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

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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 mandatory educational program conducted by the Democratic Republic of Vietnam in their 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. Interestingly, we also find some long-term spillover impacts of education: impacted girls enjoyed higher household living standards, had more educated spouses, and raised more educated children. We discuss several 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 to be 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 (Vietminh) 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 of various outcomes for an individual, 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. 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 Vietminh-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.

Our study is broadly related to an emerging literature on the effects of war on human capital. For example, Shemyakina (2011) examines the link between violent conflict and the accumulation of education for school-aged students in Tajikistan. She utilizes the variations in regional and temporal exposure to the 1992-1998 armed conflict and shows that school-aged girls who lived in affected regions during the conflict had a lower probability of completing their compulsory schooling than those who were not affected by conflict. However, she finds no

evidence on the impact of conflict on education of boys. Exploiting a similar identification strategy that relies on plausibly exogenous region-by-cohort exposure to allied bombing, Akbulut-Yuksel (2014) finds that such exposure had long-term significant impacts on human capital accumulation for those who were in school age during World War II. Lee (2014) investigates the long-term impacts of pre-natal exposure to the Korean War (1950-1953) and finds that the educational attainment of those who were in utero during the worst time of the war is significantly lower, which was arguably caused by wartime disruptions to normal pre-natal development.[1]

But perhaps the most relevant studies to ours are Duflo (2001), Meng and Gregory (2002), and Wantchekon, Klasnja, and Novta (2015). 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 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. Studying the life-changing Chinese Cultural Revolution, during which most schools in urban China ceased operation over the period 1966-1977, Meng and Gregory (2002) find that the affected cohorts are less than half as likely to possess a university degree 20 years later. Furthermore, children coming from a more disadvantaged background (i.e., having parents of lower educational achievement and lower occupational status) are most negatively impacted. In a similar vein, Wantchekon *et al.* (2015) find that Beninese who benefited from access to the first schools established in colonial times are 96 percent and 10 percent respectively more likely to attain primary education and secondary education or more. These affected

[1] For other recent studies that found that war and conflicts have long-term and negative effects on education attainment, see Ichino and Winter-Ebmer (1998), Leon (2012) and Kesternich *et al.* (2014), and Swee (2015). This literature is in turn broadly related to another that investigate the impacts of natural disasters on human development (see, e.g., Caruso and Miller (2015) and Gignoux and Menéndez (2016)).

individuals and their descendants also have higher living standards, are less likely to be farmers, and are more likely to be politically active; furthermore, an uncle's education also has positive impacts on his nephews and nieces.[2]

Our study differs from 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, and were facilitated by the Indochina War), rather than the generally war-related disruptive effects.[3] During the (First) Indochina War, Vietnam's political system was turned into a bimodal regime, whereby parts of the country were occupied by the French government while some other parts were under the Vietnamese government. This in turn resulted in two different education systems in operation at the same time: the new Vietnamese system provided free, 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. Moreover, the time period that we study spans more than half a century; except for the study by Wantchekon *et al.* (2015), we are not aware of any other study on a developing country that offers such a long-run analysis.

Furthermore, in contrast with the literature's focus on the *negative* impacts of wars, we offer an investigation into a *positive* shock to education (supply) during wartime.[4] The positive shock

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[2] A later study by Duflo (2004), however, found that higher education levels resulted in more formal labor force participation but lower wages for older cohorts that were not affected by this program. Zhang, Liu, and Yung (2007) similarly did not find the Chinese Cultural Revolution to play a significantly adverse role in the returns to schooling. We mostly focus in this paper on long-term impacts on education achievement in the context of a developing country. In richer countries' context, a large and related literature exists on the impacts of compulsory school laws on other 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, forthcoming).

[3] Among the earlier cited studies, only Akbulut-Yuksel (2014) observes that a vital channel for the negative impacts of World War II was the destruction of schools, and Duflo (2001), Meng and Gregory (2002), and Wantchekon *et al.* (2015) specifically examined the impacts of education policies.

[4] From a policy making viewpoint, most interventions that aim at increasing education achievement should result in positive, rather than negative, shocks. Thus positive shocks are arguably more relevant to policy advice than negative shocks.

is caused by beneficial exposure to Vietminh's popular education program during the war. To some extent, a better understanding of positive shocks is perhaps more relevant to policies that aim at future education expansions. Another difference with our study is our focus on the education of girls, who are traditionally faced with more disadvantages in school access.

