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The multigenerational impacts of educational expansion: Evidence from Vietnam[☆]

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ABSTRACT

We investigate the multigenerational effects of a primary school expansion program in Vietnam. In the *directly affected generation*, the expansion increases educational attainment, literacy, non-agricultural economic activity, earnings and the intergenerational educational mobility. It increases human capital investments in the *children* of the directly affected generation, with increased educational expenditures, school enrollment, and health investments, and a reduction in child labor. Moreover, the expansion improves health in old age of the *parents* of the directly affected generation, an effect that seems to operate through increased financial resources, access to private health insurance and reduced alcohol consumption.

1. Introduction

Expansion and promotion of primary schooling have been shown to affect long-term individual economic and financial outcomes (Duflo, 2001; Ajayi and Ross, 2020), fertility decisions (Keats, 2018; Osili and Long, 2008), political engagement (Larreguy and Marshall, 2017), and pro-social behaviors (Chankrajang and Muttarak, 2017), amongst others.[1] Yet, due to spillovers and external effects of education, the *full* social returns of such policies could be far higher than this literature suggests with impacts going far beyond the generally well-documented effects on the individuals directly affected by the policy change. In particular, given that the *family* is an important peer group within which spillovers of education policies are likely to materialize (Kuziemko, 2014), educational expansion is likely to have impacts on the children of the directly affected generation (*downward* intergenerational spillovers) and on their parents (*upward* intergenerational spillovers; De Neve and Kawachi, 2017).

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[1] A related strand of the literature analyzes the effect of policies to promote primary schooling on primary school enrollment and completion (Deininger, 2003; Lucas and Mbiti, 2012). There is also a large literature documenting the effects of compulsory schooling laws in developed countries (see Angrist and Krueger, 1991; Harmon and Walker, 1995; Devereux and Hart, 2010; Black et al., 2008; Oreopoulos et al., 2006), in which these compulsory schooling laws usually operate at the secondary school level rather than the primary school level as is often the case in developing countries.

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Downward intergenerational spillovers are suggested by a large body of literature documenting a strong dependence of educational attainment and success later in life on parental education and family background (Björklund and Salvanes, 2011; Black and Devereux, 2011; Carneiro et al., 2013; Currie and Moretti, 2003; Holmlund et al., 2011; Lundborg et al., 2014), although the precise degree of causality is a matter of debate and varies by context (Holmlund et al., 2011). Evidence is also starting to accumulate about the effects of parental education on children's long-run health outcomes (Huebener 2018, 2019; Lundborg et al., 2014).

Upward intergenerational spillovers have been less well documented. But several arguments suggest that children's education may affect parental health and well-being in old age through several channels. First, well-educated children might invest greater resources to improve their parents' living standards (Friedman and Mare, 2014; Lavy et al., 1996). Second, higher educated children may provide their parents with more knowledgeable advice or help to make better decisions about healthcare, nutrition, lifestyle choices, and access to social services (Berniell et al., 2013; Kuziemko, 2014; P. Lundborg and Majlesi, 2018). Third, well-educated offspring may positively influence their parents' health behaviors, lifestyles and attitudes (Friedman and Mare, 2014; Torssander, 2013). Finally, parents of well-educated children may have less necessity to engage in work and may be able to retire earlier. These channels are particularly relevant in lower-income countries where parents are more likely to co-reside and to share resources with their adult children (Cameron and Cobb-Clark, 2008; De Neve and Fink, 2018), while social insurance systems are less developed (Bloom et al., 2011), prompting elderly people to work at an advanced age often under poor conditions (Cai et al., 2012; Cameron and Cobb-Clark, 2008). Evidence on such upward spillovers is still scant, and prior estimates of the relationship between children's education and parents' outcomes are mostly correlational rather than causal (De Neve and Harling, 2017; Friedman and Mare, 2014; Torssander, 2013).

In this study, we investigate the multigenerational impacts of educational expansion in a developing country context. We analyze the effects of a compulsory primary schooling reform in Vietnam on individuals of the *directly affected generation*, investments in the human capital of their *children*, and health outcomes in old age of their *parents*. The reform, introduced in 1991, mandated for the first time that Vietnamese children aged 14 or younger complete at least five years of primary education. The reform strongly increased primary school completion in areas with initially low levels of primary schooling (i.e., treatment areas), but had little impact in areas in which primary school completion was already high (i.e., control areas). The identification strategy consists of a difference-in-differences estimator based on a combination of large-scale data from Vietnam's 2009 Population and Housing Census, several waves of the Vietnam Household Living Standards Survey and the 2011 Vietnam Aging Survey. We provide support for the credibility of our identification strategy by studying parallel pre-trends, through a range of placebo tests, and by showing that the results are robust to controlling for regionally varying trends in various ways. This includes unrestricted province-by-cohort effects, and interacting linear cohort trends with rural versus urban status, and with the degree to which districts were affected by U.S. bombing during the Vietnam war.

We find that the reform has a strong positive effect of 10 percentage points on primary and secondary school completion of the *directly affected cohorts*. Through this effect, the reform increases the intergenerational educational mobility. Reform exposure also increases literacy, economic activity outside the agricultural sector, and earnings. It raises residential stability, the probability of being married, the level of education of the marital partner, and increases fertility at the extensive margin, while reducing it at the intensive margin. In terms of *downward spillovers*, we find effects on the directly affected cohorts' investments in their *children's* human capital. The results indicate that parental exposure to the reform raises children's school enrollment at ages 6–17 by about 8 percentage points. Spending on tuition fees and other education-related spending, such as on books and learning materials, increases by 10–20% of a standard deviation. Moreover, we find evidence for an increase in health investments, with higher per capita food consumption, less exposure to smoking in the household, and more expenditure on preventive health visits in the private health sector, resulting in fewer hospitalizations. We further find a reduction in child labor, suggesting that downward intergenerational spillover effects or primary school expansion may be particularly beneficial in developing countries. Heterogeneity analysis by gender reveals that the effects are concentrated more on sons than on daughters. In terms of *upward spillovers*, we find that the primary school expansion improves several measures of general health in old age of the *parents* of the directly affected generation, but has no effect on mental health. In terms of potential channels, we document effects on improved financial resources in old age, access to private health insurance and reduced alcohol consumption.

Our contributions are as follows. We provide first comprehensive causal evidence on the multigenerational impacts (including *both* downward and upward intergenerational spillovers) of primary school expansion. Besides strong educational, financial and economic returns to the directly affected generation, we document important impacts on the next generation's health and human capital and on the previous generation's health. Through these spillovers, enacting and enforcing compulsory schooling laws has high social returns. These findings are particularly policy relevant in low-income countries where primary school enrollment is still far below universal (World Bank, 2016).[2]

Our research contributes to a sparse literature on intergenerational spillover effects in human capital in developing countries (Assaad and Saleh, 2018; Grépin and Bharadwaj, 2015; Hahn et al., 2018; Keats, 2018).[3] The studies most closely related to ours is the work by Akresh et al. (2018) and Mazumder et al. (2019), who investigate downward intergenerational spillovers from primary school construction in Indonesia on educational attainment and health of the second generation. We extend their results by looking at a wider set of second-generation outcomes, including not only school enrolment but also a wide range of household expenditures on education, health outcomes, and child labor. We believe our study to be the first to document effects of parental exposure to primary school expansion on reductions in child labor among their children. Child labor is still widespread in low-income countries with a prevalence rate of roughly 19% (ILO, 2017).[4] While its reduction over the last few decades has been linked to numerous interventions (see Dammert et al., 2018 for a review), our results suggest that improved parental education is an important but so far neglected causal contributor.

Our study also extends a nascent literature on causally identified upward human capital spillovers (De Neve and Fink, 2018; P. Lundborg and Majlesi, 2018; Ma, 2019; Potente et al., 2020). While P. Lundborg and Majlesi (2018) and De Neve and Fink (2018) focus on parental survival as the main outcome of interest, in Sweden and Tanzania respectively, Ma (2019) examines the effects on parental health and cognitive ability in China. While these studies tend to find positive effects of children's schooling on parental health, Potente et al. (2020) find limited and small causal effects on parental outcomes of health and mortality in the United Kingdom. We build on these studies by significantly extend-

[2] As of the end of the United Nation's Millennium Development Goals in 2015, only 40 over 145 developing countries achieved the goal of universal primary education (World Bank, 2016). The recent rates of primary school completion in low-income countries are still very low with only 59% (UNESCO, 2019).

[3] There is also an extensive literature on downward intergenerational spillovers in developed countries (Currie and Moretti, 2003; Dickson et al., 2016; Heckman and Karapakula 2019b; Huebener, 2018, 2019; Lindeboom et al., 2009; Lundborg et al., 2014; Meghir et al., 2012; Oreopoulos et al., 2006).

[4] This compares to an incidence rate of 9% in lower-middle-income countries, 7% in upper-middle-income countries, and 1% in upper-income countries (ILO, 2017). See also Basu (1999) and Edmonds and Pavcnik (2005) for economic surveys on child labor.

ing the range of outcomes and investigating important channels of the effects.

Furthermore, we make a range of contributions to a wider set of literatures. We contribute to the literature on the determinants of parental investments in children's human capital (Attanasio, 2015; Brown, 2006; Strauss and Thomas, 1995) by showing that exposure to stricter compulsory schooling laws has a causal effect on a range of specific human capital investments. We also complement a comparatively sparse literature on the mediating channels of the intergenerational transmission of education (Carneiro et al., 2013; Piopiunik, 2014) by showing that direct monetary investments in children's health and human capital play an important role in downward intergenerational spillovers.

