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The Long-Run and Gender-Equalizing Impacts of School Access: Evidence from the First Indochina War

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The Long-Run and Gender-Equalizing Impacts of School Access: Evidence from the First Indochina War

Hai-Anh H. Dang, Trung X. Hoang, and Ha Nguyen*

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Abstract

Very few studies currently exist on the long-term impacts of schooling policies in developing countries. We examine the impacts—half a century later—of a mass education program conducted by the Democratic Republic of Vietnam in the occupied areas during the First Indochina War. Difference-in-difference estimation results suggest that school-age children who were exposed to the program obtained significantly higher levels of education than their peers who were residing in French-occupied areas. The impacts are statistically significant for school-age girls and not for school-age boys. We find beneficial spillover and inter-generational impacts of education: affected girls enjoyed higher household living standards, had more educated spouses, and raised more educated children. We discuss various robustness checks and extensions that support these findings.

Key Words: education achievement, reading literacy, school policy, popular education, difference-in-difference, long-term impact, war

JEL Codes: H0, I2, O1, P3

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I. Introduction

Education has long been known as instrumental in raising human capital value (Becker, 1962). Indicators of educational achievements such as standardized test scores or the number of completed years of schooling are frequently employed to track a country's human development outcomes. Raising educational achievements, however, is a challenging task. Policy makers, particularly in developing countries with resources constraints, are understandably keen on the design of cost-effective school policies that can be implemented at scale.

We study a low-cost and large-scale educational program implemented by the Democratic Republic of Vietnam (DRV) during the First Indochina War from 1946 and 1954, and its long-term impacts on education outcomes and household living standards. We offer an analysis for several welfare outcomes, ranging from math and reading literacy, completed education levels to various measures of household wealth. We also provide an examination of the *spillover* effects of a woman's education on her spouse and her children. Although a large body of literature has shown the social benefits of education at the aggregate level,[1] little is known about its impacts at the individual level. Furthermore, the causal link between education and development outcomes has not yet been documented well, particularly for developing countries. Our identification strategy is to exploit the temporal variation (i.e., whether an individual was at school age during the war) and the spatial variation (i.e., whether an individual was living in the French-occupied regions or the DRV-occupied regions) of an individual's exposure to the education program. These variations together allow us to employ a difference-in-difference strategy to estimate the long-term impacts of this education program.

[1] See, e.g., Castello-Climent, Chaudhary, and Mukhopadhyay (in press) for a recent study that employs more aggregated data (e.g., district-level share of education rather than household-level education outcomes) to study the impacts of education.

Our estimation results suggest that girls that resided in DRV-controlled areas during their school age have significantly stronger math and reading literacy levels and higher education achievements, five decades later, than their peers residing in French-controlled areas. The impacts are not limited to a girl's well-being alone: we find that impacted girls have higher household living standards, more educated spouses, and most interestingly, raise more educated children. These impacts are, however, not statistically significant for boys. We examine a number of alternative mechanisms that can potentially explain these results including unobserved regional trends, different treatment and control cohorts, internal and international migration, confounding impacts of war, nutrition status, and sample attrition. Estimation results, however, remain strongly statistically significant and robust across these specifications.

Few studies currently exist on the long-term impacts of education in general, and school access in particular, in a developing country context.[2] Examining the long-term, rather than the short-term, impacts can offer a more comprehensive view of human welfare outcomes such as education achievement. Indeed, finishing a college degree is a time-consuming undertaking that would require almost 20 years of continuous investment in school for most individuals. Furthermore, an individual's skill formation is considered a life cycle process since skill attainment at one stage of the life cycle can raise skill attainment at later stages of the life cycle, and that early investment can facilitate the productivity of later investment (Cunha et al., 2006; Cunha and Heckman, 2007).

[2] We mostly focus in this paper on the longer-term impacts of school access and education achievement in the context of a developing country. See Glewwe and Muralidharan (2016) for a recent review of several studies that investigate the short-term (i.e., less than 5 years) impacts of school policies for developing countries. In richer countries' context, a related literature exists on the impacts of compulsory school laws on different outcomes such as returns to education (Oreopolous, 2006), health outcomes (Clark and Royer, 2013), fertility behavior (McCrary and Royer, 2011), and domestic violence (Erten and Keskin, 2018). Also, as discussed later, a key difference with our study is the strong and voluntary participation by households in a mass education program supported by the government.

Examining a large-scale school construction program in Indonesia in the mid-to-late 1970s and identifying a child's exposure to the program through his (her) date of birth and the region of birth, Duflo (2001) estimates that the program resulted in an average increase of 0.1 to 0.2 years of education and roughly a 2 percent increase in wages around two decades later. Wantchekon *et al.* (2015) implement an innovative study by collecting longitudinal data on the first students in colonial schools in colonial times and their direct descendants and extended families in Benin. This study finds that Beninese who benefited from access to the first schools established were 96 percent and 10 percent respectively more likely to attain primary education and secondary education or more. These affected individuals and their descendants also had higher living standards, were less likely to be farmers, and were more likely to be politically active; furthermore, an uncle's education also had positive impacts on his nephews and nieces.[3]

Our study makes a fresh contribution to the existing literature in several aspects. First, we can specifically attribute the increased education achievement to the long-term impacts of schooling policies (that occurred in the context of the (First) Indochina War). During the Indochina War, Vietnam's political system was turned into a bimodal regime, in which parts of the country were occupied by the French government while some other parts were under the Vietnamese government. This in turn resulted in a unique natural policy experiment where two different education systems were concurrently in operation: the new Vietnamese system provided free,

[3] There are some other related studies on China that offer somewhat mixed results. Meng and Gregory (2002) study the life-changing Chinese Cultural Revolution, during which most schools in urban China ceased operation over the period 1966-1977 and find the affected cohorts less than half as likely to possess a university degree 20 years later. This study, however, just relies on cohorts' temporal exposure for identification. Analyzing data on twins, Zhang, Liu, and Yung (2007) did not find the Chinese Cultural Revolution to have significantly negative effects on the returns to schooling.

mandatory, and universal school access to the public while the old French system maintained the *status quo* of offering limited school access to a miniscule, elite and mostly male population group.

Second, we offer new and interesting evidence on the impacts of education on girls, who received little—if any—attention in the few existing studies. Girls were traditionally faced with (far) less school access in Vietnam, and still currently are in a number of countries. Indeed, recent estimates by UNESCO (2018) indicate that women account for two-thirds of the 750 million adults who are without basic literacy skills in the world today. Achieving gender equality ranks high as one of the key Sustainable Development Goals (SDG) supported by the United Nations.[4]

Our third contribution is that we not only examine education outcomes of girls who were at school age during the war, but also other positive *spillover* effects on her household's living conditions, on her husband's education, and most interestingly, on her children's education half a century later. In particular, we offer estimates of the long-term and causal impacts of education on these various outcomes, since we are able to instrument for an individual's education achievement using her exposure to the DRV's popular education program. We thus add to the severe dearth of evidence in the literature on the long-term causal and inter-generational impacts of a woman's education on various outcomes in the context of a developing country.

Finally, Vietnam presents a remarkable case study. Despite its modest position as a lower-middle income country, the country has recorded better education performance than what may be suggested from its income level, particularly for women. Indeed, its girls' net secondary enrollment rates caught up with and even overtook those of boys in the past decade, with the former leading the latter by as much as 10 percentage points at the upper secondary level (Dang and Glewwe, 2018). Much spotlight in the media has been given to this country's exemplar performance, but

[4] See https://sustainabledevelopment.un.org/sdgs.

little rigorous evidence has been offered on the driving factors behind this success.[5] Insights into the schooling policies that helped lead to such strong performance—especially for women—would be useful not just for Vietnam to make further progress but can produce relevant lessons for other developing countries in similar contexts as well.[6]

This paper consists of seven sections. We provide in the next section a brief overview of the natural policy experiment where two educational systems concurrently exist under the colonial French and under DRV, including a detailed description of the "mass education" program. We discuss the analytical model, our identification strategy, and the various data sets we analyze in Section III, before presenting estimation results and a number of robustness checks and heterogeneity analysis in Section IV. We then investigate other spillover effects on other family members in Section V, further reflect on other issues in Section VI, and summarize the main findings in Section VII.

II. Country Context and "Mass Education" Program

II.1. Education under the Colonial French

The French colonized Vietnam for more than 50 years, from 1887 until 1945. During this period, education was a privilege that was exclusively extended to a small group of local elites

[5] Vietnam's recent performance on the PISA (Programme for International Student Assessment) is also comparable to those of much richer countries such as the U.S. or the U.K. See also, for example, the *Economist* (2013), the *Huffington Post* (Bellos, 2015), and the *Guardian* (Ravitch, 2015) for recent media coverage of Vietnam's performance on the PISA. See also Glewwe et al. (2017) for a recent attempt to disentangle this education phenomenon.

[6] Furthermore, our study also contributes to an emerging literature that examines the negative effects of war on human capital but mostly focuses on richer countries; see, for example, Shemyakina (2011), Kesternich *et al.* (2014), and Swee (2015). But in contrast with this literature's focus on the *negative* impacts of wars, we offer an investigation into a *positive* shock to education (supply) during wartime. The positive shock is caused by beneficial exposure to the DRV's popular education program during the war. To some extent, a better understanding of positive shocks—rather than negative shocks—is perhaps more relevant to policies that aim at future education expansions. Another major difference is that while this literature (mostly) studies the generally war-related disruptive effects, we offer a specific investigation of the long-term impacts of schooling policies in the context of the Indochina War.

who would serve as civil servants.[7] As such, most Vietnamese were illiterate in this period. When Vietnam won independence in 1945, the illiteracy rate was estimated to range between as large as 80 percent (Le, 1955) and 95 percent (Pham, 1995). Alongside the French official education system, a very small number of private schools were organized by Vietnamese, but these were largely not recognized by the formal education system.

A common feature of both the formal and private systems is that girls were generally excluded from attending school. This occurrence was influenced by Confucian values imbued in the traditional Vietnamese society where formal education (i.e., including reading and writing skills) for girls was considered unimportant. Other skills such as household management were highlighted instead; furthermore, a girl's education was expected to take place mainly, if not wholly, within the family (Huu Ngoc, 1996; Tran, 2012). Some researchers estimate the female illiteracy rate to be almost 100 percent before 1945 (Nguyen, 1996). For the fortunate few who could go to school, there was unsurprisingly large gender inequality in school enrollment in this period. The percentage of female students was less than 15 percent at the primary and lower secondary levels, and approximately 10 percent at the upper secondary level over the period 1932-1944 (GSO, 2004). This stands in sharp contrast to the current "reverse gender gap" in enrollment discussed earlier.

II.2. Education under the New Government of Vietnam

Immediately after the declaration of independence in 1945, the new Government of Vietnam (known as the Democratic Republic of Vietnam or DRV in this period), launched a mass education

[7] One scholar estimated that there were about 150,000 elementary school students, 5,637 middle school students and 553 high school students out of a total population of 20 million people for Vietnam around 1940 (Nguyen, 1970). This elitist educational system appears to simply serve the main goal of bureaucratic recruitment, as seen with those under the preceding feudal systems in Vietnam and elsewhere (see, e.g., Woodside (2006)). It unsurprisingly drew sharp criticism from historians. For example, Le (1955) argued that the "French system of education aims at assimilation, and only is restricted to a minority elite to serve in the French-controlled administration".

movement called "*Bình dân học vụ*" or "Mass Education" (ME).[8] The DRV issued several new regulations to eradicate illiteracy among the whole population, which created a central agency in charge of the ME movement (Decree 17/SL) and required that every village have an ME class in 6 months with at least 30 participants (Decree 19/SL). In particular, it issued Degree 20/SL that stipulates i) mandatory and free attendance in illiteracy-eradicating classes for all (men and women), ii) within one year, everyone older than 8 be literate, or be fined, and iii) all expenses of the classes be borne by the local government.

But because of the state's budget shortages, the movement was mostly supported by voluntary efforts from the general public. Schooling was free to everyone. Teachers did not receive salaries, and each province had to supply its own teachers. ME classes were set up everywhere, for example, in a private home, at a temple, or in other public places (see Figure 1.2, Appendix 1). A classroom could be formed with just a few chairs being placed around a table, and a door or a wooden plank serving as a classroom blackboard. Teachers and students resorted to all types of materials that could be easily found and that could provide makeshift substitutes for stationery. For example, charcoal was used to write on dried banana leaves, or sticks were used on sand instead of pen and paper. Basic literacy lessons were also written up on various places that are visible to the public—such as street walls, boat sides, and even tall trees—to encourage people to learn.[9]

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[8] The DRV is also commonly known as the "Viet Minh", a more general term that can refer to a number of other local revolutionary and anti-colonial groups in this period; see Marr (2013) for a detailed discussion of this interesting chapter in the history of the country.

[9] The spirit of participants in the ME program can perhaps be illustrated by the remarkable observation that "many women brought their babies along so that they could breast feed during classes and not have to go home" (Elliott, 2006). However, while the ME program's success appears to have been mostly driven by voluntary efforts, anecdotal evidence indicates that local governments also employed various other tactics to help eradicate illiteracy among the adult population. These include plans to tax those who are illiterate, or not to assign communal land to them, or to prevent them from signing documents that they could not read aloud. In some instances, illiterate village women were forbidden from entering the marketplace, which forced them to seek out literacy instruction (Marr, 2013).