Our second 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. In particular, our new contribution is to 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 Vietminh's popular education program. To our knowledge, few, if any, studies investigate the causal and inter-generational impacts of a woman's education on various outcomes as attempted in ours, particularly in the context of a developing country.

Finally, Vietnam presents a remarkable—and perhaps even unique—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 enrolment 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, in press). Much spotlight in the media has been given to this country's exemplar performance, but interestingly enough, little rigorous evidence has been offered on the driving factors behind this success.[5] Insights into the schooling policies that

[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

helped lead to such strong performance—especially for women—would be useful not just for this country to make further progress, but can produce relevant lessons for other developing countries in similar contexts as well.

Our estimation results suggest that girls that resided in Vietminh-controlled areas during their school age have significantly stronger math and reading literacy levels and higher education achievements 50 years 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.

This paper consists of seven sections. We provide in the next section a brief overview of the educational systems under the colonial French and under Vietminh, including a detailed description of the "popular education" program. We discuss the analytical model in Section III before presenting the main estimation results in Section IV. We then analyze 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

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Huffington Post (Bellos, 2015), and the *Guardian* (Ravitch, 2015) for recent media coverage of Vietnam's performance on the PISA.

who would serve as civil servants.[6] 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, and even useless. 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 enrolment 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 enrolment discussed earlier.

II.2. Education under the New Government of Vietnam

[6] One scholar estimated that there were about 150,000 elementary school students, 5637 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".

Immediately after the declaration of independence in 1945, the new government of Vietnam (commonly referred to as "Vietminh" in this period), launched a mass education movement called "*Bình dân học vụ"* or "Mass Education" (ME). In particular, Vietminh issued three following decrees that aimed at eradicating illiteracy among the whole population

i) Decree 17/SL to create a central agency in charge of the ME movement.
ii) Decree 19/SL to require that every village have a ME class in 6 months with at least 30 participants, and
iii) Degree 20/SL to require mandatory and free attendance in illiteracy-eradicating classes for all (men and women). Within one year, everyone older than 8 would have to be literate, or would be fined. All expenses of the classes will be borne by local communes and provinces.

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.[7]

The highly inexpensive movement quickly spread across the country and produced fast results, especially in North and Central Vietnam. Vietminh'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). The ME movement in South Vietnam during Indochina War, however, was not as strong as in North and Central Vietnam (which is supported by our empirical estimation shown in a later section).[8]

One important result of the ME program is that school-age children residing under the Vietminh 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

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[7] 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, not assigning communal land to them, or preventing 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).

[8] See for example, the "History of Quang Ngai" (Provincial Government of Quang Ngai, 2015), a South Vietnamese province occupied by Vietminh during the First Indochina War.

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 Vietminh-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 eight years later; but during the war, the country was divided into French-controlled and Vietminh-controlled areas. While Vietminh continued to promote the ME movement in its areas, the education system reverted back 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 Vietminh gained control of the North of Vietnam, and could implement the ME movement for the entire North Vietnam.[9] 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]

We can thus 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

[9] In South Vietnam, for the entire Indochina War period, Vietminh had complete control over three provinces: Quang Ngai, Binh Dinh, and Phu Yen, while the remaining provinces were under French control. But 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.

[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.

child had access to school, which depends on whether their residence area was Vietminh-controlled or French-controlled. Since school-age children residing longer in Vietminh-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 unders Vietminh's control up to the end of 1954. Vietminh completely controlled three provinces in North Vietnam, which are Thanh Hoa, Nghe An, and Ha Tinh provinces, over a period of nine years from the beginning to the end of the war.[11] Vietminh also gained control of 12 additional provinces in North Vietnam from around the middle of the war. We return to more discussions 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

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

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.

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

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* is respectively the inexpensive costs of schooling and the provision of schooling (including teachers, classrooms, and so on) for a child that was created by Vietminh's ME program during the Indochina War. More formally, *P* and *Q* can take the following form

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

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

Thus, Vietminh's education policies (*ME*) can vary by (or interact with) local community characteristics (Vietminh-controlled provinces, denoted by *L*), which determines 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 Vietminh's ME program differ by whether a child resided in a Vietminh-controlled province (or the local community characteristics *L*). Consequently, we can apply a difference-in-difference (DD) strategy to estimate this model

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

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.[12] 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 Vietminh-controlled

[12] 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, forthcoming).