Furthermore, we contribute to a growing literature documenting substantial heterogeneity in intergenerational social and economic mobility across time (Chetty et al., 2017a), space (Chetty et al., 2014), and socioeconomic background (Chetty et al., 2020). Prior research has documented the importance of the neighborhood conditions during childhood (Chetty and Hendren, 2018a; R. 2018b), type of college education (Chetty et al., 2017b), the timing of parental income during childhood (Carneiro et al., 2021), and income shocks (Bütikofer et al., 2018). Our findings suggest that well-implemented compulsory schooling laws, by equalizing opportunities of access to basic schooling for all children, increase intergenerational educational mobility.[5] Finally, our findings of the effects of the human capital of the younger generation on the later-life health of parents contributes to a growing literature that seeks to identify a broad range of social and economic determinants of health in old age (Antman, 2010; Atalay et al., 2019; Barnay and Juin, 2016; Bhalotra et al., 2017; Böhme et al., 2015; Case and Paxson, 2009; De Nardi et al., 2009; Fabrizio and Franco, 2017; Fetter and Lockwood, 2018; Salm, 2011).

The remainder of our paper is structured as follows. Section 2 provides the institutional background and data. Section 3 discusses our empirical strategy, and Section 4 presents our results including the main causal effects, heterogeneity and potential mechanisms. Section 5 concludes our paper.

2. Institutional background and data

2.1. The Vietnamese 1991 compulsory schooling reform

Until 1991, there was no minimum level of compulsory schooling in Vietnam, and primary school completion rates were low and heterogeneous across regions (Glewwe and Jacoby, 1998; National Committee for EFA Assessment, 1999). In order to raise education levels in the economy, the Vietnamese government introduced a law on compulsory schooling, the Law on Universal Primary Education (LUPE). The law came into effect in 1991 and stated that with immediate effect all children aged 14 or younger had to complete five years of primary school. Consequently, children born before 1977 were not affected by the reform, while children born in 1977 and onwards were, in principle, affected. However, the implementation of the reform was "piecemeal rather than comprehensive" across the country, requiring years of preparation (MOET and JICA, 2002). Moreover, children aged 14 or younger in 1991 could have left school already and thus would have been unlikely to return to school even if technically required by the LUPE. Therefore, in our analysis, we allow up to four years for the reform to be effective and define individuals born between 1977 and 1980 as phase-in cohorts. We treat children born in 1981 and onwards as fully exposed to the reform.

The reform triggered substantial investments in primary education (Nguyen, 2004), with a significant increase in public expenditures devoted to constructing new schools, training additional teachers with enhanced qualifications and providing scholarships and financial aid to students from disadvantaged families or remote areas. These investments were financed by a mix of increased contributions and investments by the private sector and international aid from governmental and non-governmental organizations (Dang and Glewwe, 2018; Glewwe and Patrinos, 1999). As a result, infrastructure and human resources employed in primary education in Vietnam expanded considerably. From 1991 to 1998, 11,334 new schools were constructed and close to 50,000 new primary school teachers were hired, leading to an expansion of primary school classes from 262,686 to 316,968 (MOET, 1998).

Due to these developments, primary school attendance increased greatly. The aggregate number of students enrolled in primary school increased from roughly 8.1 million in 1986 to nearly 9.1 million in 1995, and primary school completion rates rose from 81% in 1979 to nearly 100% in the 2000s (World Bank, 2018). Thanks to the reform, primary school enrollment rates in the 2000s were higher in Vietnam than in other comparable countries (Dang and Glewwe, 2018).

2.2. Data

Our study is based on three nationally representative data sources for Vietnam. The first data source is a 15% sample of the 2009 Population and Housing Census of Vietnam (hereafter "the Census"). It is one of the largest existing micro datasets for Vietnam, covering 14 million individuals from 3.5 million households. It contains basic information on demographics, education, employment, mortality and housing. The second data sources are the 2010, 2012 and 2014 waves of the Vietnam Household Living Standards Survey (VHLSS). This is a biannual household survey consisting of roughly 40,000 individuals from 9000 households, drawn from the Census population. The third data source is the 2011 Vietnam Aging Survey (VNAS). It contains information on demographic characteristics, housing and assets, employment, social protection and inclusion, physical and mental health, and emotional well-being from 4007 individuals aged 50 years old and over from 12 provinces across all of 6 Vietnamese economic zones.

We restrict the Census and VHLSS data to the 1974–1984 birth cohorts to construct our samples of the directly affected generation. From the Census we extract key variables such as birth year, residential location (district and urban indicators) and educational attainment to identify treatment and control areas for our analysis. We further extract a range of outcomes for the directly affected generation from both of the Census and VHLSS. These consist of educational outcomes (primary and secondary school completion, years of schooling, and literacy), economic outcomes (being economically active, working in a non-agricultural sector, and the logs of real earnings), family outcomes (being lived in the same municipality over the last 5 years, being married, spouse's years of schooling, having at least one child, being experienced child mortality, and number of children conditional on having at least one child), and parental years of schooling (used for exploring intergenerational educational persistence).

To estimate the effect of parental exposure to the reform on investments in their children's human capital, we use the VHLSS and restrict the sample to children aged 6–17 who were born to parents from the 1974–1984 birth cohorts. For these children, we then construct three sets of dependent variables for parental investments in their children's human capital: educational investments, child labor outcomes, and health investments. We extract information on children's school enrolment, parental expenditures on school-related activities (school tuition, books and learning materials, learning tools and instruments, private tutoring, and total overall spending on education), and expenditures on children's books and maganizes and toys. We use information on children's employment to construct three measures for child labor: working for the household, working for earnings and hours of work for earnings. For investments into children's health, we extract information on household expenditures on food and children's health and health-

[5] Our results complement findings by Demirel and Okten (2020), who document that an increase in compulsory schooling in Turkey (from 5-8 years) reduced gender differences in intergenerational educational persistence.

care utilization to construct the following outcomes: monthly per capita household spending on food consumption, monthly household spending on tobacco and cigarettes, the child's health insurance coverage, number of preventive health visits, expenditures on preventive health care for public and private health services, and probability of hospitalization over the last 12 months. Monetary expenditures are measured in 1000 Vietnam Dong (VND) in 2010 prices, with the exchange rate equaling roughly 20,000 VND per 1 U.S. Dollar in 2010.

We use the VNAS to estimate the effects of the reform on the health in old age of the *parents* of the directly affected generation. One of the most important advantages of the VNAS compared to other Vietnamese data surveys is that it includes all children of the respondents, not only co-resident children but also those living outside the household, allowing us to estimate a full sample of children and thus to avoid potential sample selection bias. We exploit information on physical health, mental health, and emotional well-being to construct five measures for the health of the parents in old age over the past 12 months. These include the number days of sickness, self-reported health status (absolute and relative to others in the same age group), depression, sleep problems, and general life satisfaction. We additionally exploit information on household economic conditions and individual behaviors of the parents to construct multiple measures for potential mechanisms used to explain the main effects, spanning from financial mechanisms (dummies for poverty, income and savings, satisfaction with financial status, private health insurance, home improvements) to behavioral health mechanisms (smoking, drinking, social activities, grandchildren, and the number of children-in-law). We present summary statistics of all variables in Table A.1 in the Online Appendix.

3. Empirical strategy

3.1. Definition of treatment and control areas and enrollment trends

Before the reform, educational attainment was heterogeneous across different areas in Vietnam (Dang and Glewwe, 2018). This was due to differences in schooling preferences between local populations (Anh et al., 1998) and in regional economic development (Dell et al., 2018; Nguyen et al., 2007). Given that the compulsory schooling law mandated all areas to achieve universal primary schooling, areas with lower initial levels of primary school completion experienced a stronger expansion. Based on this idea, we define treatment areas as those with low initial school enrollments and comparison areas as those with high initial enrollments. We define areas by the interaction of districts with an urban indicator.[6] We then define the initial level of primary education (hereafter "the initial level") as the area's average rate of primary school completion for the 1977 birth cohort using the Census sample. We define treatment areas as those with a below-median initial level and comparison areas as those with an above-median initial level. Among a total of 1272 included areas, 636 are defined as treatment areas and 636 as comparison areas. For the treatment area definition, and in order to assign treatment status to individuals observed in our data, we rely on the place of residence observed for adults in the respective data waves (between 2009 and 2014) as a proxy for their place of residence when they were at school age. Our own calculations from Census data reveal a 5-year residential mobility rate across districts of 6.3%. While this rate does not appear excessively large, and not every move changes the treatment status, it would introduce some misclassification error into our treatment status definition. In Online Appendix we explore this issue further based on one wave of the VHLSS data in which we observe province of residence and province of birth. This analysis shows that the cohort trends in primary school completion rates are almost identical when defined on place of birth and place of residence, and that the reform effect remains very similar when changing the definition. This suggests that bias from using place of residence rather than place of birth is unlikely to be a concern in our study context. We discuss this in more detail in Online Appendix B, where we also devote some discussion to the possibility of selective out-migration from sampled areas to larger cities.

Fig. 1 illustrates the evolution of primary school completion rates across the cohorts born from 1974 to 1990 separately for treatment and comparison areas. The vertical bars illustrate the pre-reform, phase-in and post-reform cohorts, as defined in Section 2.1. By definition, comparison areas initially had considerably higher primary schooling rates than treatment areas, but trends are fairly parallel over pre-reform cohorts. However, this pattern gradually changes for cohorts affected by the reform. While rates in comparison areas are highly stable, rates in treatment areas increase considerably from phase-in cohorts to post-reform cohorts and then almost completely catch up with rates in the comparison areas for the youngest cohorts. The primary school completion rate in comparison areas increased by only about 3 percentage points between the pre-reform and post-reform cohorts. However, it increased by about 13 percentage points in the treatment areas, suggesting a difference-in-difference reform impact on primary school completion of roughly 10 percentage points.[7]

In the following section, we describe how we exploit this variation in a difference-in-differences regression framework to estimate reform effects on different generations. In Section 3.3 we discuss the assumptions required for a causal interpretation and we describe how we probe our identification strategy to ensure these assumptions are credible. There we also provide more information on how treatment and comparison areas differ in observed characteristics.

