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

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I. Introduction
Education has long been known as instrumental in raising human capital (Becker 1962). Raising educational achievements, however, is a challenging and expensive task. Policy makers, particularly in developing countries with resource constraints, oftentimes have to make decisions over competing priorities, such as whether to repair a road, build a hospital, or construct a new school. A good understanding of the impacts of education, especially in the longer term, plays a crucial role in this process.¹

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¹ Indeed, finishing a college degree is a time-consuming undertaking that would require almost 20 years of continuous investment in school for most individuals. An individual’s skill formation can be considered a life cycle process since early-stage investment can facilitate later-stage skills and productivity (Cunha et al. 2006; Cunha and Heckman 2007).
Our study aims to add to the nascent literature on the long-term impacts of education in developing countries. Furthermore, we make contributions in several aspects. First, we study a low-cost and large-scale educational program implemented by the Democratic Republic of Vietnam (DRV) during the First Indochina War (1946–54) and its long-term impacts on education outcomes and household living standards half a century later. Vietnam presents a remarkable case study. Despite its modest position as a lower-middle-income country, the country has recorded better education performance than what may be suggested from its income level, particularly for women. Indeed, its girls’ net secondary enrollment rates caught up with and even overtook those of boys in the past decade, with the former leading the latter by as much as 10 percentage points at the upper secondary level (Dang and Glewwe 2018). Much spotlight in the media has been given to this country’s exemplar performance, but little rigorous evidence has been offered on the driving factors behind this success.²

During the First Indochina War (1946–54), Vietnam’s political system was turned into a bimodal regime, in which parts of the country were occupied by the French while some other parts were under the DRV’s government. This in turn resulted in a unique natural policy experiment where two different education systems were concurrently in operation: the new Vietnamese system provided free, 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. This setup allows us to employ an identification strategy that exploits the temporal variation (i.e., whether an individual was at school age during the war) and the spatial variation (i.e., whether an individual was living in the French-occupied regions or the DRV-occupied regions) of an individual’s exposure to the education program. Incidentally, we also offer the first rigorous impact evaluation of this mass education program.

Second, our study also contributes to an emerging literature that examines the negative effects of war on human capital but mostly focuses on richer countries (see, e.g., Shemyakina 2011; Kesternich et al. 2014; Swee 2015). But in contrast with this literature’s focus on the negative impacts of wars, we offer an investigation into a positive shock to education (supply) during wartime. The positive shock is caused by beneficial exposure to the DRV’s popular education program during the war. Another major difference is that while this literature

² Vietnam’s recent performance on the Programme for International Student Assessment (PISA) is also comparable to those of much richer countries, such as the United States or the United Kingdom. See also, e.g., the Economist (M. I. 2013), the Huffington Post (Bellos 2015), and the Guardian (Ravitch 2015) for recent media coverage of Vietnam’s performance on the PISA. See also Dang et al. (2020) for a recent attempt to disentangle this education phenomenon.
(mostly) studies the generally war-related disruptive effects, we offer a specific investigation of the long-term impacts of schooling policies in the context of the First Indochina War.

Finally, we offer new and interesting evidence on the impacts of education on girls, who received even less attention in the few existing studies (except for the study by Hahn et al. 2018). Girls were traditionally faced with far less school access in Vietnam and still currently are in a number of countries. Indeed, recent estimates by UNESCO (2018) indicate that women account for two-thirds of the 750 million adults who are without basic literacy skills in the world today. Achieving gender equality ranks high as one of the key sustainable development goals supported by the United Nations. We offer an analysis for several welfare outcomes ranging from math and reading literacy to completed education levels and years of schooling.

Our estimation results suggest that girls who resided in DRV-controlled areas during their school age have significantly stronger math and reading literacy levels and higher education achievements, five decades later, than their peers residing in French-controlled areas. The impacts are not limited to a girl’s well-being alone; we also find that impacted girls have higher household living standards. These impacts are, however, not statistically significant for boys, although we cannot reject the null hypothesis of equal impacts between girls and boys at standard significance levels. We examine a number of alternative mechanisms that can potentially explain these results, including unobserved regional trends, different treatment and control cohorts, internal and international migration, confounding impacts of war, parental education, and sample attrition. Estimation results, however, remain strongly statistically significant and robust across these specifications.

Our note of caution, though, is that these program impacts occurred during a time of war. While this unique setting provides an interesting background for our study, it is unclear whether a similar large-scale education program may be readily replicated elsewhere.

Just a handful of studies currently exist on the long-term impacts of education in a developing country context. We briefly review the most related studies

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3 See https://sustainabledevelopment.un.org/sdgs.
4 We mostly focus in this paper on the longer-term impacts of school access and education achievement in the context of a developing country. See Glewwe and Muralidharan (2016) for a recent review of several studies that investigate the short-term (i.e., less than 5 years) impacts of school policies for developing countries. In richer countries’ context, a related literature exists on the impacts of compulsory school laws on different outcomes, such as returns to education (Oreopoulos 2006), health outcomes (Clark and Royer 2013), fertility behavior (McCrary and Royer 2011), and domestic violence (Erten and Keskin 2018). Also, as discussed later, a key difference with our study is the strong and voluntary participation by households in a mass education program supported by the government.
here. Examining a large-scale school construction program in Indonesia in the mid- to late 1970s and identifying a child’s exposure to the program through his (her) date of birth and the region of birth, Duflo (2001) estimates that the program resulted in an average increase of 0.1–0.2 years of education around two decades later. Analyzing longitudinal data on the first students in colonial schools in colonial times and their direct descendants and extended families in Benin, Wantchekon et al. (2015) find that Beninese who benefited from access to the first schools established were 96% and 10%, respectively, more likely to attain primary education or at least secondary education, respectively.

Most recently, Caicedo (2018) find that the Jesuit order’s missionaries’ education program improved education attainment in several South American countries by 10%–15% 250 years later. Chen, Kung, and Ma (2017) observe a persistent effect of imperial China’s civil examination system on human capital outcomes, where an additional highest-qualification holder per 10,000 people under this system during the Ming-Qing period (1368–1905) leads to an additional 0.7 years of schooling in the present day. Hahn et al. (2018) examine the long-term effects of a free stipend program for rural Bangladeshi women and estimate the stipend to increase the years of education for eligible girls between 14% and 25%. Studying the related but opposite phenomenon of school disruption during China’s Cultural Revolution in 1966–77, during which most schools in urban China ceased operation, Meng and Gregory (2002) find that the affected cohorts were less than half as likely to possess a university degree 20 years later.