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 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 Vietminh 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 Vietminh-controlled areas. But we also provide robustness checks when we include these cohorts and 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 Vietminh-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 1932 or after in Vietminh-controlled areas or 1941 or after 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

additional provinces in North Vietnam that Vietminh 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.[13] 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 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.[14]

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 children residing in Vietminh-controlled areas during the war have spillover impacts, we estimate the following model

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

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

[14] 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 Vietminh'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.

where the subscript k indicates individual i (k= 1), her spouse (k= 2) and child(ren) (i.e., other values for k). S_{ij1} represents individual i'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.

Since individuals' education outcomes are typically positively correlated with—or endogenous to—those of their family members,[15] 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.[16]

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).[17] Specifically, we use the different exposure to school access during the war (i.e., the interaction term $ME_t * L_j$) to

[15] 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 turns affect 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.

[16] 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.

[17] We only have crude measures of skills (i.e., reading and math literacy), so we focus on individuals' years of schooling for Equation (8).

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 (2011)'s 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 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 cluster.[18]

As earlier discussed, the popular education program was implemented in provinces under Vietminh control in North Vietnam, and it was not implemented in Vietminh-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 dataset 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

[18] 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).

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 for poverty estimates by the Government of Vietnam, international organizations, and 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).

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 use data from other data sources. In particular, we construct the variable years of Vietminh's occupation at the province level during the Indochina war based on data from different books on the history of the Vietnamese communist party and the Vietnamese army (Military History Institute of Vietnam, various years; Statistical Publishing House, various years). We also constructed the total numbers of battles and their average duration for each province over the period 1946-1954 using these books. Finally, we also analyze

UNESCO's WIDE database (UNESCO, 2017) and the World Bank's World Development Indicators database (World Bank, 2017).

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 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 Vietminh-occupied provinces and French-occupied provinces (in North Vietnam).

The average years of education in French-occupied provinces were 1.0 and 0.6 smaller than those in Vietminh-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 Vietminh-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 primary education or secondary education, whether the household head is female, Buddhist, or whether the household head belongs to the ethnic majority group. But 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 Vietminh-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 Vietminh-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 Vietminh-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 Vietminh-occupied provinces and born in French-occupied provinces appear negligible. We discuss another test for the parallel assumption and a number of additional 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 Vietminh-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 Vietminh-controlled areas are between 10 to 13 percentage points higher in the probability of achieving reading and math literacy, completing at least primary education or secondary education (Panel A, Columns 1 to 4). These children are also more likely to accomplish one and half more years 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 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 Vietminh-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 Vietminh's occupation raised an additional 0.3 years of schooling for school-age girls (Panel B, Column 10). However, estimations 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 Vietminh-controlled areas.[19] 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

The estimation results above are robust to a battery of robustness checks, which we offer below.

Implementation of ME Program in North Vietnam vs. South Vietnam

As earlier discussed, although three provinces in South Vietnam were completely occupied by Vietminh 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

[19] We provide in Table 1.2 (Appendix 1) estimates using the hybrid approach where an individual is assigned the number of years of exposure to the ME program according to the province-level years of Vietminh occupation. As earlier discussed, estimation results are qualitatively similar to those in Table 2, but statistically weaker and stronger than the first measure and the second measure respectively.

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 Vietminh-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 Vietminh-related factors (e.g., specific political regime or idealism)—that resulted in better long-term educational outcomes for those who grew up in Vietminh-controlled regions.[20]

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 Vietminh system of education. We then arbitrarily assign the 1955-1960 cohorts and the 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

[20] We offer an additional robustness check that considers whether school-age children residing in provinces that were geographically adjacent to the Vietminh-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 1.3, Panel A (Appendix 1), indicate otherwise, thus lend further support to our results. We also provide another related check that restrict the estimation sample instead to the three provinces that were under Vietminh's control throughout the war. Estimation results (Appendix 1, Table 1.3, Panel B) are qualitatively similar.

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 1.7, Appendix 1).[21]

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 1.4, Appendix 1).[22] 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 various 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)

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

where the term $\sum_{h=1}^{H} C_h * L_j$ represents all the interaction terms between birth cohorts and the dummy variable for Vietminh-controlled provinces, with the reference group being those born between 1924 and 1928. Equation (9) thus generally compares the treatment group with all the

[21] 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 difference-in-difference method works well.