3.2. Estimation

Our empirical strategy is based on generalized difference-in-differences regressions that compare across areas and cohorts, controlling for area and cohort effects, and letting cohort effects vary across provinces. The implementation of the regression differs according to whether we estimate effects on the generation directly affected by the reform, their children's generation, or their parents' generation. The three respective approaches are the following.

3.2.1. Effects on individuals from the directly affected generation

For an outcome Y_{irct}, such as primary school completion, of an individual i of the directly affected generation, living in area r within province $p(r)$, belonging to birth cohort c, and surveyed in year t, we run the regression

$$Y_{irct} = \alpha_1 + \alpha_2 \mathrm{Treat}_{ir} \times \mathrm{Phasein}_{ic} + \alpha_3 \mathrm{Treat}_{ir} \times \mathrm{Post}_{ic} + \alpha_4 {\prime}_{irct} + \theta_t + \varphi_r + \omega_c \times \pi_{p(r)} + \zeta_{irct}. \quad (1)$$

Treat_{ir} is a dummy variable indicating whether the area r where individual i lives is a treatment area. $\mathrm{Phasein}_{ic}$ is a dummy variable indicating whether the individual belongs to the phase-in birth cohort (1977–1980). Post_{ic} is a dummy variable indicating whether the individual belongs to the post-reform birth cohort (1981–1984). ${\prime}_{irct}$ is a control vector for individual characteristics, including dummies for gender and ethnicity (Kinh, Tay, Thai and other ethnicities). For the Census data, we also add religious affiliations (Buddhism, other religions and no religions) into the set of controls. θ_t, φ_r and ω_c are fixed effects for the

[6] A district is a medium administrative unit in Vietnam, which is smaller than a province but larger than a commune. Vietnam has 696 districts with an average population of roughly 125,000 people (CPHCSC, 2010).

[7] Besides the mean, the figure also shows the 25th and 75th percent of primary school completion by cohort for both the treatment and comparison areas. This shows a quite condensed distribution for the comparison areas, where the 25th percentile never falls below a 90% completion rate, and the 75th percentile reaches 100% in the post periods. In the treatment areas, there is considerable variation around the mean. Yet, both groups appear clearly distinct, with the 75th percentile of the treatment group still being clearly below the 25th percentile for the comparison group.

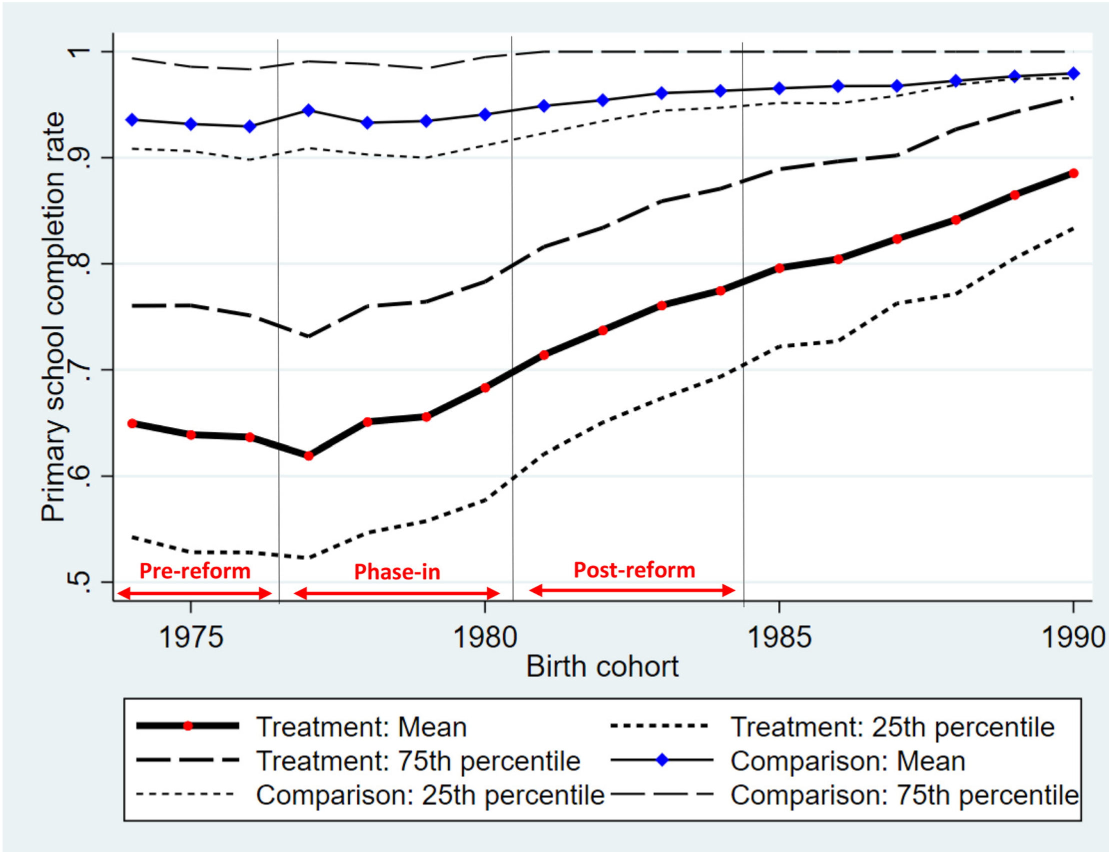

Fig. 1. Primary school completion rate by cohort for treatment and comparison areas
Data source: Population and Housing Census of Vietnam (2009, 15% sample).

survey year, area and birth cohort, $\pi_{p(r)}$ are dummies for 63 provinces, and ζ_{irct} is the error term. The survey year fixed effect θ_t controls for potential time trends across survey years, and is dropped in specifications using the Census data consisting of one year only. The area fixed effect φ_r controls for time-invariant area characteristics and the birth cohort fixed effect ω_c controls for cohort trends in the outcome. The joint inclusion of area and cohort fixed effects turns Eq. (1) into a generalized difference-in-differences regression, in which α_3 is the effect of exposure to the reform on the outcome. The interaction of birth cohort with province dummies, $\omega_c \times \pi_{p(r)}$, allows trends across cohorts to vary in unrestricted ways across provinces.

3.2.2. Effects on the children of directly affected individuals

To explore how reform exposure of individuals affects their *children*, indexed j, we focus on children observed in the VHLSS survey in year t (2010, 2012 and 2014). We run regressions of the type:

$$\begin{aligned} Y_{jrt} = {} & \beta_1 + \beta_2 \text{Treat}_{jr} \times \text{Parent Phasein}_j + \beta_3 \text{Treat}_{jr} \times \text{Parent Post}_j \\ & + \beta_4 \mathbf{X}'_{jrt} + \theta_t + \varphi_r + \omega^{cm} \times \pi_{p(r)} + \omega^{cp} \times \pi_{p(r)} + \varepsilon_{jrt}. \end{aligned} \quad (2)$$

Y_{jrt} is an outcome, such as an investment into child j's human capital. $\mathbf{X}'_{jrt}$ includes ethnicity, yearly age dummies and gender of the child. ε_{jrt} is the error term. As before, Treat_{jr} is a dummy variable indicating whether the area r where child j lives is a treatment area. ParentPhasein_j is a dummy variable indicating whether *at least one of the parents* of child j belongs to the phase-in birth cohort (1977–1980). ParentPost_{ic} is a dummy variable indicating whether *at least one of the parents* of child j belongs to the post-reform birth cohort (1981–1984). θ_t and φ_r are survey year and area fixed effects. Maternal birth cohort fixed effects, ω^{cm}, and paternal birth cohort fixed effects, ω^{cp}, are interacted with province dummies $\pi_{p(r)}$. To avoid losing children in single-parent households, we imputed the maternal/paternal birth cohort variable of a missing parent by setting it to a mid-point value (1980), and introduced two dummies in $\mathbf{X}'_{jrt}$ indicating the observations with a missing value for maternal or paternal birth cohort (which applies to 4.5% of the children's sample).

We specify treatment as *at least one of the parents* being exposed to the reform, instead of separate indicators for father being exposed and mother being exposed, for the following reasons. First, our indicator can be constructed for single-parent households. Second, maternal and paternal exposure do not have a lot of independent variation. In 80 percent of families in our sample, the husband is older than the wife, implying that if the father is treated, the then the mother is in most cases also treated.[8]

We ruled out using separate samples of mother-child pairs and father-child pairs to get separate estimates of maternal of paternal exposure, because these estimates would be confounded by the exposure of the respective other parent, which would act as an omitted variable in this approach. We also ruled out pooling together mother-child pairs and father-child pairs into a combined sample, as this would have inflated our numbers of observations by double-counting children of two-parent households.

[8] We tried the approach of including 'one parent treated' and 'both parents treated' variables and did not find evidence that effects amplify if both parents are treated. However, interpretation is complicated by the fact that the indicator for both parents being treated is highly collinear with a maternal treatment indicator. Ideally, one would want to estimate separate effects for: only the father being treated, only the mother being treated, and both parents being treated. But these indicators do not have sufficient independent variation in our sample.

