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The Long-Term Impacts of Bombing Vietnam on Occupations Over Cohorts

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Abstract

This paper investigates the persistent effects of Vietnam War (1955–1975) bombing on occupations and incomes of different cohorts in 2018. To do this, I link the US National Archives bombing data to a Vietnamese representative household survey. I employ an instrumental variable approach that leverages rounding thresholds used to target village-level airstrikes. The results show that bombing increases the share of post-war young cohorts working in low-skilled occupations by 10 percentage points and reduces their income by over 50% in 2018. The effects are even more pronounced for older cohorts who were directly exposed to the war. I estimate that heavily bombed villages lag approximately 1.3 to 1.6 generations behind in occupational transformation. My analysis indicates that educational accessibility and wartime village governance partially mediate these effects. This paper provides the first evidence that bombing distorts occupational and income structures for the post-war generation, causing bombed villages to lag in structural transformation.

JEL classification: F51, J62, N45

Keywords: bombing, cohorts, income, occupation, Vietnam War.

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1 Introduction

While extensive scholarship documents the immediate macroeconomic costs of war (Dunayev et al., 2024; Nguyen, Tran, & Van Vu, 2024), recent inquiries have turned to longer-term impacts on human capital; specifically, ways that conflict erodes the earnings of survivors (Angrist, Chen, & Frandsen, 2010; Le & Nguyen, 2020). Regarding next generations, however, researchers have predominantly focused on non-pecuniary outcomes, such as physical and psychological deficits (Santavirta, Santavirta, & Gilman, 2018; Deza & Mezza, 2025), educational attainment (Havari & Peracchi, 2019; Akresh, Bhalotra, Leone, & Osili, 2023), and subjective well-being (Kijewski, 2020). Even when they do address economic behaviors, scholars often restrict analyses to short-term household coping strategies like child or female labor (Churchill, Smyth, & Trinh, 2022; Gay, 2023), rather than to long-run career trajectories. Consequently, the extent to which conflict leaves enduring economic scars on younger generations—specifically by distorting their labor-market outcomes and suppressing long-term income—remains largely uncharted territory in the economic literature.

Two seminal studies extend this line of inquiry into the intergenerational realm, providing the closest precedents to my work. First, Campante and Yanagizawa-Drott (2015) reveal an intergenerational transmission of service, finding that a father's wartime service significantly increases the likelihood of his son enlisting. Second, examining the aftermath of WWII in Germany, Halbmeier and Schröder (2025) document that descendants of individuals born in heavily bombed areas held significantly less wealth in the early 2000s compared to those from less-damaged locales. While these studies offer foundational evidence, critical gaps remain regarding the broader economic destiny of post-war cohorts. First, Campante and Yanagizawa-Drott (2015) restrict their scope to the transmission of military occupations, leaving open the broader question of how war shapes general occupational structures and civilian labor market trends for the next generation. Second, while Halbmeier and Schröder (2025) assesses economic status, that study focuses on net wealth—a stock variable largely determined by intergenerational asset transfers. Consequently, the observed wealth gap may mechanically capture the destruction of parental physical capital (e.g., housing, land) rather than a decline in the next generation's own economic capability. In contrast, personal income serves as a direct proxy for an individual's labor market productivity and earning capacity. However, the long-term effect of war on personal income has not been fully explored. In sum, while prior research has established that war leaves economic scars, no study to date has systematically analyzed how being bombed during wartime distorts occupational trends and earnings potential of cohorts born entirely after the conflict ends.

To advance existing literature, this study scrutinizes the persistent legacy of the 1955-1975 Vietnam War bombing on the occupational and income trajectories of cohorts spanning both pre- and post-war eras. To address concerns that bombing intensity is endogenous to local economic development, I employ an instrumental variable strategy that leverages discontinuities in security score assignments, building on the framework established by Dell and Querubin (2018). The identification relies on a rounding algorithm that generates quasi-random variation: villages with virtually identical characteristics falling just below a specific security threshold in quarter t faced significantly heavier bombardment in quarter $t+1$ compared to their counterparts just above the cutoff. This design allows for a causal estimation of bombing effects by isolating variation unrelated to the intrinsic properties of the village. Crucially, distinct from previous work that primarily assesses short-run institutional impacts, my analysis shifts the focus to long-run intergenerational dynamics. This enables me to trace the persistence of the shock across distinct birth cohorts, and to determine how the scarring effects of bombing endure and when they ultimately fade away.

For conflict intensity, I utilize the granular bombing dataset constructed by Dell and

Querubin (2018), sourced from the US National Archives. This study employs "bombing frequency"—defined as the share of months within a quarter in which at least one air strike occurred—to proxy for war severity. This choice deviates from the existing literature, which predominantly relies on total ordinance volume per square kilometer at the coarser district level (see Miguel and Roland (2011); Singhal (2019); Palmer, Nguyen, Mitra, Mont, and Groce (2019); Churchill et al. (2022)). While ordinance volume arguably captures the sheer physical magnitude of destruction, bombing frequency is a better proxy for the persistent presence of conflict and sustained disruption. Furthermore, measuring frequency offers a pivotal advantage in spatial granularity, enabling identification of effects at the village level rather than the aggregated district level. Finally, to link historical shocks to contemporary welfare, I spatially merge war-time data with outcome variables from the 2018 Vietnam Household Living Standards Survey (VHLSS) and 2018 administrative boundaries.

I use two primary outcome variables—occupation and income—along with three supplementary outcomes that provide a broader picture of the war's impact across different cohorts: poverty rate, educational attainment, and population. First, I categorized the occupation variable into four groups—high-skilled jobs, middle experts, staff and sales, and low-skilled jobs—coded from Vietnam's 10-category occupational classification system. Second, I measured personal income as the logarithm of total household income divided by the household size in the survey year. Among the supplementary outcomes, educational attainment is defined as the highest degree obtained; poverty rate is measured as the proportion of households classified as poor in the survey year, based on the poverty standards published by the Government of Vietnam; and population is measured as the logarithm of absolute population size.

This paper reports several key empirical findings. *First*, a notable finding is that the occupational consequences of bombing are not limited to persons directly exposed to the war (old cohorts), but persist among cohorts born after the war. The estimation results indicate that exposure to the mean bombing intensity (frequency of 0.292 strikes per quarter) increases the probability of working in low-skilled occupations by over 10 percentage points for both old and young cohorts. Specifically, this labor force is displaced into the agriculture, forestry, and fishery sectors, and into occupations requiring basic skills such as assembly, machine operating, craft work, or manual labor. My rough estimates suggest that it takes 1.3 to 1.6 generations for these adverse effects on occupational orientation to completely dissipate. *Second*, bombing resulted in severe income reductions. As of 2018, the individual income of both pre- and post-war cohorts in bombed areas was more than 50% lower than their potential levels. Although this impact tends to diminish for the younger group—implying that the labor market and the economy are gradually recovering over time—the remaining legacy of bombing is still significant enough in 2018 to increase the poverty rate at the village level by 5.6 percentage points. The adverse impact of bombing on occupations, incomes, and the poverty rate collectively contributes a new perspective: alongside the physical and psychological consequences documented in the medical literature (Devakumar, Birch, Osrin, Sondorp, & Wells, 2014; Santavirta et al., 2018; Deza & Mezza, 2025), the young generation also suffers surprisingly severe economic losses due to bombing. Crucially, this sluggish shift in both occupation and income indicates that bombed areas face a significant lag in structural transformation. The resulting "trapping" of human resources in low-productivity sectors anchors these villages behind the general development trend, with a lag equivalent to 1.3 to 1.6 generations. *Finally*, regarding non-economic aspects, in contrast to the bleak economic picture, I find no evidence of long-term effects of bombing on educational attainment or population size in 2018, suggesting that human capital (in terms of quantity) has recovered better than job quality. The robustness of all these results is reinforced by first-stage statistical tests, confirming that being below the security threshold is a strong and reliable predictor of

bombing frequency.

I conduct a series of robustness checks using alternative datasets and samples to validate the main findings. First, I use bomb volume per square kilometer as an alternative measure of bombing intensity. Second, I replace the 2018 VHLSS outcome data with the 2016 wave to rule out potential shocks specific to 2018. Third, I adopt an alternative cohort definition in place of the original one. The main results survive all of these robustness checks with minor changes in effect magnitudes.

This study contributes to the existing literature along three core dimensions: First, I bridge a critical gap in the research on the long-term economic consequences of aerial bombardment by providing the first empirical evidence of its impact on the occupational structure and incomes of peacetime-born generations. A review of existing literature reveals a predominant focus on macroeconomic aspects or human capital. Specifically, global studies on bombing, such as Brakman, Garretsen, and Schramm (2004) and Akbulut-Yuksel, Tekin, and Turan (2022), primarily examine the recovery of urban structures and city growth in post-WWII Germany; meanwhile, Freh, Dallos, and Chung (2013), Haddad and Okuyama (2016), and Saing and Kazianga (2020) focus on casualties and spatial displacement in Iraq, Lebanon, and Cambodia. In Vietnam specifically, while the research stream is well-developed, it leaves a gap regarding the labor market, especially among the new generations. Classical studies like Miguel and Roland (2011) document a miraculous recovery in consumption and infrastructure at the district level; Dell and Querubin (2018) focuses on political institutions in South Vietnam immediately post-war; while a series of micro-level studies delve into human impairment such as mental health (Singhal, 2019), disability (Palmer et al., 2019), and educational mobility (Vu & Bue, 2019). Even the recent work of Churchill et al. (2022), which touches upon labor, restricts its scope to child labor as a short-term household coping mechanism, rather than examining the long-term occupational orientation of adults. Moving beyond these immediate survival mechanisms, I uncover a harsher economic reality: bombing campaigns shattered local economic structures, effectively trapping younger generations—including those never directly exposed to war—within low-skilled, low-productivity sectors. Crucially, I quantify the persistence of this conflict drag, estimating that it takes approximately 1.3 to 1.6 generations for these distortions to fade and for the occupational and income gaps to finally close.

Second, I address the issue of aggregation bias commonly found in conflict economics by exploiting bombing intensity data at the most granular geographic level: the village. Predecessor studies (Miguel & Roland, 2011; Singhal, 2019; Churchill et al., 2022; Vu & Bue, 2019) mainly rely on district- or provincial-level data, which contain the risk of smoothing variations in bombing intensity across villages within the same district. By adopting a more micro approach, I am able to precisely identify individual effects (occupation, income) that aggregate district-level data have inadvertently obscured.

Third, this study bridges the disconnect between macro-resilience and micro-stagnation, offering a novel explanation for Vietnam's recovery puzzle. By identifying a structural transformation lag, I show that, while the broader economy converges, bombed villages remain anchored in low-productivity traps that are effectively invisible in coarser datasets. While aggregated district-level growth drives the neutrality documented in previous studies, granular village-level analysis reveals substantial and statistically significant economic scars. This implies that aggregate recovery masks pockets of persistent lags. Crucially, the finding of a developmental lag extending 1.3–1.6 generations challenges the optimistic view of Vietnam's complete recovery, demonstrating that behind positive growth figures lie communities still struggling to escape structural traps. Globally, this underscores a harsh reality: while macro indicators may recover, the burden of inequality continues to weigh on specific communities that were once the epicenter of conflict.

The paper proceeds as follows. Section 2 provides the historical background, detailing the context of the Vietnam War and post-conflict reconstruction. Section 3 introduces the data sources constructed for this analysis. Section 4 outlines the identification strategy employed to estimate the long-term effects of bombing on the occupational and income trajectories of different cohorts. Section 5 presents the main empirical results, followed by a series of robustness checks in Section 6. Section 7 discusses mechanisms of the impacts. Section 8 provides further discussions about meaning of resutls found. Finally, Section 9 is the conclusion.

2 Background

2.1 The Vietnam War (1955-1975)

Vietnam endured several wars prior to what is known as the Vietnam War (1955-1975). After the first Indochina War (1946-1954) and the cessation of French colonial rule in the region, Vietnam was divided into two distinct entities: communist-leaning North Vietnam and pro-Western South Vietnam. Following the Geneva Accords, in 1954 the 17th parallel was established as a dividing line between North and South Vietnam. In 1960, the National Liberation Front, also known as the Viet Cong, was founded. That same year, a communist insurgency began in South Vietnam, led by the Viet Cong and supported by the North Vietnamese army.

In the wake of a series of maritime incidents in the South China Sea during the summer of 1964, the United States initiated a strategic shift, launching a diplomatic mission to Southeast Asia. Under the leadership of President Lyndon Johnson, a concerted effort was made to foster regional stability and expand American influence throughout the region. In 1965, a contingent of 200,000 American soldiers was dispatched to South Vietnam, symbolizing a pivotal moment in US foreign policy. Subsequent years witnessed a remarkable escalation, with the US troop presence in Vietnam rising to well over a million by the end of 1969 (Dell & Querubin, 2018). General William DePuy in Sheehan (1998) emphasized a doctrine of overwhelming firepower: "[t]he solution in Vietnam is more bombs, more shells, more napalm" (p. 619). Charles Colson once remarked: "[get] the people by the balls and their hearts and minds will follow" (Kodosky (2007), p.175). The US military strategy underscored that excessive force could reduce insurgent forces, disrupt their operations, and break their morale.

Regarding the scale of destruction, Vietnam War-era bombing has been quantified as being at least three times more extensive in terms of tonnage than the bombing of World War II and roughly fifteen times greater than the bombing during the Korean War (Miguel & Roland, 2011). This makes it one of the most intense and devastating aerial campaigns in the annals of military history. The Vietnam War ended on April 30, 1975, with the fall of Saigon, leading to the reunification of Vietnam under communist control. This marked the official end of decades of conflict and the dissolution of South Vietnam. The aftermath saw the country renamed the Socialist Republic of Vietnam in 1976. The war left Vietnam heavily scarred, with significant destruction and loss of life. Nearly all military bombing strikes during the war occurred in South Vietnam, where the main focus of this paper lies.

2.2 South Vietnam Economy

Historically, the South Vietnamese economy was predominantly agricultural. During the 1955–1975 period, farming engaged 60% of the labor force, while the industrial sector accounted for only 5%. Wartime conditions distorted the labor market, necessitating increased participation by women, youth, and the elderly, and eventually resulted in reduced overall productivity. During the war, the industrial base in the region was small, concentrated pri-

marily around Saigon, and dominated by four key sectors—food, beverages, tobacco, and textiles—which collectively accounted for 70% of overall output (CIA, 1973). Despite heavy fighting, most industrial facilities remained intact due to dispersal of factories to avoid airstrikes. However, the macro-economy relied heavily on US aid, particularly for imports. Conversely, exports collapsed, and food deficits widened, driven by military spending (Vietnam Government, 2024). Although agriculture, especially rice production, remained the economic backbone, the sector faced disruption as many rural farmers migrated to urban centers amid intense conflict in the countryside.

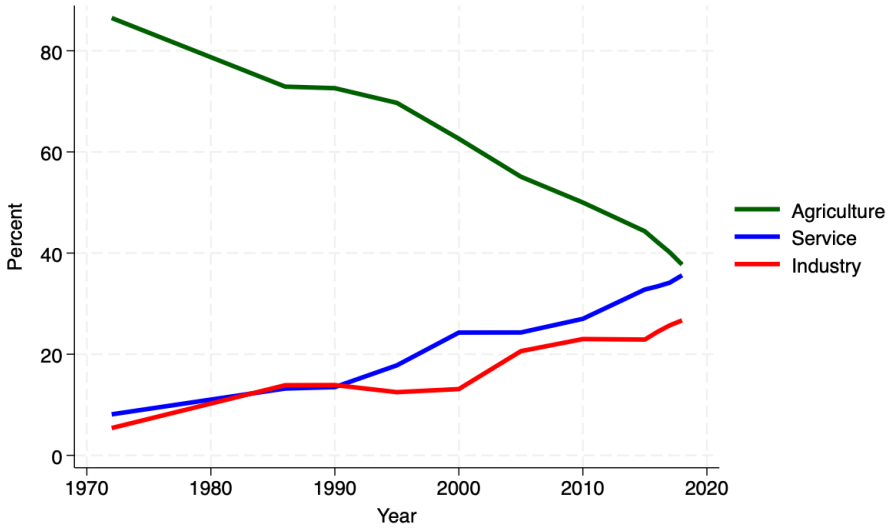


Figure 1: Labor force structure in South Vietnam (1972–2018)

Note: The figure displays the sectoral composition of the labor force in South Vietnam, including agriculture, industry, and services. While agricultural employment decreased sharply over the long term, industrial employment remained relatively stagnant from 1972 to 2000, followed by a marked increase thereafter. Service employment gradually increased from 10% in 1972 to around 35% in 2020. Data sources: CIA (1973), Mai and Van (2019), Tuan (2022).

The economic structure of South Vietnam, specifically the evolution of its labor force in the post-unification period, is illustrated in Figure 1. Following reunification in 1975, the structural transformation of the economy was sluggish. As depicted in the stagnant industrial trend line in the figure, Vietnam remained predominantly agricultural with minimal industrial development; 81% of the population lived in rural areas (Nguyen, 2012). Economic recovery was slow, with industrial output in 1980 merely matching 1976 levels and agriculture growing at only 2% annually (Van Song et al., 2020; Han, 2022). Significant industrial development did not materialize until after 2000, reflecting Vietnam's prolonged path to industrialization.