The highly inexpensive movement quickly spread across the country and produced fast results, especially in North and Central Vietnam. The DRV's Department of Education reported that more than 2.5 million people could read and write near the end of 1946 in North and Central Vietnam (Marr, 1984). Other researchers even provided a much higher figure—around 10 million—on the number of previously uneducated Vietnamese that were now literate and considered this achievement unprecedented in the history of South East Asian countries (Woodside, 1983). The numbers of enrolled students were also estimated to increase, from the pre-war period 1939-1940 to post-war 1954, by around twice at the primary school level, four times at the lower secondary school level, and around nine times at the upper secondary school level (Ministry of Education, undated).

One important result of the ME program is that school-age children residing under the DRV's regime had the opportunity to go to school at barely any cost. This was in contrast with their older, less unfortunate cohorts who were born and grew up under the colonial regime and had virtually no opportunity for schooling. Another important feature is that, as specified by Degree 20/SL, illiteracy eradication programs were not only (far) more accessible but also mandatory for everyone, which would include girls and other traditionally disadvantaged population groups such as ethnic minorities. This greatly differs from the education system under the colonial French, where admission was mostly available to privileged boys.

II.3. Education during the First Indochina War

In 1946, the French army returned to Vietnam, attempting to reoccupy the country and reestablish its institutions. They faced stiff resistance of an increasingly stronger DRV-led local army, who fought vigorously for their country's newly-established independence. The First Indochina War started in 1946 and ended with the Geneva Accord in 1954; but during the war, the country was divided into French-controlled and DRV-controlled areas. While the DRV continued

to promote the ME movement in its areas, the education system reverted to its previous elitist form in French-controlled areas. Put differently, the ME program did not exist in French-controlled areas during the war. This bimodal system lasted until the end of the war in 1954, when the DRV gained control of the North of Vietnam and could implement the ME movement for the entire North Vietnam. As a result, more than 90 percent of the adult population in North Vietnam were reported to be literate by the end of 1958 (Ministry of Education, 1995).[10] The ME movement in South Vietnam during the Indochina War, however, was not as strong as in North Vietnam (which is supported by our empirical estimation shown in a later section).[11] Consequently, the Indochina War offers a unique natural experiment for us to study the impacts of education access under two independent and diametrically different education systems.

We can employ two different identification approaches to evaluate the long-term impacts of the ME program. Our first identification approach relies on whether the (average) school-age child had access to school, which depends on whether their residence area was DRV-controlled or French-controlled. Since school-age children residing longer in DRV-controlled areas might also have had more school access, our second identification approach makes use of the differential duration of exposure to school access that children had. Figure 1 plots the number of years that each province in Vietnam was under the DRV's control up to the end of 1954. The DRV completely controlled three provinces in North Vietnam, which are Thanh Hoa, Nghe An, and Ha Tinh

[10] Since individuals can catch up on their education (i.e., going back to schools as adults), we would expect to capture the net impacts of the ME program only. We return to more discussion in a later section.

[11] See, for example, the "History of Quang Ngai" (Provincial Government of Quang Ngai, 2015), a South Vietnamese province occupied by the DRV during the First Indochina War. In South Vietnam, for the entire Indochina War period, the DRV had complete control over three provinces: Quang Ngai, Binh Dinh, and Phu Yen, while the remaining provinces were under French control. South Vietnam followed a different political regime and a different model of education for the subsequent two decades following the end of the Indochina War (i.e., during 1954-1975). See, for example, Herring (2013) for a narrative on the history of Vietnam, and Elliott (2000) for a captivating biographical story of four generations of a Vietnamese family spanning this period.

provinces, over a period of nine years from the beginning to the end of the war.[12] The DRV also gained control of 12 additional provinces in North Vietnam from around the middle of the war. We return to more discussion in the next section.

III. Analytical Model

III.1. Conceptual Framework

Our study fits well under the standard education production function framework. In particular, we slightly modify the theoretical model provided by Glewwe and Kremmer (2006) to suit our context. Assume a learning production function that takes the following structural relationship

$$A = a(S, Q, C, H) \quad (1)$$

where A is learned skills, S is years of schooling, Q is a vector of school and teacher characteristics (quality), C is a vector of child characteristics (including "innate ability"), H is a vector of household characteristics that includes household investment in their child(ren)'s education. Q, C, and H are considered exogenous under this framework, while S is an endogenous (choice) variable.

An important set of variables that can be added to Equation (1) is the prices (costs) of schooling, denoted by the vector P. These prices include various school expenses including school fees, school supplies (and perhaps even the opportunity cost of sending a child to school, or the wages paid for child labor). In our context, P and Q are respectively the inexpensive costs of schooling and the provision of schooling (including teachers, classrooms, and so on) for a child that was created by the DRV's ME program during the Indochina War. More formally, P and Q can take the following form

$$P = b(ME, L) \quad (2)$$

[12] These three provinces are observed to be a traditional strong base for the DRV government and their geographical conditions are favorable for self-defense (Ngo, 2001).

$$Q = c(ME, L) \quad (3)$$

Thus, the DRV's education policies (*ME*) can vary by (or interact with) local community characteristics (DRV-controlled provinces, denoted by *L*), which determine the costs of schooling as well as the provision of schooling.

Note that *P* does not appear in Equation (1) because it has no direct effect on learning, instead it affects household decisions on the number of years they can send their children to school (i.e., the endogenous years of schooling *S*), which is a function of *Q*, *C*, *H* as well

$$S = f(Q, C, H, P) \quad (4)$$

Plugging Equations (2) and (3) into Equation (4), and the results to Equation (1), we have the reduced form relationships

$$S = g(ME, L, C, H) \quad (5)$$

$$A = h(ME, L, C, H) \quad (6)$$

A couple of observations are in order. First, under this conceptual framework, Equations (5) and (6) suggest a direct and estimable linkage between education policies and children's education outcomes. Second, different from the production function in Equation (1), the impacts of education policies in these equations are net impacts, and thus offer estimates that are most practically relevant to policy makers. These two equations together provide the theoretical base for our empirical models in the next section.

III.2. Empirical Model and Identification Strategies

As earlier discussed, the impacts of the DRV's ME program differ by whether a child resided in a DRV-controlled province (or the local community characteristics *L*). Consequently, we can apply a difference-in-difference (DD) strategy to estimate this model

$$O_{ijt} = \alpha + \beta\left(ME_t * L_j\right) + \gamma X_{ijt} + bp_j + \tau_t + \varepsilon_{ijt} \quad (7)$$

where the dependent variable O_{ijt} represents both *A* and *S* for brevity. O_{ijt} denotes a number of education outcomes such as reading and math literacy and years of schooling, for an individual *i* born in province *j* in year *t*. Reading literacy is represented by a dummy variable which equals 1 if an individual can read without any difficulty, and 0 otherwise. Math literacy is similarly represented by a dummy variable with the value of 1 for an individual who can do a calculation without difficulty, and 0 otherwise. Since students usually had to take an examination to complete a degree at the end of each school level (before 2000) (see, for example, Pham (1995)), achieving a school degree could be an important landmark for an individual's education.[13] As such, we also include in the outcome variable O_{ijt} dummy variables that indicate whether the individual accomplished a primary school degree or a lower secondary school degree (which requires finishing five and nine years of schooling respectively).

We will focus our attention on the treatment coefficient β. Our hypothesis is that the ME program helps increase education attainment for individuals who resided in DRV-controlled areas during their school ages than their peers in French-controlled areas. As earlier discussed, we offer two measures for the treatment variable (interaction term) $ME_t * L_j$. Our first, and preferred, measure assigns ME_t as a dummy variable that equals 1 if individual i was born between 1940 and 1945 (treatment group) and equals 0 if individual i was were born either before the war in 1924-1935 (first control group) or after the war in 1950-1961 (second control group). Individuals in the treatment group would be of prime school age (i.e., between 6 and 14 years old) during the

[13] In addition, our calculation using the 1997-98 VLSS shows that the average years of schooling for a Vietnamese adult (age 15 and above) hovered around 6.4. This number, however, has increased in recent years (Dang and Glewwe, in press). We focus mostly on the quantity of education and long-term life outcomes such as household living standards in this paper, since like most household consumption surveys, the V(H)LSSs do not offer test data on the quality of education. But note that estimation results in Dang and Glewwe (in press) point to stronger-than-average performance on international standardized test scores for Vietnamese students compared with other countries at a similar income level.

war. While individuals in the first control group would have attended schools that were still under the French system, those in the second control group would have attended schools that completely operated under the DRV government after the war in 1954. We combine both control groups to increase the sample size for our analysis, but also provide estimation results that compare the treatment group against each of these control groups. We also provide robustness checks when we further disaggregate these control groups. In this approach, we leave out of the estimation sample the cohorts that were born during 1936-1939 or 1946-1949, who were partially exposed to increased school access (i.e., the "contaminated" treatment cohorts) if they were living in DRV-controlled areas. But we also provide robustness checks when we include these cohorts; we return to more discussion in the robustness checks section.

For the second measure, we explicitly take into account the differential length of exposure to the ME program that school-age children residing in DRV-controlled areas might have benefited from increased school access, which can vary depending on their age. Table 1.1 in Appendix 1 provides an illustration of this exposure, where a child born in 1935 or later in DRV-controlled areas or 1944 or later in French-controlled areas would have had five years of exposure. The number of years of exposure is similarly adjusted for children born in the provinces in North Vietnam that DRV gained control from around the middle of the war. The reason we restrict the number of years of exposure to five is that, children would likely drop out of school if they miss the crucial years of schooling at the primary school level.[14] Our second measure thus includes all the "contaminated" treatment cohorts in the estimation sample. This would likely weaken the impacts of the ME program, although the decrease may be somewhat mitigated due to larger

[14] Various education studies suggest that late school enrollment is observed to lead to more school dropouts in a number of countries around the world (Wils, 2004; UIS, 2005; Chen, 2015; No et al., 2016).

estimation sample sizes. Consequently, the second measure may represent more conservative (i.e., downward biased) estimates of the impacts of the ME program than the first measure.[15]

X_{ijt} is a set of other control variables that include dummy variables indicating gender, ethnicity, and religion (i.e., whether the individual is Buddhist, Christian or has other religion beliefs). To reduce any selection bias due to migration, we incorporate in Equation (7) the province of birth fixed-effects (bp_j). Furthermore, we exclude from our estimation sample the individuals who had migrated when they were younger than 15 years old (but we will also produce robustness checks when these individuals are included in the estimation sample). We also control for potential secular trends in Equation (7) with the birth year fixed effects (τ_t).

To further evaluate whether the potentially increased education outcomes for school-age girls residing in DRV-controlled areas during the war have spillover impacts, we estimate the following model

$$Y_{ijk} = \theta + \delta S_{ij1} + \eta X_{ijk} + rp_j + \pi_t + \mu_{ijk} \qquad (8)$$

where the subscript *k* indicates girl *i* (*k*= 1), her spouse (*k*= 2) and child(ren) (i.e., other values for *k*). S_{ij1} thus represents a girl's years of schooling. The dependent variable Y_{ijk} represents the outcomes of interest for individual *i*, such as her living standards, and her spouse's and children's education outcomes. Other control variables are similar to those in Equation (7), except that we now control for current province of residence fixed-effects (rp_j) since the outcome variables concern a woman's current residence and her household members.[16]

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[15] We also tried a third and hybrid measure, which assigns to all school-age children residing in province *j* the number of years this province was under the DRV's occupation during the war. This hybrid model would likely provide statistically weaker results than the first measure, but stronger results than the second measure. We return to discussing estimation results in the next section.

[16] As discussed earlier, we only have crude measures of skills (i.e., reading and math literacy), so we focus on individuals' years of schooling for Equation (8). Also, since unobserved factors in the current province of residence may be possibly correlated with a woman's living standards (say, through her decision to migrate to a certain

Since individuals' education outcomes are typically positively correlated with—or endogenous to—those of their family members,[17] non-instrumented regressions for Equation (8) would provide biased estimation results. In our setting, the bias would likely be downward toward zero compared to IV estimates, since it has long been observed that institutional features like compulsory schooling or the accessibility of schools more likely affects the schooling choices of individuals who would otherwise have relatively low schooling (Card, 2001). Put differently, the low costs of the ME program could have encouraged individuals—who would have dropped out because of the school cost barrier (rather than, say, expected lower returns to education)—to attend school. The IV estimates can better identify such individuals and thus are likely larger than the naive OLS estimates.[18]

As such, we estimate Equation (8) with the IV method where the selection equation for individual *i*'s years of schools (S_{ij1}) is estimated as in Equation (7). Specifically, we use the different exposure to school access during the war (i.e., the interaction term $ME_t * L_j$) to instrument for individual *i*'s education outcome in Equation (8). We also provide OLS estimation results for this equation for comparison purposes.

Following Cameron, Gelbach, and Miller's (2011) suggestion, we provide robust standard errors for all estimation results with two-way clustering. The first level of clustering is at the birth province level (i.e., to account for correlations in outcomes for individuals born in the same birth

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destination), we also ran robustness checks on women's outcomes using the province of birth FE in Equation (8). The estimation results (not shown) are qualitatively similar, albeit slightly statistically weaker. Note, however, that this concern is not relevant to her spouse and children.