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

other birth cohorts, including the contaminated treatment cohorts. Estimation results, provided in Table 1.5 in Appendix 1 for both boys and girls (and for girls only, Table 1.6), further support the long-term impacts on the treatment group.

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 1.7) as well as expanding the treatment group to include some contaminated treatment years (Panel C, Table 1.7) for the girls' sample. Estimation results are, unsurprisingly statistically weaker, but qualitatively similar.

Internal Migration and Parental Education

Since school-age girls had more school access in Vietminh-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 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 Vietminh-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 Vietminh-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 suggests 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. To offer another measure of the intensity of the war, we also use another variable which is the average number of days of battle. Estimation results provided in Table 1.9 (Appendix 1) are also qualitatively similar.

Other Concerns: Nutrition, International Migration, and Sample Attrition

Our robustness checks up to this point have ruled out a number of different issues that may potentially bias 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 Vietminh-controlled provinces that are not directly connected to the ME movement that can bias our estimates.

For example, Vietminh-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 1.10 (Appendix 1). The treatment effect is marginally statistically significant at the 10 percent level for our first measure of exposure, but statistically significant for our second measure of exposure. However, the estimated impact is negative, suggesting that children residing in Vietminh-controlled provinces had lower nutritional status. This indicates that our estimates are likely conservative.[23]

Another concern is that, school-age children residing in Vietminh-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

[23] 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. Furthermore, 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.

estimates.[24] To check on this hypothesis, we provide estimates on whether individuals are more likely to receive remittance from their relatives living overseas more than 50 years later (i.e., in 1997-1998). Estimation results provided in Table 1.7, 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 tradeoff with doing so is that the sample size is smaller, which can result in weaker estimates. Still, estimation results (Table 1.11, Panel A) are highly statistically significant for several indicators such as completing at least primary education and years of 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 1.11, Column 10), but estimates are not significant for the other indicators.[25]

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

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

[25] 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 to two groups: group 1 consist 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).

(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 1.11, 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.

IV.3. Heterogeneity Analysis

Besides the gender dimension, we further examine the heterogeneous effects of Vietminh's ME program on different ethnic and religious groups. 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 Vietminh's program. Individuals that 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), whose magnitude is more than twice that of the Kinh-Hoa groups. While the small sample size could be a reason for the

lack of statistical significance, it is also likely that ethnic minority groups were faced with more obstacles in further developing their initial educational boost.[26]

The Buddhist group, the Catholic group, and the non-religious groups all seem to benefit from the education policy, which suggest that there might have been no discrimination in Vietminh'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 the 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 are 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 Vietminh'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.[27] 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,

[26] See, e.g., Dang (2012) for a recent investigation of the differences in welfare between ethnic majority and minority groups.

[27] 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 is equal to 1 if main construction of wall is made of concrete, brick, stone or wood, and 0 otherwise. Floor quality is also a dummy variable which is equal to 1 if 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 Vietminh'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 a positive coefficient. One additional year of education caused by the exposure to Vietminh's ME program increases households' durable assets 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).[28] 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 as a result of exposure to Vietminh's ME program during Indochina War helps explain higher income, wealth and expenditure of the individuals' households in 1998, four decades later. This illustrates the

[28] 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 hovers around 8 percent in 1998 (Zhang et al., 2005) and 10 percent for the Asia region (Psacharopoulos and Patrinos, 2004).

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 between 3 and 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 Vietminh'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.[29] 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 for both boys and girls, and 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 Vietminh's ME program one generation later.

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 enrolment in the past decade. This pattern is consistent with the country's stronger-than-average

[29] We include all the children that live outside the household in our estimation samples.

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 the country'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.[30] Vietnam has also reached virtually universal primary school enrolment 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 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

[30] Our calculation based on the data for Figure 1 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.

war), when those born during 1924-1955 reached adulthood. The proportion of men that ever served in the army was estimated to range between 35 and approximately 75 percent, depending on the age cohorts, while the corresponding figure for women was just 4 percent (Teerawichitchainan, 2009).[31] 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 is 68 percent higher than that for sons of fathers with no education or a primary school education (Merli, 2000).[32]

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 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

[31] 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).

[32] 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).

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.

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 Vietminh'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 to 13 percentage points, and their education by an additional 1.5 years of schooling. Vietminh'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 education attainments of the girls' children by 0.3 to 0.5 more years.