This paper consists of five sections. In section II, we provide a brief overview of the natural policy experiment, where two educational systems concurrently existed under the colonial French and under the DRV, including a detailed description of the mass education program. We discuss the analytical model, our identification strategy, and the various data sets we analyze in section III before presenting estimation results and a number of robustness checks and heterogeneity analysis in section IV. We then offer further reflections on other issues and conclude in section V.

Caicedo (2018) offers a review of the literature on the long-term economic effects of missions. See also a recent study by Castello-Climent, Chaudhary, and Mukhopadhyay (2018), which employs more aggregated data (e.g., district-level share of education rather than household-level education outcomes) to study the impacts of education.

A related study by Zhang (2018) finds high school completion to increase one’s probability of working off-farm by 17% and one’s probability of working in a white-collar job by 13% a decade later. Yet it appears there may be no consensus on school disruption caused by the Chinese Cultural Revolution. For example, analyzing data on twins, Zhang, Liu, and Yung (2007) did not find the Chinese Cultural Revolution to have significantly negative effects on the returns to schooling.
II. Country Context and Mass Education Program

A. Education in the First Half of the 20th Century

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 either the children of local French colonists or a small group of local elites who would serve as civil servants. As such, most Vietnamese were illiterate in this period. When Vietnam won independence in 1945, the illiteracy rate was estimated to range between 80% (Le 1955) and 95% (Pham 1995). The enrollment rate of any age cohort at Franco-Vietnamese schools was estimated to be no more than 10% throughout the colonial period (Kelly 1982).

In this period, girls were traditionally excluded from attending school, with the female illiteracy rate estimated to be almost 100% before 1945 (Nguyen 1996). This occurrence was influenced by Confucian values imbued in the traditional Vietnamese society, where formal education for girls was considered unimportant (Huu Ngoc 1996; Tran 2012). While the French established public schools for girls at the beginning of the 20th century (Tran 2010), there was an unsurprisingly large gender gap in school enrollment in this period. The percentage of female students was less than 15% at the primary and lower secondary levels and approximately 10% at the upper secondary level over the period 1932–44 (General Statistics Office 2004). This stands in sharp contrast to the current reverse gender gap in enrollment discussed earlier.

Immediately after the declaration of independence in 1945, the new Government of Vietnam (known as the DRV in this period) offered a new education system with a new curriculum. The DRV also launched a mass education movement called Bình dân học vụ, or Mass Education (ME), and issued a degree stipulating mandatory and free attendance in illiteracy-eradicating classes for all (men and women) and literacy for everyone older than age 8 within 1 year. As such, school-age children residing under the DRV’s regime had the opportunity to go to school at barely any cost. But perhaps more importantly, 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

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7 One scholar estimated that there were about 150,000 elementary school students, 5,637 middle school students, and 553 high school students out of a total population of 20 million people for Vietnam around 1940 (Nguyen 1970). This elitist educational system appears to simply serve the main goal of bureaucratic recruitment, as seen with those under the preceding feudal systems in Vietnam and elsewhere (see, e.g., Le 1955; Woodside 2006). Notably, the numbers of enrolled students were estimated to increase (from the prewar period 1939–40 to postwar 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).
system under the colonial French, where admission was mostly available to privileged boys, and shares certain similarities with the present-day system, where (almost all) public schools are widely accessible and heavily subsidized by the government.

The movement was strongly supported by the public and quickly spread across the country, especially in North and Central Vietnam. It was estimated to result in between more than 2.5 million (Marr 1984) and 10 million new literates, which was considered unprecedented in the history of Southeast Asian countries (Woodside 1983). The Vietnamese government also reports that more than 90% of the adult population in North Vietnam were literate by the end of 1958 (Ministry of Education 1995).

B. Education during the First Indochina War

In 1946, the French army returned to Vietnam, which started the First Indochina War. During the war, the country was divided into French-controlled and DRV-controlled areas. The DRV continued to promote the ME movement in its areas. In response to the changing circumstances, the French also implemented a number of measures to reform their previous elitist education system in an attempt to make it more accessible to the population and maintain their cultural influence. In particular, these measures include reserving a larger education budget, establishing more schools, and recruiting more education staffs (Nguyen 2013). French schools also began to admit a larger number of children of Vietnamese and other non-French nationalities (Vasavakul 1994). Yet the French did not offer any program that is similar to the ME program in their areas during the war. This bimodal system lasted until the end of the war in 1954, when the DRV gained control of North Vietnam and could implement the ME movement for the entire North Vietnam. Consequently, the Indochina War offers a unique natural experiment for us to study the impacts of education access under two independent and diametrically different education systems.

We employ two different identification approaches to evaluate the long-term impacts of the ME program. Our first identification approach relies on whether the (average) school-age child had access to school, which depends on whether her residence area was DRV controlled or French controlled. Since school-age children residing longer in DRV-controlled areas might also have had more

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* The ME movement in South Vietnam during the Indochina War, however, was not as strong as in North Vietnam. South Vietnam followed a different model of education for the subsequent two decades following the end of the Indochina War (i.e., during 1954–75) that are more similar to the US system. See Vasavakul (1994) for a comparative analysis of schools under the two systems during 1945–65.
school access, our second identification approach makes use of the differential duration of exposure to school access that children had. Figure 1 plots the number of years that each province in Vietnam was under the DRV’s control up to the end of 1954. The DRV completely controlled three provinces in North Vietnam—Thanh Hoa, Nghe An, and Ha Tinh provinces—over a period of 9 years from the beginning to the end of the war. These three provinces are observed to be a traditional strong base for the DRV government, and their geographical conditions are favorable for self-defense (Ngo 2001). The DRV also gained control of 12 additional provinces in North Vietnam from around the middle of the war. We return to more discussion in section III.