[17] Abundant evidence exists for assortative mating where more educated men tend to marry more educated women (see, e.g., Becker (1973) and Fernandez and Rogerson (2001), which in turn affects inter-generational outcomes (see, e.g., Ermisch, Francesconi, and Siedler (2006) and Guell, Mora, and Telmer (2015)). See also Black and Devereux (2011) for a recent review of studies related to inter-generational mobility.

[18] An alternative interpretation is offered by Heckman *et al.* (2006), who suggest that IV estimates of the returns to schooling are often larger than those of OLS estimates because of heterogeneous returns to an activity and people sort into different activities based on those returns.

province), and the second level at the commune of current residence (i.e., to account for the primary sampling unit effects). There are around 150 communes but 30 birth provinces; thus, to be cautious, we also apply finite sample adjustment to address potential concerns with a small number of clusters.[19]

As earlier discussed, the popular education program was implemented in provinces under DRV control in North Vietnam, and it was not implemented in DRV-control provinces in South Vietnam. Therefore, we restrict our analysis to individuals that were born in North Vietnam, but we also report estimation results for those born in South Vietnam as robustness checks.

III.3. Data

The main data set that we analyze is the 1997-1998 round of the Vietnam Living Standards Survey (VLSS), which was implemented by Vietnam's General Statistical Office (GSO) with technical assistance from the World Bank. The 1997-98 VLSS is nationally representative and collects data on about 6,000 households across the country. The survey offers a rich set of variables such as household consumption and assets, as well as information on each household member's demographics, education, health, labor market outcomes, and anthropometric measures. The survey also collects data on children of household heads, regardless of whether or not these children were living in the household at the time of the survey. This survey has good data quality and has been widely analyzed by the Government of Vietnam, international organizations, and academic researchers. A particularly useful feature for our analysis is that this survey offers information on respondents' date of birth, place of birth, whether they have moved from their birth place or not, and the age at migration (if they have moved from their birth place).

[19] For easier interpretation, we estimate Equations (7) and (8) using the linear regression model, using the "*ivreg2*" command in Stata (Baum, Schaffer, and Stillman, 2010).

We also supplement our analysis with two other surveys that have information on an individual's birth place: i) the 1992-93 VLSS, which was the first household consumption survey that was implemented for the country, and ii) the 2014 Vietnam Household Living Standards Survey (VHLSS). There are, however, limitations with these surveys. The 1992-93 VLSS has a smaller sample size and collects data on around 4,800 households; most of these households were resurveyed in the 1997-98 VLSS. In addition, we discuss later some potential issues with data quality of certain variables in this survey. The 2014 VHLSS, on the other hand, has a larger sample size but was implemented further away from the Indochina War period; thus, sample attrition issues with this survey pose more severe challenges (e.g., a child age 6 in 1945 was 75 years old in 2014). Still, it may be useful to provide some limited robustness checks using these surveys. Besides the V(H)LSS data, we also analyze UNESCO's WIDE database (UNESCO, 2017) and the World Bank's World Development Indicators database (World Bank, 2017).

But importantly, we generate for the first time several new variables related to the First Indochina war that help construct our treatment measures. In particular, since each province in Vietnam produced a book on the history of its local communist party and its army, we manually glean data from these books to construct these variables. The most interesting variable is the years of the DRV's occupation at the province level, which allows us to construct different measures of exposure to the ME program as earlier discussed: whether a school-age child was exposed to it, and the durations of exposure (both at the individual and province levels). Furthermore, we are also able to construct a variable that indicates the number of battles that were fought in each province, which will be employed for a robustness check on the potential confounding effects of

the war.[20] We use more than 30 books that have been published over the past two decades with various publishers; these books are listed in detail in Appendix 2.

IV. Long-run Impacts of School Policies

IV.1. Estimation Results

A key assumption underlying the DD model is the parallel trend assumption, whereby the difference in the outcome variable between the treatment group and the control group remains essentially the same over time (in the absence of the treatment). Without this assumption, the change for the treatment group may not be attributed to the treatment, but it can result from other time-varying unobservable factors. We check this assumption in Table 1, which shows the balance tests for the differences across different cohorts from the pre-treatment period 1924-35 to the post-treatment period 1950-61 between DRV-occupied provinces and French-occupied provinces (in North Vietnam).

The average years of education in French-occupied provinces were 1.1 and 0.6 smaller than those in DRV-occupied provinces respectively for the cohorts of 1924-35 and those of 1950-61, and are highly statistically significant. However, these two numbers are not statistically significantly different from each other. This suggests that the gap in the years of education between French-controlled provinces and DRV-controlled provinces before and after the treatment years remains similar. Table 1 also reports the balance tests for a number of other variables, including the percentage of the population who have reading and math literacy, who have completed at least

[20] As an example, the history book for Bac Kan Province records that the French attacked and occupied Phu Thong town of Bach Thong District (in Bac Kan Province) on October 15, 1947. A battle took place in Phu Thong town in March 1948, and the French were defeated in July 1948. The French attacked and occupied Cho Don District on October 7-8, 1947; there was also a battle between the French and the Vietnamese in late October 1947. In 1949, Bac Kan province was completely liberalized from French rule. We list all the battles occurring in each district within each province, and then count the total number of battles at the province level for each province in northern Vietnam. The number of battles per province ranges from 4 to 137 and averages 37.9.

primary education or secondary education, whether the household head is female, Buddhist, or whether the household head belongs to the ethnic majority group. The results are qualitatively similar.

We further check the parallel trend assumption separately for men and women. Figure 2 plots the completed years of education by birth cohort for those who were born in DRV-occupied provinces versus those who were born in French-occupied provinces. This figure shows an (approximately) parallel trend in the number of years of education for the female control cohorts, including from the cohorts of 1924-29 to those of 1930-35, and from the cohorts of 1950-55 to those of 1956-61. This holds regardless of whether they were born in DRV-occupied provinces or French-occupied provinces. The treatment years, in contrast, saw a considerable increase of two more years of education for the female cohorts of 1940-1945 born in DRV-occupied provinces compared with their peers in French-occupied provinces. This increase also holds to some extent for the contaminated treated female cohorts of 1946-1949. For all the male birth cohorts, the difference between those born in DRV-occupied provinces and born in French-occupied provinces appears negligible. We return to discuss additional tests for the parallel assumption and a number of related robustness checks in Section IV.2.

We provide in Table 2 the estimation results, based on Equation (7), for the whole population (Panel A), girls (Panel B), and boys (Panel C). Using the first measure of exposure to schooling policy during the war (i.e., by interacting a dummy variable for DRV-controlled areas with the treatment period), we find that the treatment effects are positive and statistically significant for different indicators of educational attainment. Specifically, school-age children residing in DRV-controlled areas were between 10 to 13 percentage points higher in the probability of achieving reading and math literacy and completing at least primary education or secondary education (Panel

A, Columns 1 to 4). These children also accomplished one more year of schooling (Panel A, Column 5), which is a sizable increase of 28 percent from the average 3.6 years of schooling in the pre-treatment period.

The results are qualitatively similar, although somewhat statistically weaker, for the second measure of exposure to schooling policy during the war (Panel A, Columns 6-10). In particular, the probability of accomplishing math competency becomes marginally statistically significant at the 10 percent level, and the probability of completing secondary education or higher becomes statistically insignificant. This is consistent with our earlier discussion that the second measure would provide more conservative estimates of the impacts of the ME program than the first measure.

We further disaggregate the gains in educational attainment between boys and girls. School-age girls living in DRV-controlled areas during the war attained an additional 1.5 years of schooling compared to their peers in French-controlled areas (Panel B, Column 5); similarly, one more year of the DRV's occupation raised an additional 0.3 years of schooling for school-age girls (Panel B, Column 10). However, estimation results for school-age boys (Panel C) are not statistically significant for any measure of educational attainment, which suggests that these boys did not gain from the ME policy in DRV-controlled areas. We will thus focus more on the girls' sample in the rest of the paper and will come back to more discussion on the results for boys in a later section.

IV.2. Robustness Checks

We offer in this section a battery of robustness checks, which examine potential channels that may affect our estimation results.

Implementation of ME Program in North Vietnam vs. South Vietnam

As earlier discussed, although three provinces in South Vietnam were completely occupied by DRV during the war, the ME program was hardly implemented in those provinces. This offers us

an important falsification test for the impacts of the ME program, where our hypothesis is that a weak or no implementation of the ME program would result in no (long-term) effect on school-age children residing in these provinces. Indeed, Table 3 shows that the ME program does not have statistically significant impacts on the educational attainment of school-age children living in DRV-controlled areas in South Vietnam, except for the single case of completing primary education for boys. This result remains the same for both the girl sample and the boy sample. The finding generally supports our hypothesis that it is the ME program—rather than other DRV-related factors (e.g., specific political regime or idealism) or unobserved regional trends—that resulted in better long-term educational outcomes for those who grew up in DRV-controlled regions.[21]

Different Birth Cohorts

Both Figure 2 and our estimation results in Table 2 point to the beneficial and statistically significant long-term impacts of the ME program on girls who were of school age during the war. We also control for birth year fixed effects when producing these results. But to further check whether these results may be driven by other unobserved birth-cohorts-specific factors beyond the birth year fixed effects, we offer three additional sets of robustness checks.

First, we conduct a falsification test by restricting our estimation sample to girls who were born between 1955 and 1966 (i.e., after the First Indochina War) and enjoyed the same access to school under the DRV system of education. We then arbitrarily assign the 1955-1960 cohorts and the

[21] Estimation results using a triple differences strategy that interacts the treatment variable with a dummy variable indicating the South of Vietnam (i.e., adding the South of Vietnam in the estimation sample for Table 2) provide similar results (not shown). We also offer an additional robustness check that considers whether school-age children residing in provinces that were geographically adjacent to the DRV-controlled provinces have better long-term education outcomes. If they do, this would suggest that the beneficial impacts of the ME program may have been caused by some other (unobserved) factors that were not related to the ME program. Estimation results, provided in Table 1.2, Panel A (Appendix 1), indicate otherwise, thus lending further support to our results. We also provide another related check that restricts the estimation sample instead to the three provinces that were under the DRV's control throughout the war. Estimation results (Appendix 1, Table 1.2, Panel B) are qualitatively similar.

1961-1966 cohorts to the control group and the treatment group respectively. Our hypothesis is that we should not see any statistically significantly different results between these two groups. Estimation results provided in Table 4, Panel A indeed support this hypothesis where all five measures of educational attainment—for both measures of exposure to the ME program—show statistically insignificant impacts. Another falsification test where we assign the cohorts of 1936-1939 to the treatment group, keeping the control group the same as in Table 2 (i.e., cohorts 1924-1935 and 1950-1961), provides qualitatively similar results (Panel A in Table 1.6, Appendix 1).[22]

Second, instead of grouping together as the control group for all those who were born during the pre-treatment period 1924-1935 and the post-treatment period 1950-1961, we break this control group into four different and smaller control groups that are composed of those who were born in the periods 1924-1929, 1930-1935, 1950-1955, and 1956-1961. Although the estimation sample sizes were strongly reduced, estimates remain strongly statistically significant for this indicator of education achievement, and other indicators as well (Table 1.3, Appendix 1).[23] Furthermore, we keep fixed the treatment group, and vary the control group in various ways, by either restricting it to pre-war or post-war periods or different combinations of smaller birth cohorts. Figure 3 plots the estimation results and their 95 percent confidence intervals for the years of school variable and shows that estimation results are still qualitatively similar.

Finally, following Duflo (2001) we estimate a more general version of Equation (7)

$$O_{ijt} = \alpha + \beta_h\left(\sum_{h=1}^{H} C_h * L_j\right) + \gamma X_{ijt} + bp_j + \tau_t + \varepsilon_{ijt} \qquad (9)$$

[22] Another falsification test (not shown), where we assign the cohorts of 1949-1954 and the cohorts of 1955-1960 to the control group and the treatment group respectively, provides qualitatively similar results. Some of the coefficients are statistically significant however they provide the wrong and negative signs. Those tests suggest that our findings are strong and the difference-in-difference method works well.

[23] We provide estimates for female cohorts only in this table, since estimates are not statistically significant for all male cohorts.

where the term $\sum_{h=1}^{H} C_h * L_j$ represents all the interaction terms between birth cohorts and the dummy variable for DRV-controlled provinces, with the reference group being those born between 1924 and 1928. Equation (9) thus generally compares the treatment group with all the other birth cohorts, including the contaminated treatment cohorts. We expect the treatment cohort of 1940-1945 to benefit the most from the ME program. Indeed, estimation results, provided in Table 1.4 in Appendix 1 for both boys and girls (and for girls only, Table 1.5), further support this result.