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Table 1. Balance Tests for Vietminh-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 Vietminh 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 Vietminh 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 Vietminh-controlled area					Years of exposure to Vietminh				
	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 Vietminh-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 Vietminh-controlled area					Years of exposure to Vietminh				
	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. Sample includes migrants with age of less than 15 in the North and 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 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 5. Heterogeneity Analyses for Girls

	Dummy variable for Vietminh-controlled area					Years of exposure to Vietminh				
	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 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 6. Impacts of Female Education on Household Living Conditions

	Dummy variable for Vietminh-controlled area						Years of exposure to Vietminh					
	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 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 7. Impacts of A Wife's Education on Her Husband's Education

	Dummy variable for Vietminh-controlled area					Years of exposure to Vietminh				
	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 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 8. Impact of A Mother's Education on Her Children's Education

	Dummy variable for Vietminh-controlled area			Years of exposure to Vietminh		
	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 *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 9. Impact of A Mother's Education on Her Children's Education, for Sons vs. Daughters

	Dummy variable for Vietminh-controlled area			Years of exposure to Vietminh		
	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 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. Map of Vietnam under Vietminh and the French Occupation during the First Indochina War, 1946-1954

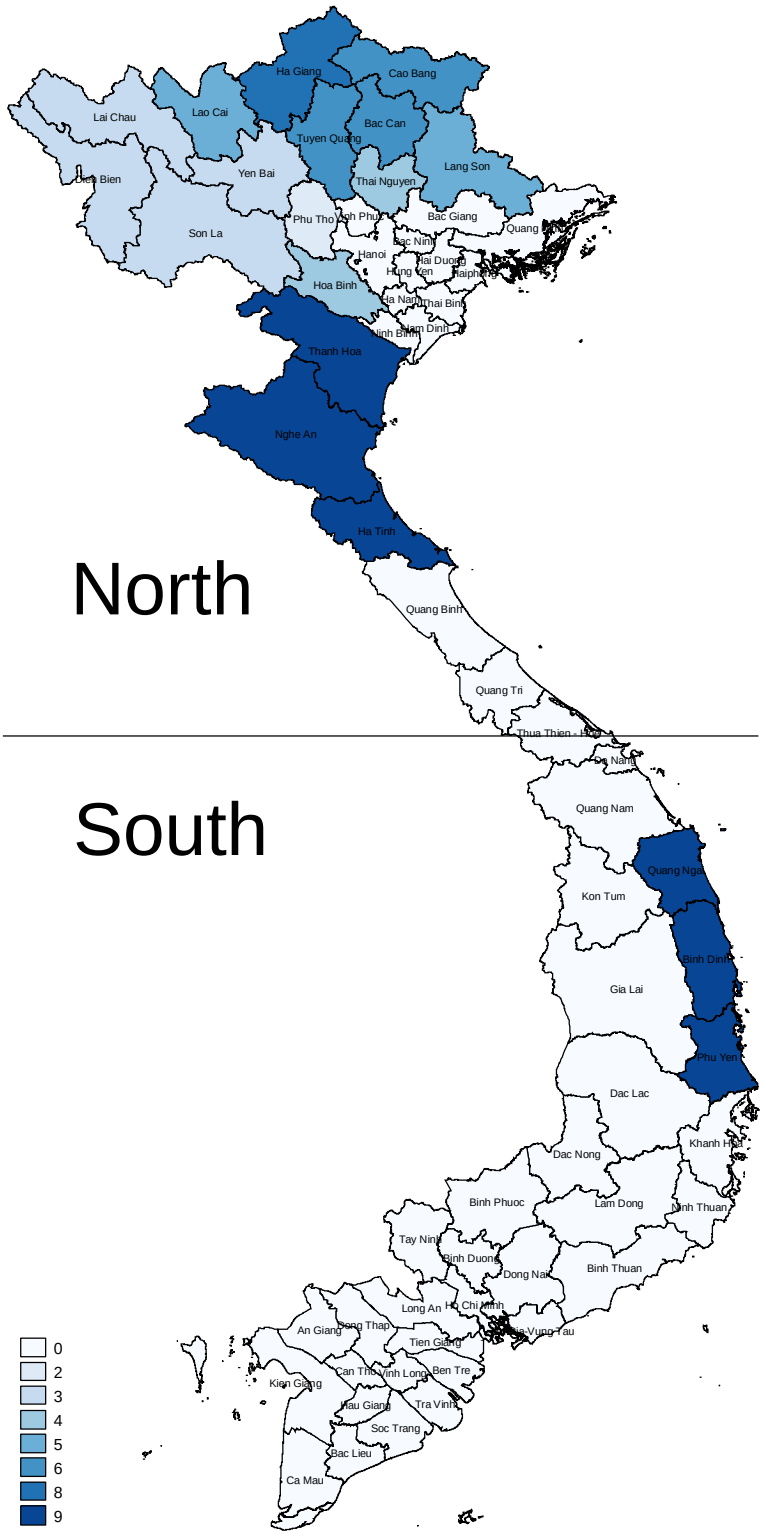


Figure 2. Years of School for Those Born in Vietminh-occupied Provinces versus Those Born in French-occupied Provinces