3.2.3. Effects on the parents of directly affected individuals

To explore how reform exposure of individuals affects their *parents*, indexed k, we focus on parents from the 2011 VNAS survey who have at least one child born in the 1974–1984 period. We run regressions of the type:

$$\mathrm{Y_{krc}} = \gamma_1 + \gamma_2 \mathrm{Treat}_{kr} \times \mathrm{Child\ Phasein}_k + \gamma_3 \mathrm{Treat}_{kr} \times \mathrm{Child\ Post}_k + \gamma_4 \mathbf{X}'_{\mathrm{krc}} + \varphi_{\mathrm{r}} + \omega_{\mathrm{c}} \times \pi_{\mathrm{p(r)}} + \sum_{\tau} \rho_\tau NC_\tau + \epsilon_{\mathrm{krc}}. \tag{3}$$

$\mathrm{Y_{krc}}$ is an outcome, such as health status of a parent k. $\mathbf{X}'_{\mathrm{krc}}$ includes ethnicity and gender of the parent, and ϵ_{krc} is the error term. As before, $\mathrm{Treat_{jr}}$ is a dummy variable indicating whether the area r where parent k lives is a treatment area. $\mathrm{ChildPhasein_k}$ is a dummy variable indicating whether *at least one of the children* of parent *k* belongs to the phase-in birth cohort (1977–1980). $\mathrm{ChildPost_k}$ is a dummy variable indicating whether *at least one of the children* of parent *k* belongs to the post-reform birth cohort (1981–1984). As before, φ_{r} are area fixed effects, and $\omega_{\mathrm{c}} \times \pi_{\mathrm{p(r)}}$ is an interaction between parent *k*'s birth cohort and province dummies.[9] Finally, NC_τ is the number of children born in year τ to control for family size and age structure of the children, with associated regression coefficients ρ_τ.[10] As an extension of specification (3) we will also report results when we let the effect vary by whether parents have one treated child, versus more than one treated children (i.e., a different dose of the treatment).

For most variables in our analysis, missing values are rare, and we therefore proceed with a complete case analysis. The only exceptions are information on income from the VHLSS and information on parents' occupations from the Census. For these two variables, substantial shares of observations are missing, and we impute missing values with the mean value. This imputation cannot drive any of the reform effects because the mean of the variable is absorbed by the regression constant. However, the imputation has the advantage of making use of the full sample for the other variables in the regression model.

For a more straightforward interpretation of the regression analysis, unless otherwise stated, we standardize continuous variables with a mean of 0 and a standard deviation of 1. Moreover, standard errors are clustered at the district level to account for potential correlations across birth cohorts within the same district (Bertrand et al., 2004).[11] Economic theory and a large body of literature on labor market returns to schooling and on intergenerational educational spillovers lead us to expect positive signs on the returns and spillovers. Given this clear expectation, we use one-sided hypothesis testing, following for example (Heckman and Karapakula, 2019a,b).[12]

3.3. Identification

Interpretation of α_3, β_3 and γ_3 in Eqs. (1), (2) and (3) as causal effects of primary school expansion requires the parallel trends assumption to hold. In other words, there should be no diverging cohort trends in the outcomes between treatment and comparison areas that would lead to nonzero estimates of α_3, β_3 or γ_3 in the absence of the reform. Suppose for example that the treatment areas would be generally poorer and less developed, or that in the past they had been more negatively affected by the Vietnam war. If this would have attracted more public investments into these areas, then this could have triggered a catch-up process for the treatment areas that would have led to convergence with the comparison areas even in absence of the schooling reform. We address such identification concerns in a variety of ways.

First, we provide evidence of how treatment and control areas differ in terms of their observed characteristics. Online Appendix Table A.2 presents the results of cross-sectional, area-level regressions of areas' treatment statuses on their average observed characteristics. These characteristics include the age of the population, the composition of the population in terms of ethnicity and religious affiliation, an urban indicator, the affectedness of districts by U.S. bombing during the Vietnam war,[13] and the location of the area in Vietnam's six major economic zones (Southeast, Red River Delta, Mekong River Delta, Northern and Southern Central Coast, Central Highlands and Northern Midlands and Mountains). As expected, the treatment status of a area is strongly affected by its initial primary school completion rate across all specifications. Among the remaining regressors in column 2, the most significant associations are with the major economic zone dummies, and one ethnicity dummy appears weakly significant. Overall, this suggests relatively few systematic differences in observables apart from the initial primary school completion rate and broad geographic location.

Second, we conduct balancing tests to check the degree to which the identifying area-by-cohort variation in primary school completion correlates with trends in observables. For this purpose, we regress the area-by-cohort share of primary school completion on area-by-cohort specific observables, conditioning on cohort and area fixed effects. Given that the Census data is from 2009, we must be careful not to use observables that are likely to be outcomes of the reform, such as education, earnings or employment. We therefore use demographic characteristics of ethnicity and religious affiliation that can change in a area over time but are unlikely to change at the individual level in response to the reform. As shown in Online Appendix Table A.3, the only covariate that is statistically significantly correlated with the area-by-cohort share of primary school completion is the Tay ethnicity. Yet, this is with a negligibly small effect size that is unlikely to be of practical relevance.[14] We thus conclude that our identifying variation is not related to demographic trends in ethnicity or religious affiliation.

Third, we check for diverging pre-trends using an event study approach. We do this for the primary school completion rate of the directly affected generation, for schooling and child labor outcomes of their children, and health status of their parents. We regress each of the outcomes on treatment status, birth cohort dummies of the directly affected generation, and the corresponding cohort dummies interacted with treatment status (omitting the first phase-in cohort of 1977 as the reference group). For each cohort we then plot the coefficients of the treatment-cohort interaction terms in the event study graphs of Online Appendix Figure A.1. Across all outcomes there are small pre-treatment differences, moderate differences opening up in the phase-in cohorts, and more substantial differences emerging in the post-reform cohorts. Importantly, differences in pre-trends are mostly insignificant, with the main exception of the primary school completion rate of the directly affected generation, where even very small differences are statistically

[9] Because parents' birth cohorts are less densely populated, differently from the previous equations, in equation (3) birth cohort c is measured in 5-year intervals.

[10] To be precise, τ indexes every single birth year from 1974 to 1984 as well as the categories of before 1974 and after 1984.

[11] If instead we implement a two-way clustering at district level and region-by-year level, we find almost identical standard errors.

[12] There will be some outcomes, such as fertility or marriage outcomes, where the expected sign is ambiguous. In these cases, two-sided tests can be obtained by doubling the one-sided p-value. Thus, an outcome significant at the 1-percent level in a one-sided test will be significant at the 2-percent level in a two-sided test, and an outcome significant at the 5-percent level in a one-sided test, will be significant at the 10-percent level in a two-sided test.

[13] This is defined as the number of bombs, missiles, and rockets dropped by American air forces per km^2 during 1965-75 at the district level. This dataset is constructed by Miguel and Roland (2011).

[14] In 98% of the regions, the Tay ethnicity share changed by less than 6 percentage points between the pre-reform cohorts (1981–1984) and the post-reform cohorts (1974–1976). The coefficients of −0.0015 (Table A.3) would suggest that the extreme event of a 6-percentage point increase in the Tay ethnicity share would affect the primary school completion rate by only 0.9 percentage points (−0.0015*6=−0.009), which is about one tenth of the reform effect we show in Table 1.

significant because of the large sample size of the Census (1.6 million observations).

Fourth, as another way of verifying the validity of the common trends assumption, we define a placebo reform that assumes the reform took place in 1982, nine years prior to the actual reform in 1991. We estimate a similar model as Eq. (1) using only the 1965–1975 pre-reform birth cohorts, neither of which was affected by the real reform.[15] The results in Panel B of Online Appendix Table A.4 show that exposure to the placebo reform has no effect on the probability of completing primary school for both males (columns 1–2) and females (columns 3–4). As a further placebo test, in Panel A of the same table, we use again the original reform definition, but use the occupation of the parents of the directly affected generation as a placebo *outcome*. We use an indicator variable equal to 1 if the parent holds a highly skilled occupation, and equal to 0 otherwise. Since parents' occupational choices were shaped before the reform, there should be no reform effect on this outcome. The results confirm that the reform has no effect on parents' occupation.

Finally, despite all the encouraging evidence presented in this section, we make our regression models robust against a range of differential trends. As shown in Eqs. (1), (2) and (3), we include birth cohort effects interacted with province dummies throughout our analysis to allow cohorts trends to vary in unrestricted ways across provinces. Moreover, in robustness checks presented in the Online Appendix (Tables A.5, A.9-A.15) we augment Eqs. (1)-(3) by allowing for a linear cohort trend interacted with an urban-rural indicator, and a linear cohort trend interacted with a district's affectedness by U.S. bombing in the Vietnam war. This controls for the possibility of a general catch-up of rural areas or areas more affected by the war. All our results are remarkably robust against this inclusion, suggesting that a general catch-up is not driving our results. Overall, the results of these robustness checks and placebo tests make us confident that our findings are indeed driven by the real reform, not by violations of the common trends assumption due to diverging trends in unobservables.[16]

4. Results

4.1. Reform effect on the directly affected generation

We start by presenting the effect of exposure to the reform on the *directly* affected generation in Table 1, using a mix of outcomes from both the Census and VHLSS datasets. The results are based on regression Eq. (1) with additional interaction effects by gender in order to separate effects for men and women. Panel A of the table reports results on educational and economic outcomes.

The results in columns 1–3 of Panel A show that for both males and females exposure to the reform increases the probability of completing primary and secondary by about 10 percentage points, and that this translates into roughly one additional year of schooling. To further verify whether this affects their actual skills, we estimate effects on an indicator for literacy (the full ability to read and write) in column 4 of Panel A. Exposure to the reform improves literacy by about 1.3–1.4 percentage points for both males and females. This effect is highly statistically significant. Compared to the baseline illiteracy rate of about 3.3% for pre-reform cohorts in treatment areas (own calculations from Census data) this is a sizable effect, implying that reform exposure almost halved the illiteracy rate.

In the remainder of Panel A, we present results for economic activity, economic sector,earnings, and occupation. As shown in column 5, reform exposure increases the probability of being economically active by 1.4 percentage points for women, and 5 percentage points for men. For both men and women, it increases the probability of being active in the non-agricultural sector by 7 percentage points (column 6). Moreover, there is a strong increase in earnings of about 21–27 log points, which is slightly stronger for men (column 7). This is a sizable effect compared to conventional returns to schooling estimates. However, differently from conventional returns to schooling, our earnings effect includes a labor force participation effect because economically inactive individuals are included in the earnings variable with zero earnings.[17] Moreover, reform exposure affects earnings not only through an individual's own education, but potentially also through spillovers from the fact that many individuals in a local area are treated by the reform. In Appendix Table A.19 we explore effects on household expenditure per capita and don't find any effects. This might suggest that additional earnings are partly used for non-consumptive purposes, such as increased savings or transfers to family members. In column 8, we show that reform exposure also increases the probability of having a high-skilled occupation among the directly affected individuals by 2.2 percentage points for females and 5.2 percentage points for males.