In addition, Tables 1.5 and 1.6 also suggests that the contaminated treatment group—those born during 1946-1950—also benefit from the ME program, although to a lesser extent as earlier discussed (e.g., the estimated treatment effect on reading literacy for this group is marginally statistically significant at the 10 percent level, and the treatment effects are slightly weaker). We further experiment with using the contaminated treatment group as the treatment group (Panels B and D, Table 1.6) as well as expanding the treatment group to include some contaminated treatment years (Panel C, Table 1.6) for the girls' sample. Estimation results are, unsurprisingly statistically weaker, but qualitatively similar.[24]

Internal Migration and Parental Education

Since school-age girls had more school access in DRV-controlled provinces than in French-controlled provinces, migration from the former to the latter, or vice versa, would likely reduce the impacts of the ME program. In other words, including in our estimation sample these migrants could dilute the treatment effect. Indeed, when we include in the estimation sample girls who

[24] In addition to the various robustness checks for different control (and treatment) groups discussed above, we offer an additional robustness check that combines the matching technique with the difference-in-difference model (MDID) (Blundell and Dias, 2009). The MDID model employs a weaker parallel trend assumption that is conditional on observed characteristics instead of all (observed and unobserved) characteristics. Estimation results provided in Table 1.7, Appendix 1 are qualitatively similar.

migrated when younger than 15 years old, the estimated treatment effect $\hat{\beta}$ becomes smaller across all indicators of education achievement (Table 4, Panel B). For example, compared to Table 2, school-age girls living in DRV-controlled area were 3 percentage points less likely to achieve reading literacy (Column 1), and obtained 0.3 fewer years of schooling (Column 5). Furthermore, $\hat{\beta}$ also becomes statistically weaker, particularly for the model specifications using our second measure. This result provides further supportive evidence for the long-term beneficial impacts of schooling policies in the DRV-occupied provinces.

Parental education plays an instrumental role in their children's education. However, we have no information on parental education for almost half (40 percent) of the individuals in our sample. But as a robustness check, we rerun our estimates on those that have parental education and provide estimation results in Table 4, Panel C. Estimates become somewhat stronger and more statistically significant. For example, compared to Table 2, girls are 5 percentage points more likely to achieve reading literacy and to attain 0.5 more years of education (Columns 1 to 5). Furthermore, the treatment effect also becomes more strongly statistically significant for the second measure of exposure to schooling policies (Columns 6 to 10). These results suggest that our estimates of the impacts of the ME program in Table 2 are conservative.

Potential Confounding Impacts of War

Could children residing in areas that were exposed to more fighting during the war have been more affected? To investigate this question, we add to Equation (7) as an additional control variable

the number of battles fought in each province during the Indochina war. Estimation results, shown in Table 1.8 (Appendix 1), are qualitatively similar.[25]

Other Concerns: Nutrition, International Migration, Sample Attrition, and Hybrid Modeling Approach

Our robustness checks up to this point have examined a number of issues that reinforce our results, including the implementation of the ME program in North Vietnam versus that in South Vietnam, falsification tests for different birth cohorts, and migration issues. Still, one concern remains that there might have been other factors related to DRV-controlled provinces that are not directly connected to the ME movement that can bias our estimates.

For example, DRV-controlled areas could be wealthier, which could have had upward bias on education (supply and) achievement. To investigate this possibility, we examine other outcomes that may not be directly connected to the ME movements, but could be connected to the overall economic development of a region. One good indicator of wealth is adult height, since better nutrition during early life leads to better adult height (Steckel, 1995; Case and Paxson, 2008). This in turns can leads to better education outcomes; for instance, Neelsen and Stratmann (2011) find that the 1941-42 Greek famine had adverse effects on literacy and schooling for the cohorts who were exposed to the famine before the age of three. Consequently, if height is not statistically significantly different between individuals in the treatment group and the control group, the concern about other contaminated factors might be mitigated. We re-run regressions of Equation (7) with height as the dependent variable and provided estimation results in Table 1.9 (Appendix 1). The treatment effect is marginally statistically significant at the 10 percent level for our first

[25] Examining the impacts of intensive US bombing on Vietnam in the subsequent Vietnam war, Miguel and Roland (2011) found no impacts on either the province or district literacy rates. They observed that this can be due to the flexible adaptation by teachers and students that includes dispersal into small groups to avoid strikes, and school provision of foxholes and helmets for student protection during U.S. attacks.

measure of exposure, but statistically significant for our second measure of exposure. However, the estimated impact is negative, suggesting that children residing in DRV-controlled provinces had lower nutritional status. This indicates that our estimates are likely conservative.[26]

Another concern is that school-age children residing in DRV-controlled areas may have migrated out of the country in the intervening 50 years or so between the Indochina War and the VLSS implementation in 1997-1998. In this case, our estimation sample can provide biased estimates.[27] To check on this hypothesis, we provide estimates on whether individuals are more likely to receive remittances from their relatives living overseas more than 50 years later (i.e., in 1997-1998). Estimation results provided in Table 1.6, Columns 2 and 4 suggest that this is not the case.

Another more technical concern is that, the 1997-98 VLSS may not capture well those that should be in our estimation sample. For example, people who were born in 1924-1935 could be between more than 60 and 70 years old in 1997-1998, and some of them may have died and were thus not surveyed. To address this concern, we provide estimation results for the same female individuals that were surveyed in the 1992-1993 VLSS. However, since this survey collected data on fewer households, one trade-off with doing so is that the sample size is smaller, which can result in weaker estimates. Still, estimation results (Table 1.10, Panel A) are highly statistically significant for several indicators such as completing at least primary education and years of

[26] Notably, the treatment effect for height is not stably statistically significant for all cohorts; for example, it is statistically insignificant when we compare the treatment group with the first control group. Furthermore, since French-controlled provinces were generally better off economically than Vietminh-controlled provinces (Marr, 1984), any such difference in wealth between Vietminh-controlled provinces and French-controlled provinces would generally render our estimation results conservative. But any such difference would need to equally hold for North Vietnam and South Vietnam and/ or different birth cohorts (i.e., have effects beyond our preceding robustness checks) in order to bias our results downward toward zero. Given the fast-changing historical circumstances in Vietnam from the start of the Indochina war, this assumption would be unlikely to hold.

[27] If migrants were more educated individuals, our estimates would be conservative (i.e., biased downward).

schooling for the first measure of exposure to school policies. The years of schooling variable is also statistically significant for the second measure of exposure (Table 1.10, Column 10), but estimates are not significant for the other indicators.[28]

We also examine the 2014 VHLSS, which is the most recent survey round that collects data on individuals' birth place. Clearly, given the (much) longer time interval, sample attrition issues (for example, because of death) are more severe with this survey. Nevertheless, we experiment with estimating Equation (1) for those who were born in 1946-1949 as the (contaminated) treatment group, and those who were born in 1950-1961 as the control groups. While we expect any estimated treatment impact to be severely downward biased, the treatment impact (Table 1.10, Panel B, Appendix 1,) is still positive and statistically significant at the 5 percent level for completing at least primary education for both the first and second measures of exposure to school access. The other education outcome variables are statistically insignificant but have positive coefficients.

Finally, we provide in Table 1.11 in Appendix 1 estimates using the hybrid approach where an individual is assigned the province-level, instead of the individual-level, number of years of exposure to the ME program (which is determined according to the province-level years of the

[28] A number of individuals report different years of birth and years of education in the panel data of the 1992-1993 and 1997-1998 VLSSs, which raises concerns about measurement errors. To reduce the measurement errors, we restrict our estimation sample of the 1992-1993 VLSS to the panel individuals whose differences in their reported birth years and years of education between the two rounds are less than 2 years. To further investigate these measurement issues, we divided the panel sample in 1992-1993 into two groups: group 1 consists of individuals who reported the same birth years in both survey rounds, and group 2 the remaining individuals. We subsequently check the age and education profiles of these two groups. We find that group 1 is significantly younger and more educated than group 2 (e.g., achieving an average of 7.2 years of school versus 5.7 years of school for group 2). This supports our hypothesis that individuals in group 2 might have provided incorrect birth years. This result is consistent with previous studies on survey recall bias. For example, recall bias was observed to decrease for more educated survey respondents in various countries including India (Das, Hammer, and Sánchez-Paramo, 2012), Malaysia (Beckett et al., 2001), Sweden (Kjellsson, Clarke, and Gerdtham, 2014), and the US (Kennickell and Starr-McCluer, 1997).

DRV occupation). As earlier discussed, estimation results are qualitatively similar to those in Table 2, but statistically, are weaker than the first measure and stronger than the second measure.

IV.3. Heterogeneity Analysis

Besides the gender dimension, we further examine the heterogeneous effects of the DRV's ME program on different ethnic and religious groups. Previous studies have pointed to ethnic gaps in living standards in the country;[29] consequently, it would be useful to understand whether different population groups responded differently to the ME program (as a policy intervention). Panels A and B of Table 5 present the impact on educational outcomes for the ethnic majority groups Kinh-Hoa (Panel A) and for the remaining ethnic minority groups (Panel B). Panels C to E of Table 5 present the impact on education outcomes for Buddhist, Catholic, and the remaining non-religious groups.

The Kinh-Hoa ethnic groups form the majority of the population, and consistent with the estimation results for the whole population (Table 2), they clearly benefit from the DRV's program. Individuals who were exposed to the program are more likely to achieve reading and math literacy, complete primary and secondary education, and have more years of education; these results hold for both measures of exposure. The remaining ethnic groups, however, do not seem to consistently benefit from the program. The only education outcome with a statistically significant treatment effect is reading literacy (Panel B, Column 1), the magnitude of which is more than twice that of the Kinh-Hoa groups. While the small sample size could be a reason for the lack of statistical

[29] Several studies find that the ethnic differentials in the returns to endowments (including differentials in quality of schooling, individual ability, or labor market discrimination) can account for a considerable share of the consumption and earning differentials between ethnic groups in Vietnam (van de Walle and Gunewardena (2001), Baulch et al. (2007), and Dang (2012)). Recent evidence also points to the role of language barriers in explaining the ethnic gaps (Nguyen et al., 2017).

significance, it is also likely that ethnic minority groups were faced with more obstacles in further developing their initial educational boost.

The Buddhist group, the Catholic group, and the non-religious groups all seem to benefit from the education policy, which suggests that there might have been no discrimination in the DRV's ME program. The strongest treatment effect manifests itself most clearly in math and reading literacy. Note, however, that the vast majority of the respondents are not religious, and the Buddhist group and the Catholic group constitute only a small fraction of the population.

V. Spill-over Impacts on Related Outcomes

We investigate long-term impacts of women's education achievement on their household living standards, and their spouses' and their children's education and health outcomes. These females were school-age children in the Indochina War. We estimate Equation (8) using IV regressions, where S_{ij1} is a woman's completed years of education and is instrumented for with our two measures of her exposure to the DRV's ME program. Since IV estimates are consistent, we expect the two measures to provide similar estimation results, except for some differences that may be caused by somewhat different estimation sample sizes.[30] For comparison purposes, we also provide estimation results using OLS regressions.

V.1. Households' Outcomes

We provide in Table 6 estimates for a variety of measures of living standards, including the value of the household's durable assets, and the household's expenditure, income, and living area,

[30] In the local IV terminology, our two measures of exposure should affect very similar population groups; thus, they should provide (asymptotically) similar results.

all of which are on a per capita basis and on logarithmic scale. We also offer estimates for other physical indicators of house quality such as its structure and floor quality. House structure is a dummy variable which equals 1 if the wall is (mostly) made of concrete, brick, stone or wood, and 0 otherwise. Floor quality is also a dummy variable which equals 1 if the flooring material is marble, tile, cement, and 0 otherwise.

Two remarks are in order for Table 6. First, the IV estimates reveal that households with female members exposed to the DRV's ME program have higher indicators of living standards. The two indicators that are not statistically significant are (log of) the household's income per capita and living area per capita, but these variables have positive coefficients. One additional year of education caused by the exposure to the DRV's ME program increases households' durable assets value per capita by 15 percent, their expenditure per capita by 10 percent (Table 6, Columns 1 and 2), and the quality of their house structure and floor respectively by 4 and 13 percentage points (Table 6, Columns 5 and 6).[31] As discussed earlier, estimation results using the second measure of exposure as the IV have similar magnitudes. Second, consistent with our earlier theoretical discussion, the IV estimates are larger than the OLS estimates, with the magnitude ranging from more than half a times for durable assets (Column 1) to six times larger for the floor quality (Column 10).

These results suggest that women's better education achievements because of exposure to the DRV's ME program during Indochina War have positive effects on their households' income,

[31] The estimated coefficient on the household per capita expenditure and income may also be roughly interpreted as some (crude) indicator of the rate of returns to education. For comparison, the return to education in urban China is estimated to hover around 8 percent in 1998 (Zhang et al., 2005) and 10 percent for the Asia region (Psacharopoulos and Patrinos, 2004).

wealth, and expenditure roughly five decades later. This illustrates the long-lasting and beneficial impacts of education, not only for individuals, but also for their households.

V.2. Outcomes for Spouses

Our earlier discussion of the existing literature suggests that under assortative mating, a woman's education should be correlated with her husband's education and other outcomes. Yet, to our knowledge, the issue of assortative mating has never been studied in the context of Vietnam. We thus turn next to investigating whether a woman's education has any impact on her husband's education outcomes. Estimation results provided in Table 7 are consistent with our earlier results, and suggest that a woman's education is significantly positively correlated with her husband's outcomes. In particular, if she attains one additional year of schooling, this would be associated with a 3 to 5 percentage points increase in the probability that her husband attains reading and math literacy (Table 7, Panel B, Columns 1 and 2), and with him having 0.6 more years of schooling (Table 7, Panel B, Column 5). Also consistent with our previous results, the IV estimates are larger than the OLS estimates and IV estimates using the second measure of exposure to the DRV's ME program are rather similar.