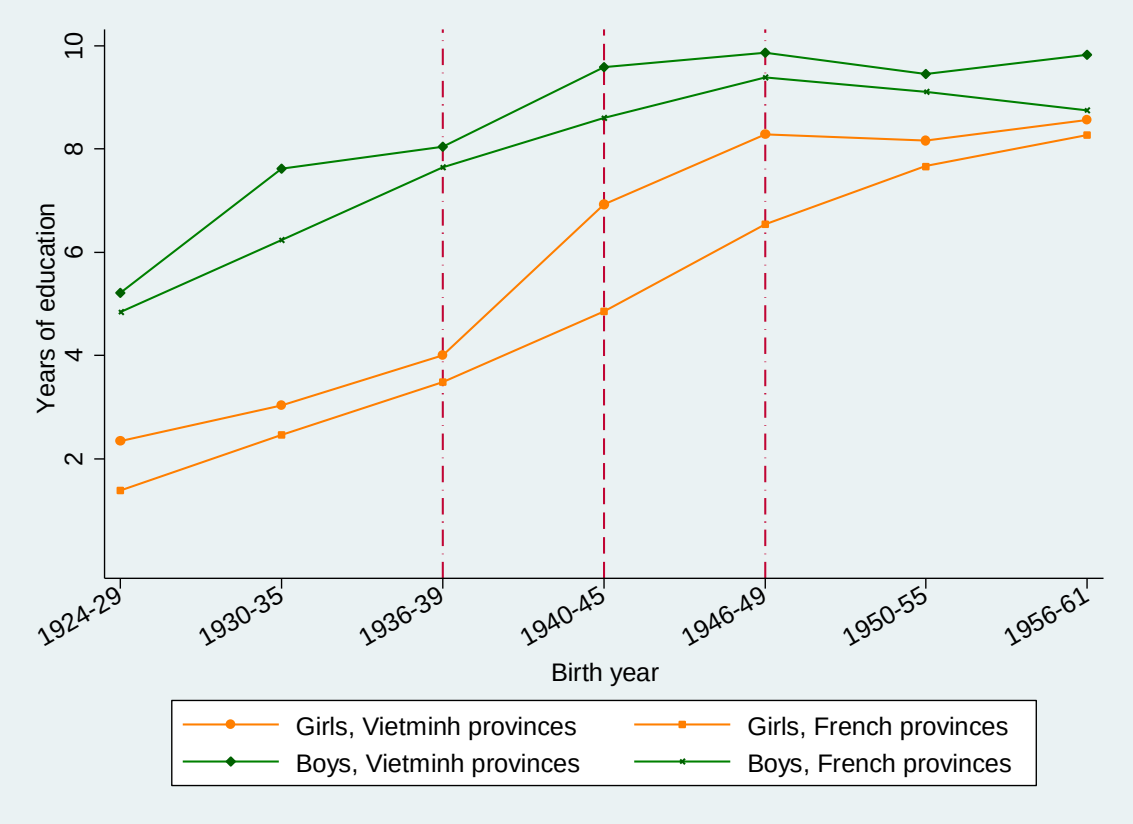


Figure 3. Robustness Checks for Different Combinations of Control Cohorts

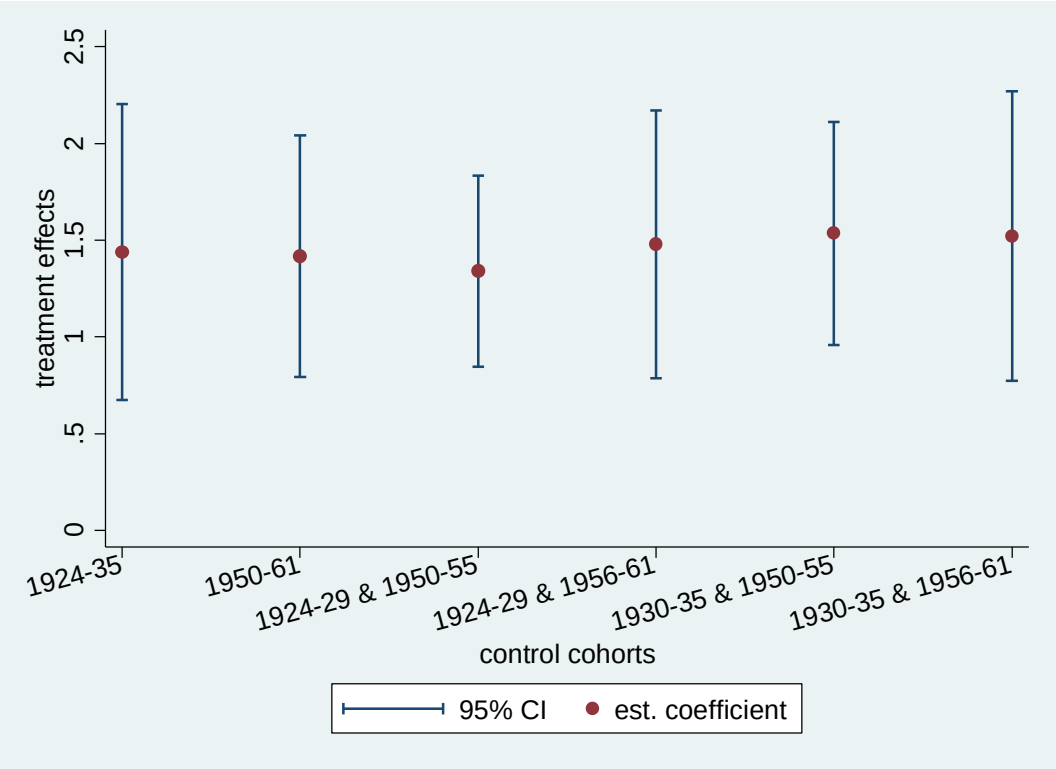


Figure 4. Averaged Years of Schooling of Adult Population vs. Country Income Level

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

Appendix 1. Additional Tables and Figures

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
1927	0	0	0	0
1928	1	0	0	0
1929	2	0	0	0
1930	3	0	1	0
1931	4	0	2	0
1932	5	0	3	0
1933	5	0	4	0
1934	5	0	5	1
1935	5	0	5	2
1936	5	0	5	3
1937	5	1	5	4
1938	5	2	5	5
1939	5	3	5	5
1940	5	4	5	5
1941	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 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. 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, 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 1.3. Robustness Checks with Different Province Samples in North Vietnam, Girls' Sample

	Dummy for Vietminh-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 Vietminh-controlled provinces					
Treated birth years*Vietminh	0.202**	0.192**	0.253***	0.193**	1.437***
	(0.075)	(0.089)	(0.067)	(0.083)	(0.297)
Treated birth years* adj_ Vietminh	-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*Vietminh	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 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 1.4. Robustness Checks for Different Control Cohorts, Girls' Sample

	Dummy variable for Vietminh-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 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, All Population