III. Analytical Model

A. Empirical Model and Identification Strategies

We estimate the following empirical model, where the impacts of education policies are net impacts (i.e., while individuals can drop out of school as children, they may go back to school as adults), thus offering estimates that are most practically relevant to policy makers.\(^9\) We can apply a difference-in-differences strategy to estimate this model:

\[
O_{ijt} = \alpha + \beta (ME_t \times L_j) + \gamma X_{ijt} + \beta p_j + \tau_t + \varepsilon_{ijt},
\]  

(1)

where the dependent variable \(O_{ijt}\) 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 that equals 1 if an individual can read a short passage in Vietnamese without any difficulty and 0 otherwise. Math literacy is similarly represented by a dummy variable with a value of 1 for an individual who can do some basic calculations without difficulty and 0 otherwise. Examples of these basic calculations include addition (e.g., add 19 and 12), subtraction (e.g., subtract 16 from 45), multiplication (e.g., multiply 13 and 7), and division (e.g., divide 75 and 5). (Also see World Bank [2001] for more details on these tests). Since students usually had to take an examination to complete a degree at the end of each school level (before 2000; see, e.g., Pham 1995), achieving a school degree could be an important landmark for one’s education.\(^{10}\) As such, we also include in the outcome

\(^9\) We discuss in a working paper (Dang, Hoang, and Nguyen 2018) a simple theoretical model based on Glewwe and Kremer (2006) that suggests a direct and estimable linkage between education policies and children’s education outcomes.

\(^{10}\) We focus mostly on the quantity of education and long-term life outcomes, such as household living standards, in this paper, since like most household consumption surveys, the VLSS and VHLSS do not offer test data on the quality of education. But note that estimation results in Dang and Glewwe (2018) point to stronger than average performance on international standardized test scores for Vietnamese students compared with other countries at a similar income level.
Figure 1. Map of Vietnam under the DRV and the French Occupation during the First Indochina War (1946–54). Shading indicates the number of years under the DRV’s occupation. Country borders or names do not necessarily reflect the World Bank Group’s official position. The dividing line for the north and the south of Vietnam after the First Indochina War was established by the Geneva Accords of 1954 and is symbolically drawn at the 17th parallel north. This map is for illustrative purposes and does not imply the expression of any opinion on the part of the World Bank concerning the legal status of any country or territory or concerning the delimitation of frontiers or boundaries. A color version of this figure is available online.
variable $O_{ij}$ dummy variables that indicate whether the individual accomplished a primary school degree or a lower secondary school degree (which requires finishing 5 and 9 years of schooling, respectively).

The control variables $X_{ij}$ include dummy variables indicating gender, ethnicity, religion (i.e., whether the individual is Buddhist, is Christian, or has other religion beliefs), and province of birth fixed effects ($b_p$).\footnote{Using the current province of residence fixed effects yields similar results (see table A.13). We return to further robustness checks with migration in sec. IV.B.} Furthermore, we exclude from our estimation sample 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 (1) with the birth year fixed effects ($\tau_t$).

We will focus our attention on the treatment coefficient $\beta$. Our hypothesis is that the ME program helps increase education attainment for individuals who resided in DRV-controlled areas (i.e., $L_j = 1$) during their school ages more than their peers in French-controlled areas (i.e., $L_j = 0$). As discussed earlier, we offer two measures for the treatment variable (interaction term) $ME_i \times L_j$. Our first measure assigns $ME_i$ as a dummy variable that equals 1 if individual $i$ was born between 1940 and 1945 (treatment period) and equals 0 if individual $i$ was born before the war in 1924–35 (control period). These periods are depicted in figure 2. Individuals in the treatment period would be of prime school age (i.e., 6–11 years old) during the war, while individuals in the control period would have attended schools that were still under the French system. This is our preferred measure for the main estimation results unless specified otherwise.

We also provide robustness checks when we either disaggregate the control group or change it substantially. In this approach, we leave out of the estimation sample the cohorts who were born during 1936–39, who were partially exposed to increased school access (i.e., the partially treated cohorts) if they were living in DRV-controlled areas. But we also provide robustness checks when we include these cohorts; we return to more discussion in section IV.B.

For the second measure, we explicitly take into account the differential length of exposure to the ME program that school-age children residing in DRV-controlled areas might have benefited from increased school access, which can vary depending on their age. Table 1 provides an illustration of this exposure, where a child born in 1940 or later in DRV-controlled areas (col. 2) would have had 5 years of exposure. The number of years of exposure is similarly adjusted for children born in the provinces in North Vietnam that DRV gained control from around the middle of the war (e.g., two examples are given in cols. 4 and 5). The reason we restrict the number of years of exposure to 5 years is that...
children would likely drop out of school if they miss the crucial years of schooling at the primary school level. Various education studies suggest that late school enrollment is observed to lead to more school dropouts in a number of countries around the world (Wils 2004; UIS 2005; Chen 2015; No et al. 2016). Our second measure thus includes all the partially treated cohorts in the estimation sample. This would likely weaken the impacts of the ME program but may yield smaller standard errors, given somewhat larger estimation sample sizes.

For robustness checks, we also tried a third and hybrid measure, which assigns to all school-age children residing in province $j$ the number of years this province was under the DRV’s occupation during the war. This hybrid model would likely provide statistically weaker results than the first measure but stronger results than the second measure. We return to discussing estimation results in section IV.

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

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12 Varying this restriction on the numbers of years of exposure (e.g., 3 or 4 years) yields similar results (see table A.14).
B. Data

The main data set that we analyze is the 1997–98 round of the Vietnam Living Standards Survey (VLSS), which was implemented by Vietnam’s General Statistics Office, 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 these children were living in the household at the time of the survey. This survey has good data quality and has been widely analyzed by the Government of Vietnam, international organizations, and academic researchers. A particularly useful feature for our analysis is that this survey offers information on respondents’ date of birth, place of birth, whether they have moved from their birth place, and 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: (1) the 1992–93 round of the VLSS, which was the first household consumption survey that was implemented for the country, and (2) the 2014 Vietnam Household Living Standards Survey (VHLSS).
There are, however, limitations with these surveys. While there is a panel component between most of the 1992–93 VLSS and the 1997–98 VLSS, the former has a smaller sample size and collects data on only around 4,800 households. In addition, we discuss later some potential issues with data quality of certain variables in the 1992–93 VLSS. 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 VLSS and VHLSS data, we also analyze UNESCO’s World Inequality Database on Education (UNESCO 2017) and the World Bank’s World Development Indicators database (World Bank 2017).