3 Data

In this section, I describe four datasets used in this paper, including household survey data, bombing data, shapefile data, and administrative data. Other data used for robustness checks are introduced in Section 6.

3.1 Survey Data

2018 Vietnam Household Living Standard Survey

I utilize the 2018 Vietnam Household Living Standard Survey (VHLSS) as the primary source of outcomes and some control variables. VHLSS is a nationally representative survey that collects data on household income, expenditures, and socioeconomic indicators. Although VHLSS is conducted biennially, its content varies from one round to the next. I chose the 2018 round for specific reasons: this round of the survey provides a larger sample size, including over 70,000 households — far more than the 2016 round, which had 46,995 households. Because it was conducted later, the 2018 data also encompasses a broader post-war cohort. Crucially, the 2018 data align with a third-level administrative shapefile of Vietnam for the same year, which enables me to connect the bombing data from the 1965-1975 period with the survey data from 2018. The number of shapefiles with third-level data in Vietnam is limited; however, a file was available in 2018. I present summary statistics of several variables extracted from the VHLSS survey in 2018 in Table A.1 and Table A.2 in the appendices.

Outcomes data

All outcome data in this paper are from the 2018 VHLSS survey. This study focuses on several key village-level outcomes, including occupation and personal income, as well as poverty rate, educational attainment, and population. First, my measure of occupation is the share of people in each village employed in a specific occupation. Personal income, calculated as the logarithm of total household income divided by household size, is the second outcome, as a proxy for individual economic well-being. Third, I define the poverty rate as the proportion of households classified as poor in the sample.[1] Fourth, I measure the outcome of educational attainment by the share of the population achieving at least a specific level of education, usually a diploma.[2] Finally, the fifth outcome is the logarithm of the exact population.

Occupations classification

I rely on the occupational classification standards established by the Vietnamese General Statistics Office (GSO). There are two alternatives for categorization: one based on specific occupational types (ten groups) and the other on industrial sectors (twenty-one industries). I adopt the former alternative, detailed in Table A.3. To mitigate potential measurement error inherent in fine-grained classifications and enhance interpretability, I aggregate the ten original categories into four broad clusters based on skill: high-skilled jobs, middle experts, staff and sales, and low-skilled jobs[3].

3.2 Bombing Data

Geographical unit of bombing

[1] In the 2018 VHLSS survey, the Vietnamese government compared monthly income per person in a household with the income-based poverty line to determine whether a household is poor.

[2] The share of a specific educational diploma refers to the proportion of people who hold that diploma, but it is not mutually exclusive of other higher diplomas. For example, individuals holding a high school diploma may also hold higher diplomas, such as a university degree or a master's degree. I consider four levels of educational attainment, including secondary school, high school, college, and university. In Vietnam, universities grant academic bachelor's degrees over four to six years, whereas colleges provide three-year associate degrees focused on vocational skills.

[3] This classification relies on task similarity, educational requirements, and skill levels. High-skilled jobs include leaders, high-level experts, and armed forces; middle experts comprise mid-level professionals; staff and sales cover office and service workers; and low-skilled jobs encompass agricultural, manual, machine-operating, and unskilled laborers.

I utilize the bombing data sourced from the US National Archives, a dataset first introduced by Dell and Querubin (2018). These records encompass US military bombing data in South Vietnam during the period from 1965 to 1975. This data is primarily collected at the hamlet level, a geographical unit that Vietnamese administrators no longer use in 2018. Thus, using the coordinates of each bombing point during the war, I determine which modern village in 2018 corresponds to a specific historical hamlet. As a result, some villages resemble a single historical hamlet (I define these as single-hamlet villages). In contrast, others are relatively larger and correspond to several hamlets (I define them as multi-hamlet villages). To ensure that the bombing data is as accurate as possible for analysis and is not biased by the aggregation of bombing, I focus on single-hamlet villages and exclude multi-hamlet villages. Despite that, I conduct some tests to ensure that the attributes of single-hamlet villages are not systematically different from those of multi-hamlet villages, and that my focus on the single-hamlet analysis is not misleading.

Bombing frequencies

The spatial distribution of conflict intensity is illustrated in Figure 2, which maps the bombing frequency for single-hamlet villages. On this map, darker red shades correspond to areas subjected to higher bombing frequencies, while lighter shades denote lower intensity.[4] To construct this metric, I follow the methodology of Dell and Querubin (2018), using the information on strikes at a hamlet to compute the share of months in a quarter during which strikes took place. I adopt this bombing frequency as the primary measure of war severity throughout this paper.[5]

3.3 Shapefile and Administrative Data

Vietnam shapefile 2018

I utilize a 2018 Vietnam third-level administrative shapefile from the Global Administrative Areas 2018 dataset that covers a total of 11,163 villages.[6] Although the shapefile covers the entire country, the analysis in this paper focuses exclusively on South Vietnam, where most bombing occurred. Thanks to this shapefile, I can embed bombing during wartime in modern villages in 2018 (as shown in the map Figure 2). A visual representation of this Vietnam administrative boundary shapefile at the village level can be found in the Appendices, Figure A.18.

Administrative data

[4]Similarly to Figure 2, I present personal income in 2018 for single-hamlet villages in the Appendices Figure A.19. These two maps enable me to observe the raw relationships between bombing and personal income before investigating their link further in Section 4 and Section 5.

[5]Here, I describe the process used to calculate bombing frequency. Essentially, the US established the Hamlet Evaluation System (HES) to assess and categorize the pacification status of villages throughout South Vietnam. Each geographical unit, including provinces, districts, and hamlets, was assigned a HES identification number and located by spatial coordinates (Kalyvas & Kocher, 2003). The HES system recorded whether air or artillery strikes occurred near populated areas of villages in each month. Based on this information, Dell and Querubin (2018) calculated bombing frequency as the share of months during a quarter with strikes.

[6]Third-level units in Vietnam in 2018 include similar localities smaller than a district: communes, wards, and towns. Since these units are less common in literature terminology, I consider them all "villages", a label for geographical units used in this paper.

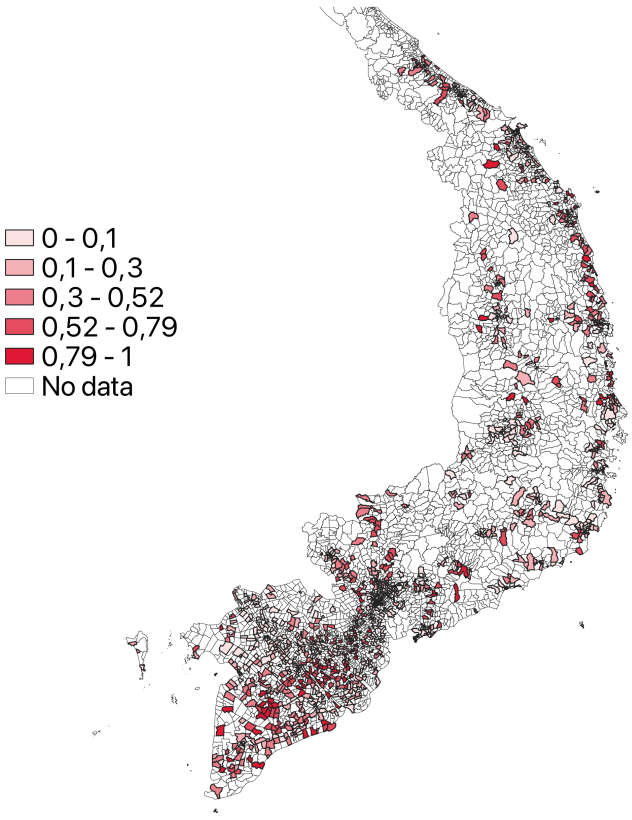


Figure 2: Bombing frequency

Note: The figure presents village-level bombing frequency for single-hamlet villages in South Vietnam during the period 1965-1975. Darker red shades indicate a higher share of months per quarter during which bombing occurred. Several of the white-shaded areas also experienced bombing during wartime; however, they are multi-hamlet villages, which I do not focus on in this paper. Bombing data are sourced from Dell and Querubin (2018), which covers only South Vietnam.

Although the shapefile visualizes village-level bombing, inconsistency of identifiers between the bombing dataset and the 2018 VHLSS survey prevents me from creating a unique dataset that combines bombing and outcomes. Thus, I ultimately use an administrative data source that supports me in connecting these two sources.[7]

4 Identification Strategy

4.1 OLS Regressions: How is Bombing Correlated with Outcomes?

The OLS regressions in this section provide an overview of the potential long-term associations between bombing and village-level outcomes, including occupation, personal income, poverty rate, education, and population. I construct the following regression model for estimation:

$$y_j = \delta bomb_j + \theta X_j + \epsilon_j \tag{1}$$

in which $bomb_j$ is the bombing frequency of village j during the period 1965-1975. It

[7]To connect bombing and outcome data, I need to create a common identifier set for both types of data. Initially, the bombing data contains half of the identifiers, the outcome data contains the other half of the identifiers, and the administrative data contains all the identifiers, which then becomes the common identifier set.

is the share of months during a quarter with strikes.[8] y_j is either the share of people in a given village that work in a specific occupational group[9], the logarithm of personal income (in Vietnam Dong), the proportion of poor households in villages, the share of people in a village obtain at least a diploma, or the logarithm of a village population. X_j is the control set, which contains village characteristics, and controls represent historical characteristics.[10] I estimate these OLS specifications at the village level.

4.2 Single-Hamlet Villages: The Instrumental Variable Approach

My OLS estimations may be subject to an endogeneity problem related to bombing frequency, as bombing may be correlated with pre-existing economic conditions or insurgent activity. Therefore, I employ an instrumental variable (IV) approach to identify the effects of Vietnam War bombing on listed outcomes and to address this concern. I adopt the IV strategy developed by Dell and Querubin (2018) and estimate the first stage in the following equation:

$$\begin{aligned} bomb_{j,t+n} =& \gamma below_{jt} + \sum_{d=1}^{4} \delta_d D_{jtd} + \sum_{d=1}^{4} \mu_d D_{jtd} f_d(dist_{jt}) + \\ &+ \sum_{d=1}^{4} \psi_d D_{jtd} f_d(dist_{jt}) below_{jt} + \alpha_t + \beta X_{jt} + \epsilon_{jt} \end{aligned} \tag{2}$$

$bomb_{j,t+n}$ is the bombing frequency of village j in quarter(s) $t+n$ and $below_{jt}$ is the instrumental variable. The instrument presents the status of whether a village was below a security threshold in terms of four thresholds when the village security score was calculated at the end of quarter t.[11] Villages falling just below any threshold experienced a significant increase in bombing likelihood. These village security scores were not systematically used to allocate other resources except for bombing, and the instrument *below* is strongly correlated with the frequency of bombing in the next quarter. In line with Dell and Querubin (2018), $f_d(dist_{jt})$ is an RD polynomial in distance to the nearest threshold, D_{jtd} is a set of indicators that equal one if threshold d is the nearest threshold, X_{jt} includes a set of control variables[12],

[8] For a check on multi-hamlet villages, I calculate the bombing frequency at the village level by averaging the bombing frequencies of all hamlets within a single village. OLS regressions on these villages show a strong relationship between bombing and outcomes; however, I do not calculate IV estimations, as the instrument variable estimation is only valid for single-hamlet villages.

[9] When calculating the village-level share of people in a specific occupational group, the average village sample size is approximately 55 individuals, ensuring statistical power calculation in the estimates.

[10] I took controls representing historical attributes from Dell and Querubin (2018) because they studied short-run impacts of Vietnam bombing on a range of outcomes and collected numerous historical attributes during wartime.

[11] Dell and Querubin (2018) define the security scores, the four security thresholds, detailing the mechanics of the instrumental variable *below* as follows. During the war, the US military collected monthly and quarterly responses from villages to 169 indicators, which they grouped into 19 submodels and processed via Bayes' rule to compute continuous scores. They then rounded these submodel scores to the nearest whole number, creating the discontinuities. The US military system further aggregated these values to obtain an overall security score, which served as the basis for classifying each village into one of five security classes, including A (very secure), B, C, D, and E (very insecure). Dell and Querubin (2018) computed the locations of four thresholds AB, BC, CD, and DE and the distance from each village to the nearest threshold. The status of a village being below a threshold is used as an instrument in their paper, which pools all villages that are below any of the four thresholds defined. In this current paper, I also use *below* as the instrumental variable, and the identification strategy above is valid since I include only single-hamlet villages.

[12] The control set includes historical attributes, and for the second stage, also village and household characteristics.

and α_t is a quarter-year fixed effect. I then plug the bombing frequency estimated in equation (2) into the second stage, which allows me to observe how bombing affects the outcomes of interest.

This IV strategy is restricted to single-hamlet villages, where each unit corresponds to both a historical hamlet and a modern village, thereby preserving the validity of the instrument from Dell and Querubin (2018). I run most of the regressions on the single-hamlet villages, except in cases in which comparison is necessary. Individual-level observations number in the thousands, but aggregate to hundreds at the village level.

First stage results

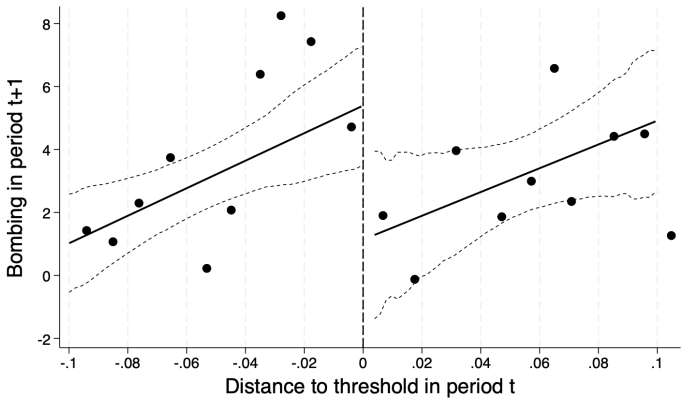


Figure 3: First stage

Note: The figure illustrates the first-stage regression, plotting bombing frequency in quarter t+1 against the distance to the nearest security threshold. Each point represents the average value within a bin. The solid line depicts the fitted linear regression, with dashed lines indicating the 95% confidence interval. The vertical dashed line marks the security threshold; a negative distance indicates that a village was classified as below the threshold in quarter t.

Figure 3 presents the bombing in quarter $t+1$ against the distance to the nearest threshold in quarter t using a local linear polynomial regression and the single-hamlet villages sample. A negative distance to the nearest threshold corresponds to a village being classified as below the security threshold. The figure shows that a village below the security threshold in quarter t increases the share of months in the next quarter with bombing or artillery fire by approximately 28.4%.[13] Numerically, the first-stage is strong and consistent with the first stage in Dell and Querubin (2018), which finds that being below any security thresholds in quarter t increases the bombing frequency in hamlets in the next quarter by 4.4%. I am aware of the differences in magnitude of effects in the first stages between this paper and the study by Dell and Querubin (2018), as my paper focuses on single-hamlet villages only and faces the issue of a small sample size. My future study might aim to address this problem.

Cohorts definitions

The definition of old or young cohorts is subjective to the researchers. In my paper, following Beshir (2019); Churchill et al. (2022), who also studied the Vietnam War, I initially focus on two cohorts: young (born after 1975), old (born before 1975), and pooled cohorts. Accordingly, the young cohort is people aged 15–43 in the VHLSS 2018 (the legal working age minimum and maximum birth year post-war), while the old cohort is 44 and older. The

[13]The sole tables of the first stages are not shown here. Please refer to the first-stage results in Table 1 - Table 3

cohorts can be further subdivided into 20- or 10-year birth ranges, yielding five overlapping or six separate cohort groups. I present the main findings for the two cohorts and the overlapping cohorts in Section 5, and I use six separate cohort divisions for a robustness check in Section 6.

Discussion of relevance, exclusion restrictions, and selection bias

A crucial issue for inference of the long-term impacts of bombing is the non-random US bombing of regions of Vietnam, because the US may have targeted regions with more economic development than others to diminish their military strength, or may have targeted areas in which the Viet Cong insurgency was on the rise. The IV identification strategy in this study is expected to overcome the endogeneity problem. The village security scores are strongly associated with the bombing of villages in the next quarter, as shown in the first-stage result. There is no further evidence that the HES scores were used for any purpose other than allocating airstrikes (Dell & Querubin, 2018). Meanwhile, the village scores affect the outcomes of interest only through the bombing allocated to the villages in the past. The relevance and exclusion restriction assumptions are more likely to be satisfied in this setup.

To address selection bias from excluding multi-hamlet villages, I conduct balance tests confirming that excluded villages and individuals resemble those in single-hamlet villages, ensuring the IV focus is unbiased. The balance test results are generally satisfactory (see Appendix Figure A.9). Histograms comparing group characteristics are shown in the Appendices Figure A.10 to Figure A.17, and indicate no systematic differences. Additionally, I replicate Dell and Querubin (2018) by checking pre-war village characteristics, and confirm balance (Appendix Table A.4).