V.3. Intergenerational Impacts on Children's Education Achievement

The next question we ask is whether there are any inter-generational impacts of education for Vietnam? And if such impacts exist, what are the magnitudes? A recent study by Emran and Shilpi (2011) suggests that intergenerational occupational mobility exists in Vietnam, but not much is known about intergenerational education mobility.

We provide in Table 8 estimates for three education outcomes, which are completion of primary education or more, completion of secondary education or more, and years of education.[32] The latter two outcomes are statistically significant (although marginally significant at a weaker level for the second measure for secondary school completion). Primary completion is not statistically significant, perhaps due to successes with policies aimed at a universal primary education by the government. It is interesting to note that our OLS estimate of the intergenerational (years of) education mobility for Vietnam is 0.3 (Panel A, Column 3), which is not very different from the global average correlation between parent and child's schooling of 0.4 estimated by Hertz *et al.* (2007). Our IV estimates suggest that one more year of schooling for the mother can raise her child's education by between 0.3 and 0.5 additional years (Panel B, Columns 3 and 6).

We further provide IV estimates separately for boys and girls in Table 9, which show that the years of schooling are strongly statistically significant for both boys and girls, but other outcomes become statistically insignificant or marginally statistically significant at the 10 percent level, possibly due to smaller estimation samples. The intergenerational impacts of mothers' education are rather similar for boys and girls. One more year of schooling attained by the mother raises her daughter(s)'s education achievement by 0.3-0.4 more years, and her son(s)'s education by 0.3-0.5 more years. Given the gender inequality in education access for the mothers residing in French-occupied provinces, this result provides supportive evidence for the beneficial gender-equalizing impacts of the DRV's ME program one generation later.[33]

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[32] We include all the children who live outside the household in our estimation samples.

[33] We focus on identifying the intergenerational impacts of a mother's education on her children in this paper. A father's education can have beneficial impacts on his children's education, and identifying this causal relationship is beyond the scope of this paper. Still, since our estimation results in Table 7 also suggests that there is a strong correlation between mothers' and fathers' years of education, we can offer a check on the combined impacts of parental education by replacing a mother's years of education with the average years of education of the mother and the father. Estimation results (Table 1.12, Appendix 1) are qualitatively similar to those shown in Table 8. Using the maximal

We offer further reflections on some related issues in the next section.

VI. Further Reflections on Related Issues

As discussed earlier, Vietnam has witnessed a reverse gender gap in secondary school enrollment in the past decade. This pattern is consistent with the country's stronger-than-average gender equality for other education outcomes as well. Figure 4 plots the average male and female years of schooling against countries' (log of) per capita GDP, which shows that Vietnam's male years of schooling is somewhat higher than the overall trend. But remarkably, its female years of schooling is one year more than the global trend.[34] Vietnam has also reached virtually universal primary school enrollment and higher gender equality than the global trend at its income level (see Figure 1.1, Appendix 1).

Our study offers an interesting historical and institutional perspective that may help shed light on this remarkable performance. The current strong performance in education may be traced back to the beneficial impacts of ME policy, which helped lay the foundations of Vietnam's current education system at its declaration of independence more than half a century ago. Importantly, the ME policy provided an unprecedented opportunity to help level access to schools not only for the whole population of school-age children, but also school-age girls who had rarely been granted the privilege of school attendance.

However, we did not find statistically significant impacts of the ME policy on boys' education. In other words, the available data do not allow us to rule out the hypothesis that the ME policy has

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years of education of the mother and the father instead of the average also provides qualitatively similar results (not shown).

[34] Our calculation based on the data for Figure 2 also shows that Vietnam's (unconditional) mean female years of schooling is 8.6, which is 0.7 years more than the global corresponding figure. The steeper slope in Panel B in Figure 4 also suggests that it generally takes a higher level of income for women to achieve the same years of schooling as that of men.

no impacts on boys' schooling. Yet, another hypothesis to help explain this result can be put forward if we are to make additional assumptions. Since boys were found to have enrolled in school at a much higher rate than girls before 1945 (Section II.1), the ME policy may have had weaker impacts on boys. Furthermore, analysis of a smaller survey that covers three provinces, Ha Nam, Nam Dinh, and Ninh Binh, in North Vietnam suggests that a far larger share of men than women were enrolled in the army during the Second Indochina war (or the Vietnam war), when those born during 1924-1955 reached adulthood. The proportion of men who ever served in the army was estimated to range approximately between 35 and 75 percent, depending on the age cohorts, while the corresponding figure for women was just 4 percent (Teerawichitchainan, 2009).[35] Notably, the same study also finds that enlisted men were predominantly *more* educated: young men with up to five years of schooling were respectively 44 percent and 19 percent less likely to be inducted than those who had between six and nine years of education and 10 years of education or more. As such, the (much) larger war mortality for (educated) men (Hirsch et al., 1995) may have further reduced any potentially positive impacts of the ME policies. Indeed, analyzing the same data, another study offers further corroborative evidence that during the same war, the death rate for sons of fathers with six or more years of education was 68 percent higher than that for sons of fathers with no education or a primary school education (Merli, 2000).[36]

Interestingly enough, while the specific context of the mass education program implemented in Vietnam half a century ago is unique, similar movements—albeit at a different level—also

[35] In general, women's participation in armed activities are much more likely to be informal than that of men (see, e.g., Turner-Gottschang and Phan, 1998; Taylor, 1999). The larger male war mortality rate also concurs with evidence for other countries (Obermeyer *et al.*, 2008).

[36] Another related hypothesis is that since more than two-thirds of men were estimated to be enlisted when younger than 20 years old during the Vietnam war (Teerawichitchainan, 2009), this may also have disrupted men's education and thus reduced their education attainment. However, we have no data on veterans who were encouraged to go back to school after the Vietnam war. Studies for the US have suggested that preferential government policies on this population group can significantly increase their college education (see, e.g., Bound and Turner, 2002; Stanley, 2003).

occurred in other countries. For example, the US underwent a spectacular educational transformation, which witnessed the percentage of the 18-year-old population with a high school diploma jumping by more than five times, from 9 percent to more than 50 percent between 1910 and 1940. This mass education achievement set the US far ahead of European countries for most of the 20th century in terms of human capital stock and was attributed to investment by both the family and the local state government (Goldin and Katz, 2009).

As such, while it may not be possible to exactly replicate a similar school policy in the same context in Vietnam or elsewhere, the historical lesson remains relevant. In particular, if a school policy can win unanimous approval and support from the government and all society's different walks of life, it may be able to offer record-breaking achievements at relatively low costs. Our findings thus suggest that a similar approach, at least in spirit, to certain policies may be fruitful. For example, today information technology skills and foreign languages skills have become increasingly relevant in a globalizing world, and these skills may be considered indispensable for any individual's success perhaps just like reading literacy in the old times. Indeed, some countries represent models of success where bilingualism or trilingualism have been made either official (e.g., Singapore) or *de facto* practice (e.g., India or the Netherlands). Vietnam may be able to perform another miracle and achieve literacy in foreign languages if the country can bring to action the type of efforts it did more than half a century ago.

For a specific example, Vietnam's English proficiency is currently estimated to be at a moderate level, which ranks it in 34th place out of 80 countries around the world and is roughly equal to the average country in Asia (EF, 2018). These numbers suggest much potential for the country to tap into if it is to achieve literacy in this increasingly popular language. They also suggest that the country is perhaps in a relatively better position—compared to its very high

illiteracy rate more than 60 years ago—to scale up its classes and eradicate English illiteracy for its whole population.

Furthermore, notwithstanding Vietnam's impressive achievement with girls' education, we still recommend that the country ensure equal, if not compensatory, access for its female population at higher levels of education. For example, the number of female university students has been historically lower than that of male university students, and this discouraging trend, however, reversed only as recently as 2015 (GSO, 2017). It would be useful for this trend to continue. Besides societal concepts of gender equality, the evidence that we uncovered on the long-term inter-generational and spillover impacts of girls' education would perhaps help justify further investment in girls.

VII. Summary

Our study helps address the dearth of studies on the long-term impacts of education policies on developing countries. We find that school-age children's exposure to the DRV's ME program that was implemented during the first Indochina War helped raise their probability of achieving reading and math literacy, completing at least primary education, and completing at least secondary education by between 10 and 13 percentage points, and their education by an additional 1.5 years of schooling. the DRV's ME program had especially long-term beneficial impacts on girls' household living standards, and their husband and children.

Specifically, one additional year of schooling, more than 50 years later, increased their households' durable assets per capita by 15 percent, their expenditure per capita by 10 percent, and the quality of their house structure and floor respectively by 5 and 13 percentage points. One additional year of schooling also resulted in girls getting married to husbands who are 3 to 5 percentage points more likely to attain reading and math literacy, who have 0.6 more years of

schooling. One more year of schooling also raised the education attainments of the girls' children by 0.3 to 0.5 more years.

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Table 1. Balance Tests for DRV-occupied provinces and French-occupied provinces, pre-treatment period 1924-35 and post-treatment period 1950-61

(1)	(2)	(3)	(4)	(5)
Difference between French controlled provinces and DRV controlled provinces	Cohorts of 1924-35	Cohorts of 1950-61	Difference in difference	N
Years of education	1.074***	0.568	-0.506	2550
	(0.224)	(0.577)	(0.539)	
Reading literacy	0.056	0.003	-0.053	2559
	(0.052)	(0.027)	(0.059)	
Math competency	0.046	-0.018	-0.064	2559
	(0.044)	(0.041)	(0.064)	
Primary education completion or above	0.079	0.018	-0.060	2549
	(0.049)	(0.042)	(0.073)	
Secondary education completion or above	0.076***	0.052	-0.023	2549
	(0.020)	(0.082)	(0.070)	
Household head is female	-0.012	-0.014	-0.002	2559
	(0.031)	(0.025)	(0.021)	
Household head belongs to major ethnic group	0.090*	0.134**	0.044	2559
	(0.052)	(0.052)	(0.029)	
Household head is Buddhist	-0.011	-0.024	-0.013	2559
	(0.041)	(0.029)	(0.030)	

Note: Each cell presents the results from a separate regression of the dependent variable shown in the first column on a dummy variable indicating whether an individual in the specified birth cohorts (columns 2 and 3) resides in DRV controlled provinces. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 2. Population Education Program and Educational Attainment in North Vietnam

	Dummy variable for DRV-controlled area					Years of exposure to DRV				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Whole sample										
Treated	0.117***	0.106***	0.126***	0.120*	0.957**	0.021**	0.016*	0.026**	0.016	0.174**
	(0.039)	(0.041)	(0.048)	(0.067)	(0.444)	(0.008)	(0.009)	(0.010)	(0.012)	(0.077)
N	3016	3016	3007	3007	3008	3683	3683	3674	3674	3674
adj. R^2	0.368	0.373	0.424	0.348	0.444	0.353	0.364	0.406	0.335	0.430
Panel B. Female										
Treated	0.204***	0.192**	0.263***	0.187**	1.474***	0.028	0.021	0.043**	0.023	0.253**
	(0.073)	(0.089)	(0.065)	(0.084)	(0.297)	(0.017)	(0.019)	(0.016)	(0.014)	(0.094)
N	1622	1622	1618	1618	1619	1979	1979	1975	1975	1976
adj. R^2	0.460	0.460	0.521	0.395	0.535	0.414	0.425	0.481	0.371	0.499
Panel C. Male										
Treated	0.030	0.015	-0.035	0.033	0.290	0.015	0.011	0.008	0.006	0.059
	(0.046)	(0.046)	(0.067)	(0.060)	(0.831)	(0.010)	(0.009)	(0.011)	(0.013)	(0.119)
N	1394	1394	1389	1389	1389	1704	1704	1699	1699	1698
adj. R^2	0.194	0.198	0.274	0.291	0.282	0.203	0.200	0.258	0.274	0.273

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Robustness Check: ME Program in North Vietnam vs. South Vietnam

	Dummy for DRV-controlled area				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)
Panel A. Whole sample (South Vietnam)					
Treated	-0.019	0.011	0.040	-0.038	-0.057
	(0.056)	(0.069)	(0.060)	(0.042)	(0.602)
N	2996	2996	2988	2988	2988
adj. R^2	0.317	0.296	0.229	0.116	0.286
Panel B. Female (South Vietnam)					
Treated	-0.033	0.012	0.006	-0.018	-0.053
	(0.058)	(0.089)	(0.094)	(0.068)	(0.709)
N	1679	1679	1677	1677	1677
adj. R^2	0.308	0.263	0.194	0.101	0.263
Panel C. Male (South Vietnam)					
Treated	-0.008	0.015	0.121**	-0.052	0.078
	(0.054)	(0.059)	(0.046)	(0.037)	(0.599)
N	1317	1317	1311	1311	1311
adj. R^2	0.286	0.241	0.167	0.101	0.223

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Further Robustness Checks for Girls: Different Cohorts, Migration, and Parental Education