	Reading Literacy	Math competency	Primary education completion or above	Secondary education completion or above	Years of education
	(1)	(2)	(3)	(4)	(5)
Vietminh*cohort 2933	0.032	0.028	0.025	0.002	0.532
	(0.069)	(0.073)	(0.105)	(0.055)	(0.347)
Vietminh*cohort 3439	0.019	-0.069*	0.005	0.040	0.312
	(0.037)	(0.035)	(0.076)	(0.069)	(0.342)
Vietminh*cohort 4045	0.127***	0.088**	0.122**	0.154**	1.406**
	(0.042)	(0.036)	(0.053)	(0.071)	(0.616)
Vietminh*cohort 4650	0.044	0.053	0.117**	0.075	1.203**
	(0.051)	(0.049)	(0.050)	(0.054)	(0.521)
Vietminh*cohort 5155	0.026	-0.010	-0.018	-0.009	0.148
	(0.059)	(0.075)	(0.044)	(0.046)	(0.397)
Vietminh*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 1.6. 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)
Vietminh*cohort 2933	-0.024	0.003	-0.050	-0.040	0.003
	(0.063)	(0.055)	(0.049)	(0.030)	(0.334)
Vietminh*cohort 3439	-0.016	-0.107	0.003	0.016	-0.009
	(0.063)	(0.065)	(0.075)	(0.080)	(0.446)
Vietminh*cohort 4045	0.173***	0.148**	0.258***	0.225**	1.524***
	(0.051)	(0.058)	(0.071)	(0.093)	(0.427)
Vietminh*cohort 4650	0.045	0.104	0.216***	0.140*	1.535*
	(0.069)	(0.066)	(0.063)	(0.075)	(0.872)
Vietminh*cohort 5155	-0.007	-0.035	-0.004	0.019	-0.173
	(0.069)	(0.099)	(0.053)	(0.051)	(0.575)
Vietminh*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: 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 1.7. Robustness Checks with Contaminated Treatment Cohorts, Girls' Sample