In Panel B of Table 1, we investigate a range of family outcomes. We start with an indicator of having lived in the same commune over the past 5 years (an inverse measure of migration). Reform exposure makes it more likely for individuals to have remained resident in the same commune by 7 percentage points for women, and 3 percentage points for men (column 1). This could be a result of a stronger local economy due to higher local levels of education and economic activity and expansion of the non-agricultural sector. As columns 2 and 3 show, it also increases the probability of being married (4 percentage points for women, 12 percentage points for men), and it raises the average quality of the spouse as proxied by their years of schooling by around half a year for both women and men. This latter finding is in line with previous findings from both developed and developing countries (Hahn et al., 2018; Pencavel, 1998). These factors might be related, as the availability of spouses of higher quality in the local area might induce couples to marry earlier, and as a result be more likely to settle down in the local geographic area. This process might also partly explain the higher likelihood for women of having at least one child (8.3 percentage points – column 4). Another contributing factor to this could be the decrease in having experienced child mortality by 9.2 percentage points, as shown in column 5. If we control for having at least one child, on the other hand, we find that reform exposure *reduces* the number of children by approximately 0.04 (column 6). Positive fertility effects at the extensive margin and negative effects at the intensive margin can be rationalized by the theory developed in Aaronson et al. (2014). As these authors show, interventions that decrease the cost of investing in child quality, for example because of increased access to schooling or decreased child mortality, have positive fertility effects at the extensive margin, because

[15] In the placebo reform, the pre-placebo cohorts include individuals born between 1965–1967, the phase-in cohorts include individuals born between 1968–1971, and the post-placebo reform cohorts include those born between 1972–1975. The regions' primary school completion rates for the 1968 cohort are used as the initial level to define the placebo treatment and comparison regions, as done with the real reform. For the estimation, we obtain in total a sample for fathers of 726,409 observations and a sample for mothers of 748,326 observations from the Census data. The placebo reform cannot be applied to child outcomes, because children of the placebo reform cohorts may well be affected by the actual reform.

[16] It may be tempting to use the compulsory schooling reform as an instrumental variable (IV) to estimate the causal effect of primary school education on investments in children's human capital and on parental health. We refrain from implementing such an IV strategy because, as pointed out by Holmlund et al. (2011), the compulsory schooling reform is unlikely to meet the exclusion restriction that is required for it to be a valid instrument. This is because not only will a given focal individual gain a higher level of schooling with the reform, but also will other individuals of the same generation in the same regions. And these other individuals could directly or indirectly affect the children or the parents of the focal individual, violating the IV exclusion restriction.

[17] The log transformation of earnings is then performed after adding a constant of one.

Table 1
Reform effects on the directly affected generation.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. Education and economic outcomes							
	Primary school completion	Secondary school completion	Years of schooling	Literacy	Economically active	Non-agricultural sector	Log earnings	High skilled occupation
Treat × Post × Female	0.096***	0.110***	0.995***	0.014***	0.014***	0.068**	0.214*	0.022***
	(0.005)	(0.007)	(0.057)	(0.002)	(0.004)	(0.032)	(0.131)	(0.005)
Treat × Post × Male	0.101***	0.118***	1.052***	0.013***	0.051***	0.069***	0.269**	0.052***
	(0.005)	(0.007)	(0.055)	(0.001)	(0.004)	(0.028)	(0.148)	(0.004)
Data source:	*Census*	*Census*	*Census*	*Census*	*Census*	*VHLSS*	*VHLSS*	*Census*
Observations	1656,579	1656,579	1656,579	1656,579	1656,579	17,471	17,471	1656,579
	Panel B. Family outcomes							
	Lived in same municipality past 5 years	Married	Spouse's years of schooling	At least one child	Experienced child mortality	No. of children		
Treat × Post × Female	0.068***	0.040***	0.500***	0.083***	−0.092***	−0.041***		
	(0.004)	(0.005)	(0.044)	(0.006)	(0.006)	(0.011)		
Treat × Post × Male	0.027***	0.119***	0.456***	*n.a.*	*n.a.*	*n.a.*		
	(0.005)	(0.006)	(0.045)					
Data source:	*Census*	*Census*	*Census*	*Census*	*Census*	*Census*		
Observations	1656,579	1656,579	853,164	832,706	832,706	832,706		
	Panel C. Intergenerational persistence							
Dep. variable:	*Years of schooling of directly affected generation*							
Sample:	Child-parent	Son-mother	Daughter-mother	Son-father	Daughter-father			
Parental schooling	0.391***	0.363***	0.397***	0.391***	0.399***			
	(0.007)	(0.010)	(0.015)	(0.009)	(0.015)			
Treat × Post × Parental schooling	−0.084***	−0.069***	−0.070***	−0.027**	−0.011			
	(0.010)	(0.015)	(0.023)	(0.014)	(0.023)			
Data source:	*Census*	*Census*	*Census*	*Census*	*Census*			
Observations	415,370	236,127	91,600	187,957	71,804			

Notes: All specifications include Treat × Phase-in and fixed effects for cohorts-by-province, area, and (for VHLSS data) survey year. Further control variables are dummies for gender and ethnicity (Kinh, Tay, Thai and others). For the Census data, dummies for religious affiliations (Buddhism, others and no religious affiliations) are included into the set of control variables. Earnings are measured by the log of Vietnam Dong 1000 (in 2010 prices). The sample includes individuals born in 1974–1984. In column (1) of Panel C (child-parent) each child is included once, with parental schooling measured as the highest level of schooling among the parents. Significance levels are based on one-sided hypothesis tests. Robust standard errors clustered at the district level are in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Data source: Population and Housing Census of Vietnam (2009, 15% sample) and Vietnam Household Living Standards Survey (2010, 2012 and 2014).

taking advantage of the increased opportunities to invest in child quality requires having at least one child. At the intensive margin, such policies have negative effects, in line with the traditional quantity-quality trade-off.[18] One caveat to bear in mind when interpreting these findings is that the sample includes individuals aged 25–35. The marriage and fertility effects are therefore not necessarily 'completed' effects, and they might partly result from changes in marriage and birth timing.

We further examine the reform effects on the intergenerational persistence of schooling between individuals of the directly affected generation and their parents. Our estimation is based on an augmented version of Eq. (1) with years of schooling of an individual of the directly affected generation on the left hand side. The right hand side is augmented by adding parental years of schooling and its interactions with $\mathrm{Treat} \times \mathrm{Post}$, $\mathrm{Treat} \times \mathrm{Phasein}$, Treat, Post and $\mathrm{Phasein}$. While the coefficient for parental schooling indicates the baseline intergenerational persistence of schooling, the coefficient for $\mathrm{Treat} \times \mathrm{Post} \times \mathrm{Parental\ Schooling}$ measures how exposure to the reform affects the intergenerational persistence. The coefficient in Panel C of Table 1 on parental schooling indicates a baseline intergenerational persistence of about 0.4 years of schooling. The interaction effect shows that reform exposure reduces the dependence of schooling of the directly affected generation on their parents' schooling. The coefficient in column 1 shows a reduction of approximately 0.08 for the overall child-parent pairs. The four remaining columns of Panel C in Table 1 show the results broken down by child and parental gender pairs. Here we find the reduction of the intergenerational persistence to be about 0.07 for both son-mother and daughter-mother pairs in columns 2 and 3 (both statistically significant at the 1% level), and about 0.03 for son-father pairs in column 4 (statistically significant at the 5% level), with no significant moderating effect for daughter-father pairs (column 5).[19]

Overall, our results suggest that the directly affected generation benefitted from the reform through increased educational attainment, literacy, and economic activity (with a shift away from agriculture), as well as higher labor market earnings. It also increased the likelihood of being married, the quality of the marital partner, and the probability of having children. Conditional on having children, however, it decreased family size. Moreover, the reform increased intergenerational educational mobility for the directly affected generation. In Online Appendix Table A.5, we find these effects to be highly robust against adding linear cohort trends interacted with a rural indicator and U.S. bombings in the Vietnam war.

4.2. Intergenerational spillover effects on the children's generation

The reform effects documented in the previous section might generate spillover effects on school attainment and investments into the human capital of the *children* of the generation directly affected by the reform. The improved education and skills may enable the directly affected generation to better understand the importance of human capital investments for their children's long-term success (Lundborg et al., 2014; Lundborg et al., 2018) and the implications for their children's ability to support parents in old age (Becker et al., 2016). Better education may also lead to being better informed about school choices and enrollment processes, and it may foster attitudes and values in favor of educational investments (Figlio et al., 2019; Piopiunik 2014). The increased probability of economic activity, a high-skilled occupation and higher earnings might allow the directly affected generation to invest more monetary resources into their children (Bruins, 2017; Calero, 2009).[20] This effect could be amplified by the decreased family size (conditional on having children), as the theory of quantity-quality trade-offs would suggest (Hanushek, 1992; Rosenzweig and Zhang, 2009). Our finding that the directly affected individuals also choose higher educated marital partners could further amplify downward spillover effects, given that higher educated spouses may increase their partner's own economic outcomes (Huang et al., 2009; Lefgren and McIntyre, 2006) and contribute directly to overall household resources. In this section we therefore explore downward intergenerational spillover effects on school attainment and investments into the human capital of the children of the directly affected generation. We start with the main results, followed by gender heterogeneity.