But importantly, we generate for the first time several new variables related to the First Indochina War that help construct our treatment measures. In particular, since each province in Vietnam produced a book on the history of its local communist party and its army, we manually glean data from these books to construct these variables. The most interesting variable is the years of the DRV’s occupation at the province level, which allows us to construct different measures of exposure to the ME program, as discussed earlier: whether a school-age child was exposed to it and the durations of exposure (at both the individual and the province levels). Furthermore, we are also able to construct a variable that indicates the number of battles that were fought in each province, which will be employed for a robustness check on the potential confounding effects of the war. For example, we list all the battles occurring in each district within each province and then count the total number of battles at the province level for each province in northern Vietnam. The number of battles per province ranges from 4 to 137 and averages 37.9. We use more than 30 books that have been published over the past two decades with various publishers; these books are listed in detail in appendix B (apps. A and B are available online).

IV. Long-Run Impacts of School Policies
A. Estimation Results
A key assumption underlying the difference-in-differences model is the parallel trend assumption, whereby the difference in the outcome variable between the treatment group and the control group would have remained essentially the same in the absence of treatment. Without this assumption, the change for the treatment group may not be attributed to the treatment, but it can result from other time-varying unobservable factors. While it would be impossible to check this assumption directly in the treatment period (since the treatment could
change the outcome), we can check it in the periods before the treatment (the
pretrends) given the available data.

We check the parallel pretrend assumption separately for men and women in figure 2, which plots the completed years of education by birth cohort for those who were born in DRV-occupied provinces versus those who were born in French-occupied provinces. This figure shows an (approximately) parallel pretrend in the number of years of education for the female control cohorts (light gray lines), including from the cohorts of 1924–29 compared with those of 1930–35. This holds regardless of whether they were born in DRV-occupied provinces (solid light gray line) or French-occupied provinces (dashed light gray line). The treatment years, in contrast, saw a considerable increase of two more years of education for the female cohorts of 1940–45 born in DRV-occupied provinces compared with their peers in French-occupied provinces. This increase also holds to some extent for the female cohorts of 1946–49 before tapering off for younger cohorts born in 1950–55 or 1956–61. For all the male birth cohorts, the difference between those born in DRV-occupied provinces and born in French-occupied provinces appears negligible. We return to discuss a number of related robustness checks in section IV.B.13

We provide in table 2 the estimation results—based on equation (1) for the first measure of exposure (i.e., by interacting a dummy variable for DRV-controlled areas with the treatment period)—for the whole population (panel A), girls (panel B), and boys (panel C). The treatment effects are positive for the different indicators of educational attainment. Specifically, school-age children residing in DRV-controlled areas had an 8–15 percentage point higher probability of achieving reading and math literacy and completing at least primary education and secondary education (panel A, cols. 1–4); these children also accomplished one more year of schooling (panel A, col. 5). However, these estimates are statistically significant only for math literacy and are marginally statistically significant for reading literacy at the 10% level; the corresponding 95% confidence intervals (CIs) are 2–14 and 0–20 percentage points, respectively.

We further disaggregate the gains in educational attainment between boys and girls. School-age girls living in DRV-controlled areas during the war had a 17–29 percentage point higher probability of achieving reading and math literacy and completing at least primary education and secondary education

13 We offer an additional robustness check for the parallel assumption that combines the matching technique with the difference-in-differences model (MDID) (Blundell and Dias 2009). The MDID model employs a weaker parallel trend assumption that is conditional on observed characteristics instead of all (observed and unobserved) characteristics. Estimation results (table A.8) are qualitatively similar to the main estimation results in table 2.
|                       | Reading Literacy | Math Competency | Primary Education Completion or Above | Secondary Education Completion or Above | Years of Education |
|-----------------------|-----------------|-----------------|---------------------------------------|----------------------------------------|-------------------|
|                       | (1)             | (2)             | (3)                                   | (4)                                    | (5)               |
| **A. Whole Sample**   |                 |                 |                                       |                                        |                   |
| Treated               | .100*           | .080**          | .127                                  | .146                                   | 1.032             |
|                       | (.052)          | (.030)          | (.080)                                | (.102)                                 | (.712)            |
| N                     | 1,259           | 1,259           | 1,260                                 | 1,260                                  | 1,260             |
| Adjusted $R^2$        | .335            | .354            | .356                                  | .299                                   | .435              |
| Mean of dependent variable | .593         | .538            | .403                                  | .244                                   | 4.911             |
| **B. Female**         |                 |                 |                                       |                                        |                   |
| Treated               | .182***         | .171***         | .293***                               | .237**                                 | 1.438***          |
|                       | (.058)          | (.061)          | (.067)                                | (.109)                                 | (.390)            |
| N                     | 699             | 699             | 700                                   | 700                                    | 700               |
| Adjusted $R^2$        | .263            | .259            | .281                                  | .206                                   | .342              |
| Mean of dependent variable | .409         | .339            | .217                                  | .113                                   | 3.227             |
| **C. Male**           |                 |                 |                                       |                                        |                   |
| Treated               | .045            | -.002           | -.060                                 | .011                                   | .461              |
|                       | (.084)          | (.085)          | (.107)                                | (.097)                                 | (1.164)           |
| N                     | 560             | 560             | 560                                   | 560                                    | 560               |
| Adjusted $R^2$        | .141            | .145            | .178                                  | .257                                   | .245              |
| Mean of dependent variable | .823         | .786            | .636                                  | .407                                   | 7.016             |
| p-value for testing equality of treatment effects between females and males | .174 | .077 | .035 | .135 | .721 |

**Note.** We employ the first measure of exposure whereby the treatment group consists of individuals who belong to the birth cohorts of 1940–45 and who resided in DRV-controlled provinces during their school age; the control group consists of the birth cohorts of 1924–35. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religious 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 individuals who had migrated when they were younger than age 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune).  
* $p < .10$.  
** $p < .05$.  
*** $p < .01$.  

They also attained an additional 1.4 years of schooling compared with their peers in French-controlled areas (panel B, col. 5; 95% CI, 0.7–2.2), which is a sizable increase of 44% from the average 3.2 years of schooling in the pretreatment period. Notably, all these impacts are strongly statistically significant. However, estimation results for school-age boys (panel C) are not statistically significant for any measure of educational attainment.