	Dummy variable for Vietminh-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: 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 1.8: Robustness Checks, Controlling for the Number of Battles Fought in Each Province

	Dummy variable for Vietminh-controlled area					Years of exposure to Vietminh				
	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 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. 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: Robustness Checks, Controlling for the Duration of Battles Fought in Each Province

	Dummy variable for Vietminh-controlled area					Years of exposure to Vietminh				
	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 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. 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 1.10. Further Robustness Check, Nutrition Status and Remittances, Girls' Sample

	Dummy variable for Vietminh-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

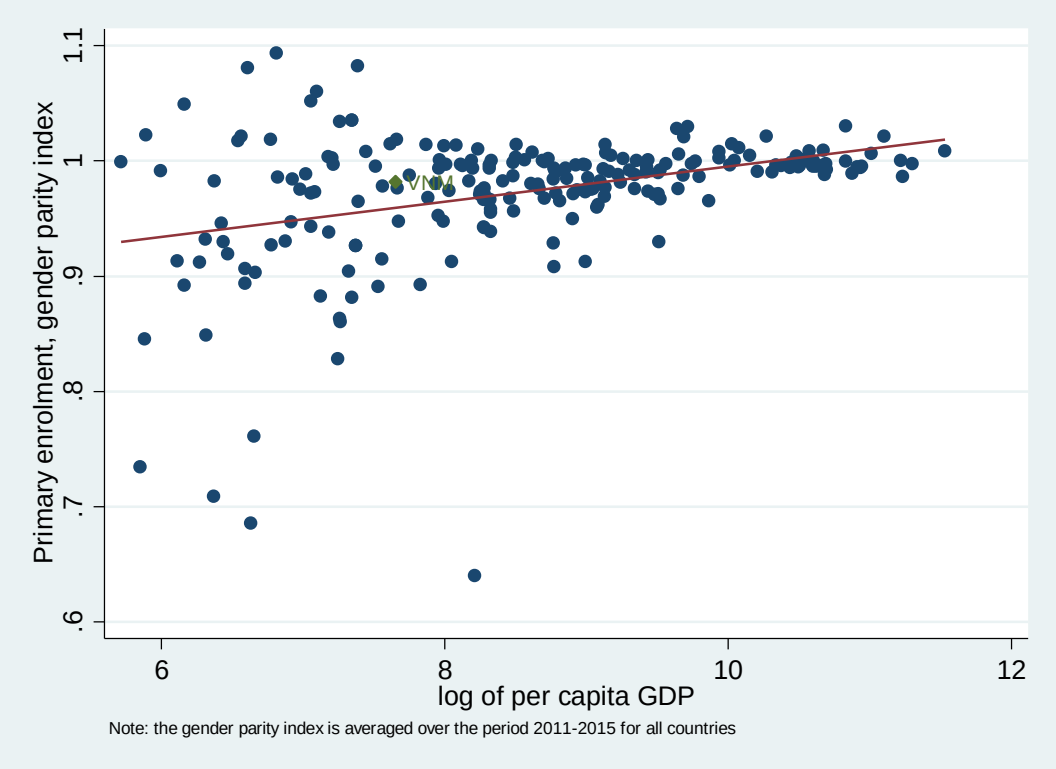
Note: The treatment group consists of the birth cohorts of 1940-45 and the control group consists of the birth cohorts of 1924-1935 and 1950-1961. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religion groups (including Buddhism and Christianity), 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 1.11: Further Robustness Checks, with VLSS 1992-93 and VHLSS 2014, Girls' Sample

	Dummy variable for Vietminh-controlled area					Years of exposure to Vietminh				
	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 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]