	Dummy variable for DRV-controlled area					Years of exposure to DRV				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Falsification tests (The treatment group is the cohorts of 1961-1966 and control group is the cohorts of 1955-1960)										
Treated	0.001	0.031	-0.017	-0.022	0.326					
	(0.049)	(0.045)	(0.047)	(0.041)	(0.294)					
N	1087	1087	1084	1084	1084					
adj. R^2	0.273	0.233	0.243	0.225	0.298					
Panel B. Sample includes migrants with age of less than 15 in the North (The treatment group is the cohorts of 1940-45 and control group is the cohorts of 1924-1935 and 1950-1961)										
Treated	0.166**	0.157	0.216***	0.168**	1.171***	0.024	0.017	0.035**	0.018	0.179
	(0.081)	(0.098)	(0.065)	(0.078)	(0.359)	(0.018)	(0.020)	(0.016)	(0.013)	(0.108)
N	1830	1830	1826	1826	1827	2237	2237	2233	2233	2234
adj. R^2	0.451	0.443	0.492	0.373	0.508	0.404	0.405	0.456	0.351	0.475
Panel C. Adding parental education (The treatment group is the cohorts of 1940-45 and control group is the cohorts of 1924-1935 and 1950-1961)										
Treated	0.249***	0.239**	0.319***	0.291***	1.983***	0.029*	0.031*	0.053***	0.041**	0.338***
	(0.085)	(0.095)	(0.108)	(0.101)	(0.420)	(0.017)	(0.017)	(0.018)	(0.018)	(0.106)
N	1000	1000	996	996	997	1234	1234	1230	1230	1231
adj. R^2	0.400	0.416	0.520	0.410	0.526	0.386	0.400	0.494	0.395	0.513

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Heterogeneity Analyses for Girls

	Dummy variable for DRV-controlled area					Years of exposure to DRV				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Kinh and Hoa groups										
Treated	0.104***	0.099**	0.135**	0.124	0.970*	0.018**	0.016*	0.026**	0.018	0.164*
	(0.036)	(0.043)	(0.053)	(0.076)	(0.488)	(0.008)	(0.009)	(0.011)	(0.013)	(0.086)
N	2620	2620	2612	2612	2613	3199	3199	3191	3191	3191
adj. R^2	0.345	0.358	0.418	0.363	0.431	0.329	0.351	0.397	0.343	0.411
Panel B. Ethnic minority groups										
Treated	0.223**	-0.071	0.014	0.099	0.442	0.008	-0.014	-0.021	-0.002	0.063
	(0.094)	(0.122)	(0.091)	(0.083)	(0.645)	(0.057)	(0.060)	(0.041)	(0.037)	(0.377)
N	396	396	395	395	395	484	484	483	483	483
adj. R^2	0.456	0.414	0.417	0.184	0.466	0.441	0.393	0.412	0.187	0.463
Panel C. Buddhist Groups										
Treated	0.054	0.214**	0.110	0.067	1.940**	0.019**	0.052***	0.036*	0.013	0.383*
	(0.047)	(0.076)	(0.093)	(0.132)	(0.809)	(0.009)	(0.015)	(0.021)	(0.029)	(0.201)
N	277	277	275	275	275	352	352	350	350	350
adj. R^2	0.379	0.404	0.413	0.330	0.485	0.367	0.395	0.455	0.312	0.462
Panel D. Catholic Groups										
Treated	0.229*	0.338**	0.330*	0.074	1.584	0.019*	0.038*	0.050*	0.018	0.244
	(0.119)	(0.144)	(0.175)	(0.292)	(1.068)	(0.011)	(0.019)	(0.025)	(0.046)	(0.159)
N	220	220	222	222	222	278	278	281	281	281
adj. R^2	0.298	0.344	0.221	0.251	0.364	0.263	0.356	0.254	0.264	0.372
Panel E. Non-religious groups										
Treated	0.108**	0.062**	0.135***	0.117	0.934*	0.020**	0.009	0.025**	0.010	0.146*
	(0.042)	(0.029)	(0.043)	(0.077)	(0.531)	(0.009)	(0.008)	(0.010)	(0.013)	(0.078)
N	2519	2519	2510	2510	2511	3053	3053	3043	3043	3043
adj. R^2	0.384	0.379	0.440	0.346	0.444	0.369	0.366	0.417	0.335	0.428

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Impacts of Female Education on Household Living Conditions

	Dummy variable for DRV-controlled area						Years of exposure to DRV					
	Log of real durable assets' value per capita	Log of real expenditure per capita	Log of real income per capita	Log of living area per capita	House structure	Floor quality	Log of real durable assets' value per capita	Log of real expenditure per capita	Log of real income per capita	Log of living area per capita	House structure	Floor quality
OLS regressions	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Years of education	0.091***	0.048***	0.056***	0.018***	0.013***	0.019***	0.089***	0.047***	0.049***	0.015***	0.013***	0.019***
	(0.007)	(0.004)	(0.008)	(0.004)	(0.005)	(0.005)	(0.006)	(0.004)	(0.007)	(0.003)	(0.004)	(0.004)
IV regressions	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Years of education	0.153**	0.099**	0.118	0.041	0.044*	0.127***	0.222**	0.093*	0.093	0.064	0.064**	0.126***
	(0.059)	(0.046)	(0.099)	(0.034)	(0.023)	(0.016)	(0.085)	(0.052)	(0.078)	(0.047)	(0.028)	(0.041)
F-test of the excluded instrument	101.55	107.03	83.31	107.03	107.03	107.03	20.43	19.65	19.72	19.65	19.65	19.65
N	1607	1618	1491	1618	1618	1618	1962	1975	1835	1975	1975	1975

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and current province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Impacts of A Wife's Education on Her Husband's Education

	Dummy variable for DRV-controlled area					Years of exposure to DRV				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A. OLS regressions										
Years of education	0.020***	0.026***	0.040***	0.052***	0.451***	0.019***	0.025***	0.037***	0.049***	0.459***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.031)	(0.003)	(0.004)	(0.003)	(0.004)	(0.029)
IV regressions	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Years of education	0.041***	0.049***	0.051**	0.033	0.603***	0.031***	0.035***	0.032**	0.055***	0.653***
	(0.007)	(0.008)	(0.023)	(0.027)	(0.099)	(0.007)	(0.009)	(0.015)	(0.014)	(0.125)
F-test of the excluded instrument	25.14	25.14	25.39	25.39	25.39	72.07	72.07	71.15	71.15	71.15
N	1159	1159	1155	1155	1155	1430	1430	1426	1426	1425

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and current province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Impact of A Mother's Education on Her Children's Education

	Dummy variable for DRV-controlled area			Years of exposure to DRV		
	Primary education completion or above	Secondary education completion or above	Years of education	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A. OLS regressions						
	OLS	OLS	OLS	OLS	OLS	OLS
Years of education of mother	0.011**	0.025***	0.320***	0.012***	0.028***	0.349***
	(0.004)	(0.004)	(0.036)	(0.003)	(0.003)	(0.029)
PANEL B. IV regressions						
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Years of education of mother	0.008	0.020**	0.463***	0.006	0.021*	0.345***
	(0.017)	(0.009)	(0.098)	(0.013)	(0.011)	(0.090)
F-test of the excluded instrument	30.06	30.06	30.10	119.38	119.38	119.35
N	2541	2541	2533	3322	3322	3314

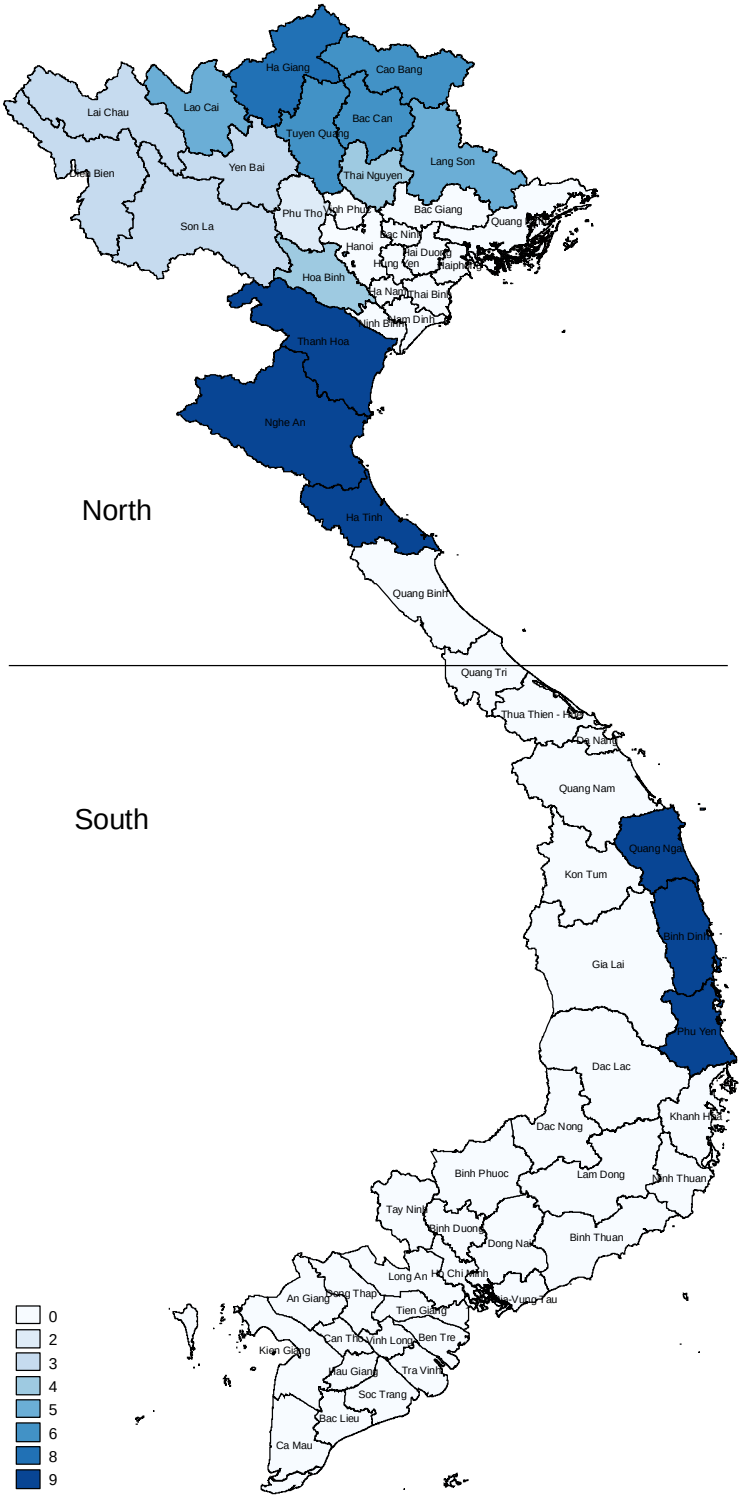
Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and current province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Impact of A Mother's Education on Her Children's Education, for Sons vs. Daughters

	Dummy variable for DRV-controlled area			Years of exposure to DRV		
	Primary education completion or above	Secondary education completion or above	Years of education	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A. Son						
Years of education of mother	0.004	0.043*	0.524***	0.010	0.032**	0.309***
	(0.014)	(0.022)	(0.129)	(0.012)	(0.015)	(0.106)
N	1272	1272	1269	1672	1672	1669
PANEL B. Daughter						
Years of education of mother	0.011	0.003	0.389***	0.000	0.009	0.329***
	(0.017)	(.)	(0.124)	(0.018)	(0.011)	(0.114)
N	1269	1269	1264	1650	1650	1645

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and current province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Figure 1. Map of Vietnam under DRV and the French Occupation during the First Indochina War, 1946-1954



Note: The number of years under the DRV's occupation is indicated by different colors.
Disclaimer: Country borders or names do not necessarily reflect the World Bank Group's official position. The dividing line for the North and the South of Vietnam after the First Indochina War was established by the Geneva Accords of 1954, and is symbolically drawn at the 17th parallel north. This map is for illustrative purposes and does not imply the expression of any opinion on the part of the World Bank, concerning the legal status of any country or territory or concerning the delimitation of frontiers or boundaries.