5 Results

5.1 Effects of Bombing on Occupations

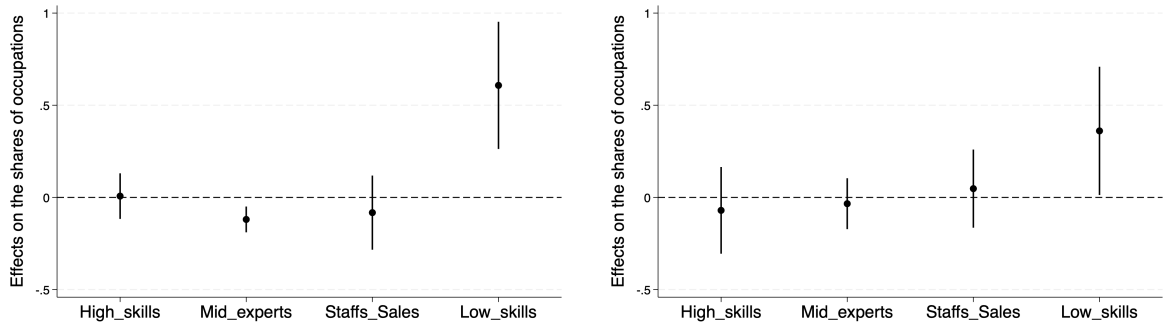
Two cohorts estimations

Table A.5 in the Appendices shows my OLS estimates of the effects of bombing during the Vietnam War on occupations in 2018. I use the village-level shares of people who worked in four occupational groups (including high-skilled jobs, middle experts, staff and sales, and low-skilled jobs) as the first outcome. The estimates show that many people in all age cohorts work in low-skilled positions as a consequence of bombing, even decades after the war ended (column 4). This estimated coefficient is around 15% for both cohorts, which shows that moving from no strikes to the sample mean of 0.292[14] strikes per quarter increases the share of low-skilled laborers by 4.38 percentage points (0.15×0.292). Additionally, column 2 of Table A.5 shows that significantly fewer members of the old cohort from villages that experienced bombing are middle-level experts in any field. Despite indicating a potential link between bombing and cohort occupations, my OLS estimates are subject to an endogeneity problem. The IV estimates below seek to address this concern.

I have documented the long-lasting effects of bombing on occupations of both cohorts, using the IV estimates. The results hold even when I consider more than two cohorts. Figure 4 below displays the IV estimations for two cohorts. Panel a, Figure 4 focuses on the old cohort and indicates that bombing during the Vietnam War substantially contributed to many persons from this cohort working in low-skilled jobs. The coefficient of low-skilled jobs

[14] 0.292 is the sample mean of the variable bombing. See statistics of variables used in this paper in Appendices, Table A.2.

estimation is 60.8%, corresponding to a 17.7 percentage points increase in the low-skilled share of the old population when moving from zero strikes to 0.292 strikes per quarter, which is sizable and significantly differs from zero. This highlights that working opportunities for the old cohort suffer heavily from strikes. Even more importantly, these effects remain visible more than four decades later for younger people: Panel b of Figure 4 illustrates that the young cohort still faces consequences of bombing that increased the likelihood of them working in low-paid, low-skilled jobs, by 36.1%, equivalent to an increase of 10.54 percentage points when moving from no strike to the mean of 0.292 strikes per quarter. Bombing also significantly reduces the old cohort's share of middle-expert jobs by 3.49 percentage points (-11.96×0.292), but small interval confidences imply limited observations. No significant effects are observed for high-skilled, staff, or sales occupations in either cohort.



(a) Old cohort (born before 1975)

(b) Young cohort (born after 1975)

Figure 4: Impacts of Bombing on Occupations: Two Cohorts

Note: The figures present IV estimates of the impact of bombing on the shares of different occupations in 2018, including high-skilled jobs, middle-skilled expert roles, staff and sales positions, and low-skilled jobs. The left panel reports results for the older cohort (born before 1975), while the right panel shows results for the younger cohort (born after 1975). A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects on the corresponding occupational share. Standard errors are clustered at the village level.

Across the four main occupational groups examined (including high-skilled jobs, middle experts, staff and sales, and low-skilled jobs), the analysis reveals that the impact of bombing is heterogeneous, concentrating entirely at the bottom of the skill hierarchy. Specifically, I find no clear evidence that bombing affects the probability of participation in the three higher-tiered occupational groups. This implies that the share of personnel holding leadership positions (in government or enterprises), high- and medium-level professionals in knowledge-based sectors (science, education, business), knowledge-intensive jobs, and clerical and commercial service workers, remains unaltered by the history of conflict.

Conversely, bombing displaced a significant portion of the labor force: an increase of over 10 percentage points for both old and young cohorts clustering in low-skilled positions. Specifically, this category encompasses three main labor segments: (i) Traditional agricultural labor: including cultivation, forestry, and fisheries; (ii) Blue-collar workers: comprising assemblers, machine operators, craft workers, garment workers, and manual laborers; and (iii) Elementary occupations: consisting of essential but low-skilled service tasks such as domestic work, environmental sanitation, food preparation, and other casual labor.

The common characteristics of these low-skilled jobs are their manual, routine nature, low marginal productivity, and limited value added. This enduring pattern of occupational structure over cohorts highlights a crucial point: bombing has long-lasting consequences that continue to limit economic opportunities and to push people at all ages into lower-paid occupations to this day. The consequent trapping of workers in these occupations provides a

direct and robust explanation for the severe income decline discussed in the subsequent section.

Five cohorts IV estimations

In Figure 5, I analyze bombing's differential effects across cohorts using multiple birth cohorts beyond the initial old (pre-war) and young (post-war) groups. I create five overlapping 20-year birth cohorts—two pre-war, two post-war, and one spanning the war period. Aligned with the two-cohort estimations, these results in Figure 5 reveal a consistent trend: bombing increased the likelihood of working in low-skilled jobs across all cohorts, with effects ranging from 60% (17.52 percentage points) for the oldest cohort to 30% (8.76 percentage points) for the youngest. This effect is most potent among older cohorts born before, during, and shortly after the war (Panel a to Panel c), and weakens for those born after unification (Panel d and Panel e). The smaller coefficient on the youngest cohort might result from a shorter birth window with fewer observations; however, the increasing pattern persists. The results shed light on the fact that although the occupational effects of bombing gradually diminish, they persist by driving higher levels of low-skilled employment across cohorts, even among the youngest.

Reduced-form estimations

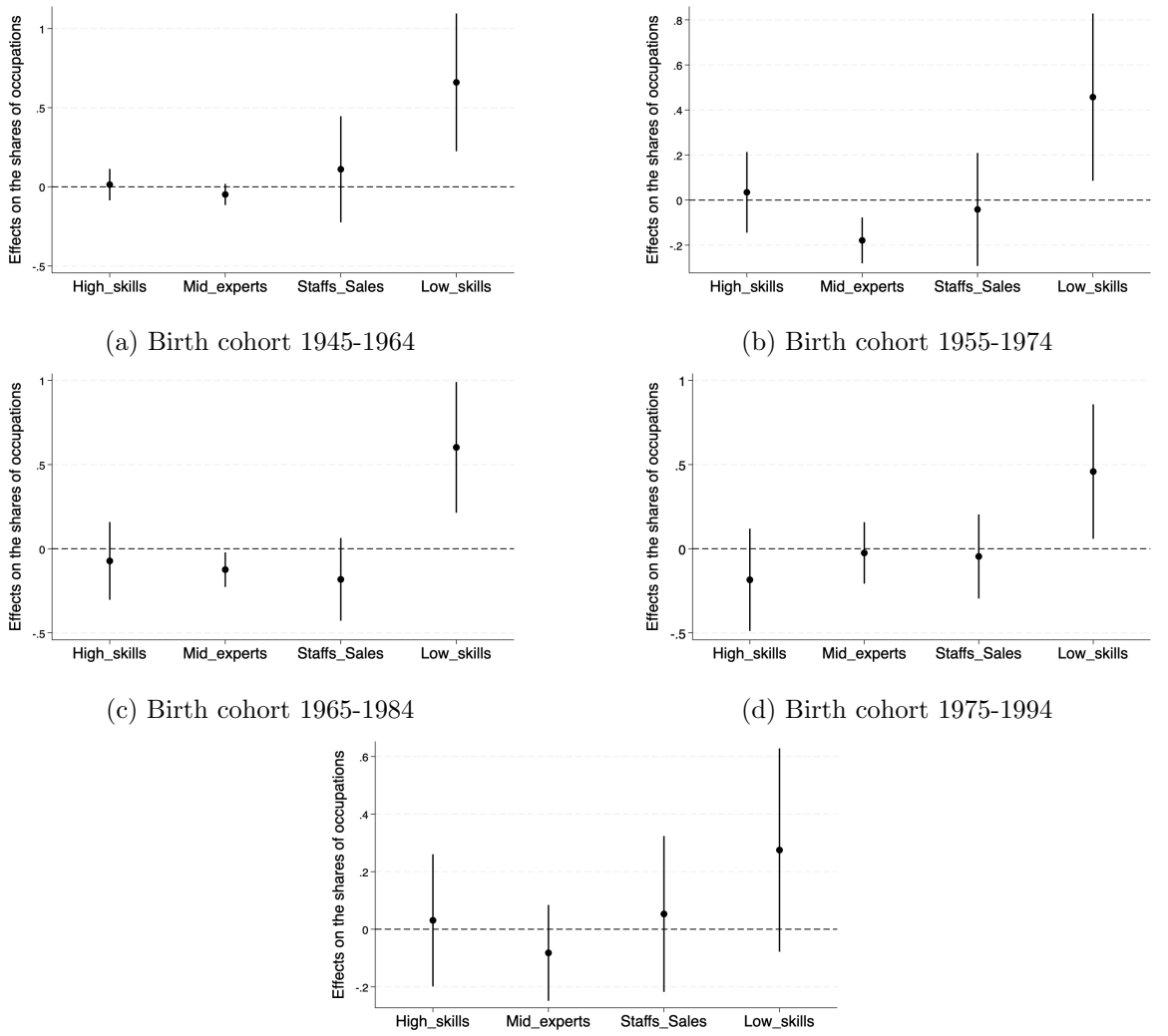
Figure 6 shows the reduced-form relationship between the IV and the share of occupations among overlapping cohorts. The status of a village being below any of the four security thresholds is the instrument. Panels a to d again show a prominent outcome: being below any security thresholds increases the likelihood of working in low-skilled jobs from 13% to 18% across most cohorts. Panel e shows a positive effect, albeit not statistically different from zero, suggesting that though they are generally persistent, the effects of bombing do gradually fade over time. The effects from the reduced forms, however, are less significant than the IV estimates presented in Figure 4 and Figure 5.

In sum, in terms of occupations, bombing during the Vietnam War shows long-run adverse effects on both older people who directly experienced the war and on younger ones who have lived during peacetime. This emphasizes that even though history has passed, its consequences continue to harm the economic situation of very young people in general. In what follows, I provide evidence that bombing also lowers the personal income of old and young people in 2018.

5.2 Effects of Bombing on Incomes

Table 1 presents the village-level estimates of the effects of bombing on personal income in 2018. Column 1 is the estimate for all cohorts, and columns 2 and 3 are the estimates for separate old and young cohorts. The OLS estimates in Panel A show a strong correlation between bombing and personal income: a movement from no strike to the sample mean of 0.292 strikes per quarter during wartime declines personal income in 2018 by approximately 4.2% (calculated as $100 \times (e^{-0.148 \times 0.292} - 1)$). The personal income declines are 11.0% and 8.3% for the sole old and young cohorts in columns 2 and 3.

Panel B reports IV estimates: The results suggest that moving from no bombing to the sample mean decreased personal income by around 38.2% for all cohorts in column 1, 53.6% for the sole old cohort in column 2, and 52.5% for the sole young cohort in column 3 relative



(a) Birth cohort 1945-1964

(b) Birth cohort 1955-1974

(c) Birth cohort 1965-1984

(d) Birth cohort 1975-1994

(e) Birth cohort 1985-2003

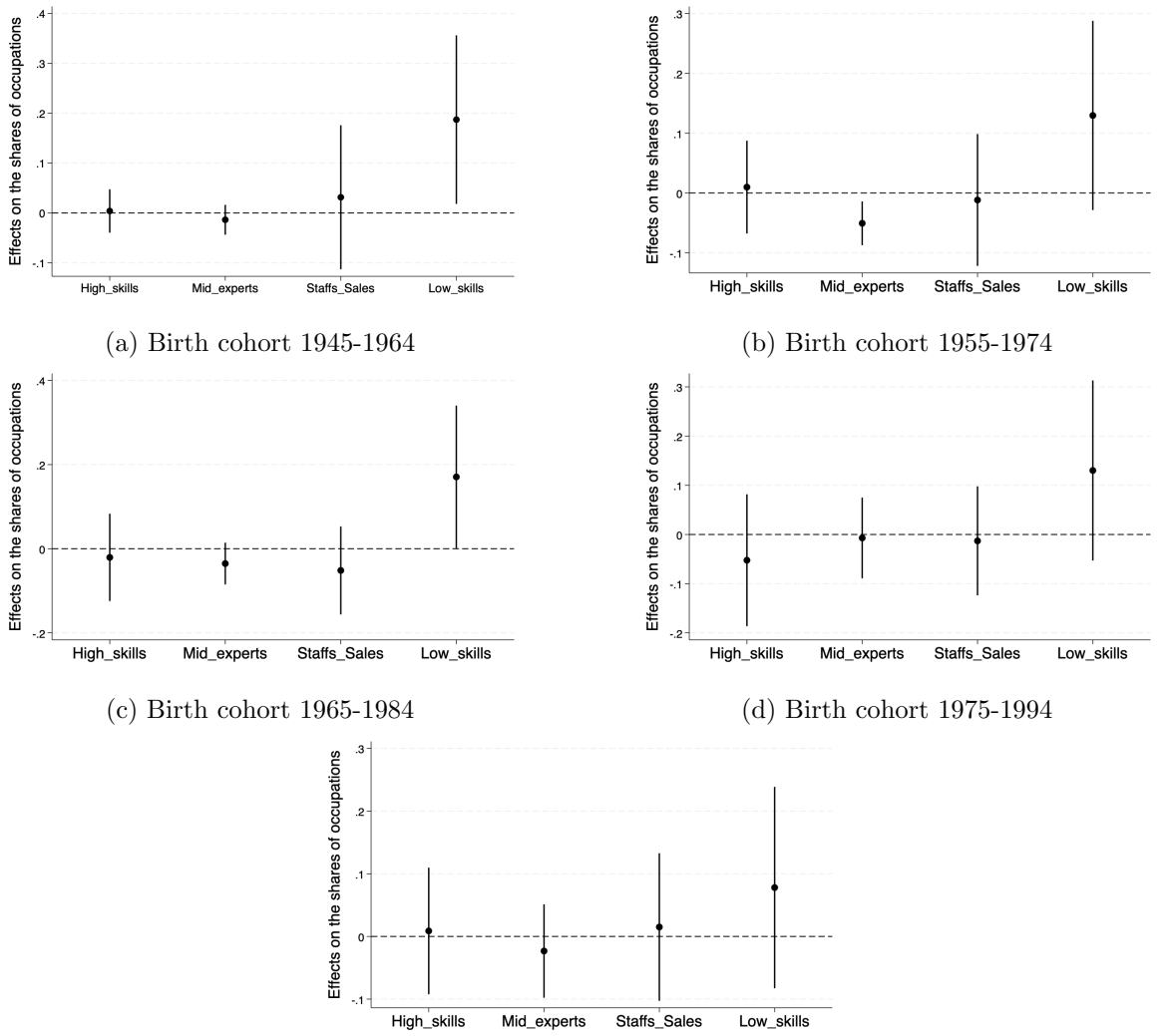
Figure 5: Impacts of Bombing on Occupations: Five Cohorts

Note: The figures present IV estimates of the impacts of bombing on the shares of different occupations in 2018, including high-skilled jobs, middle-skilled expert roles, staff and sales positions, and low-skilled jobs, across five overlapping cohorts. Each cohort represents an overlapping 20-year birth window: two older cohorts born before 1975, two younger cohorts born after 1975, and one cohort spanning the war period. The youngest cohort spans an 18-year birth window, ending in 2003, as those born later are excluded from the sample due to their inability to legally work in Vietnam. A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects on the corresponding occupational share. Standard errors are clustered at the village level.

to an overall sample mean of 7.862 - 7.938 personal income in logarithms. To place these findings in perspective, the estimated income effects are economically substantial and notably larger than those typically documented in the broader conflict literature. I reserve a detailed discussion of these magnitudes for Section 8. The reduced-form estimates in Panel C show consistent results despite minor effects.

Panel D shows first-stage estimates, in which being below any of the four security thresholds statistically increases the share of months with strikes in the next quarter from 18.0% in column 3 to 28.4% in column 1.