We also provide formal t-tests for equality of the treatment effects between girls and boys in table 2 (last row), which suggest that these effects are not statistically significantly different between the two genders except for the probability of completing primary school or higher. It appears that the estimates for boys are noisier than those for girls. We will thus focus more on the girls’ sample in the rest of the paper and will mostly present results for boys in appendix A.

The results are qualitatively similar for the second measure of exposure, with one more year of exposure to the ME program increasing the probability of accomplishing math competency and completing secondary education or higher by 3–5 percentage points as well as 0.3 more years of schooling for girls (table 3, panel B). However, estimates become somewhat statistically weaker for girls. In particular, the probability of accomplishing math competency and the probability of completing secondary education or higher becomes statistically insignificant for girls (panel B, cols. 2, 4), yet estimation results for boys and girls together are somewhat more statistically significant (panel A, cols. 1, 3, 5). These mixed results are consistent with our earlier discussion that the second measure may provide more conservative estimates of the impacts of the ME program than the first measure, but it can also offer more efficient estimates because of the larger sample sizes.

B. Robustness Checks and Heterogeneity Analysis

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

1. Implementation of ME Program in North Vietnam versus South Vietnam

As discussed earlier, although three provinces in South Vietnam were completely occupied by the DRV during the war, the ME program was hardly implemented in those provinces. This offers us an important falsification test for the impacts of the ME program, where our hypothesis is that a weak or no implementation of the ME program would result in no effect—or even a negative

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14 The corresponding 95% CIs for these estimates are 0.3–5.7, 2.5–7.9, and 0.1–0.4.
|                  | Reading Literacy (1) | Math Competency (2) | Primary Education Completion or Above (3) | Secondary Education Completion or Above (4) | Years of Education (5) |
|------------------|----------------------|---------------------|------------------------------------------|---------------------------------------------|------------------------|
| **A. Whole Sample** |                      |                     |                                          |                                             |                        |
| Treated          | .024**               | .017**              | .030**                                   | .019                                        | 221**                  |
|                  | (.009)               | (.007)              | (.011)                                   | (.016)                                      | (.097)                 |
| N                | 1,592                | 1,592               | 1,594                                    | 1,594                                       | 1,594                  |
| Adjusted R²      | .340                 | .369                | .349                                     | .289                                        | .429                   |
| Mean of dependent variable | .604               | .548                | .412                                     | .246                                        | 5.035                  |
| **B. Female**    |                      |                     |                                          |                                             |                        |
| Treated          | .030**               | .023                | .052***                                  | .028                                        | 253***                 |
|                  | (.014)               | (.013)              | (.014)                                   | (.018)                                      | (.080)                 |
| N                | 877                  | 877                 | 878                                      | 878                                         | 878                    |
| Adjusted R²      | .233                 | .241                | .241                                     | .173                                        | .307                   |
| Mean of dependent variable | .413               | .338                | .218                                     | .108                                        | 3.298                  |
| **C. Male**      |                      |                     |                                          |                                             |                        |
| Treated          | .025                 | .017                | .012                                     | .006                                        | .191                   |
|                  | (.016)               | (.016)              | (.017)                                   | (.017)                                      | (.177)                 |
| N                | 715                  | 715                 | 716                                      | 716                                         | 716                    |
| Adjusted R²      | .159                 | .160                | .174                                     | .235                                        | .246                   |
| Mean of dependent variable | .839               | .806                | .651                                     | .415                                        | 7.165                  |

**Note.** We employ the second measure of exposure whereby treatment is defined as the number of years of exposure to the ME program for individuals who belong to the birth cohorts of 1940–45 and who resided in DRV-controlled provinces in their school age; the control group consists of the birth cohorts of 1924–35. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religious 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 age 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune).

* * p < .10.
** ** p < .05.
*** *** p < .01.
effect—on school-age children residing in these provinces. Put differently, in the absence of the ME program, girls may have attained no more—or even less—education achievement. Indeed, table A.1 (tables A.1–A.15 are available online) shows that the ME program has either no statistically significant impacts or some negative impacts on the educational attainment of school-age children living in DRV-controlled areas in South Vietnam, except for the single case of completing primary education for boys. The finding generally supports our hypothesis that it is the ME program—rather than other DRV-related factors (e.g., specific political regime or idealism) or unobserved regional trends—that resulted in better long-term educational outcomes for those who grew up in DRV-controlled regions.

We also offer another robustness check that considers whether school-age children residing in provinces that were geographically adjacent to the DRV-controlled provinces have better long-term education outcomes. If they do, this would suggest that the beneficial impacts of the ME program may have been caused by some other (unobserved) factors that were not related to the ME program. Estimation results (table A.2, panel A) indicate otherwise, thus lending further support to our results. We also provide another related check that restricts the estimation sample instead to the three provinces that were under the DRV’s control throughout the war. Estimation results (table A.2, panel B) are qualitatively similar.

2. Different Birth Cohorts

Both figure 2 and our estimation results in table 2 (and table 3) point to the beneficial and statistically significant long-term impacts of the ME program on girls who were of school age during the war. We also control for birth year fixed effects when producing these results. But to further check whether these results may be driven by other unobserved birth-cohorts-specific factors beyond the birth year fixed effects, we offer three additional sets of robustness checks.

First, we conduct a falsification test by restricting our estimation sample to girls who were born between 1955 and 1966 (i.e., after the First Indochina War) and enjoyed the same access to school under the DRV system of education. We then arbitrarily assign the 1955–60 cohorts and the 1961–66 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 panel A of table 4 indeed support this hypothesis, where all five measures of educational attainment—for both measures of exposure to the ME program—show statistically insignificant impacts. Another falsification test—where we assign the cohorts of 1936–39 to the treatment group instead, keeping the control group the same as in table 2 (i.e., cohorts 1924–35)—provides qualitatively similar results (table A.3).
|                                      | Reading Literacy (1) | Math Competency (2) | Primary Education Completion or Above (3) | Secondary Education Completion or Above (4) | Years of Education (5) |
|--------------------------------------|----------------------|---------------------|--------------------------------------------|---------------------------------------------|------------------------|
| **A. Falsification Tests**            |                      |                     |                                            |                                             |                        |
| Treated                              | .001                 | .031                | −.017                                      | −.022                                       | .326                   |
|                                      | (.049)               | (.045)              | (.047)                                     | (.041)                                      | (.294)                 |
| N                                    | 1,087                | 1,087               | 1,084                                      | 1,084                                       | 1,084                  |
| Adjusted $R^2$                       | .273                 | .233                | .243                                       | .225                                        | .298                   |
| Mean of dependent variable           | .925                 | .893                | .864                                       | .673                                        | 8.408                  |
| **B. Sample Includes Migrants Younger than Age 15** |                      |                     |                                            |                                             |                        |
| Treated                              | .130*                | .128*               | .264***                                    | .248**                                      | 1.185***               |
|                                      | (.064)               | (.070)              | (.062)                                     | (.096)                                      | (.357)                 |
| N                                    | 776                  | 776                 | 777                                        | 777                                         | 777                    |
| Adjusted $R^2$                       | .272                 | .260                | .280                                       | .196                                        | .332                   |
| Mean of dependent variable           | .428                 | .357                | .237                                       | .125                                        | 3.420                  |
### C. Adjusting for Number of Years of Exposure with Migration Age

