically significant. Interestingly, the effects on the father and mother in group 4 are very similar, while in other treatment groups the effects on the father are always larger.

These results indicate that schooling effects of bombing exposure vary by age of exposure, but they vary differently for males and females. Specifically, exposure in utero appears to only affect males. This might be due to the fact that female infants tend to be less vulnerable in utero and at an early age than are male infants.

Figure B1: Effects of bombing exposure on schooling by age of exposure



Note: the graph displays treatment effects estimated for each treatment group. The control group includes those who were conceived in 1973 or later. Treatment group 1 includes those who were conceived in 1972 but born in 1973. Treatment group 2 includes those conceived in 1965–72 but born before 1973. Treatment group 3 includes those conceived in 1963–64. Treatment Group 4 are those conceived in 1960–62. The model controls for parental province of birth, cohort, and age fixed effects, as well as child's age and gender. Standard errors are clustered at the parental province of birth. The sample includes parents conceived in 1960–78 and is drawn from the VHLSS 2014–16.

Source: authors' estimations based on data from the VHLSS 2014–16.