In Table 1, I report first-stage F statistics, ranging from 4.54 to 9.96, which are below



(a) Birth cohort 1945-1964

(b) Birth cohort 1955-1974

(c) Birth cohort 1965-1984

(d) Birth cohort 1975-1994

(e) Birth cohort 1985-2003

Figure 6: Reduced Forms: Five Cohorts

Note: The figures present reduced form estimates of the impacts of bombing on the shares of occupations in 2018, including high-skilled jobs, middle-skilled expert roles, staff and sales positions, and low-skilled jobs over five overlapping cohorts. Each cohort represents an overlapping 20-year birth window: two older cohorts born before 1975, two younger cohorts born after 1975, and one cohort spanning the war period. The youngest cohort spans an 18-year birth window, ending in 2003, as those born later are excluded from the sample due to their inability to legally work in Vietnam. In the reduced form regressions, *below* is the exogenous indicator variable, which indicates whether a village is randomly assigned to be below any of the four security thresholds by the US army. I expect that a village below a security threshold will have had more strikes in the next quarter. See Section 4.2 for details on how a village is determined to be below a threshold and how this implies bombing in the next quarter. This reduced form estimates the relationship between *below* and the shares of occupations regardless of bombing. A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects of bombing on the corresponding occupational share. Standard errors are clustered at the village level.

ideal for IV estimates—likely due to the small single-hamlet sample. Recent studies (e.g., Lee, McCrary, Moreira, and Porter (2022)) question the $F > 10$ criterion that the rule might no longer be valid. To confirm instrument strength, I additionally use Anderson-Rubin tests, which validate my IV. These tests are applied to all IV regressions; I report these statistics in Table 1 and any Tables with IV estimates. In most cases, these AR tests yield a P-value less than 0.05, indicating that the instrument used is not weak.

To conclude, the adverse effect of bombing on personal income is present not only for

Table 1: Impacts of Bombing on Income: Two Cohorts

Sample	(1) *All cohorts*	(2) *Old cohort*	(3) *Young cohort*
Dep. Variable	Income	Income	Income
Panel A. OLS estimations			
	-0.148***	-0.398*	-0.299*
	(0.053)	(0.221)	(0.162)
Panel B. IV estimations			
Bombing	-1.650***	-2.629**	-2.505**
	(0.549)	(1.310)	(1.022)
Mean of Dep. Var.	7.885	7.938	7.862
Panel C. Reduced form			
Below	-0.468**	-0.690**	-0.379*
	(0.224)	(0.319)	(0.196)
Panel D. First stage			
Dep. Variable	Bombing	Bombing	Bombing
Below (IV)	0.284***	0.211**	0.180***
	(0.090)	(0.099)	(0.060)
F stat.	9.96	4.54	8.99
AR Test (P-value)	0.00	0.04	0.03
Controls	✓	✓	✓
Observations	213	155	161

Note: The table presents village-level estimations of the impacts of bombing on personal income in 2018. The dependent variable is personal income, measured as the logarithm of total household income divided by household size. For older or younger cohort analyses, household income includes only earnings from people in the respective cohort. Column 1 is the estimate for all cohorts, and columns 2 and 3 are the estimates for the sole old and young cohorts. In the first stage (Panel D), the status of a village that is *below* any threshold in terms of the four security thresholds serves as the instrumental variable. The reduced form (Panel C) regresses personal income on the instrument (*below*), including all controls. Control variables include sex, age, educational attainment, household size, urban/rural status, occupation, and other controls from Dell and Querubin (2018). Robust standard errors clustered at the village level in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

older but also for younger cohorts in 2018. My paper is one of the rare studies indicating that war still has persistent consequences on personal income across generations. My finding aligns with a recent paper of Halbmeier and Schröder (2025), who studied WWII and found that people born in heavily bombed areas own 10 percent less wealth than those from less-damaged localities in the early 2000s. Together, these studies shed light on an unexplored long-term effect that war may have serious economic consequences for very young people. In what follows, I show that bombing also created high poverty rates for villages in 2018. Nevertheless, bombing does not affect the educational attainment of people of all ages or the village population numbers.

5.3 Effects of Bombing on the Poverty Rate

In Table A.7, I estimate the effects of bombing on village-level poverty rates in 2016 and 2018. I construct village-level poverty rates as the proportion of households classified as poor in the survey year, based on poverty lines defined by the Vietnamese government.[15] The results in

[15]In Vietnam, the poverty threshold for 2016 and 2018 was defined under Decision No. 59/2015/QĐ-TTg. Accordingly, a poor household in a rural area is one with an average per capita monthly income of 700,000 VND or less, and simultaneously lacking in more than three indicators measuring deprivation in access to basic

Table A.7 show a strong correlation between bombing and the poverty rates of villages. A movement of bombing from zero strikes to the sample mean increases the poverty rate in 2016 by 11.4 (0.389×0.292) percentage points, (column 1) and in 2018 by 5.6 (0.193×0.292) percentage points, (column 2). Another paper, Miguel and Roland (2011), found a signal of the effects of Vietnam bombing on poverty rates of provinces/districts; however, the effects are not sizable. My findings, when focusing on granular data, suggest that even after four decades have passed, bombing significantly increased village-level poverty. This evidence complements my findings in Table 1, where bombing is associated with reductions in personal income.

5.4 Effects of Bombing on Educational Attainment

Figure A.1 and Table A.6 present my OLS and IV estimates of the effects of bombing on educational attainment by cohorts in 2018. Table A.6 considers two cohorts samples. Panel A's OLS results (columns 1–4) indicate a decline in general and tertiary education shares within the old cohort and a signal of a long-term reduction in tertiary education for the young cohort. Panel B's IV estimates (columns 1–2), however, show effects solely on the old cohort: moving from zero strikes to the sample mean decreased general education shares for the old cohort by 7.0 - 9.6 percentage points. There are no significant effects of bombing on the education of the young cohort. Figure A.1 in the appendices considers five overlapping cohort samples. The figure shows that bombing reduced the general education of only the two oldest cohorts, those born in the periods 1945-1964 and 1955-1974, although many of these persons completed secondary school before the intense bombing period (1965–1975).[16] My findings align with Acosta, Baez, Caruso, and Carcach (2023), who found that people who experienced the civil war in El Salvador (1980-1992) achieved 0.8 fewer years of education than others. Because I found only a small effect of bombing on the education of older people, my findings contradict Havari and Peracchi (2019), who found that children of parents who endured war have lower educational attainment than those who did not. In summary, I provide new evidence that bombing has no long-term effect on the educational attainment of young generations.

5.5 Effects of Bombing on Population

Figure A.2 in the appendices presents OLS and IV estimates of bombing's effects on population size for old and young cohorts. Results from these estimations show negative but statistically insignificant effects. These are in line with prior findings: Miguel and Roland (2011) report no significant link between US bombing intensity and district or province population density growth of Vietnam in 2002; Appy (2015) note that despite town destruction, Vietnamese populations largely remained, explaining the lack of long-term population change due to bombing, and Dell and Querubin (2018) find no short-term effect of bombing during the Vietnam War on quarterly population growth.

In summary, this section demonstrates that bombing during the Vietnam War has had long-lasting adverse effects on the economic situation of both old and young people, pushing

social services. In urban areas, this number is 900,000 VND.

[16] Appendix Figure A.4 presents the education effects of bombing for six separate cohorts, giving a potential explanation why bombing affected only these two cohorts: the low secondary attainment of cohort 1945–1954 likely reflects limited education access pre-war in Vietnam, while bombing significantly lowers general education for cohort 1965–1974, born during peak bombing, which created the low educational attainment for 20-year cohort 1955-1974. Again, in these figures, bombing does not significantly affect the education of younger cohorts.

them to work in low-skilled (i.e., low-paid) occupations and reducing their personal income by more than 50%. Strikes had minimal effects on the educational attainment of all cohorts and had no impact on population size in the long run.

6 Robustness Checks

I conduct several regressions using different samples and alternative data to assess the robustness of the results found in Section 5.

Alternative outcome data

First, I re-estimate the effects of bombing on most outcomes (occupation, personal income, and educational attainment) but using the 2016 VHLSS data instead of the 2018 VHLSS data. The VHLSS data lacks an actual panel structure, with only half of the sample retained between rounds. To address concerns about potential sampling bias or contemporaneous shocks explicitly related to the year 2018, I re-estimate the regressions in Section 5 using the 2016 VHLSS data. In the appendices, Figure A.5 presents the impacts of bombing on occupations of two cohorts in 2016. While some estimates lose statistical significance compared to 2018, the overall pattern—especially the rise in low-skilled employment in heavily bombed areas—persists. Table A.8 and Figure A.6 illustrate the adverse effects of bombing on cohorts' personal income and the minimal impact on educational attainment. In summary, the baseline estimates in Section 5 are highly robust across the entire 2016 dataset.

Alternative bombing data

Secondly, to address the concern regarding the use of bombing frequency (share of months with strikes during a quarter) rather than bombing quantity, as is more common, I examine the effects of bombing using an alternative bombing dataset from Miguel and Roland (2011). In detail, bombing (quantity) refers to the total number of US bombs, missiles, and rockets per square kilometer recorded during the Vietnam War by the US military. This dataset covers the entire country but aggregates data at the district level, offering less geographical precision than the village-level data from Dell and Querubin (2018). The data has been widely used in economic history studies such as Singhal (2019); Palmer et al. (2019); Churchill et al. (2022). Table 2 presents the estimates of the effects of bombing on the' personal income of two cohorts. The results indicate that shifting from zero bombing to the sample mean intensity of 32.3 bombs, missiles, and rockets per square kilometer corresponds to a significant reduction in personal income at the district level. Specifically, this exposure lowers income by approximately 9.2% $\left(100 \times \left(e^{32.3 \times -0.003} - 1\right)\right)$ for young cohorts and 6.3% for old cohorts in 2018.[17] These estimates prove that my results also hold when using an alternative dataset of bombing intensity. Note that because this bombing data differs, I must use an alternative identification strategy to ensure compatibility. I follow Miguel and Roland (2011), who used this data first, and adopt the north–south distance to the 17th parallel of Vietnam as an instrumental variable for bombing intensity. The first-stage results confirm that this IV strongly predicts bombing exposure, aligning with the first stage of Miguel and Roland (2011).[18]

[17] The absolute effects vary slightly for the old cohort when the number of observations is reduced. However, the two coefficients estimated for separate cohorts are not too different.

[18] Note that Miguel and Roland (2011) do not investigate the effects of bombing Vietnam on personal income in 2002 but instead focus on district-level consumption.

Table 2: Impacts of Bombing on Income: Two Cohorts

Sample	(1) *All cohorts*	(2) *Old cohort*	(3) *Young cohort*
Dep. Variable	Income	Income	Income
Panel A. IV estimations			
Bombing (quantity)	-0.003***	-0.002***	-0.003**
	(0.001)	(0.001)	(0.001)
Mean of Dep. Var.	8.423	8.354	8.459
Panel B. First stage			
Dep. Variable	Bombing	Bombing	Bombing
Distance to 17 (IV)	-16.832***	-17.020***	-16.832***
	(3.444)	(3.521)	(3.444)
F stat.	23.89	23.38	23.89
AR Test (P-value)	0.00	0.02	0.00
Controls	✓	✓	✓
Clusters	248	246	248
Observations	248	246	248

Note: The table presents estimations of the impacts of bombing on personal income using district-level bombing data from Miguel and Roland (2011). The dependent variable is personal income in 2018, measured as the logarithm of total household income divided by household size. For older or younger cohort analyses, household income includes only earnings from individuals in the respective cohort. Bombing (quantity) is the total US bombs, missiles, and rockets per square kilometer. The average bombing intensity is 32.3 bombs, missiles, and rockets per square kilometer. All results are obtained at the district level using instrumental estimations, in which the instrument variable is the distance from a district to the 17th parallel north. This instrument has been used in several studies, including Palmer et al. (2019); Singhal (2019); Churchill et al. (2022). Control variables include sex, age, educational attainment, household size, urban/rural status, occupation, and other controls from Miguel and Roland (2011). Robust standard errors clustered at the district level in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Separate cohorts samples

Third, I re-estimate the effects of bombing on the occupation and educational attainment of cohorts in 2018 using six separate 10-year birth cohorts instead of the five overlapping cohorts used in Section 5. Figure A.3 shows the effects of bombing on occupations of cohorts. I observe noisier results when using these subsamples, but in general, bombing causes more people of all ages to work in low-skilled (low-paid) occupations, in line with my baseline results. Figure A.4 presents the effects of bombing on educational attainment across six cohorts. Again, this suggests that the bombing had minor effects on the general education of older cohorts, consistent with my previous findings.

Cluster at different level

Finally, I test the robustness of my baseline results by replicating the estimations in Section 5, clustering standard errors at the *district* level instead of the village level. Because bombing varies at the village level in my sample, clustering at the district level enables testing of broader patterns and ensures robustness against higher-level spatial correlation. Figure A.7, Figure A.8, and Table A.9 show that the bombing coefficients and their significance remain

consistent as in the baseline results, with only minor changes in standard errors. In conclusion, my results in Section 5 hold throughout the numerous robustness checks.

7 Mechanisms

7.1 Combat-Era Circumstances as Mechanisms

In this section, I investigate whether specific channels mediate the adverse long-term effects of war-era bombing on occupations and personal incomes in 2018. A potential mechanism should correlate with both bombing and the outcomes of interest. Furthermore, inclusion of the possible channel should either decrease the magnitude of the bombing coefficient or render the result insignificant. The former determines a partial mediation while the latter is a complete mediation (Churchill et al., 2022).

Bombing caused severe disruptions during and after the war—damaged infrastructure, altered government operations, and stalled economic development. These short-term shocks proved difficult to reverse and gradually became long-term constraints. In this section, I utilize all available data on village conditions during wartime and postwar from Dell and Querubin (2018), focusing on infrastructure, governance, and economic indicators, and examine whether these short-term damages mediated the adverse effects of bombing on occupation and personal income in 2018.

I exploit a range of variables that encapsulate the historical prestige of villages. This set includes variables containing the following information: (1) Whether the children of village residents were able to attend primary school classes in the past - i.e., in 1970/71, when HES71 was conducted (primary school accessibility). (2) Whether a government-accredited secondary school was accessible in the past (secondary school accessibility). (3) Whether a village was in the high-security class.[19] (4) Whether a village was in the high administration latent class [20] (proxy of village governance). (5) Whether manufactured goods were for sale at the local markets in the past. (6) Whether there was a surplus of goods produced in this village for sale outside the village.

To study how much of the relationship between bombing and outcomes (occupation and personal income in 2018) is mediated through the potential channels listed, I first examine the correlation between bombing and each of these potential channels. I found that bombing influences almost all potential factors significantly, except for the accessibility to secondary school. I present the estimations of the relationships between bombing and potential channels in the appendices, Table A.10.

However, not all potential channels listed in Table A.10 are mechanisms that may explain the effects of bombing on outcomes. I observe that primary school accessibility and local village governance are two mechanisms that may explain contemporary occupational trends and low personal income. I present evidence about these mechanisms in Table 3. Column 2 shows that accessibility to primary school in the past could reduce the negative impacts of bombing on income. Because bombing worsened accessibility to primary school in the past (Table A.10, column 1), this accessibility shortage mediated the adverse effects of bombing on income in 2018. Column 4 of Table 3 indicates that a high level of village governance

[19] The term "high-security class" refers to hamlets where the Government of the Republic of Vietnam maintained dominant administrative and military control, with minimal to no insurgent activity or influence from the Viet Cong .

[20] The term "administration latent class" refers to a summary measure created through Latent Class Analysis by the US military to evaluate the performance and presence of the South Vietnamese local government. It incorporates several factors, including the government's ability to collect taxes, whether village committee positions are fully staffed, and the frequency with which the village chief visits neighborhoods to interface with citizens.

Table 3: Potential Mechanisms

Sample	(1) *All cohorts*	(2) *All cohorts*	(3) *Old cohort*	(4) *Old cohort*
Dep. Variable	Income	Income	Mid-experts jobs	Mid-experts jobs
Panel A. IV estimations				
Bombing	-1.650***	-1.147**	-0.120***	-0.109***
	(0.549)	(0.473)	(0.036)	(0.034)
Mean of Dep. Var.	7.885	7.885	0.018	0.018
Pri.school acces	–	1.070***	–	–
	–	(0.266)	–	–
Well-organized village	–	–	–	0.037**
	–	–	–	(0.018)
Panel B. First stage				
Below (IV)	0.284***	0.293***	0.284***	0.286***
	(0.090)	(0.089)	(0.090)	(0.089)
F stat.	9.96	11.23	9.96	10.43
AR Test (P-value)	0.00	0.00	0.00	0.00
Controls	✓	✓	✓	✓
Observations	213	213	213	213

Note: The table presents the impacts of bombing on different outcomes in 2018 (Income: columns 1 and 2; Occupation: columns 3 and 4. Columns 2, 3, and 4 each include a variable that represents a potential channel. In the first stage, the status of a village that is *below* any threshold in terms of the four security thresholds serves as the instrumental variable. The regressions in both stages include a set of control variables, including sex, age, household size, urban/rural status, and educational attainment, as well as other controls from Dell and Querubin (2018). Robust standard errors clustered at the village level in parentheses. Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in the past may mean more older cohort workers in middle expert positions. Since bombing reduced the governance of villages in the past (Table A.10, column 4), this collapse mediated the adverse effects of bombing on middle-expert occupation in 2018.