4.2.1. Main results

We derive the effects of exposure to the reform on investments into children's human capital, based on regression Eqn 2. In Table 2 we investigate school enrollment, school tuition, and other educational expenditures and, in Table 3 child labor, and in Table 4 health investments.

Column 1 in Table 2 displays the effects of exposure on school enrollment for children aged 6–17. Exposure of at least one parent to the reform increases the probability of a child's school enrollment by 8.4 percentage points (column 1), an effect that is highly statistically significant at the 1% level. We verified that this effect operates entirely via secondary school attendance, as one would expect because primary school is compulsory for everyone in the children's generation.[21] Given very high levels of school attendance at baseline (just above 90% of children of pre-reform parents in treatment areas), the effect size implies that the reform brought school attendance of the children's generation at primary and secondary school to a universal level.

Reform exposure of at least one parent also increases payments for children's school tuition, by 18% of a standard deviation, statistically significant at the 1% level (column 2). A range of school types in Vietnam charge tuition fees, and higher tuition fees are thought to indicate a higher quality in terms of skill development, educational services, teaching resources and facilities (Glewwe and Patrinos, 1999). In principle, the increase in tuition fees could reflect parents choosing higher quality schools, holding enrolment constant. But it could also be simply driven by the increase in enrolment itself, given that non-enrolled children pay zero tuition fees. To check this, we estimate the tuition equation conditional on enrolment (column 3), and we find that there is still a weakly statistically significant effect of 9.5% of a standard deviation on tuition fees.

For further types of educational expenditure in the remaining columns of Table 2, we find that reform exposure of at least one parent increases spending on children's books and learning materials, learning tools and instruments, and total school-related spending. Effect sizes are roughly similar around 20% of a standard deviation and statistically significantly at the 5% level. There is no effect, however, on private tutoring. While private tutoring has been found to be effective in improving children's academic performance in developing countries

[18] The reform we study not only potentially lowers the cost of investing in child quality, but it also provides improved earnings and employment opportunities for mothers, from which one would expect a negative extensive margin effect (Aaronson et al., 2014). Our empirical results suggest that the positive extensive margin effect outweighs the negative one.

[19] For column (1) of Panel C, we measure parental education by the highest level of education obtained by one of the parents. One caveat for the analysis in Panel C is that child-parent pairs can only be observed in the Census if they live in the same household. As sons are more likely to live with their parents than daughters, our sample includes more sons. The data also contains more mother-child pairs than father-child pairs because of the higher life expectancy of females, and because absent fathers are more likely than absent mothers. Nevertheless, the magnitude of our intergenerational persistence estimate is broadly in line with international evidence. For earlier birth cohorts (1929-1978), Hertz et al. (2007) find a higher estimate of 0.6 for Vietnam and they document a falling trend over cohorts. Our smaller value of 0.4 estimated on later birth cohorts therefore seems very plausible.

[20] This mechanism may work stronger in developing countries where numerous children reside in financially-restricted families while there have been typically insufficient public investments (Strauss and Thomas, 1995).

[21] We found that effects for children of primary and lower secondary school age are close to zero and statistically insignificant, while the effect at upper secondary school age is 0.097 and statistically significant at the 1%-level.

Table 2
Reform effects on the children's generation: Investments into schooling and learning.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		School related expenditures						Home expenditures	
	School enrolment	Tuition	Tuition, conditional on enrolment	Books and learning materials	Learning tools and instruments	Private tutoring	Total school related spending	Children's books and magazines	Children's toys
At least one treated parent × Post	0.084***	0.180**	0.095*	0.201**	0.224**	−0.008	0.209**	0.223**	0.176*
	(0.027)	(0.091)	(0.073)	(0.095)	(0.126)	(0.099)	(0.106)	(0.096)	(0.121)
Observations	9237	9237	8694	9237	9237	9237	9237	9237	9237

Notes: All specifications include At least one treated parent × Phase-in and fixed effects for survey year, paternal cohort-by-province, maternal cohort-by-province, and area. Control variables for child characteristics: dummies for gender (son), ethnicities (Kinh, Tay, Thai and others), and ages. Expenditure variables are based on log expenditure (in 2010 prices), standardised into a mean of zero and a standard deviation of one. The sample includes children aged 6–17 of parents born in 1974–1984. Significance levels are based on one-sided hypothesis tests. Robust standard errors clustered at the district level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
Data source: Vietnam Household Living Standards Survey (2010, 2012 and 2014).

Table 3
Reform effects on the children's generation: Child labor outcomes.

	(1) Working for the household	(2) Working for earnings	(3) Hours of work for earnings
At least one treated parent × Post	−0.056***	−0.037***	−0.400***
	(0.021)	(0.012)	(0.133)
Observations	9237	9237	9237

Notes: All specifications include At least one treated parent × Phase-in and fixed effects for survey year, paternal cohort-by-province, maternal cohort-by-province, and area. Control variables for child characteristics: dummies for gender (son), ethnicities (Kinh, Tay, Thai and others), and ages. The sample includes children aged 6–17 of parents born in 1974–1984. Significance levels are based on one-sided hypothesis tests. Robust standard errors clustered at the district level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
Data source: Vietnam Household Living Standards Survey (2010, 2012 and 2014).

Table 4
Reform effects on the children's generation: Investments into health.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Per capita household spending on food consumption	Household spending on tobacco and cigarettes	Health insurance coverage	Number of preventive health visits	Preventive health care expenditure - Public	Preventive health care expenditure - Private	Hospital-ization
At least one treated parent × Post	0.230***	−0.147	0.038	0.189	−0.143	0.145*	−0.026*
	(0.096)	(0.130)	(0.041)	(0.218)	(0.127)	(0.107)	(0.017)
Observations	9237	9237	9237	9237	9237	9237	9237

Notes: All specifications include At least one treated parent × Phase-in and fixed effects for survey year, paternal cohort-by-province, maternal cohort-by-province, and area. Control variables for child characteristics: dummies for gender (son), ethnicities (Kinh, Tay, Thai and others), and ages. Expenditure variables are based on log expenditure (in 2010 prices), standardised into a mean of zero and a standard deviation of one. The sample includes children aged 6–17 of parents born in 1974–1984. Significance levels are based on one-sided hypothesis tests. Robust standard errors clustered at the district level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
Data source: Vietnam Household Living Standards Survey (2010, 2012 and 2014).

(Banerjee et al., 2007; Dang and Rogers, 2008), it has traditionally been uncommon in many areas in Vietnam (Dang, 2007). Increased parental exposure to compulsory schooling in our treatment areas does not seem to have noticeably increased this practice. The last two columns of Table 2 include results on educational expenditures related to the home rather than school. They show statistically significant effects on children's books and magazines (22.3% of a standard deviation) and toys (17.6% of a standard deviation).

In Table 3, we investigate child labor. We interpret it as a negative investment in children's human capital, as it has been shown to be detrimental to educational attainment (Beegle et al., 2009), academic performance (Goulart and Bedi, 2008) and health (O'Donnell et al., 2005). We find strong and statistically significant effects on child labor of the next generation. Columns 1 and 2 show that children of parents who were exposed to the reform are also less likely to work for the household and to have a paid job outside of the household, with an effect size of 5.6 and 3.7 percentage points, respectively (both statistically significant at the 1% level). Finally, as column 3 shows, parental exposure also reduces children's daily hours of paid work, by close to 0.4 h (statistically significant at the 1% level).

In Online Appendix Table A.6, we show that these effects also exist for children aged 6–14, below the legal minimum age of 15 for labor market participation. We find that, although the effects are smaller than in the overall sample (slightly smaller for the probability of working for the household, and nearly half for working for earnings and the hours or work), parental exposure continues to have statistically significant effects for all of these child labor outcomes.

Health is an important component of human capital and ill-health can hinder school attendance and skill acquisition. In Table 4, we explore how parents' exposure to the school expansion reform affects investments into their children's health. Given that children's malnutrition has negative effects on long-run outcomes and is still a concern in developing countries (Currie and Vogl, 2013; Strauss and Thomas, 1998), we start by considering the household's per capita food consumption as our first proxy for health investments and find a strongly significant positive effect of 23% of a standard deviation (column 1). In column 2, we find a reduction in household spending on tobacco and cigarettes though the estimate is not statistically significant.

In columns 3 and 4 we investigate the child's health insurance coverage, and the number of preventive health visits to health centers. While we find positive point estimates for both, these are not statistically significant. Columns 5 and 6 reveal no statistically significant effect on preventive *public* health care expenditures (although point estimates point to a decrease) and a positive effect on preventive *private* health care expenditures (statistically significant at the 10% level). This points towards parents spending more on their children's health within the private health care system which is reputed as having a higher quality than the public system in Vietnam (Nguyen and Wilson, 2017). Finally, we look at the probability of the child having been hospitalized within the past year. Acute ill health requiring hospitalization may in part be caused by a lack of parental health investments, and hospitalization potentially involves absenteeism from school. For these reasons, we interpret the reduction in hospitalizations of 2.6 percentage points (column 7, weakly statistically significant) as a beneficial effect on health investments.

In the Online Appendix, we present additional evidence to rule out that the effects presented in this section are affected by differences in children's ages. If exposure to the compulsory schooling reform causes parents to postpone or speed up child bearing, the children of treated and untreated parents would have a different age at the time of the survey. If educational investments or incidences of child labor vary by child's age (e.g., are higher in secondary than in primary school), this could affect or even drive our results (although this concern is already mitigated by controlling for age fixed effects). Reassuringly, as we show in Online Appendix Table A.7, parental exposure to the reform has no effect on children's age at the time of the survey.

4.2.2. Heterogeneity by gender

We now investigate whether the results reported so far are heterogeneous by the children's gender. Improving understanding of sex differences in parental investments in children's human capital is important as it may help explain gender gaps in social and economic outcomes in adulthood (Baker and Milligan, 2016; Booth and Nolen, 2012; Brenøe and Lundberg, 2018).