Figure 2. Years of School for Those Born in DRV-occupied Provinces versus Those Born in French-occupied Provinces

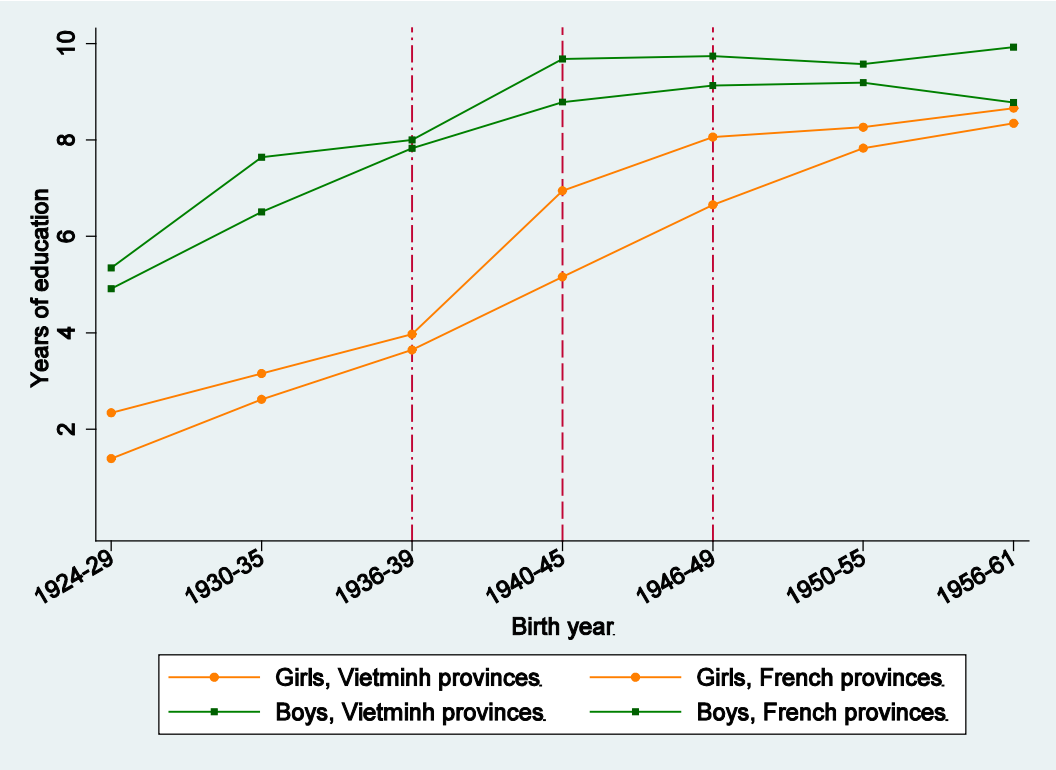


Figure 3. Robustness Checks for Different Combinations of Control Cohorts

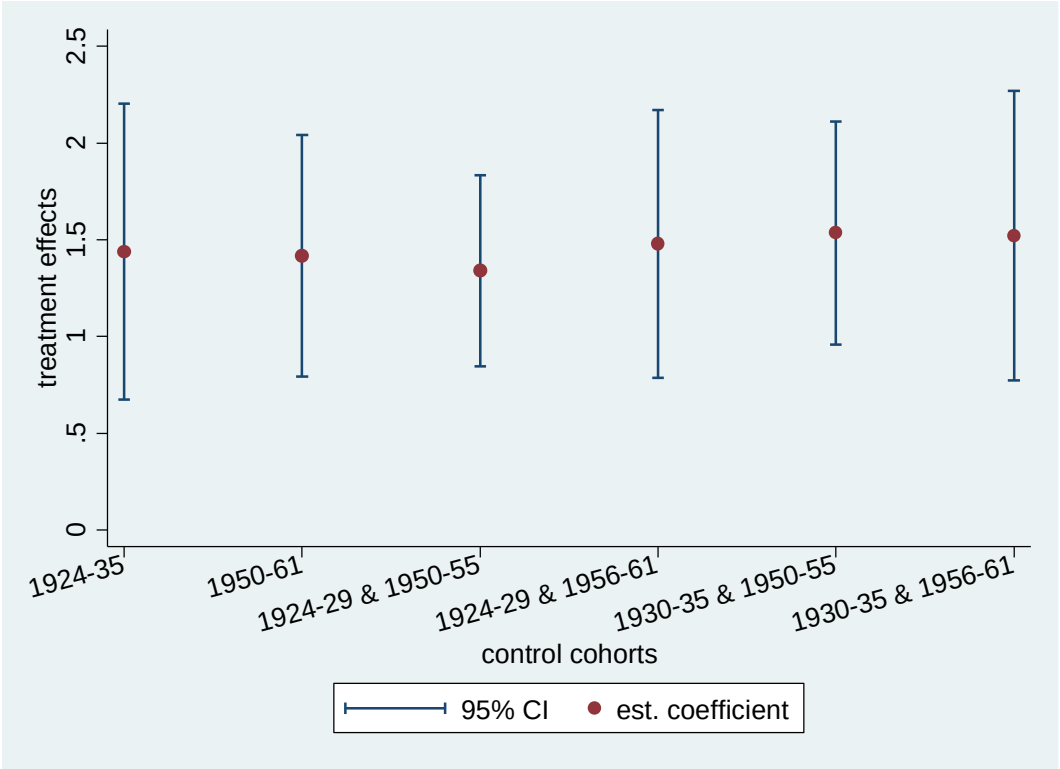
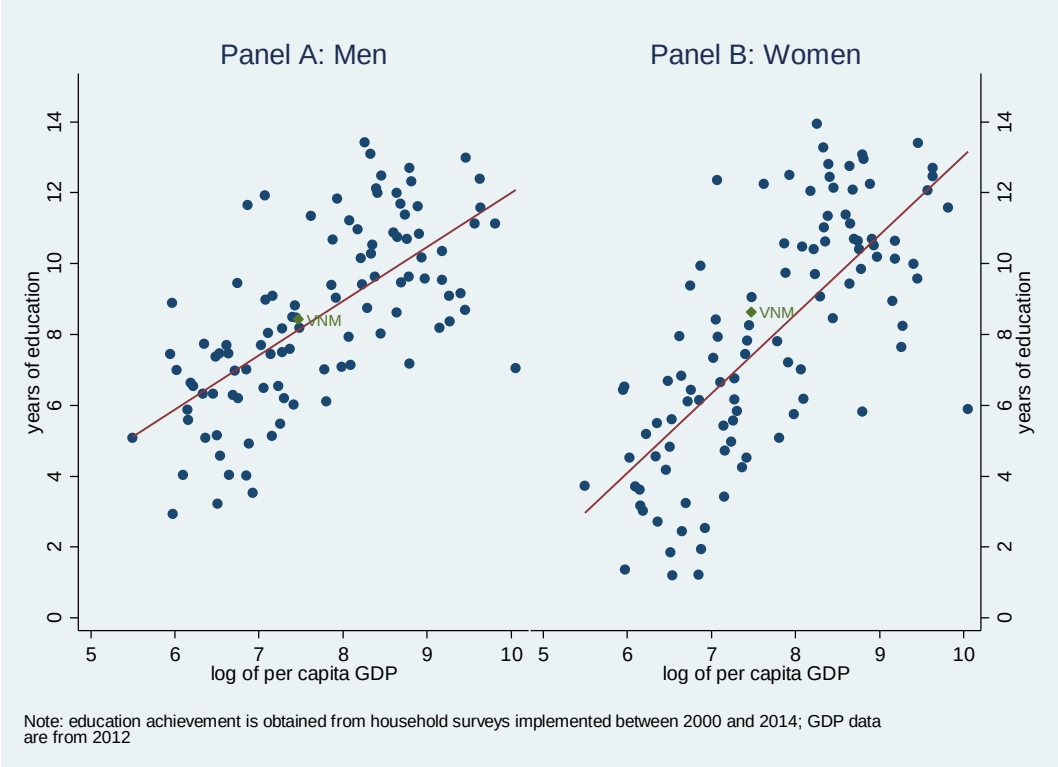


Figure 4. Averaged Years of Schooling of Adult Population vs. Country Income Level



Data source: UNESCO's WIDE database and World Bank's WDI database.

Appendix 1. Additional Tables and Figures (for online publication)

Table 1.1: Number of Years That Individuals Are Potentially Exposed to the New Education Policy

Birth Year	Vietminh-occupied Provinces	French-occupied Provinces	Mixed-occupation Provinces	
			Example 1	Example 2
1924	0	0	0	0
…	0	0	0	0
1934	0	0	0	0
1935	1	0	0	0
1936	2	0	0	0
1937	3	0	1	0
1938	4	0	2	0
1939	5	0	3	0
1940	5	0	4	0
1941	5	0	5	1
1942	5	0	5	2
1943	5	0	5	3
1944	5	1	5	4
1945	5	2	5	5
1946	5	3	5	5
1947	5	4	5	5
1948	5	5	5	5
…	5	5	5	5
1961	5	5	5	5

Note: This table shows the number of years of general schooling that are potentially affected for those who reached primary and secondary school age during the 1946-54 Indochina war. An individual is assumed to start attending primary school at 6 years old. The maximal number of years of potential exposure is restricted to 5 years, or the number of years of schooling required to achieve a primary school degree, and individuals are assumed to stop going to school if they are never enrolled at age 11. For the mixed-occupation provinces, Example 1 and Example 2 show the number of years of potential exposure for provinces that are occupied by Vietminh starting from 1947 and 1951 respectively.

Table 1.2. Robustness Checks with Different Province Samples in North Vietnam, Girls' Sample

	Dummy for DRV-controlled area				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)
Panel A. Include provinces that are adjacent to DRV-controlled provinces					
Treated birth years*DRV	0.202**	0.192**	0.253***	0.193**	1.437***
	(0.075)	(0.089)	(0.067)	(0.083)	(0.297)
Treated birth years* adj_ DRV	-0.015	-0.002	-0.080	0.050	-0.305
	(0.106)	(0.136)	(0.133)	(0.184)	(0.906)
N	1622	1622	1618	1618	1619
adj. R^2	0.460	0.459	0.521	0.395	0.534
Panel B. Remove incompletely-occupied provinces during the war 1946-54					
Treated birth years*DRV	0.189**	0.177*	0.260***	0.175*	1.379***
	(0.077)	(0.095)	(0.073)	(0.091)	(0.423)
N	1377	1377	1375	1375	1376
adj. R^2	0.442	0.441	0.503	0.394	0.513

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 1.3. Robustness Checks for Different Control Cohorts, Girls' Sample

	Dummy variable for DRV-controlled area				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)
Panel A. control group is the cohorts of 1924-29					
Treated	0.082*	0.045	0.251***	0.214*	1.200***
	(0.047)	(0.062)	(0.068)	(0.105)	(0.422)
N	445	445	446	446	446
adj. R^2	0.386	0.338	0.327	0.207	0.401
Panel B. control group is the cohorts of 1930-35					
Treated	0.276**	0.282***	0.339***	0.287**	1.798***
	(0.100)	(0.094)	(0.067)	(0.117)	(0.487)
N	493	493	494	494	494
adj. R^2	0.171	0.203	0.254	0.188	0.279
Panel C. control group is the cohorts of 1950-55					
Treated	0.149**	0.147	0.197***	0.187**	1.399***
	(0.068)	(0.109)	(0.064)	(0.083)	(0.339)
N	610	610	608	608	609
adj. R^2	0.132	0.164	0.288	0.215	0.291
Panel D. control group is the cohorts of 1956-61					
Treated	0.247***	0.218**	0.276***	0.139	1.442***
	(0.089)	(0.105)	(0.073)	(0.086)	(0.395)
N	791	791	790	790	790
adj. R^2	0.249	0.241	0.346	0.292	0.363

Note: The treatment group consists of the birth cohorts of 1940-45. Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 1.4. Robustness Checks with All Birth Cohorts, All Population

	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)
DRV*cohort 2933	0.032	0.028	0.025	0.002	0.532
	(0.069)	(0.073)	(0.105)	(0.055)	(0.347)
DRV*cohort 3439	0.019	-0.069*	0.005	0.040	0.312
	(0.037)	(0.035)	(0.076)	(0.069)	(0.342)
DRV*cohort 4045	0.127***	0.088**	0.122**	0.154**	1.406**
	(0.042)	(0.036)	(0.053)	(0.071)	(0.616)
DRV*cohort 4650	0.044	0.053	0.117**	0.075	1.203**
	(0.051)	(0.049)	(0.050)	(0.054)	(0.521)
DRV*cohort 5155	0.026	-0.010	-0.018	-0.009	0.148
	(0.059)	(0.075)	(0.044)	(0.046)	(0.397)
DRV*cohort 5661	-0.019	-0.045	-0.032	0.068	0.481
	(0.052)	(0.058)	(0.042)	(0.049)	(0.335)
N	3683	3683	3674	3674	3674
adj. R^2	0.353	0.365	0.406	0.336	0.430

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 1.5. Robustness Checks with All Birth Cohorts, Girls' Sample

	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)
DRV*cohort 2933	-0.024	0.003	-0.050	-0.040	0.003
	(0.063)	(0.055)	(0.049)	(0.030)	(0.334)
DRV*cohort 3439	-0.016	-0.107	0.003	0.016	-0.009
	(0.063)	(0.065)	(0.075)	(0.080)	(0.446)
DRV*cohort 4045	0.173***	0.148**	0.258***	0.225**	1.524***
	(0.051)	(0.058)	(0.071)	(0.093)	(0.427)
DRV*cohort 4650	0.045	0.104	0.216***	0.140*	1.535*
	(0.069)	(0.066)	(0.063)	(0.075)	(0.872)
DRV*cohort 5155	-0.007	-0.035	-0.004	0.019	-0.173
	(0.069)	(0.099)	(0.053)	(0.051)	(0.575)
DRV*cohort 5661	-0.081	-0.080	-0.021	0.076	-0.017
	(0.080)	(0.084)	(0.069)	(0.066)	(0.478)
N	1979	1979	1975	1975	1976
adj. R^2	0.415	0.428	0.484	0.372	0.501

Note: Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 1.6. Robustness Checks with Contaminated Treatment Cohorts, Girls' Sample

	Dummy variable for DRV-controlled area				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)
Panel A. treatment group is cohorts of 1936-1939; control group is cohorts of 1924-1935 and 1950-1961					
Treated	0.026	-0.017	0.091	-0.005	0.092
	(0.109)	(0.138)	(0.085)	(0.110)	(0.723)
N	1561	1561	1556	1556	1557
adj. R^2	0.481	0.503	0.557	0.436	0.562
Panel B. treatment group is cohorts of 1946-1949; control group is cohorts of 1924-1935 and 1950-1961					
Treated	0.050	0.148**	0.184***	0.112	1.529**
	(0.065)	(0.069)	(0.049)	(0.094)	(0.745)
N	1562	1562	1557	1557	1558
adj. R^2	0.487	0.491	0.533	0.388	0.535
Panel C. treatment group is cohorts of 1940-1949; control group is cohorts of 1924-1935 and 1950-1961					
Treated	0.137**	0.174***	0.231***	0.158*	1.524***
	(0.053)	(0.060)	(0.048)	(0.084)	(0.392)
N	1801	1801	1797	1797	1798
adj. R^2	0.433	0.432	0.489	0.363	0.504
Panel D. treatment group is cohorts of 1936-1945; control group is cohorts of 1924-1935 and 1950-1961					
Treated	0.135	0.112	0.198***	0.115***	0.955**
	(0.082)	(0.100)	(0.064)	(0.036)	(0.381)
N	1800	1800	1796	1796	1797
adj. R^2	0.433	0.447	0.508	0.401	0.526

Note: Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 1.7. Robustness Checks with Combined Matching and Difference-in-Difference Model, Girls' Sample

	Dummy variable for DRV-controlled area				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)
Panel A. Whole sample					
Treated	0.106**	0.084*	0.136*	0.087	0.796
	(0.042)	(0.042)	(0.070)	(0.084)	(0.620)
N	3016	3016	3007	3007	3008
Panel B. Female					
Treated	0.180***	0.153**	0.234***	0.144	1.192**
	(0.064)	(0.075)	(0.082)	(0.098)	(0.519)
N	1621	1621	1617	1617	1618
Panel C. Male					
Treated	0.054	0.031	0.013	0.017	0.437
	(0.050)	(0.053)	(0.092)	(0.078)	(0.437)
N	1394	1394	1389	1389	1389

Note: Each cell presents the results from a separate regression that employs for matching dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, and his/ her age. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted at the birth province; *** p<0.01, ** p<0.05, * p<0.1. Estimates are obtained using the user-written "*diff*" Stata program (Villa, 2016).

