The Multigenerational Impacts of Educational Expansion: Evidence from Vietnam

Thomas Cornelissen and Thang Dang[*]

March 2021

Abstract

We exploit a compulsory schooling reform in Vietnam to investigate the multigenerational effects of public primary school expansion. In the *directly affected generation*, the expansion increases educational attainment, literacy, non-agricultural economic activity, earnings and the intergenerational educational mobility. It increases marriage rates and fertility at the extensive margin, but reduces fertility at the intensive margin. The expansion 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. Finally, the expansion improves the health in old age of the *parents* of the directly affected generation. They benefit from improved general health, but not mental health, as well as improved financial resources, access to private health insurance and reduced alcohol consumption.

[*] Cornelissen: Department of Economics, University of Essex, CReAM and IZA, Wivenhoe Park, Colchester CO4 3SQ, United Kingdom (e-mail: t.cornelissen@essex.ac.uk); Dang: Department of Economics, University of York, Heslington, York YO10 5DD, United Kingdom (e-mail: thang.dang@thangdang.org).

1

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, forthcoming), 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).

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;

---

[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.

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 regions with initially low levels of primary schooling (i.e., treatment regions), but had little impact in regions in which primary school completion was already high (i.e., control regions). 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 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

3

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 9 percentage points. Spending on tuition fees increases by 14-24% of a standard deviation and other education-related spending, such as on books and learning materials, by 14-20% 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 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.,

---

[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).

4

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 are able to 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 labour. 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; Lundborg and Majlesi, 2018; Ma, 2019; Potente et al., 2020). While 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 extending 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

---

[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, 2019; 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.

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 socio-economic background (Chetty et al., 2020). Prior research has documented the importance of the neighborhood conditions during childhood (Chetty and Hendren, 2018a, 2018b), type of college education (Chetty et al., 2017b), income shocks (Bütikofer et al., 2018), disease control (Bütikofer and Salvanes, 2020), and the timing of parental income during childhood (Carneiro et al., 2021). 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 at., 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

---

[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.

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 regions. 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 source 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 9,000 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 4,007 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 regions 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

8

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 healthcare 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 1,000 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 regions and event study of pre-trends

Before the reform, educational attainment was heterogeneous across different regions 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 at., 2007). Given that the compulsory schooling law mandated all regions to achieve universal primary schooling, regions with lower initial levels of primary school completion experienced a stronger expansion. Based on this idea, we define treatment regions as those with low initial school enrollments and comparison regions as those with high initial enrollments. We define regions 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 region's average rate of primary school completion for the 1977 birth cohort using the Census sample. We define treatment regions as those with a below-median initial level and comparison regions as those with an above-median initial level. Among a total of 1,272 included regions, 636 are defined as treatment regions and 636 as comparison regions. For the treatment region definition, and in order to assign treatment status to individuals observed in our data, we rely on the region of residence observed for adults in the respective data waves (between 2009 and 2014) as a proxy for their region of residence when they were at school age. As has been argued by Dell et al. (2018), supported by several empirical sources, residential mobility has traditionally been low in Vietnam. 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. To the extent that residential mobility is random, we would expect it to cause attenuation bias into our estimates, meaning that we can interpret our results as lower bound estimates.

Figure 1 illustrates the evolution of primary school completion rates across the cohorts born from 1974–1990 separately for treatment and comparison regions. The vertical bars illustrate the pre-reform, phase-in and post-reform cohorts, as defined in Section 2.1. By definition, comparison regions initially had considerably higher primary schooling rates than treatment regions, but trends are fairly parallel over pre-reform cohorts. However, this pattern gradually changes for cohorts affected by the reform. While rates in comparison regions are highly stable, rates in treatment regions increase considerably from phase-in cohorts to post-reform cohorts and then almost completely catch up with

---

[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).

10

rates in the comparison regions for the youngest cohorts. The primary school completion rate in comparison regions 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 regions, suggesting a difference-in-difference reform impact on primary school completion of roughly 10 percentage points.

[Insert Figure 1 about here]

Given that we have access to several pre-reform cohorts, we can 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 Figure 2.

[Insert Figure 2 about here]

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 significant because of the large sample size of the Census (1.6 million observations). While the absence of strong pre-trends is encouraging, we make our regression models robust against a range of differential trends. As we explain in more detail below, our regressions will allow for cohort dummies to vary by province, and we will show that our results are robust against allowing for differential time trends by rural and urban areas. We further probe the validity of our identification strategy in the next section, and present placebo tests at the end of Section 3.3.

3.2   Exogeneity of the treatment

In this section, we provide evidence of how treatment and control regions differ in terms of their observed characteristics. We also conduct balancing tests to check the degree to which the identifying region-by-cohort variation in primary school completion correlates with trends in observables.

11

Table 1 presents the results of cross-sectional, region-level regressions of regions' 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, and the location of the region 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 and by construction, the treatment status of a region 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.

[Insert Table 1 about here]

The results in Table 1 help us understand how treatment and comparison regions differ. Yet, it is important to note that even if there were differences in time-constant regional characteristics, they would be taken into account by our difference-in-difference identification strategy in which we condition on region and cohort fixed effects (see Section 3.3).

In the following we establish that the differential expansion of primary school completion across regions, which we use to identify the reform effect, is unrelated to trends in other observables. For this purpose, we regress the region-by-cohort share of primary school completion on region-by-cohort specific observables, conditioning on cohort and region fixed effects. Given that the Census data is from 2009, we must be careful not 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 region over time but are unlikely to change at the individual level in response to the reform.