Note that not all of my findings in Section 5 can be explained by a corresponding mechanism. The results presented here must be interpreted with caution, as village attributes during and after the war (which I consider the channels) are limited both in scope and coverage. The mechanism that I detected here explains only a small part of the total effects of bombing on the listed outcomes.

7.2 State-Directed Economic Planning

Vietnam's centrally planned economy (1976–1985) likely trapped many people from the old cohort in agriculture and low-skilled jobs due to limited labor market options rather than personal choice. After the war, the state expanded agricultural production by relocating people to rural areas while severely restricting the industrial sector. Private enterprise was banned, and workers had little incentive to work under collective farming, where land was state-owned, and productivity did not affect wages or food access (Van Song et al., 2020). Farmers were compelled to join cooperatives and deliver all output to the state. Industrial output stagnated; in Ho Chi Minh City alone, 30,000 businesses closed within three months of trade bans in 1978 (Desbarats, 1987). Investment in private firms was illegal (Van Song et al., 2020), and by 1980, industrial production remained at 1976 levels while agriculture grew by just 2% annually (Kimura, 1986).

This forced reliance on agriculture lasted at least a decade. Although the 1986 Doi Moi reforms reintroduced market incentives, many in the old cohort remained in low-skilled sectors. The persistence of these patterns suggests that path dependence is shaped by state policy,

rather than occupational choices.

The 1986 Sixth Party Congress launched major reforms, including the transfer of land from cooperatives to households, 15-year land-use rights, and the legalization of surplus trade (Thayer, 1987). These changes enabled the development of a market economy in agriculture. For much of the old cohort—with decades of farming experience and limited industrial skills—this likely reinforced their continued work in cultivation. In contrast, the young cohort, more adaptable to post-reform shifts, faced weaker effects. As shown in Figure 4 and Figure 5, bombing had less impact in terms of pushing younger people into low-skilled jobs. Some were able to move beyond agriculture, despite potentially inheriting land from their older family members.

7.3 Selective Migration

Numerous historical sources document large-scale migration across Vietnam during and after the war. While some migration was voluntary, forced displacement during wartime and state-directed redistribution afterward dominated. In the South, rising rural insecurity from bombing pushed millions into urban areas[21]. Post-war, the government reversed these flows through mass resettlement programs, relocating poor and unemployed urban dwellers to rural villages and New Economic Zones (NEZs) (Desbarats, 1987; Banister, 1992). Though around 20% eventually returned to cities, a majority remained rural, often possessing low human capital, which contributed to the persistence of low-skilled occupations among the old cohort.

Between 1965–1974, about 10 million people—nearly 47% of the South's population—were displaced (Desbarats, 1987). After reunification, the state implemented population redistribution to decongest cities and expand agriculture (Jones, 1982; Anh, 2003; UNFPA, 2010). Over 3 million urban residents and 2 million minority citizens were relocated to NEZs (Desbarats, 1987; Banister, 1992). Because return migration to cities was illegal (Forbes, 1996; Anh, 2003), roughly 80% remained in rural areas (Banister, 1985) despite poor conditions.

Given the imbalance in wartime rural–urban migration and its partial reversal post-war, bombed villages likely absorbed many returnees and NEZ settlers with limited skills. These populations, constrained by poor infrastructure and government-imposed agricultural mandates, were unlikely to transition to higher-skilled work even after market reforms in 1986. Thus, the old cohort—comprising those who lived through both phases—is most exposed to the long-term occupational effects of bombing and forced migration.

8 Discussion

8.1 Unpacking the Magnitude of Income Loss

Survey data from 2018 reveals an alarming reality: individual incomes in areas heavily affected by bombing are over 50% below their potential levels. This is a severe decline, especially when compared to studies on other conflicts worldwide.

Internationally, economists have quantified the impacts of conflict and pointed to significant declines in GDP per capita, although the magnitude of these effects varies. Fund (2020), in a survey of numerous civil wars, concluded that GDP per capita falls by approximately 28% ten years after the conflict; private consumption drops by about 25%. Halbmeier and Schröder (2025), examining the descendants of those exposed to World War II in Germany, found an association between a father's birthplace in a war zone and a 17% reduction in the

[21] In contrast, the North evacuated urban residents to rural areas in response to US bombing (Banister, 1985; Desbarats, 1987; Banister, 1992).

offspring's wealth. Similarly, Keše ljević and Spruk (2024) showed that the Syrian Civil War wiped out 14% of the country's GDP per capita.

In Vietnam, research on the long-term impact of bombing on individual income remains relatively modest, though some related works have laid important foundations. Nguyen et al. (2024) showed that a 1% increase in unexploded ordnance density reduced nighttime light intensity—a proxy for district-level economic growth—by 0.46%. Le and Nguyen (2020) analyzed the density of ordnance and identified an income disparity of up to 30% during 2010–2014. This gap exists between school-age individuals in the most heavily targeted districts compared to those in areas with average exposure. This figure implies that if the comparison were extended between heavily destroyed areas and those with absolutely no bombing, the income discrepancy would be significantly larger. The paper, however, does not mention to new-generation incomes.

In this study, I record a decline of over 50% in individual income in 2018 for both the old cohort (directly exposed to the war) and the young cohort (born after the war). There are three main mechanisms that explain this:

First, the double-penalty mechanism: The increase in the share of low-skilled labor due to bombing consequences implies that workers are pushed into a lower income equilibrium. They not only lose the opportunity to work in higher-skilled sectors (as they might have in a peaceful region) but are also trapped at the bottom of the income distribution. Figure 7 clearly illustrates this stratification: low-skilled workers have median incomes that are significantly lower than those of other occupational groups. For individuals in the lowest quantile of the low-skilled group, an income shock of over 50% is entirely plausible.

Second, the sensitivity of the model to sample size: When I pool cohorts, the estimated income decline is approximately 38%. However, when I subsample to examine each cohort separately, the decline rises to over 50%. I suggest that the reduced number of observations in the subsample models may have partially inflated the magnitude of the impact. Nevertheless, the fact that the estimated coefficients remain statistically distinguishable from zero is robust evidence confirming that the negative impact of bombing on income is present and undeniable.

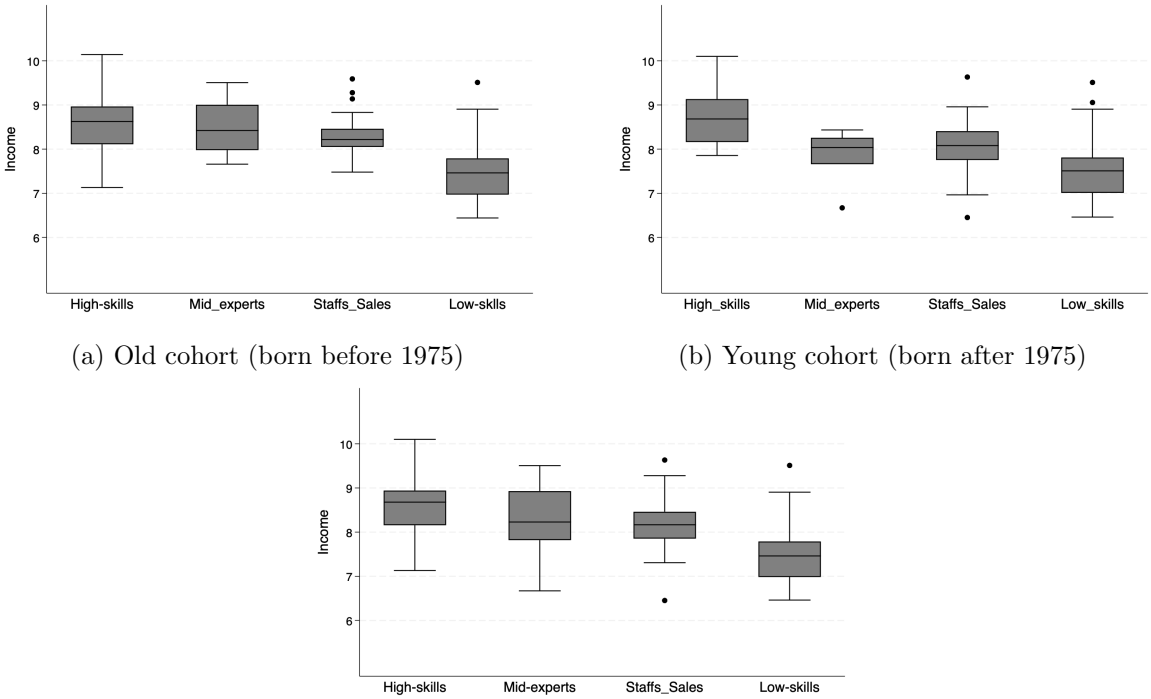
Third, the interpretation of the marginal effect: It should be noted that the income decline of approximately 50% reflects the hypothetical impact associated with an increase in bombing intensity from zero to the sample mean (0.292 strikes per quarter). In reality, the bombing intensity experienced by many villages may be lower than this mean, implying that the actual income loss in those locations would be smaller than the estimated figure.

Another potential factor is measurement error stemming from the complexities of quantifying agricultural income, particularly when a significant portion of earnings is derived from livestock assets.

In summary, the legacy of bombing on individual income in 2018 is an observable reality, and while the precise magnitude may be open to debate, the possibility that the actual damage is severe cannot be ruled out.

8.2 The Multi-Generational Burden of the Vietnam War

This study provides evidence that bombing during the Vietnam War left an occupational impact on multiple population cohorts born between 1945 and 1984 (see Section 5). However, the paper does not specifically quantify how many generations actually bore these adverse consequences. Determining the extent to which the impact persists across generations holds significant implications for economists, particularly given that research on the impact of conflict on second and third generations—especially regarding labor market outcomes—remains relatively limited. To address this issue, a key prerequisite is to define the length of a gener-



(a) Old cohort (born before 1975)

(b) Young cohort (born after 1975)

(c) All cohorts

Figure 7: Incomes over Four Occupational Groups

Note: The figure compares the income distribution across four occupational groups: High-skilled, middle experts, staff and sales, and low-skilled. The vertical axis represents income, defined as the logarithm of total real household income divided by household size. The horizontal axis denotes the occupational categories. The horizontal line within each box indicates the median income. Panel (a) displays the distribution for the old cohort (born before 1975); Panel (b) depicts the young cohort (born after 1975); and Panel (c) presents the pooled sample of both cohorts. Overall, median income is positively correlated with skill level, with the low-skilled group exhibiting the lowest income among the four categories examined.

ation. This number is not fixed but varies depending on the country and historical period. In economics, the mean age of mothers at childbearing is often used as a standard proxy for generation length; this age is generally higher in developed countries and lower in developing countries.

Studies in developed nations during the 1975–2000 period suggest that a generation can span nearly 30 years. In France in 1977, the mean age of mothers at birth was 26.5, and this figure increased to 29.4 by 2000 (Pison, 2001). Similarly, in the US, the mean age of mothers for all births was 24.6 in 1979 and rose to 27.2 in 2000 (Mathews & Hamilton, 2002).

In Vietnam, despite a lack of comprehensive statistics, existing reports still record significant fluctuations in the maternal age at childbearing throughout the 1945–2003 period. 1945–1949, Vietnamese women gave birth to their first child at an average age of 26.1 (Johansson, Hoang, Lap, Diwan, & Eriksson, 1996). However, this figure dropped to just 23.5 for women giving birth during the intense war period from 1953 to 1977 (GSO & Macro, 2003). Another analysis based on the 1988 Vietnam Demographic and Health Survey (DHS) data also indicated that the majority of women gave birth to their first child before the age of 20 (Luc, Thang, Swenson, & San, 1993).

Based on this evidence, I argue that a time frame of 25–30 years is appropriate to represent the length of a generation in Vietnam for those born during the 1945–2003 period. This approach allows me to roughly estimate the number of generations in Vietnam that were affected by bombing, based on the identified cohorts.

Given that bombing during the Vietnam War affected individual occupations in 2018 for

cohorts born between 1945 and 1984, this period spans approximately 39 years. Assuming a generation length of 25 years, this implies that approximately 1.6 generations bore the significantly adverse impact of the bombing on their occupations. If the generation length is extended to 30 years, the number of generations negatively affected in terms of occupation is approximately 1.3. This result once again confirms the persistent nature of war shocks, which are not easily erased even for generations born after hostilities have completely ceased. Nevertheless, the youngest cohort in this study (born 1994–2003) appears to have escaped the negative impacts on occupations. This indicates that the legacy of war, while persistent, gradually fades over time as the labor market recovers, leading to a situation where the difference in occupational trends between bombed and non-bombed areas dwindles to invisibility.

8.3 Bombing Impeded Structural Transformation

Areas heavily affected by bombing face stagnation in the transformation process, which is reflected in the sluggish transition of occupational structures and incomes across generations as of 2018. To clarify this mechanism, it is necessary to reference established foundations in economic theory. Structural transformation is theoretically grounded in the reallocation of economic activity across broad sectors (Herrendorf, Rogerson, & Valentinyi, 2014), necessitating a shift in the labor market towards tasks requiring higher cognitive and technical skills (Acemoglu & Autor, 2011). According to (McMillan & Rodrik, 2011), growth-enhancing structural transformation occurs only when labor moves from low-productivity to high-productivity activities, marking a transition from a low-income agrarian economy to a more prosperous urban industrial economy (Sen et al., 2019).

The fact that a large segment of the population, including the second generation in former war zones, remains trapped in low-skilled and low-income occupational groups (in Section 5, bombing increased the share of young cohorts working in low-skilled occupations by about 10 percentage points and reduced their income by over 50% in 2018) is a valid indicator that the war disrupted the process of structural transformation. Based on calculations of generation length, it can be asserted that bombed villages lag in structural transformation by approximately 1.3 to 1.6 generations, as it requires a corresponding amount of time for the disparities in occupation and income between villages to be eliminated. In summary, this paper provides evidence that the legacy of bombing not only impairs the income of multiple generations but also causes a structural lag at the village level.

8.4 Meaning of Persistence: Long-lasting Scars, Slow Convergence, and Place-based Traps

My empirical results paint a picture of the legacy of war: recovery is ongoing, but at a sluggish pace. The fact that the young cohort still suffers significant adverse effects (albeit smaller than the old cohort) provides robust evidence of long-lasting scars, refuting the hypothesis that war is merely a temporary shock to physical capital. This persistence reflects a mechanism of slow convergence through intergenerational transmission. The legacy of bombing does not vanish with the silencing of the bombs; it permeates the family structure and transmits poverty to the next generation. While the coefficient differential between the two generations confirms a recovery trend, the substantial magnitude of the impact on the young cohort indicates that the friction of structural transformation is significant, impeding the labor market's self-correcting capacity. The persistent lag at the village level suggests the existence of a place-based trap or low-level equilibrium trap. Bombing may have devastated the local economic structure:

When a village's occupational structure is locked into agriculture and low-skilled activities due to bombing, opportunities for social mobility for the next generation are stifled at their place of origin. Therefore, the differential between the two generations should not be viewed merely as evidence of successful recovery, but rather as a measure of structural sluggishness. At the current pace, it takes approximately 1.3 to 1.6 generations for the economic scars to fade, showing that the legacy of war is a long-term burden hindering poverty reduction and sustainable development efforts.

9 Concluding Remarks

Economists have extensively documented the immediate trauma and economic damage inflicted on populations exposed to war. However, evidence regarding the economic legacy on peacetime cohorts—those born and living in peace—remains scarce. Beyond studies inspecting psychological sequelae, relatively few contributions investigate how war relates to long-term economic indicators, such as employment and income, for the next generation. Addressing this gap, I explore bombing during the Vietnam War (1955–1975) to examine how conflict intensity shapes the occupations and incomes of cohorts born after the war.

This paper provides robust evidence that the war, despite ending over four decades ago, continues to exert adverse effects on the economic outcomes of young people in 2018. To identify these effects, I exploit an instrumental variable strategy based on security rounding thresholds, where villages falling just below a specific cutoff faced significantly more airstrikes than those just above it.

I find substantial effects of bombing on all age cohorts, including on the young generation born entirely after the conflict. Bombing increased the share of young cohorts working in low-skilled occupations by approximately 10 percentage points and severely reduced their income by over 50% in 2018. These distortions imply that heavily bombed villages lag behind in structural transformation by approximately 1.3 to 1.6 generations. This finding is critical, as few existing studies have quantified such intergenerational economic lags beyond the well-documented psychological trauma and loss of human capital.

Overall, my results shed light on the persistence of war shocks. While the adverse effects are shown to diminish gradually over time, the bombing has effectively trapped many workers in low-productivity sectors, creating a long-term structural lag that continues to constrain the occupations and incomes of future generations.