|                | Treated | N     | Adjusted $R^2$ | Mean of dependent variable |
|----------------|---------|-------|----------------|----------------------------|
|                |         |       |                |                            |
| Treated        | .023*   | .018  | .047***        | .300                       |
|                | (.013)  | (.012)| (.014)         | (.018)                     |
| N              | 974     | 974   | 975            | 975                        |
| Adjusted $R^2$ | .238    | .237  | .245           | .164                       |
| Mean of dep. var | .431    | .357  | .236           | .120                       |

### D. Controlling for Number of Years Residing in Current Province since Last Move

|                | Treated | N     | Adjusted $R^2$ | Mean of dependent variable |
|----------------|---------|-------|----------------|----------------------------|
|                |         |       |                |                            |
| Treated        | .168**  | .153**| .263***        | .218*                      |
|                | (.067)  | (.071)| (.074)         | (.107)                     |
| N              | 699     | 699   | 700            | 700                        |
| Adjusted $R^2$ | .264    | .261  | .294           | .214                       |
| Mean of dep. var | .409    | .339  | .217           | .113                       |

### E. Adding Parental Education

|                | Treated | N     | Adjusted $R^2$ | Mean of dependent variable |
|----------------|---------|-------|----------------|----------------------------|
|                |         |       |                |                            |
| Treated        | .203**  | .222**| .380***        | .332**                     |
|                | (.081)  | (.095)| (.093)         | (.134)                     |
| N              | 348     | 348   | 348            | 348                        |
| Adjusted $R^2$ | .224    | .231  | .326           | .243                       |
| Mean of dep. var | .506    | .428  | .276           | .155                       |

**Note.** For panel A, we employ the first measure of exposure whereby the treatment group is randomly assigned to the birth cohorts of 1961–66 and consists of individuals who resided in DRV-controlled provinces in their school age; the control group consists of the birth cohorts of 1955–60. For all other panels, the treatment group consists of individuals who belong to the birth cohorts of 1940–45 and who resided in DRV-controlled provinces in their school age; the control group consists of the birth cohorts of 1924–35. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religious 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 age 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune).

* $p < .10$.
** $p < .05$.
*** $p < .01$. 
Second, instead of grouping together as the control group all those who were born during the pretreatment period 1924–35, we break this control group into four different and smaller control groups that are composed of those who were born in the periods 1924–29, 1930–35, 1919–24, and 1919–35. Although the estimation sample sizes were strongly reduced, estimates remain strongly statistically significant for this indicator of education achievement and other indicators as well. Figure 3 plots the estimation results and their 95% CIs for the completed years of schooling and shows that estimation results are still qualitatively similar, with the estimated impacts ranging from 1.2 to 1.8 more years of schooling, depending on the specific control cohort. Furthermore, we keep fixed the treatment group and vary the control group in various ways, either restricting it to prewar or postwar (posttreatment) periods or using different combinations of smaller birth cohorts in both periods. Estimation results (fig. A.2; figs. A.1–A.3 are available online) remain qualitatively similar.

Finally, following Duflo (2001), we estimate a more general version of equation (1):

$$O_{ijt} = \alpha + \beta_{b} \left( \sum_{h=1}^{H} C_{h} \times L_{j} \right) + \gamma X_{ijt} + b_{p} + \tau_{t} + \epsilon_{ijt},$$

where the term $\sum_{h=1}^{H} C_{h} \times L_{j}$ represents all the interaction terms between birth cohorts and the dummy variable for DRV-controlled provinces, with the reference
group being those born between 1924 and 1928. Equation (2) thus generally compares the treatment group with all the other birth cohorts, including the partially treated cohorts. We expect the treatment cohort of 1940–45 to benefit the most from the ME program. Indeed, estimation results further support this result for girls (table A.4) but not for boys (table A.5). Further analysis that adds to the control groups other cohorts, especially the postwar cohorts 1950–61, are provided in Dang et al. (2018).

3. Internal Migration and Parental Education
Since school-age girls had more school access in DRV-controlled provinces than in French-controlled provinces, migration from the former to the latter, or vice versa, would likely reduce the impacts of the ME program through measurement errors with exposure. In other words, including these migrants in our estimation sample 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 becomes smaller across almost all indicators of education achievement (table 4, panel B). For example, compared with table 2, school-age girls living in DRV-controlled area were 5 percentage points less likely to achieve reading literacy (col. 1) and obtained 0.3 fewer years of schooling (col. 5). Furthermore, becomes statistically weaker. Since most city dwellers in French-controlled areas migrated to Vietminh-controlled rural areas as the war broke out in 1946 (Turley 1975; Nguyen 2006; Goscha 2013), school-age children born in the former areas were also more likely to migrate to the latter areas. Because these children would be included in the control group (when we do not exclude migrants), this would bias estimation results downward and helps explain both the smaller treatment effects and the weaker statistical significance shown above.