In Table 5, we focus on a selection of the outcomes from the previous Tables 2-4. A more comprehensive analysis for all outcomes is in Online Appendix Table A.8. While point estimates show beneficial effects of parental exposure to the reform for children of both genders, it is striking that effect sizes are relatively larger for sons than for daughters. For school enrolment (column 1), the effect for sons exceeds that for daughters by a factor of 1.4. For school tuition, educational spending and private health expenditures this rises to a factor of 2–2.5, and for child labor to a factor of 4. Furthermore, the effects on tuition, total educational spending, child labor, and health expenditure are statistically significant for boys only. The third row of coefficients in Table 5 reports the differences in the effects between sons and daughters. With the exception of total school related spending and working for earnings, we cannot reject the hypothesis that the effects are equal between genders. Nevertheless, it is intriguing that all effect differences point to the same direction, favoring boys.

These imbalances may well reflect preferences for sons in Asian cultures such as Vietnam (Bharadwaj and Lakdawala, 2013), which may affect how parents choose to distribute additional monetary investments between sons and daughters (Barcellos et al., 2014; Choi and Hwang, 2015; Jayachandran and Kuziemko, 2011).[22] In the last row of coefficients in Table 5 we show, however, that the (unconditional) pre-reform difference in these investments was not in all cases skewed in favor of boys. Prior to the reform boys experienced lower school enrollment and school-related spending, and more hours of child labor. Judging from the point estimates, the reform effects would then have closed or in some outcomes even have over-compensated these differences in favor of boys.

Overall, we conclude from this section that the compulsory schooling reform not only improved the quantity and quality of schooling of the *children* of the directly affected generation, as well as the monetary investments in their education, but also had a significant preventative effect on child labor, and positive effects on health investments. Moreover, these effects tend to be larger for sons compared to daughters. To our knowledge, such comprehensive intergenerational spillover effects of primary school expansion have not been shown in the literature before. In further robustness checks presented in Online Appendix Tables A.9-A.12, we show that the results presented in this section are highly robust against including linear paternal and maternal cohort trends interacted with rural/urban status and with districts' affectedness by U.S. bombings during the Vietnam war.

4.3. Intergenerational spillover effects on the parent's generation

Previous literature has argued that having more children contributes to economic security in old age due to intergenerational support from offspring to parents (Banerjee et al., 2010; Chen and Fang, 2018; Oliveira, 2016). Recent studies have emphasized that the *quality* of children (e.g., their level of schooling) also matters for upward intergenerational support and thus may affect the later-life outcomes of parents (De Neve and Fink, 2018; P. Lundborg and Majlesi, 2018; Ma, 2019). The role of children for old-age parents is likely to be particularly strong in developing countries where social security systems are poorly functioning and financial markets are underdeveloped. In this section, we investigate upward intergenerational spillover effects, on old age health outcomes of the *parents* of the generation directly affected by the reform. We start with the main results, followed by an analysis of mechanisms.

4.3.1. Main results

Table 6 presents effects of exposure to the reform on one's parents' old age health outcomes based on regression Eq. (3). We present the baseline effect in Panel A and heterogeneity by parental gender in Panel B. The age range of parents included in the sample is from 60 to 91, with an average age of 68.5.

With respect to the *general* health outcomes in the first three columns, we find that having at least one child exposed to the reform reduces the annual days of sickness by 10 days (column 1). We also find a roughly 10 percentage points increase in general self-reported health, both absolute (column 2) and relative to peers of the same age range (column 3). With respect to *mental* health outcomes in the last three columns, Panel A shows no overall effects on depression, sleep problems, or general life satisfaction. With respect to the gender of the parent, Panel B shows that even though there are some gender differences for specific outcomes there is no general pattern that would indicate that the effects benefit only mothers or only fathers. The difference in the effects for mothers and fathers are not statistically significant, except for sleep problems, where there is a beneficial effect for fathers but not for mothers of treated children (column 5, Panel B). To sum up, we find positive overall effects on *general* health, and some indication of a beneficial mental health effect for fathers.

[22] In other cultural settings, the results may be different. For example, Baker and Milligan (2016) find that parents spend time on teaching activities more with daughters than with sons in Canada, the United States and the United Kingdom.

Table 5
Reform effects on the children's generation: Heterogeneity by child gender.

	(1) School enrolment	(2) Tuition	(3) Total school related spending	(4) Hours of work for earnings	(5) Preventive health care expenditure - Private	(6) Hospital-ization
At least one treated parent × Post × Daughter	0.065**	0.111	0.117	−0.084	0.094	0.034
	(0.031)	(0.101)	(0.116)	(0.157)	(0.122)	(0.033)
At least one treated parent × Post × Son	0.093***	0.232**	0.292***	−0.330**	0.211**	0.044
	(0.029)	(0.107)	(0.116)	(0.148)	(0.127)	(0.045)
Effect difference (son-daughter)	0.028	0.121	0.176**	−0.246*	0.117	0.009
	(0.026)	(0.102)	(0.095)	(0.162)	(0.120)	(0.033)
Unconditional pre-reform gender difference(son-daughter)	−0.022***	−0.027	−0.076***	0.121***	0.033	0.007
	(0.006)	(0.023)	(0.023)	(0.036)	(0.023)	(0.004)
Observations	9237	9237	9237	9237	9237	9237

Notes: All specifications include At least one treated parent × Phase-in and fixed effects for survey year, paternal cohort-by-province, maternal cohort-by-province, and area. Control variables for child characteristics: dummies for gender (son), ethnicities (Kinh, Tay, Thai and others), and ages. Expenditure variables are based on log expenditure (in 2010 prices), standardised into a mean of zero and a standard deviation of one. The sample includes children aged 6–17 of parents born in 1974–1984. Significance levels are based on one-sided hypothesis tests. Robust standard errors clustered at the district level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
Data source: Vietnam Household Living Standards Survey (2010, 2012 and 2014).

Table 6
Reform effects on the parent's generation: Health outcomes.

	(1) Days of sickness	(2) Good self-reported health status	(3) Relative good health status (compared to others)	(4) Depression	(5) Sleep problems	(6) Satisfied with life
	Panel A. Baseline					
At least one treated child × Post	−10.031**	0.088***	0.120***	−0.001	−0.020	0.042
	(5.634)	(0.036)	(0.048)	(0.003)	(0.042)	(0.048)
	Panel B. Heterogeneity by parental gender					
At least one treated child × Post × Mother	−7.683**	0.075**	0.076***	−0.001	0.025	0.039
	(3.389)	(0.034)	(0.032)	(0.003)	(0.040)	(0.042)
At least one treated child × Post × Father	−3.553	0.082*	0.141***	0.002	−0.127***	0.063
	(5.782)	(0.053)	(0.054)	(0.006)	(0.050)	(0.054)
Effect difference (father-mother)	4.130	0.007	0.065	0.003	−0.152**	0.024
	(6.350)	(0.068)	(0.064)	(0.007)	(0.067)	(0.067)
Unconditional pre-reform gender difference(father-mother)	4.552	0.101***	0.097***	0.003	−0.087***	−0.061**
	(2.880)	(0.025)	(0.027)	(0.004)	(0.023)	(0.025)
Observations	1818	1818	1818	1818	1818	1818

Notes: All specifications include At least one treated child × Phase-in and fixed effects for area and five-year-age-interval-by-province (age intervals defined as 60–64, 65–69, 70–74, 75–79, and >=80 years old). Control variables: gender, ethnicity (Kinh), and dummies indicating the number of children by birth cohort category (before 1974, 1974, 1975, 1976, 1977, 1978, 1979, 1980, 1981, 1982, 1983, 1984, and after 1984). Sample incluses the individuals aged 60 and above who have children born in 1974–1984. Significance levels are based on one-sided hypothesis tests. Robust standard errors clustered at the district level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
Data source: Vietnam Aging Survey (2011).

4.3.2. Mechanisms

One important mechanism why old age parents could be in better health if their children had access to better education is an improved financial situation, which could allow them access to better health care or earlier retirement. A second important class of mechanisms are better health behaviors (Ma, 2019). According to these broad classes of mechanisms, we split our analysis up into *financial* mechanisms (Table 7) and *behavioral* mechanisms (Table 8).

With regards to the overall effects in Panel A of Table 7, we find no reform effects on indicators for poverty, for whether the income meets personal needs, or whether parents receive income support from their children, although these estimates all point into the direction of improvement. We do however find positive effects in columns 4–6 on an indicator for having savings, on household assets (an index summing up items such as having a refrigerator, a water heater, an improved sanitation, etc.) and on satisfaction with the financial status. One reason why assets and financial satisfaction might be improved despite no significant improvements in income could be if better educated children require less financial support from their parents, allowing the parents to increase savings and other assets. Column 7 further shows an effect

Table 7
Reform effects on the parent's generation: Potential financial mechanisms.

	(1) Poverty	(2) Income meets personal needs	(3) Regular income support from children	(4) Savings	(5) Household assets	(6) Satisfied with financial status	(7) Private health insurance
	Panel A. Baseline						
At least one treated child × Post	−0.003	0.059	0.019	0.088***	0.203**	0.066**	0.017*
	(0.039)	(0.050)	(0.040)	(0.027)	(0.089)	(0.030)	(0.012)
	Panel B. Heterogeneity by parental gender						
At least one treated child × Post × Mother	0.009	0.024	0.018	0.034*	0.130**	0.034*	0.010
	(0.030)	(0.042)	(0.037)	(0.023)	(0.071)	(0.026)	(0.012)
At least one treated child × Post × Father	0.025	0.058	−0.029	0.071**	0.123*	0.069**	0.020**
	(0.039)	(0.050)	(0.060)	(0.032)	(0.096)	(0.035)	(0.012)
Effect difference (father-mother)	0.016	0.034	−0.047	0.037	−0.007	0.035	0.010
	(0.050)	(0.063)	(0.073)	(0.036)	(0.115)	(0.041)	(0.015)
Unconditional pre-reform gender difference(father-mother)	−0.042**	0.039	−0.073***	−0.014	0.081	0.049***	−0.002
	(0.020)	(0.026)	(0.026)	(0.016)	(0.069)	(0.018)	(0.006)
Observations	1818	1818	1818	1818	1818	1818	1818

Notes: All specifications include At least one treated child × Phase-in and fixed effects for area and five-year-age-interval-by-province (age intervals defined as 60–64, 65–69, 70–74, 75–79, and >=80 years old). Control variables: gender, ethnicity (Kinh), and dummies indicating the number of children by birth cohort category (before 1974, 1974, 1975, 1976, 1977, 1978, 1979, 1980, 1981, 1982, 1983, 1984, and after 1984). Sample incluses the individuals aged 60 and above who have children born in 1974–1984. Significance levels are based on one-sided hypothesis tests. Robust standard errors clustered at the district level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
Data source: Vietnam Aging Survey (2011).