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A Appendices

Table A.1: Summary Statistics

	Obs	Frequency	Percent
Population			
old cohort	9,210	4,394	47.71
young cohort	9,210	4,816	52.29
Below (IV)			
Yes	9,210	4,601	49.96
No	9,210	4,609	50.04
Old cohort			
Urban			
Urban	4,394	1,991	45.31
Rural	4,394	2,403	54.69
Sex			
Male	4,394	2,036	46.34
Female	4,394	2,358	53.66
Occupations			
Leaders	3,003	44	1.47
Hi. experts	3,003	119	3.96
Mid. experts	3,003	76	2.53
Off. staffs	3,003	66	2.20
Ser. & sales	3,003	644	21.45
Pri. sector	3,003	285	9.49
Manual worker	3,003	336	11.19
Assem. & oper.	3,003	133	4.43
Low-skilled	3,003	1,293	43.06
Armed forces	3,003	7	0.23
Education			
No degree	3,259	875	26.85
Primary	3,259	1,037	31.82
Secondary	3,259	680	20.87
Hi. school	3,259	421	12.92
College	3,259	52	1.60
University	3,259	173	5.31
Master or higher	3,259	21	0,64
Young cohorts			
Urban			
Urban	4,816	2,154	44.73
Rural	4,816	2,662	55.27
Sex			
Male	4,816	2,471	51.31
Female	4,816	2,345	48.69
Occupations			
Leaders	3,727	33	0.89
Hi. experts	3,727	359	9.63
Mid. experts	3,727	183	4.91
Off. staffs	3,727	119	3.19
Ser. & sales	3,727	688	18.46
Pri. sector	3,727	173	4.64
Manual worker	3,727	518	13.90
Assem. & oper.	3,727	424	11.38
Low-skilled	3,727	1,221	32.76
Armed forces	3,727	9	0.24
Education			
No degree	3,773	315	8.35
Primary	3,773	927	24.57
Secondary	3,773	1,073	28.44
Hi. school	3,773	900	23.85
College	3,773	114	3.02
University	3,773	428	11.34
Master or higher	3,773	16	0.42

Note: The table presents summary statistics of main variables used in this paper, using data from the 2018 VHLSS survey and Dell and Querubin (2018). Observations are the count of individuals. For some variables, the data are summarized for separate young and old cohorts.

Table A.2: Summary Statistics (cont)

	Obs	Mean	Sd	Min	Max
Bombing	9,210	0.292	0.381	0	1
log(personal income)	9,210	8.050	0.622	6.450	10.020
Household size	9,210	4.195	1.587	1	11
Age	9,210	43.059	17.559	15	107
Old cohort	9,210	58.233	11.144	44	107
Young cohort	9,210	29.215	8.637	15	43

Note: The table presents summary statistics of main variables used in this paper, using data from the 2018 VHLSS survey and Dell and Querubin (2018). Observations are the count of individuals. For some variables, the data are summarized for separate young and old cohorts.

Table A.3: Occupational/ Industrial Classification

Classification by Occupations	**Classification by Industries**
1. Leader in all fields and levels	1. Agriculture, forestry, and aquaculture
2. High-level experts	2. Mining and quarrying
3. Mid-level experts	3. Processing and manufacturing industries
4. Office staff	4. Production and distribution of electricity, gas, hot water, steam and air-conditioners
5. Service and sales staff	5. Water supply, management and treatment of sewerage and waste
6. Skilled workers in agriculture, forestry, and fishery	6. Construction
7. Manual workers and related occupations	7. Wholesale, retail, and repair of automobiles, motorbikes, scooters, and other motorized vehicles
8. Machine assembling and operating workers	8. Transport, warehouse
9. Low-skilled labourers	9. Service of accommodation, food, and beverages
10. Members of armed forces	10. Information and communication
	11. Finance, banking, and insurance
	12. Business in real estates
	13. Professionalism, science, and technology
	14. Administration and supporting services
	15. Activities of the communist party and socio-political organizations, state management, public security and defense compulsory social assurance
	16. Education and training
	17. Healthcare and social assistance
	18. Art, recreation, and entertainment
	19. Other services
	20. Household employment generated by households, household self-production and self-services
	21. Activities of international organizations and bodies

Note: The table presents the occupational and industrial groups from the 2018 VHLSS survey data classified by Vietnam's General Statistics Office (GSO). In this paper, I extracted the information on the occupations of people from household questionnaires.

Table A.4: Balance Check

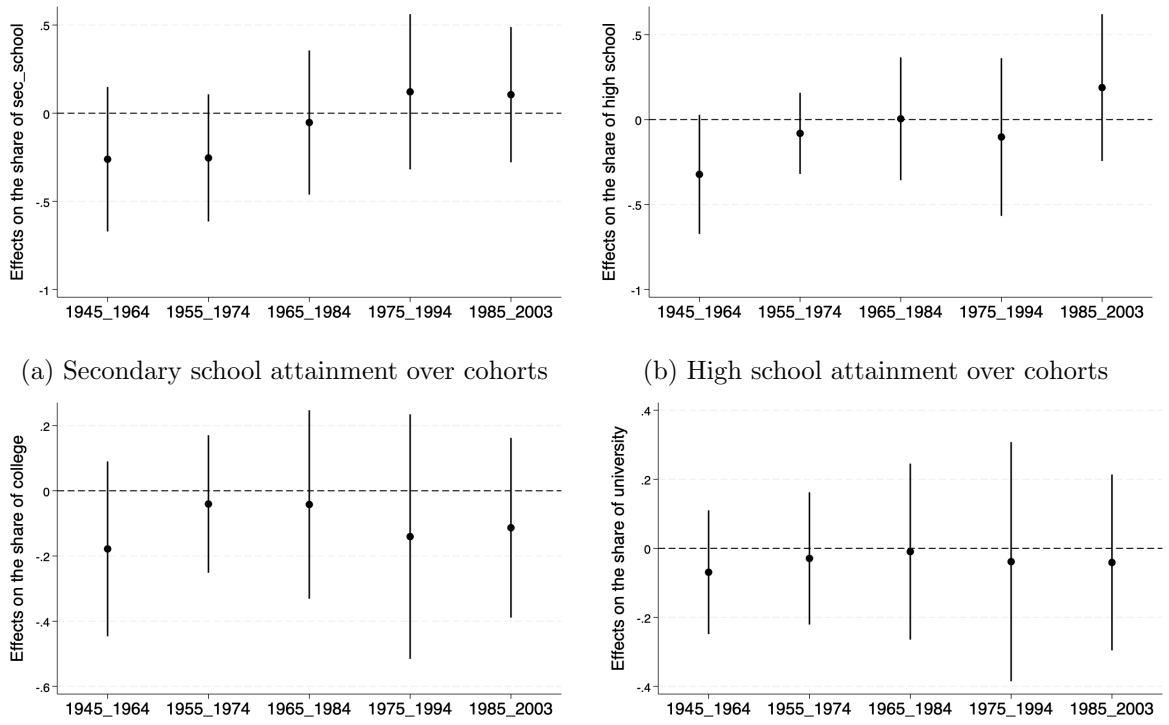
Pre-period characteristic	(1) RD coeff.	(2) Std. error
Bombing	0.023	0.077
Security		
Security LCA	-0.067	0.044
Enemy forces present	0.081	0.058
Village guerrilla squad	0.110	0.064
VC main force squad	-0.000	0.067
VC base nearby	0.080	0.057
VC attack	0.067	0.052
Active VC infrastructure	-0.106	0.072
% households participate VC	-0.017	0.025
VC propaganda	0.035	0.038
VC taxation	-0.199	0.092
Troops		
Friendly forces nearby	-0.004	0.021
US-initiated attacks	-0.004	0.022
US-deaths	0.006	0.053
SVN deaths	0.974	0.538
VC deaths	-6.471	4.849
Economic		
Economic LCA	0.002	0.056
Nonrice food available	-0.029	0.078
Manufactures available	-0.053	0.074
Surplus goods produced	0.036	0.087
Fields fallow due to insecurity	-0.151	0.078
HH with motorized vehicle	-0.046	0.034
HH require assistance to subsist	0.044	0.016
Hamlet population growth	-0.003	0.067
Urban	0.048	0.070
Governance		
Administration LCA	-0.275	0.233
Local government taxes	-0.127	0.088
Village committee filled	-0.126	0.079
Local chief visits hamlet	-0.054	0.062
Education LCA	0.067	0.034
Primary school access	0.013	0.039
Secondary school access	0.007	0.057
Health LCA	0.057	0.030
Public works under construction	0.140	0.087
Civic society		
Civic society LCA	0.055	0.051
HH participation in civic orgs	0.080	0.047
HH participation in PSDF	0.098	0.053
HH participation in econ training	0.032	0.029
HH participation in devo projects	0.013	0.050
Self devo projects under way	0.007	0.065
Youth organization exists	-0.002	0.806
Council meets regularly with citizens	0.085	0.093

Note: The table reports the balance check on pre-period (before bombing) characteristics of villages, using these attributes as the dependent variables in equation (2) and data in quarter $t-1$, immediately before bombing began. Column (1) presents the coefficient on the variable *below* in RD regressions. Column (2) is the standard errors at the village level. Most of the coefficients of *below* are statistically insignificant, which indicates that the village pre-period attributes are balanced. In this table, LCA refers to latent class analysis, VC represents the Northern Vietnamese troops, and HH denotes the household. PSDF is the People's Self-Defense Force. Data is from Dell and Querubin (2018).

Table A.5: Impacts of Bombing on Occupations: OLS Estimations

Dep. Variables	(1) High-skilled	(2) Middle experts	(3) Staffs & Sales	(4) Low-skilled
Panel A. OLS estimations: old cohort				
Bombing	-0.033	-0.049***	0.021	0.145*
	(0.024)	(0.018)	(0.053)	(0.075)
Panel B. OLS estimations: young cohort				
Bombing	-0.058	-0.038	-0.001	0.154*
	(0.050)	(0.027)	(0.055)	(0.084)
Controls	✓	✓	✓	✓
Observations	213	213	213	213

Note: The table presents OLS estimates of the impact of bombing on occupations, including high-skilled jobs, middle-skilled expert roles, staff and sales positions, and low-skilled jobs, of two cohorts in 2018 (the older cohort born before 1975, the younger cohort born after 1975). Control variables in these regressions include age, gender, household size, urban, and other controls from Dell and Querubin (2018). All regressions are at the village level. Robust standard errors clustered at the village level in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



(a) Secondary school attainment over cohorts

(b) High school attainment over cohorts

(c) College attainment over cohorts

(d) University attainment over cohorts

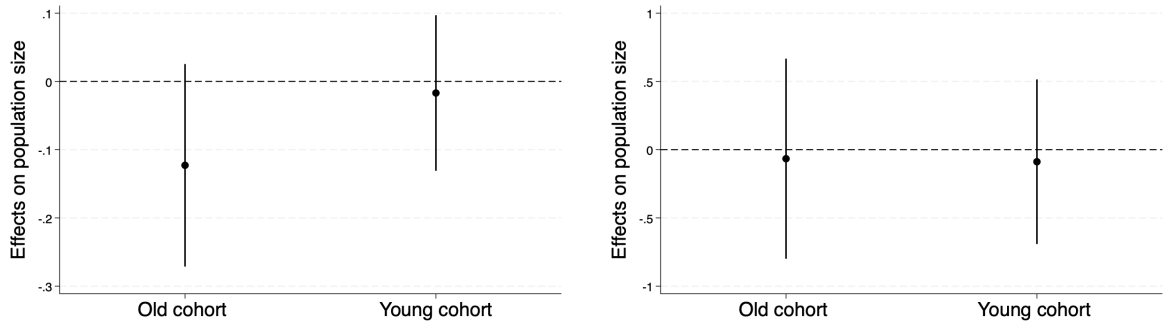
Figure A.1: Impacts of Bombing on Education: Five Cohorts

Note: The figure presents the IV estimation impacts of bombing on the shares of different diplomas over overlapping cohorts in 2018. Each cohort represents an overlapping 20-year birth window: two older cohorts born before 1975, two younger cohorts born after 1975, and one cohort spanning the war period. A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects of bombing on the corresponding educational attainment. Standard errors are clustered at the village level. Panels (a) to (d) show no clear evidence of bombing effects across all levels of educational attainment of cohorts in 2018.

Table A.6: Impacts of Bombing on Education: Two Cohorts

Dep. Variables	(1) Secondary	(2) High school	(3) College	(4) University
Panel A. OLS estimations: old cohort				
Bombing	-0.033	-0.057**	-0.024*	-0.025**
	(0.041)	(0.028)	(0.014)	(0.011)
Panel B. OLS estimations: young cohort				
Bombing	-0.017	-0.051	-0.050**	-0.041**
	(0.047)	(0.039)	(0.023)	(0.020)
Controls	✓	✓	✓	✓
Observations	214	214	214	214
Panel C. IV estimations: old cohort				
Bombing	-0.328*	-0.242**	-0.126	-0.072
	(0.168)	(0.119)	(0.101)	(0.078)
Mean of Dep. Var.	0.300	0.147	0.054	0.042
Reduced form				
Below	-0.093	-0.069	-0.036	-0.020
	(0.071)	(0.052)	(0.044)	(0.035)
Panel D. IV estimations: young cohort				
Bombing	0.074	0.043	-0.104	-0.018
	(0.194)	(0.205)	(0.145)	(0.136)
Mean of Dep. Var.	0.533	0.309	0.120	0.096
Reduced form				
Below	0.021	0.012	-0.029	-0.005
	(0.083)	(0.088)	(0.065)	(0.060)
Controls	✓	✓	✓	✓
Observations	213	213	213	213
Panel E. First stage				
Dep. Variable	Bombing	Bombing	Bombing	Bombing
Below (IV)	0.284***	0.284***	0.284***	0.284***
	(0.090)	(0.090)	(0.090)	(0.090)
Controls	✓	✓	✓	✓
F stat.	9.96	9.96	9.96	9.96
AR Test (P-value)	0.00	0.00	0.00	0.00
Observations	213	213	213	213

Note: The table presents estimates of the impact of bombing on educational attainment, measured by the shares of individuals holding different educational degrees in 2018. In all columns (1)-(4), the share of a specific educational diploma is the share of people who have that diploma, but it is not mutually exclusive of other diplomas. For example, the share of young cohorts with a high school diploma refers to those holding a high school diploma, who may also have a university degree or a master's degree. The IV results indicate that the older cohort attained approximately 9.6 percentage points (-0.328×0.292) less general education, while there is no clear effect on the educational attainment of the younger cohort. In the first stage (Panel E), the status of a village that is *below* any threshold in terms of four security thresholds serves as the instrumental variable. Control variables in these regressions include age, gender, household size, urban/rural status, occupation, and other controls from Dell and Querubin (2018). All regressions are at the village level. Robust standard errors clustered at the village level in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



(a) OLS estimation (b) IV estimation

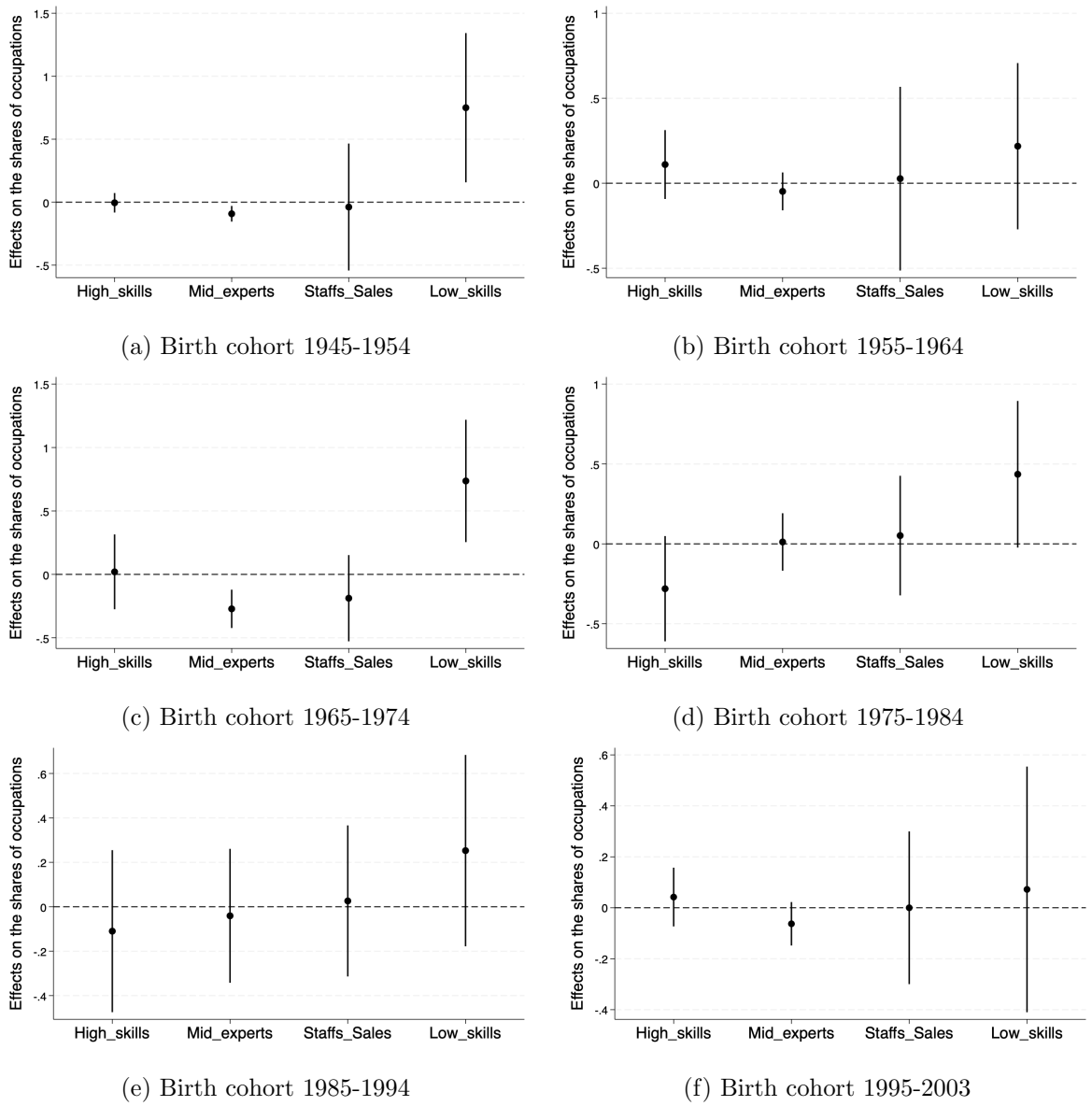
Figure A.2: Impacts of Bombing on Population Size: Two Cohorts

Note: The figure presents the OLS and IV estimations of the impacts of bombing on the population size of two cohorts in 2018 (older cohort born before 1975, younger cohort born after 1975). A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects of bombing on the population size of the corresponding cohort. Standard errors are clustered at the village level. Panels (a) and (b) show no clear evidence of bombing effects on the population in 2018.