The available data do not provide information about the destinations for children who migrated. But for a conservative check, we can make an extreme assumption that if treated children migrate, all of them migrate to French-controlled areas and thus no longer benefit from the ME program. Estimates would then provide the lower bounds of the impacts of the ME program. To implement this check, we adjust the number of years of exposure by a child’s age at migration (i.e., if the child is more than 6 years old when migrating, subtract from the number of years of exposure the difference between six and the age at migration). Estimation results (table 4, panel C) are still strongly statistically significant for the probability of completing at least primary school and the completed number of years of schooling and marginally statistically significant at the 10% level for the probability of achieving reading literacy. The corresponding lower bound estimates are 5 percentage points more likely to
complete at least primary school, 0.2 additional years of schooling, and 2 percentage points more likely to achieve reading literacy. For another check, we control for the number of years of residence in the current province since the last move (which could help address this bias due to migration to some extent), and $\beta$ becomes statistically stronger (table 4, panel D) and more similar in magnitude to the main results in table 2. These results provide further supportive evidence for the long-term beneficial impacts of schooling policies in the DRV-occupied provinces.

Parental education plays an instrumental role in their children’s education. However, we have no information on parental education for almost half (40%) of the individuals in our sample. But as a robustness check, we rerun our estimates on those who have parental education and provide estimation results in table 4, panel E. Estimates become somewhat stronger and even more statistically significant. For example, compared with table 2, girls are 2 percentage points more likely to achieve reading literacy and to attain 0.5 more years of education (cols. 1–5).

Estimation results using the second measure of exposure are qualitatively similar (table A.6). Estimation results for boys are, however, statistically insignificant for both measures of exposure (table A.7).

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

Another concern is that school-age children residing in DRV-controlled areas may have migrated out of the country in the intervening 50 years or so between the Indochina War and the VLSS implementation in 1997–98. In this case, our estimation sample can provide biased estimates. (But note that if migrants were more educated individuals, our estimates would be biased downward). To check on this hypothesis, we provide estimates on whether individuals are more likely to receive remittances from their relatives living overseas more than 50 years later (i.e., in 1997–98). Estimation results (tables A.9, A.10) for both measures of exposure suggest that this is not the case for both girls and boys.

Another concern is that the 1997–98 VLSS may not capture well those who should be in our estimation sample. For example, people who were born in 1924–35 could be between more than 60 and 70 years old in 1997–98, 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 who were surveyed in the 1992–93 VLSS. However, since this survey collected data on much fewer households, one trade-off with doing so is that the sample size is smaller, which can result in less precise estimates. Still, estimation results (table A.11, panel A) are highly statistically significant for several indicators, such
as completing at least primary education (col. 3) and years of schooling (col. 5), and are marginally statistically significant for completing secondary education or higher for the first measure of exposure to school policies. The point estimates are somewhat weaker but quite similar to those in table 2; for example, the estimated impact on the years of schooling is 1.2 (col. 5), which is close to that of 1.4 more years of schooling in table 2. The years of schooling variable is also statistically significant for the second measure of exposure (panel A, col. 10), but estimates are not significant for the other indicators.\footnote{A number of individuals report different years of birth and years of education in the panel data of the 1992–93 and 1997–98 VLSS, which raises concerns about measurement errors. To reduce the measurement errors, we restrict our estimation sample of the 1992–93 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. Indeed, further analysis suggests that younger and more educated individuals are more likely to report correct birth years, which 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), and Sweden (Kjellsson, Clarke, and Gerdtham 2014).}

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 (e.g., because of death) are more severe with this survey. Nevertheless, we experiment with estimating equation (1) for those who were born in 1946–49 as the partially treated group and those who were born in 1950–61 as the post-war control groups. While we expect any estimated treatment impact to be severely downward biased, the treatment impact is still positive and statistically significant at the 5% level for completing at least primary education for the first measure of exposure to school access (table A.11, panel B, col. 3). The other education outcome variables are statistically insignificant but have positive coefficients.

Finally, we employ the hybrid approach where an individual is assigned the province-level, instead of the individual-level, number of years of exposure to the ME program (which is determined according to the province-level years of the DRV occupation). Estimation results are weaker in magnitude than those for the second measure of exposure in table 3 but are statistically stronger (table A.12). The estimated impacts are 2 and 1 percentage points more likely to achieve reading literacy and math competency, 3 percentage points more likely to complete at least primary education, and 0.1 more years of schooling.

5. Heterogeneity Analysis

Besides the gender dimension, we further examine the heterogeneous effects of the DRV’s ME program on different ethnic groups. Previous studies have pointed
to ethnic gaps in living standards in the country; consequently, it would be useful to understand whether different population groups responded differently to the ME program (as a policy intervention).\textsuperscript{16} We interact the treatment variables (for both measures of exposures) with the Kinh-Hoa ethnic groups, which form the majority of the population, and present estimation results on both this interaction term and the treatment variables in table 5. Consistent with the estimation results for the whole population (table 2), the Kinh-Hoa clearly benefit from the DRV’s program. Individuals belonging to these ethnic groups who were exposed to the program are 70 percentage points more likely to achieve math literacy, are 55 and 35 percentage points more likely to complete primary and secondary education, and have 2.1 more years of education (table 5, panel A).\textsuperscript{17} The remaining ethnic groups, however, do not seem to consistently benefit from the program. The only education outcome for which ethnic minority groups have a statistically significant treatment effect is reading literacy, the magnitude of which is more than twice that of the Kinh-Hoa groups (i.e., 43 percentage points vs. 25 percentage points). However, this result does not hold for the second measure of exposure, and neither does the completed years of schooling variable. 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.

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 (1) as an additional control variable the number of battles fought in each province during the Indochina war. Estimation results (table A.15) are qualitatively similar.\textsuperscript{18}

V. Discussion and Conclusion

Our study contributes to the literature on the long-term impacts of education policies on developing countries. Furthermore, we also offer the first rigorous impact evaluation of a large-scale mass education program that was implemented in North Vietnam during the first Indochina War. We find that school-age

\textsuperscript{16} Several studies find that the ethnic differentials in the returns to endowments (including differentials in quality of schooling, individual ability, or labor market discrimination) can account for a considerable share of the consumption and earning differentials between ethnic groups in Vietnam (van de Walle and Gunewardena 2001; Baulch et al. 2007; and Dang 2012).

\textsuperscript{17} The corresponding 95\% CIs for these estimates are 50–89, 40–70, 26–45, and 1.3–2.9.