Table 8
Reform effects on the parent's generation: Potential behavioural health mechanisms.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Smoking	Alcohol consumption, at least…			Social activities	At least one grand-child	Number of children-in-law	Household size
		2–3 times per week	4–6 times per week	once per day				
	Panel A. Baseline							
At least one treated child × Post	−0.018	−0.059**	−0.055**	−0.038*	−0.019	0.007	0.273***	0.129
	(0.035)	(0.026)	(0.027)	(0.026)	(0.049)	(0.012)	(0.099)	(0.209)
	Panel B. Heterogeneity by parental gender							
At least one treated child × Post × Mother	−0.017	−0.003	0.005	0.015	−0.034	0.001	0.291***	−0.028
	(0.026)	(0.019)	(0.017)	(0.017)	(0.037)	(0.014)	(0.089)	(0.150)
At least one treated child × Post × Father	−0.048	−0.103***	−0.128***	−0.114***	0.075*	0.006	0.142*	0.159
	(0.043)	(0.043)	(0.044)	(0.041)	(0.056)	(0.011)	(0.093)	(0.195)
Effect difference (father-mother)	−0.031	−0.100*	−0.133***	−0.129***	0.109*	0.005	−0.149	0.187
	(0.050)	(0.053)	(0.051)	(0.048)	(0.060)	(0.017)	(0.127)	(0.250)
Unconditional pre-reform gender difference(father-mother)	0.327***	0.279***	0.234***	0.216***	−0.065**	−0.002	0.132	0.070
	(0.021)	(0.018)	(0.017)	(0.017)	(0.027)	(0.006)	(0.112)	(0.116)
Observations	1818	1818	1818	1818	1818	1818	1818	1818

Notes: All specifications include At least one treated child × Phase-in and fixed effects for area and five-year-age-interval-by-province (age intervals defined as 60–64, 65–69, 70–74, 75–79, and >=80 years old). Control variables: gender, ethnicity (Kinh), and dummies indicating the number of children by birth cohort category (before 1974, 1974, 1975, 1976, 1977, 1978, 1979, 1980, 1981, 1982, 1983, 1984, and after 1984). Sample incluses the individuals aged 60 and above who have children born in 1974–1984. Significance levels are based on one-sided hypothesis tests. Robust standard errors clustered at the district level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
Data source: Vietnam Aging Survey (2011).

of 1.7 percentage points on having a private health insurance which is expected to provide the elderly with better health care compared to a public health insurance. Heterogeneity results in Panel B suggest that these effects are not statistically significantly different by parental gender.

Looking at behavioral mechanisms, Table 8 reveals no effect on smoking of old age parents, but very sizable reductions in alcohol consumption, driven entirely by fathers. As columns 2–4, Panel B, show, the probability of both moderate and daily alcohol consumption reduces by 10 percentage points for fathers. In the remaining columns we investigate social contacts within and outside the family, which may affect mental health (Meng and Xue, 2020). There is evidence for an increase in social activities outside the house for fathers of treated children (column 5, Panel B). Finally, while there is no effect on having at least one grandchild (column 6) or on overall household size (column 8), there is an increase in the number of children in law (i.e., one's children being married—column 7). This result matches the positive effect on the marriage probability of the directly affected generation (Table 3 above). A higher number of children in law might affect parental outcomes positively in line with the strong evidence on the positive effects of the number of own children on old-age support (Oliveira, 2016).

To sum up, we find some positive effects of the primary school expansion on the old age health of the parents of the directly affected generation. These appear to be primarily physical rather than mental health effects, and relevant mechanisms include a better financial situation, private health insurance and, for fathers, reduced drinking and improved social activities. In the Online Appendix (Tables A.13-A.15), we find the results presented in this section to be highly robust against allowing age effects to differ between rural and urban areas and by districts' affectedness by U.S. bombings during the Vietnam war. In a further check to our baseline specification, we allow the effect to vary by whether parents have one treated child or more than one treated child (Online Appendix Tables A.16-A.18). Arguably, parents with more than one treated child receive a higher dose of the treatment. In line with this idea, we find that the effects tend to be stronger when more than one child is treated.

5. Discussion and conclusion

We have studied the multigenerational effects of primary school expansion in Vietnam sparked by the country's 1991 schooling reform. Our results show that the reform had a strong positive effect of 10 percentage points on primary and secondary school completion of the *directly affected cohorts*. Through this effect, the reform increased the intergenerational educational mobility. Reform exposure also increased literacy, economic activity outside the agricultural sector, and earnings. It raised residential stability, the probability of being married, the level of education of the marital partner, and it increased fertility at the extensive margin, while reducing it at the intensive margin. In terms of *downward spillovers*, we document effects on the affected cohorts' investments in their children's human capital. The results indicate that parental exposure to the reform raised children's school enrollment and parental spending on school-related expenses (tuition fees, textbooks) and other educational spending (children's books and magazines, toys). Moreover, we find evidence for an increase in health investments and a reduction in child labor. Most of these downward spillover effects benefit sons more than daughters. This may be a driver of gender gaps in achievement in adulthood (Barcellos et al., 2014; Chetty et al., 2016), and the gender pattern mirrors results reported in causal studies on the intergenerational transmission of human capital (Black et al., 2005; Lundborg et al., 2014). Our findings complement a comparatively sparse literature on the mediating channels of the intergenerational transmission of education. While we focus on monetary investment in children's health and human capital, Carneiro et al. (2013) focus on the home environment and parental investments in children's cultural knowledge, musical skills, reading ability, computer skills, and joint home activities. Piopiunik (2014) isolates the degree to which parents value education as an important channel. By showing that *downward* intergenerational spillovers operate through parental investments in their children's human capital, we also contribute to the literature on the determinants of parental investments (Attanasio, 2015). The evidence available on the link between parental schooling and their investments in child human capital is mainly correlational (Brown, 2006; Strauss and Thomas, 1995). The literature has highlighted determinants such as child endowments (Adhvaryu and Nyshadham, 2016), parental beliefs (Kinsler and Pavan, 2016), fertility (Rosenzweig and Zhang, 2009), gender and race (Jayachandran and Kuziemko, 2011; Thompson, 2018), household characteristics (Newman and Holupka, 2014), and labor market conditions (Majlesi, 2014). Our results suggest exposure to primary school expansion as an additional causal determinant of investments into one's children's human capital through the various channels we identify.

In terms of *upward spillovers*, we find that the primary school expansion had positive general health effects in old age for the *parents* of the directly affected generation. As potential channels of these effects, our results indicate improved financial resources in old age, access to private health insurance and reduced alcohol consumption. Prior research on the causal determinants of health in old age has emphasized the roles of economic conditions (De Nardi et al., 2009), early-life conditions (Bhalotra et al., 2017; Case and Paxson, 2009), retirement decisions (Atalay et al., 2019; Fabrizio and Franco, 2017), child migration (Antman, 2010; Böhme et al., 2015), home care (Barnay and Juin, 2016), social pensions and government assistance programs (Fetter and Lockwood, 2018; Salm, 2011). Our results add to this literature by focusing on the human capital of the younger generation as an important driver of the old age individuals' health and well-being.

While educational attainment has long been recognized as one of the most powerful and sustainable tools for transforming human lives and promoting economic development (Krueger and Lindahl, 2001; Manuelli and Seshadri, 2014; Oreopoulos and Salvanes, 2011), fully understanding the social returns of educational policies is very much a matter of ongoing research. Prior literature has often emphasized spillovers on economic outcomes, including aggregate productivity and economic growth (Lange and Topel, 2006; Moretti, 2004a). Micro-level studies have documented spillovers on plant productivity (Moretti, 2004b), crime (Lochner and Moretti, 2004), and voter participation (Moretti et al., 2003). Our paper shows that the *family* is an important social group in which spillovers materialize across generations, including on non-economic outcomes such as health.

Our findings show that downward and upward intergenerational spillover effects constitute important components of the social returns of compulsory schooling policies such as primary school expansion. Moreover, we show that in a developing country, increased monetary investments in children's health and education and decreased instances of child labor may be important drivers of the downward spillover effect, while the upward spillover effect on parental health seems to be driven by both financial resources and health behaviors. These channels have received little attention in the literature trying to disentangle the channels of intergenerational spillover effects of human capital.

Overall, our findings imply that through both long-term effects on the directly affected generation and external effects within families on the next and the previous generation, enacting and enforcing compulsory primary schooling has high social returns. Given recent estimates for low-income countries that put the average primary school completion rate at 59% (UNESCO, 2019) and the prevalence rate of child labor at 19% (ILO, 2017), our results carry particular policy relevance for those countries and call for increased efforts in achieving universal primary school completion. More generally, however, given that positive externalities of education are among the predominant economic rationales for the provision of publicly funded education (Hanushek 2002), our results reinforce the economic rationale for providing free compulsory primary school education, irrespective of the particular context.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.labeco.2022.102243.

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