Table A.7: Impacts of Bombing on Poverty

	(1)	(2)
Dep. Variables	Pov. rate 2016	Pov. rate 2018
Panel A. IV estimation		
Bombing	0.389**	0.193**
	(0.136)	(0.087)
Mean of Dep. Var	0.061	0.048
Controls	✓	✓
Observations	213	213
Panel B. First stage		
Dep. variables	Bombing	Bombing
Below (IV)	0.284***	0.284***
	(0.090)	(0.090)
Controls	✓	✓
F stat.	9.96	9.96
AR Test (P-value)	0.00	0.00
Observations	213	213

Note: The table reports estimates of the impact of bombing on village-level poverty rates in 2016 and 2018. Poverty is measured as the proportion of households classified as poor—based on the official criteria of the General Statistics Office (GSO) of Vietnam—relative to the total number of households in the sample. In the first stage (Panel B), the status of a village that is *below* any threshold in terms of four security thresholds serves as the instrumental variable. Control variables in these regressions include age, gender, household size, urban/rural status, occupation, and other controls from Dell and Querubin (2018). The estimates (Panel A) show that bombing increased village poverty rates in 2016 by 11.4 percentage points (0.389×0.292) and in 2018 by 5.6 percentage points (0.193×0.292). Robust standard errors clustered at the village level in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



(a) Birth cohort 1945-1954

(b) Birth cohort 1955-1964

(c) Birth cohort 1965-1974

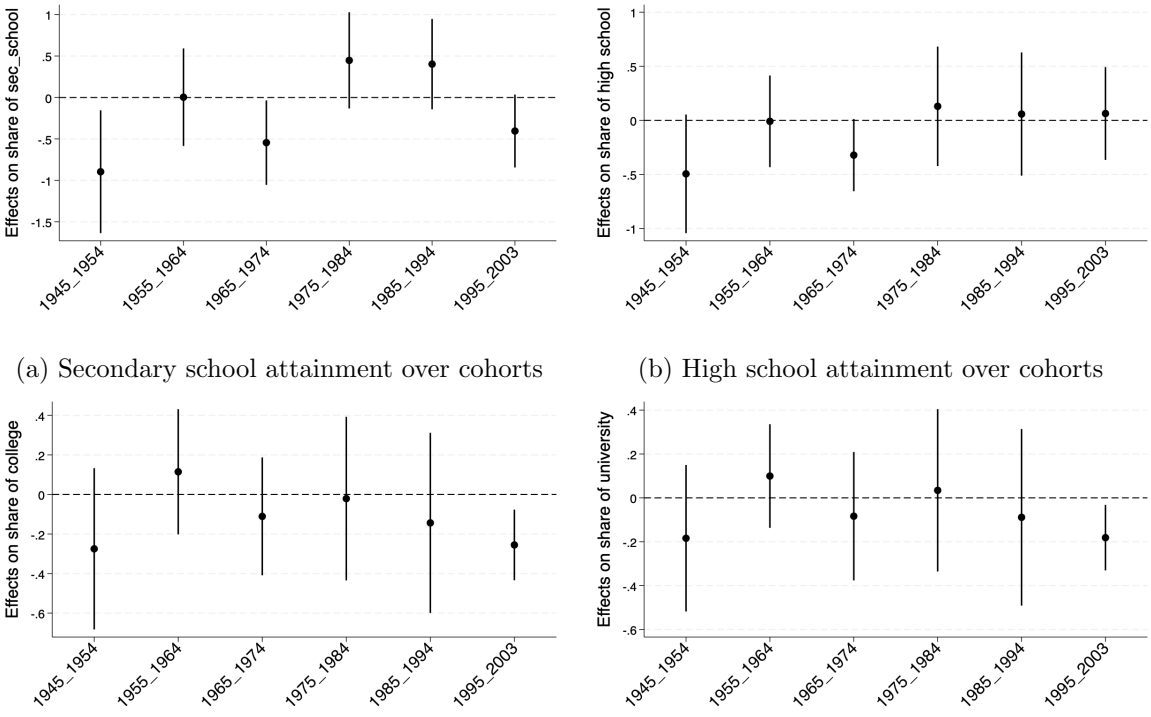
(d) Birth cohort 1975-1984

(e) Birth cohort 1985-1994

(f) Birth cohort 1995-2003

Figure A.3: Impacts of Bombing on Occupations: Six Cohorts

Note: The figures present IV estimates of the impact of bombing on the shares of different occupations in 2018, including high-skilled jobs, middle-skilled expert roles, staff and sales positions, and low-skilled jobs, across *six separate cohorts*. Each cohort represents a 10-year birth window: three older cohorts born before 1975 and three younger cohorts born in or after 1975. A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects on the corresponding occupational share. Standard errors are clustered at the village level. Panels (a) to (e) reveal a consistent trend: bombing increased the share of those working in low-skilled jobs across most cohorts except for the youngest cohort. The small sample size of the youngest cohort potentially contributes to the insignificance of this group.



(a) Secondary school attainment over cohorts

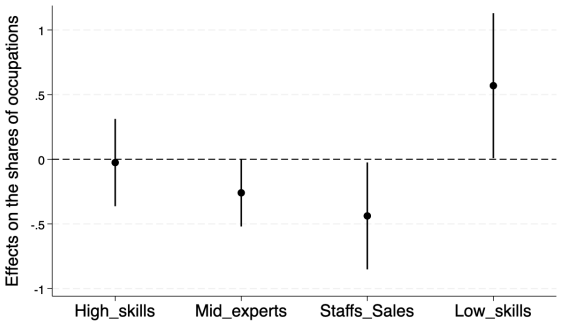
(b) High school attainment over cohorts

(c) College attainment over cohorts

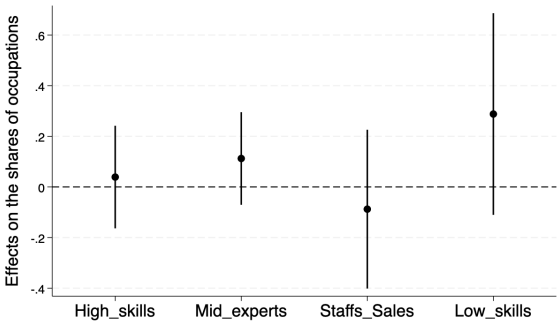
(d) University attainment over cohorts

Figure A.4: Impacts of Bombing on Education: Six Cohorts

Note: The figure presents the IV estimates of the impacts of bombing on the shares of different diplomas over *six separate cohorts* in 2018. Each cohort represents a 10-year birth window: three older cohorts born before 1975, three younger cohorts born in or after 1975. A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects of bombing on the corresponding educational attainment. Standard errors are clustered at the village level. I document some negative effects on the general education of the cohort 1965 - 1974 due to bombing in panels (a) and (b). I found no impact on the tertiary education of most cohorts in 2018. The youngest cohort shows noisy results because their education was not yet complete in 2018.



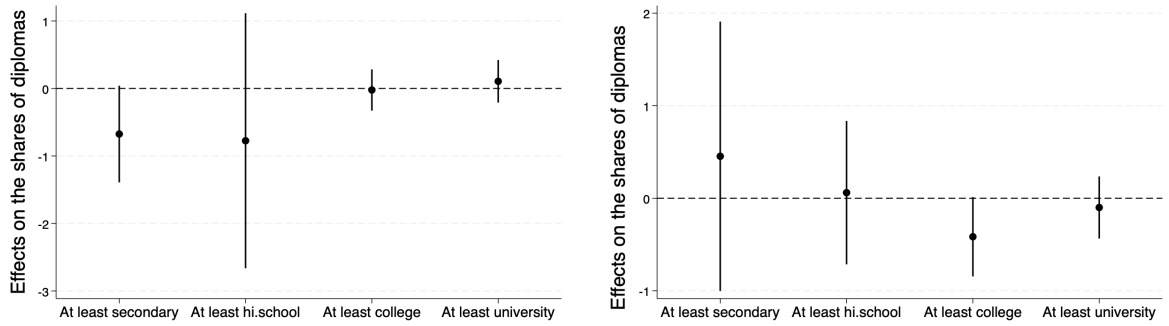
(a) Old cohort



(b) Young cohort

Figure A.5: Impacts of Bombing on Occupations in 2016: Two Cohorts

Note: The figure presents IV estimates of the impacts of bombing on the shares of occupations, including high-skilled jobs, middle-skilled expert roles, staff and sales positions, and low-skilled jobs, of two cohorts *in 2016* (older cohort born before 1975, younger cohort born after 1975). A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects of bombing on the share of the corresponding occupation. Results show that bombing increased the share of people in 2016 working in low-skilled jobs by 15.5 percentage points (0.53×0.292) for the older cohort, to 7.9 percentage points (0.27×0.292) for the younger cohort. The effects on the younger cohort, however, are less significant than in 2018. Standard errors are clustered at the village level.



(a) Old cohort

(b) Young cohort

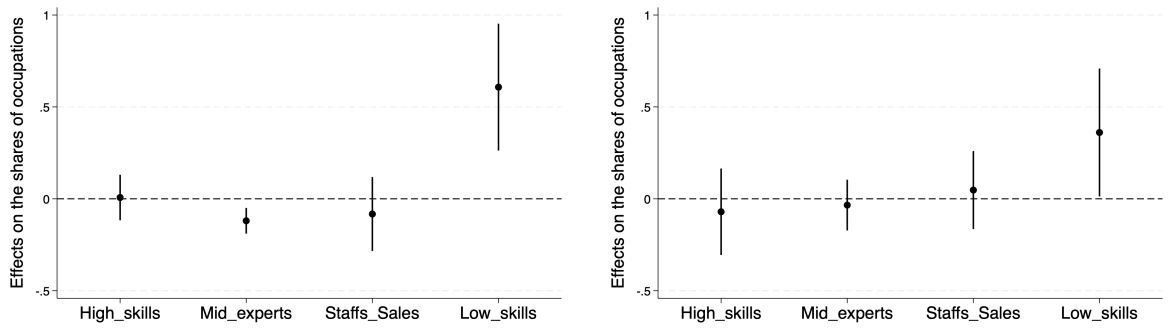
Figure A.6: Impacts of Bombing on Education in 2016: Two Cohorts

Note: The figure presents the IV estimates of the impacts of bombing on the shares of different diplomas over two cohorts *in 2016* (older cohort born before 1975, younger cohort born after 1975). A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects of bombing on the corresponding educational attainment. Standard errors are clustered at the village level. Panels (a) and (b) show no clear evidence of bombing effects across all levels of educational attainment of cohorts in 2016. There are indications of a reduction in general education for the older cohort and a lower level of tertiary education for the younger cohort due to the bombing.

Table A.8: Impacts of Bombing on Income in 2016

Sample	(1) *All cohorts*	(2) *Older cohort*	(3) *Younger cohort*
Dep. Variable	Income	Income	Income
Panel A. IV estimations			
Bombing	-1.808*	-1.082*	-1.468**
	(1.004)	(0.611)	(0.578)
Mean of Dep. Var.	8.012	7.962	8.003
Panel B. First Stages			
Dep. Variable	Bombing	Bombing	Bombing
Below (IV)	0.164*	0.189***	0.184***
	(0.088)	(0.050)	(0.047)
Controls	✓	✓	✓
F stat.	5.41	13.92	15.40
AR Test (P-value)	0.03	0.04	0.04
Observations	215	209	213

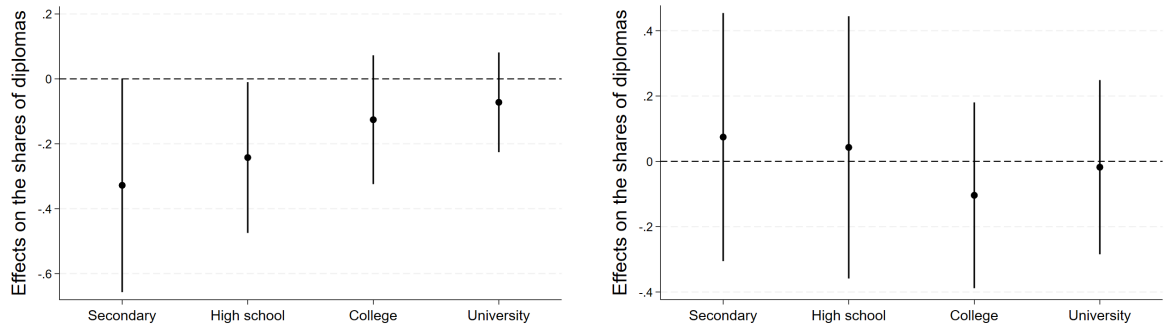
Note: The table presents estimates of the impacts of bombing on personal income *in 2016*. The dependent variable is personal income, measured as the logarithm of total household income divided by household size. For older or younger cohort analyses, household income includes only earnings from people in the respective cohort. Column (1) is the estimate for all cohorts, and columns (2) and (3) are the estimates for sole old and young cohorts. In the first stage (Panel B), the status of a village that is *below* any threshold in terms of four security thresholds serves as the instrumental variable. Control variables include sex, age, educational attainment, household size, urban/rural status, occupation, and other controls from Dell and Querubin (2018). The results in columns (2) and (3) show that bombing reduced personal income in 2016 by 27.1 percent $\left(100 \cdot \left[e^{-1.082 \times 0.292} - 1\right]\right)$ to 34.9 percent for both older and younger cohorts. Despite the magnitude differences from 2018, these effects are sizeable. Robust standard errors clustered at the village level in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



(a) Old cohort (b) Young cohort

Figure A.7: Impacts of Bombing on Occupations: Two Cohorts at the District Level

Note: The figures present IV estimates of the impact of bombing on the shares of different occupations in 2018 at the *district* level, including high-skilled jobs, middle-skilled expert roles, staff and sales positions, and low-skilled jobs. The left panel reports results for the older cohort (born before 1975), while the right panel shows results for the younger cohort (born after 1975). A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects on the corresponding occupational share. Standard errors are clustered at the *district* level. A minimal change in the standard errors when estimating the effects at the district level, and the point estimates are the same as the results in Figure 4.



(a) Old cohort (b) Young cohort

Figure A.8: Impacts of Bombing on Education: Two Cohorts at the District Level

Note: The figure presents the IV estimates of the impacts of bombing on the shares of different diplomas over two cohorts in 2018 (older cohort born before 1975, younger cohort born after 1975) at the *district* level. A 95% confidence interval accompanies each estimate. Confidence intervals that cross the horizontal dashed line indicate statistically insignificant effects of bombing on the corresponding educational attainment. Standard errors are clustered at the *district* level. Panels (a) shows that the general education of the older cohort was 8.8 percentage points (-0.3×0.292) lower due to bombing, and panel (b) shows no clear evidence of bombing on educational attainment for the younger cohort in 2018.