\textsuperscript{18} These battles took place mostly in the control provinces but not the treated provinces. Examining the impacts of intensive US bombing on Vietnam in the second Indochina war, Miguel and Roland (2011) found no impacts on either the province or district literacy rates. They observed that this can be due to the flexible adaptation by teachers and students that includes dispersal into small groups to avoid strikes and school provision of foxholes and helmets for student protection during US attacks.
|                        | Reading Literacy (1) | Math Competency (2) | Primary Education Completion or Above (3) | Secondary Education Completion or Above (4) | Years of Education (5) |
|------------------------|----------------------|---------------------|------------------------------------------|---------------------------------------------|------------------------|
|                        |                      |                     |                                          |                                             |                        |
| **A. Dummy Variable for DRV-Controlled Area** |                      |                     |                                          |                                             |                        |
| Treated × Kinh-Hoa     | -.248***             | .696***             | .550***                                  | .354***                                     | 2.132***               |
|                        | (.085)               | (.100)              | (.076)                                   | (.047)                                      | (.410)                 |
| Treated                | .426***              | -.513***            | -.247**                                  | -.110                                       | -.656                  |
|                        | (.080)               | (.083)              | (.086)                                   | (.086)                                      | (.636)                 |
| N                      | 699                  | 699                 | 700                                      | 700                                         | 700                    |
| Adjusted R²            | .262                 | .261                | .283                                     | .206                                        | .341                   |
| Mean of dependent variable | .409                 | .339                | .217                                     | .113                                        | 3.227                  |
| **B. Years of Exposure to DRV** |                      |                     |                                          |                                             |                        |
| Years of exposure × Kinh-Hoa | .027               | .070**              | .068*                                    | .071***                                     | .306                   |
|                        | (.019)               | (.031)              | (.039)                                   | (.023)                                      | (.228)                 |
| Years of exposure      | .008                 | -.035               | -.003                                    | -.029                                       | .003                   |
|                        | (.023)               | (.027)              | (.038)                                   | (.021)                                      | (.235)                 |
| N                      | 877                  | 877                 | 878                                      | 878                                         | 878                    |
| Adjusted R²            | .233                 | .245                | .246                                     | .183                                        | .309                   |
| Mean of dependent variable | .413                 | .338                | .218                                     | .108                                        | 3.298                  |

**Note.** For the first measure of exposure, the treatment group consists of individuals who belong to the birth cohorts of 1940–45 and who resided in DRV-controlled provinces in their school age; for the second measure of exposure, treatment is defined as the number of years of exposure to the ME program for individuals who belong to the birth cohorts of 1940–45 and who resided in DRV-controlled provinces in their school age. For both measures, the control group consists of the birth cohorts of 1924–35. Each cell presents the results from a separate regression that controls for dummy variables indicating gender, religious groups (including Buddhism and Christianity), birth year, and birth province fixed effects. All estimation samples exclude those who had migrated when they were younger than age 15. Robust standard errors in parentheses are adjusted for two-way clustering (birth province and current commune).

* p < .10.
** p < .05.
*** p < .01.
girls’ exposure to this program helped raise their probability of achieving reading and math literacy, completing at least primary education, and completing at least secondary education by between 17 and 29 percentage points as well as their education by an additional 1.4 years of schooling. In addition, the DRV’s ME program had especially long-term beneficial impacts on girls’ household living standards.

As discussed earlier, Vietnam has witnessed a reverse gender gap in secondary school enrollment in the past decade. This pattern is consistent with the country’s stronger-than-average gender equality for other education outcomes as well. Figure A.3 plots the average male and female years of schooling against countries’ (log of) per capita gross domestic product, which shows that Vietnam’s male years of schooling is somewhat higher than the overall trend. But remarkably, its female years of schooling is 1 year more than the global trend. Vietnam has also reached virtually universal primary school enrollment and higher gender equality than the global trend at its income level (see fig. A.1). Our study offers an interesting historical and institutional perspective that helps shed light on this remarkable performance. The current strong performance in education may be traced back to the beneficial impacts of ME policy, which provided an unprecedented opportunity to help level access to schools for children, particularly school-age girls, who had rarely been granted the privilege of school attendance.19

However, we did not find statistically significant impacts of the ME policy on boys’ education, although we cannot reject the null hypothesis of equal impacts between boys and girls at standard significance levels when the impacts are estimated separately for the two genders. There can be some tentative hypotheses that can (partially) help explain this conundrum if we are to make additional assumptions. The first hypothesis is that since the female enrollment rate was already far lower than the male enrollment rate in the prewar period, it would have been easier for the ME program to improve the former than the latter. Indeed, figure 4, using available data from Nguyen (2013) and General Statistics Office (2004), shows that the average prewar (1932–44) female enrollment rate was 4%, which is five times less than the corresponding male enrollment rate of 22%. Although the female enrollment rate subsequently doubled for the treatment cohort of 1949–50 and more than tripled for the postwar cohort of 1953—which results in the sharp increase of girls as a share of total enrollment starting from 1949–50, as displayed by the bars—it still equaled just around half of the

19 Our finding on the long-term impacts of school access also concurs with existing studies that examine the short-term beneficial effects of providing access to school for girls. For example, Andrabi, Das, and Khwaja (2013) show that Pakistani villages with a government girls’ secondary school have over twice as many educated women, and Burde and Linden (2013) find that village-based schools eliminate the gender gap in enrollment and dramatically reduce gender differences in test scores.
corresponding male enrollment rate. Another hypothesis is that men with more education had a larger enlistment rate in the army than women (Teerawichitchainan 2009); thus, their larger war mortality rate during the Second Indo-
cchina War (Hirschman, Preston, and Loi 1995; Merli 2000; Obermeyer et al.
2008) may have further reduced any potentially positive impacts of the ME pol-
icies on men. There are, unfortunately, no data that allow us to further investi-
gate these hypotheses.20

While it may not be possible to exactly replicate a similar school policy in the
same context (i.e., a time of war) in Vietnam or elsewhere, the historical lesson
remains relevant. In particular, if a school policy can win unanimous approval
and support from the government and all society’s different walks of life, it
may be able to offer record-breaking achievements at relatively low costs. Our
findings thus suggest that a similar approach, at least in spirit, to certain policies
on improving information technology skills and foreign languages skills may
be fruitful.

20 Still, another 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 have
also 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
United States have suggested that preferential government policies on this population group can sig-
nificantly increase their college education (see, e.g., Bound and Turner 2002; Stanley 2003).
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