Table 1.8. Robustness Checks, Controlling for the Number of Battles Fought in Each Province

	Dummy variable for DRV-controlled area					Years of exposure to DRV				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Whole sample										
Treated	0.108**	0.099**	0.123**	0.116	0.903*	0.019**	0.014	0.025**	0.015	0.164**
	(0.040)	(0.043)	(0.051)	(0.070)	(0.460)	(0.009)	(0.009)	(0.010)	(0.012)	(0.080)
N	2866	2866	2857	2857	2858	3490	3490	3481	3481	3481
adj. R^2	0.370	0.373	0.423	0.342	0.442	0.355	0.365	0.405	0.329	0.427
Panel B. Female										
Treated	0.187**	0.179*	0.251***	0.178**	1.391***	0.024	0.018	0.039**	0.020	0.232**
	(0.072)	(0.089)	(0.066)	(0.086)	(0.311)	(0.017)	(0.019)	(0.016)	(0.014)	(0.096)
N	1547	1547	1543	1543	1544	1885	1885	1881	1881	1882
adj. R^2	0.464	0.463	0.524	0.389	0.534	0.419	0.430	0.482	0.364	0.499
Panel C. Male										
Treated	0.022	0.006	-0.038	0.033	0.195	0.014	0.009	0.009	0.007	0.051
	(0.046)	(0.047)	(0.067)	(0.062)	(0.823)	(0.010)	(0.010)	(0.011)	(0.014)	(0.123)
N	1319	1319	1314	1314	1314	1605	1605	1600	1600	1599
adj. R^2	0.194	0.196	0.271	0.287	0.280	0.202	0.197	0.256	0.268	0.270

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for the number of battles fought in each province, and dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. One province (Thai Binh province) is excluded due to missing data. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 1.9. Further Robustness Check, Nutrition Status and Remittances, Girls' Sample

	Dummy variable for DRV-controlled area		Years of exposure to ME program	
	Height in cm	Dummy variable for overseas remittances	Height in cm	Dummy variable for overseas remittances
	(1)	(2)	(3)	(4)
Treated	-1.342*	0.006	-0.329**	0.003
	(0.755)	(0.024)	(0.140)	(0.005)
N	1590	1623	1590	1623
adj. R^2	0.207	0.042	0.207	0.042

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 1.10. Further Robustness Checks, with VLSS 1992-93 and VHLSS 2014, Girls' Sample

	Dummy variable for DRV-controlled area					Years of exposure to DRV				
	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A- VLSS 1992-93. (difference in birth year in 1993 and in 1998 less than 2)										
Treated	-0.059	-0.064	0.176***	0.117*	1.229***	0.008	0.011	0.027	0.023	0.240**
	(0.051)	(0.046)	(0.058)	(0.058)	(0.313)	(0.011)	(0.011)	(0.017)	(0.015)	(0.098)
N	1165	1165	996	996	996	1414	1414	1208	1208	1208
adj. R^2	0.320	0.324	0.428	0.315	0.390	0.282	0.294	0.413	0.308	0.374
Panel B- VHLSS 2014: The treatment group is the cohorts of 1946-49 and control group is the cohorts of 1950-1961										
Treated	N/A	N/A	0.124**	0.015	0.001	N/A	N/A	0.047**	0.040	0.243
			(0.056)	(0.082)	(0.608)			(0.022)	(0.029)	(0.182)
N			1497	1497	1497			1497	1497	1497
adj. R^2			0.254	0.176	0.214			0.254	0.177	0.215

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 1.11. Robustness Checks, Hybrid Approach with Province-level Years of Exposure

	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)
Panel A. Whole sample					
Treated	0.013***	0.010*	0.015***	0.012	0.113**
	(0.005)	(0.005)	(0.006)	(0.008)	(0.050)
N	3016	3016	3007	3007	3008
adj. R^2	0.368	0.372	0.424	0.348	0.444
Panel B. Female					
Treated	0.018*	0.015	0.026***	0.017*	0.141***
	(0.009)	(0.011)	(0.009)	(0.010)	(0.046)
N	1622	1622	1618	1618	1619
adj. R^2	0.458	0.458	0.520	0.394	0.534
Panel B. Male					
Treated	0.008	0.004	0.001	0.005	0.061
	(0.006)	(0.005)	(0.008)	(0.007)	(0.089)
N	1394	1394	1389	1389	1389
adj. R^2	0.195	0.198	0.274	0.291	0.282

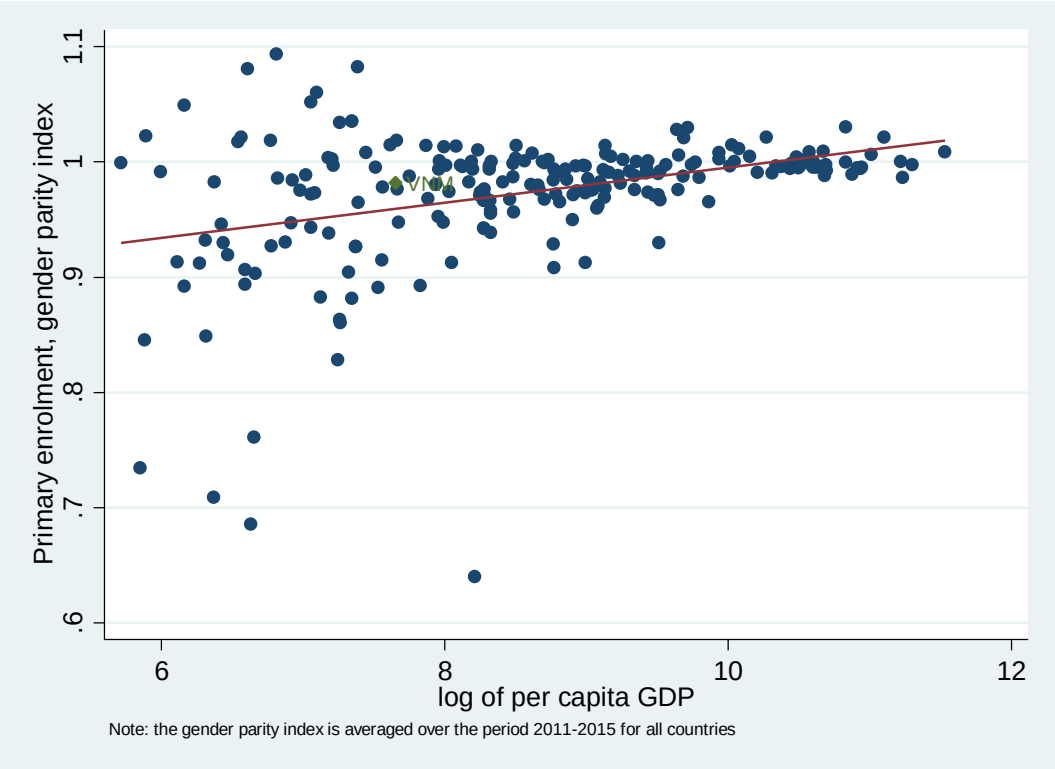
Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Table 1.12. Impact of Parental Education on Their Children's Education

	Dummy variable for DRV-controlled area			Years of exposure to DRV		
	Primary education completion or above	Secondary education completion or above	Years of education	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A. OLS regressions						
	OLS	OLS	OLS	OLS	OLS	OLS
Average years of education of mother and father	0.014***	0.029***	0.365***	0.015***	0.032***	0.379***
	(0.004)	(0.004)	(0.029)	(0.004)	(0.003)	(0.024)
PANEL B. IV regressions						
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Average years of education of mother and father	0.007	0.018*	0.420***	0.006	0.020**	0.322***
	(0.015)	(0.009)	(0.076)	(0.012)	(0.010)	(0.082)
F-test of the excluded instrument	44.28	44.28	44.39	134.34	134.34	134.24
N	2541	2541	2533	3322	3322	3314

Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religion groups (including Buddhism and Christianity), whether the individual belongs to the major ethnic group, birth year and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune); *** p<0.01, ** p<0.05, * p<0.1.

Figure 1.1. Gender Parity Index vs. Country Income Level



Data source: World Bank's WDI database.

Figure 1.2. Snapshot Images of "Popular Education" Classes

Picture A. A night time popular education class at Nghiêm Xuyên commune, Hà Tây province in 1946. Souce: Website of "Binh Dinh newspaper" [http://www.baobinhdinh.com.vn/datnuoc-connguoi/2011/12/120336/]

Picture B. Ho Chi Minh, First President of Vietnam, visited a class in Hanoi in 1956, after the first Indochina War. Source: Website of Ho Chi Minh city's Department of Culture and Sports [http://hiec.org.vn/bo-anh-trien-lam-chu-tich-ho-chi-minh-va-dao-duc-thoi-dai-moi-6960.html]

Appendix 2: List of Books on the History of the Provincial Communist Party and Army That Are Used to Construct Database on DRV's Years of Occupation and Number of Battles

No	Province	Authors	Year	Publisher
1	Lào Cai	Provincial Party Committee	2010	National Political Publishing House
2	Lai Châu	Provincial Party Committee	1999	National Political Publishing House
3	Điện Biên	Provincial Party Committee	1999	National Political Publishing House
4	Yên Bái	Provincial Party Committee	2007	National Political Publishing House
5	Sơn La	Provincial Party Committee	2002	National Political Publishing House
6	Phú Thọ	Provincial Party Committee	2000	National Political Publishing House
7	Hòa Bình	Provincial Party Committee	1993	National Political Publishing House
8	Hà Giang	Provincial Party Committee	1995	National Political Publishing House
9	Tuyên Quang	Provincial Party Committee	2000	National Political Publishing House
10	Cao Bằng	Provincial Party Committee	2003	National Political Publishing House
11	Bắc Kạn	Provincial Party Committee	2000	National Political Publishing House
12	Lạng Sơn	Provincial Party Committee	1998	Provincial Propaganda Department of Lạng Sơn
13	Thái Nguyên	Provincial Party Committee	2003	Provincial Party Committee
14	Bắc Giang	Provincial Party Committee	2003	National Political Publishing House
15	Bắc Ninh	Provincial Party Committee	2010	National Political Publishing House
16	Vĩnh Phúc	Provincial Party Committee	2007	National Political Publishing House
17	Hà Nội	Provincial Party Committee	1995	National Political Publishing House
18	Hà Tây	Provincial Military Command	1998	People's Army Publishing House
19	Hưng Yên	Provincial Party Committee	1998	National Political Publishing House
20	Quảng Ninh	Provincial Propaganda Department	1993	Provincial Propaganda Department of Quảng Ninh
21	Hải Dương	Provincial Party Committee	2008	National Political Publishing House
22	Hải Phòng	Provincial Party Committee	1991	Hải Phòng Publisher
23	Hà Nam	Provincial Party Committee	2000	Hà Nam Publisher
24	Thái Bình	Provincial Party Committee	2004	National Political Publishing House
25	Nam Định	Provincial Party Committee	2001	National Political Publishing House
26	Ninh Bình	Provincial Party Committee	2005	National Political Publishing House
27	Thanh Hóa	Provincial Party Committee	1996	National Political Publishing House
28	Nghệ An	Provincial Party Committee	1998	National Political Publishing House
29	Hà Tĩnh	Provincial Party Committee	1993	National Political Publishing House
30	Quảng Bình	Provincial Party Committee	1995	Provincial Propaganda Department of Quảng Bình
31	Quảng Trị	Provincial Party Committee	1996	National Political Publishing House

Note: All the books listed in this table are in Vietnamese. Our translation of the authors and publishers are for reference purposes only.