Table A.9: Impacts of Bombing on Income: Two Cohorts at the District Level

Sample	(1) *All cohorts*	(2) *Old cohort*	(3) *Young cohort*
Dep. Variable	Income	Income	Income
Panel A. OLS estimations			
	-0.148***	-0.398*	-0.299*
	(0.046)	(0.211)	(0.143)
Panel B. IV estimations			
Bombing	-1.650***	-2.629**	-2.505**
	(0.488)	(1.289)	(0.987)
Mean of Dep. Var.	7.885	7.938	7.862
Panel C. Reduced form			
Below	-0.468**	-0.690**	-0.379*
	(0.210)	(0.298)	(0.181)
Panel D. First stage			
Dep. Variable	Bombing	Bombing	Bombing
Below (IV)	0.284***	0.211**	0.180***
	(0.087)	(0.086)	(0.060)
F stat.	10.58	4.54	8.99
AR Test (P-value)	0.00	0.04	0.03
Controls	✓	✓	✓
Clusters	151	119	124
Observations	213	155	161

Note: The table presents the estimates of the impacts of bombing on personal income (in logarithms) in 2018, clustered at the *district* level. The dependent variable is personal income, measured as total household income divided by household size. For older or younger cohort analyses, household income includes only earnings from people in the respective cohort. Column (1) is the estimate for all cohorts, and columns (2) and (3) are the estimates for the sole old and young cohorts. In the first stage (Panel D), the status of a village that is *below* any threshold in terms of the four security thresholds serves as the instrumental variable. The reduced form (Panel C) regresses personal income on the instrument (*below*), including all controls. Control variables include sex, age, educational attainment, household size, urban/rural status, occupation, and other controls from Dell and Querubin (2018). Robust standard errors clustered at the district level in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Potential Mechanisms

Panel A. IV estimations						
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variables	Pri.s.a	Sec.s.a	Hi.s.c	Hi.a.c	Ma.good	Sur.good
Bombing	-0.470***	-0.051	-0.637***	-0.455***	-0.885**	-1.335***
	(0.157)	(0.190)	(0.239)	(0.172)	(0.387)	(0.432)
Panel B. First stage						
Dep. Variable	Bombing	Bombing	Bombing	Bombing	Bombing	Bombing
Below (IV)	0.235***	0.235***	0.235***	0.235***	0.267***	0.274***
	(0.089)	(0.089)	(0.089)	(0.089)	(0.094)	(0.091)
F stat.	10.21	10.21	10.21	10.21	8.03	9.05
AR Test (P-value)	0.00	0.00	0.00	0.00	0.01	0.00
Controls	✓	✓	✓	✓	✓	✓
Observations	213	213	213	213	210	211

Note: The table shows the impacts of bombing on potential mechanisms. Dependent variables that are the possible mechanisms include: (1) Primary school accessibility in the past, (2) Secondary school accessibility in the past, (3) Historical village was in the high-security class, (4) Historical village was in the high administration class, (5) Manufactured goods proxied by pots for sale at the local markets in the past, (6) Surplus goods provided outside village in the past. In the first stage, the status of a village that is *below* any threshold in terms of the four security thresholds serves as the instrumental variable. The regressions in both stages have a set of control variables from Dell and Querubin (2018). Robust standard errors clustered at the village level in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

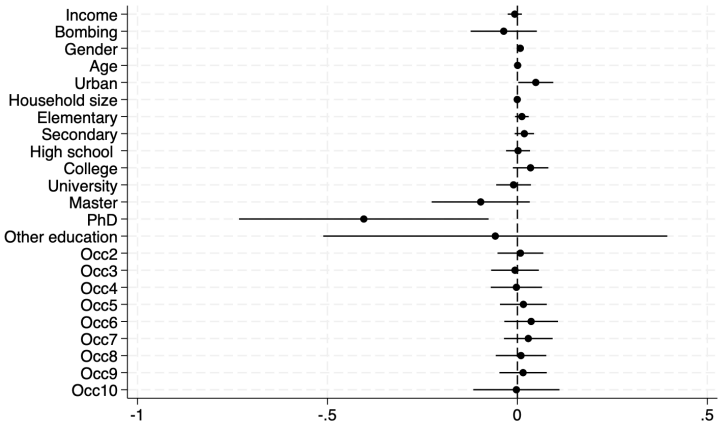


Figure A.9: Balance Check

Note: The figure shows the balance test between the two groups: single-hamlet and multiple-hamlet villages. I compare the similarities of several attributes between the groups. The sample sizes are N1 = 9,210 individuals for single-hamlet villages and N2 = 24,590 individuals for multiple-hamlet villages. Categories Occ2 - Occ10 are categories of occupations listed in Table A.3. A 95% confidence interval accompanies each coefficient estimate. Confidence intervals that cross the vertical dashed line indicate the characteristic is not significantly different between the two groups. The figure shows that there are no systematic differences in terms of attributes between single-hamlet and multiple-hamlet villages.

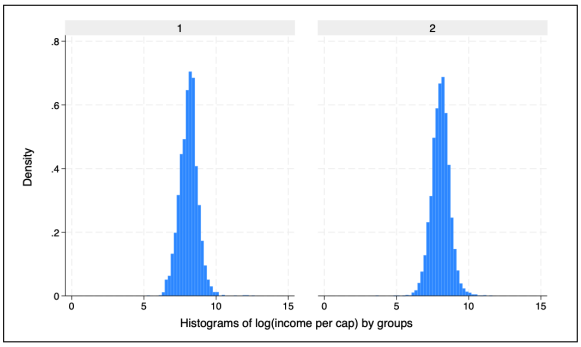


Figure A.10: Histograms of Income by Groups

Note: The figure reports the histograms of personal income of single-hamlet and multiple-hamlet villages. The numbers on the horizontal axis represent personal income in logarithms, calculated by dividing the total household income by household size. The left panel displays the distribution for single-hamlet villages, while the right panel corresponds to multiple-hamlet villages. The sample sizes are N1 = 9,210 individuals for single-hamlet villages and N2 = 24,590 individuals for multiple-hamlet villages.

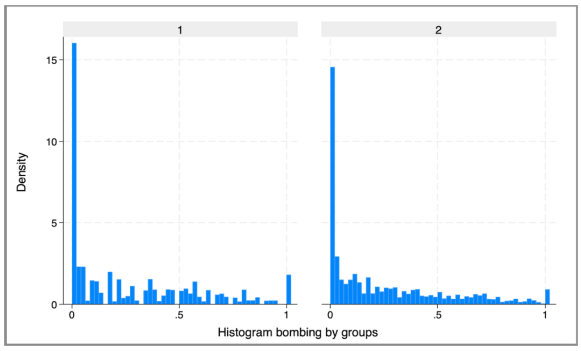


Figure A.11: Histograms of Bombing By groups

Note: The figure reports the histograms of bombing frequencies of single-hamlet and multiple-hamlet villages. The numbers on the horizontal axis represent the bombing frequencies at the village level, ranging from 0 to 1. The left panel displays the distribution for single-hamlet villages, while the right panel corresponds to multiple-hamlet villages. The sample sizes are N1 = 9,210 individuals for single-hamlet villages and N2 = 24,590 individuals for multiple-hamlet villages.

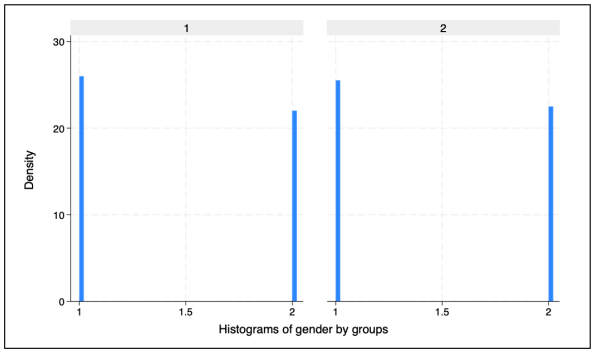


Figure A.12: Histograms of Gender by Groups

Note: The figure reports the histograms of gender distributions of single-hamlet and multiple-hamlet villages. In the horizontal axis, one refers to female and two refers to male. The left panel displays the distribution for single-hamlet villages, while the right panel corresponds to multiple-hamlet villages. The sample sizes are N1 = 9,210 individuals for single-hamlet villages and N2 = 24,590 individuals for multiple-hamlet villages.

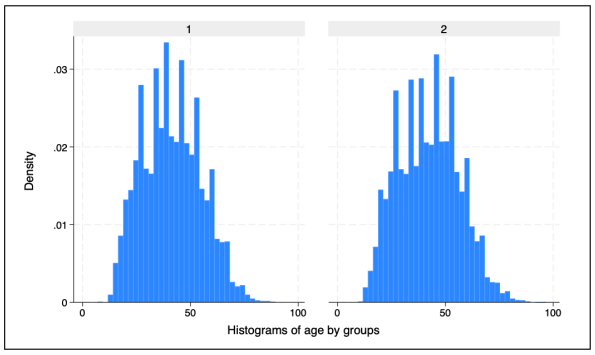


Figure A.13: Histograms of Age by Groups

Note: The figure reports the histograms of age distributions of single-hamlet and multiple-hamlet villages. The numbers on the horizontal axis are the ages of people. The left panel displays the distribution for single-hamlet villages, while the right panel corresponds to multiple-hamlet villages. The sample sizes are N1 = 9,210 individuals for single-hamlet villages and N2 = 24,590 individuals for multiple-hamlet villages.

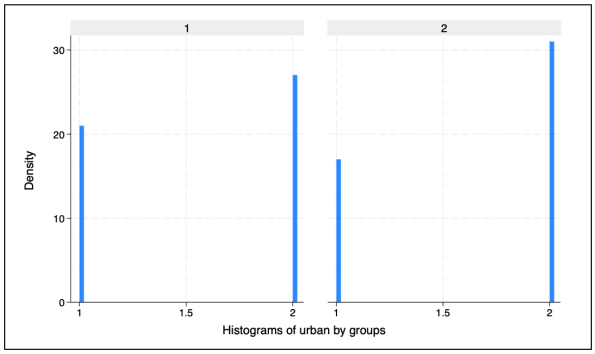


Figure A.14: Histograms of Urban/Rural Area by Groups

Note: The figure reports the histograms of urban/rural distributions of single-hamlet and multiple-hamlet villages. On the horizontal axis, 'urban' is represented by one, and 'rural' is represented by two. The left panel displays the distribution for single-hamlet villages, while the right panel corresponds to multiple-hamlet villages. The sample sizes are N1 = 9,210 individuals for single-hamlet villages and N2 = 24,590 individuals for multiple-hamlet villages.

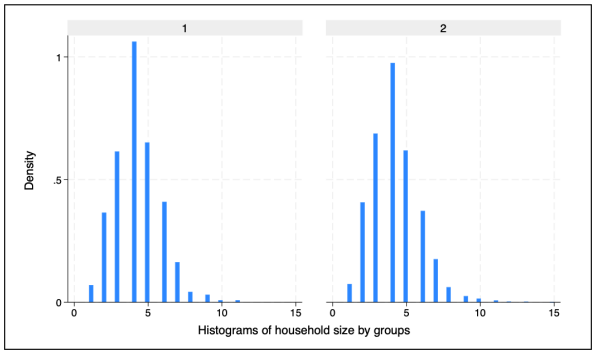


Figure A.15: Histograms of Household Size by Groups

Note: The figure reports the histograms of household size distributions of single-hamlet and multiple-hamlet villages. The numbers on the horizontal axis represent the number of people in the households. The sample sizes are N1 = 9,210 individuals for single-hamlet villages and N2 = 24,590 individuals for multiple-hamlet villages.

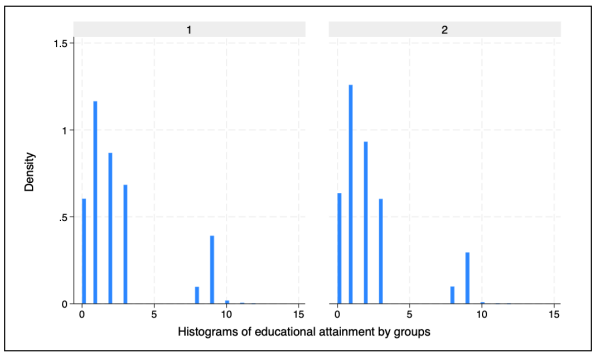


Figure A.16: Histograms of Education Attainment by Groups

Note: The figure presents histograms of educational attainment distributions for single-hamlet and multiple-hamlet villages. The numbers on the horizontal axis represent the highest degree that people obtain: zero is no degree at all, one is an elementary degree, two is a secondary school degree, three is a high school degree, eight is a college degree, nine is a university degree, ten is a master's degree, and eleven is a doctoral degree. The left panel displays the distribution for single-hamlet villages, while the right panel corresponds to multiple-hamlet villages. The sample sizes are N1 = 9,210 individuals for single-hamlet villages and N2 = 24,590 individuals for multiple-hamlet villages.

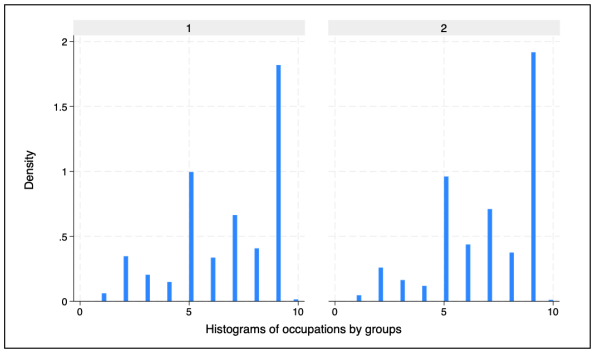


Figure A.17: Histograms of Occupations by Groups

Note: The figure presents histograms of occupational distributions for single-hamlet and multiple-hamlet villages. The numbers on the horizontal axis represent the ten occupations listed in Table A.3. The left panel displays the distribution for single-hamlet villages, while the right panel corresponds to multiple-hamlet villages. The sample sizes are N1 = 9,210 individuals for single-hamlet villages and N2 = 24,590 individuals for multiple-hamlet villages.

Figure A.18: Vietnam Shapefile in 2018

Note: This is the 2018 administrative shapefile of Vietnam at the third level, the granular level below districts. It contains about 11,162 administrative units for the entire country, with polygon geometries, names, and type attributes.

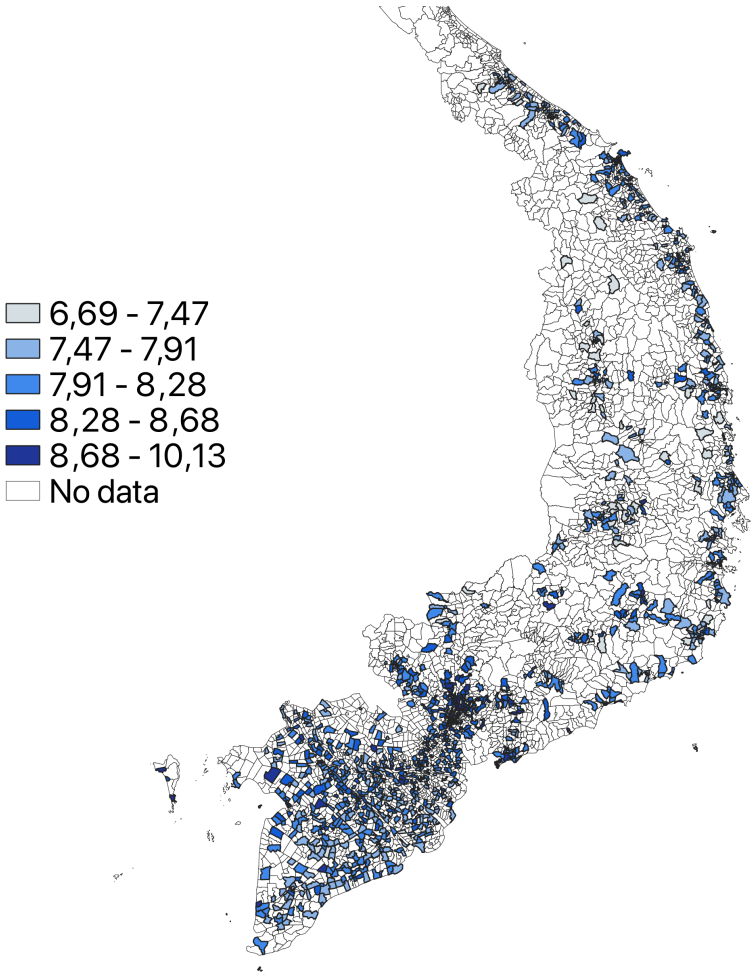


Figure A.19: Personal Income

Note: The figure presents village-level personal income in South Vietnam in 2018. I calculate personal income as the logarithm of the total household income in Vietnamese Dong divided by household size. Darker blue shades indicate higher personal income. I include only single-hamlet villages in this paper. On the map, there is personal income data in the white areas of the multiple-hamlet villages that I do not report here.

Abstrakt

Tento článek zkoumá přetrvávající dopady bombardování během vietnamské války (1955–1975) na strukturu zaměstnanosti a příjmy různých kohort v roce 2018. Za tímto účelem propojuji data o bombardování z amerického Národního archivu s reprezentativním vietnamským šetřením domácností. V analýze využívám přístup instrumentálních proměnných, který vychází z prahových hodnot zaokrouhlování používaných při cílení leteckých útoků na úrovni vesnic. Výsledky ukazují, že bombardování zvyšuje podíl poválečných mladších kohort pracujících v nízkokvalifikovaných povoláních o 10 procentních bodů a zároveň snižuje jejich příjmy v roce 2018 o více než 50 %. Tyto dopady jsou ještě výraznější u starších kohort, které byly válce vystaveny přímo. Odhaduji, že silně bombardované vesnice zaostávají v transformaci profesní struktury přibližně o 1,3 až 1,6 generace. Analýza dále naznačuje, že dostupnost vzdělání a válečné řízení na úrovni vesnic tyto efekty částečně zprostředkovávají. Tento článek přináší první důkazy o tom, že bombardování deformuje profesní a příjmovou strukturu poválečné generace a způsobuje, že bombardované vesnice zaostávají v procesu strukturální transformace.

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