[Insert Table 2 about here]

As shown in Table 2, the only covariate that is statistically significantly correlated with the region-by-cohort share of primary school completion is the Tay ethnicity. Yet, this is with such a

12

negligibly small effect size that it is unlikely to be of practical relevance.[7] We thus conclude that our identifying variation is not related to demographic trends in ethnicity or religious affiliation. We discuss further placebo tests and robustness checks to underpin the causal interpretation of our results at the end of the following section.

3.3 Estimation methods

Our empirical strategy is based on generalized difference-in-differences regressions that compare across regions and cohorts, controlling for region 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.

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 region r within province $p(r)$, belonging to birth cohort c, and surveyed in year t, we run the regression

$$Y_{irct} = \alpha_1 + \alpha_2 \text{Treat}_{ir} \times \text{Phasein}_{ic} + \alpha_3 \text{Treat}_{ir} \times \text{Post}_{ic} + \alpha_4 \mathbf{X}'_{irct}$$
$$+ \theta_t + \varphi_r + \omega_c \times \pi_{p(r)} + \zeta_{irct} \tag{1}.$$

Treat_{ir} is a dummy variable indicating whether the region r where individual i lives is a treatment region. 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). $\mathbf{X}'_{irct}$ 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 survey year, region 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

---

[7] 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 2) 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 3.

13

years, and is dropped in specifications using the Census data consisting of one year only. The region

fixed effect φ_r controls for time-invariant regional characteristics and the birth cohort fixed effect ω_c

controls for cohort trends in the outcome. The joint inclusion of region and cohort fixed effects turns

Equation 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.

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:

$$Y_{jrt} = \beta_1 + \beta_2 Treat_{jr} \times ParentPhasein_j + \beta_3 Treat_{jr} \times ParentPost_j + \beta_4 \mathbf{X}'_{jrt}$$
$$+ \theta_t + \varphi_r + \omega^{cm} \times \pi_{p(r)} + \omega^{cp} \times \pi_{p(r)} + \varepsilon_{jrt} \qquad (2).$$

Y_{jrt} is an outcome, such as an investment into child j's human capital. $\mathbf{X}'_{jrt}$ includes ethnicity, age, age

squared and gender of the child. ε_{jrt} is the error term. As before, $Treat_{jr}$ is a dummy variable indicating

whether the region r where child j lives is a treatment region. $ParentPhasein_j$ 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 region 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

14

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.

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:

$$Y_{krc} = \gamma_1 + \gamma_2 \text{Treat}_{kr} \times \text{ChildPhasein}_k + \gamma_3 \text{Treat}_{kr} \times \text{ChildPost}_k + \gamma_4 \mathbf{X}'_{krc}$$
$$+\varphi_r + \omega_c \times \pi_{p(r)} + \sum_\tau \rho_\tau NC_\tau + \epsilon_{krc} \qquad (3).$$

Y_{krc} is an outcome, such as health status of a parent k. $\mathbf{X}'_{krc}$ includes ethnicity and gender of the parent, and ϵ_{krc} is the error term. As before, Treat_{jr} is a dummy variable indicating whether the region r where parent k lives is a treatment region. 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). 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 region fixed effects, and $\omega_c \times \pi_{p(r)}$ 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]

---

[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.

[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.

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). For effects with a clear prediction of their sign from economic theory, we use one-sided hypothesis testing (Heckman and Karapakula, 2019a, 2019b), otherwise we use two-sided hypothesis testing. Table notes indicate which type of testing is used.

Causal interpretation, placebo tests and robustness

Interpretation of α_3, β_3 and γ_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 regions that would lead to nonzero estimates of α_3, β_3 or γ_3 in the absence of the reform. This assumption is supported by our discussion in Section 3.1 where we found no strong pre-trends in Figure 2, and by the evidence of uncorrelatedness of the identifying variation with trends in observables demonstrated in Section 3.2. Nevertheless, as shown in equations (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. This is possible because the treatment-by-post indicators vary within province-cohort cells as provinces include both treated and untreated regions.

Moreover, for a range of robustness checks presented in the Online Appendix (Tables A.3, A.7-A.13) we augment equations (1)-(3) by allowing for linear cohort trends interacted with an urban-rural indicator to control for the possibility of a general catch-up of rural areas. Our results are remarkably robust against this inclusion, suggesting that a general catch-up of rural areas is not driving our results.

16

We also conducted placebo tests to further verify the validity of the common trends assumption. In Online Appendix Table A.2, Panel A, we 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. Furthermore, we defined 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 Equation 1 using only the 1965–1975 pre-reform birth cohorts, neither of which was affected by the real reform.[11] The results in Panel B of Online Appendix Table A.2 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). 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.[12]

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 3, using a mix of outcomes from both the Census and VHLSS datasets. The results are based on regression equation (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.

[Insert Table 3 about here]

---

[11] 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.

[12] 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.

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 1.2 percentage points for both males and females. This effect is highly statistically significant. Compared to the baseline illiteracy rate of 3.3% for pre-reform cohorts in treatment regions (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, and earnings. As shown in column 5, reform exposure increases the probability of being economically active by half a percentage point for women, and 4.4 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 20-25 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.[13] 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 region are treated by the reform.

In Panel B of Table 3, 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 (3 percentage points for women, 11 percentage points for men), and it raises the average quality of the

---

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

spouse as proxied by their years of schooling (half a year for women, 0.8 years for 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 region 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 (6.5 percentage points – column 4). Another contributing factor to this could be the decrease in having experienced child mortality by 7 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 0.1 (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 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 tradeoff.[14] 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 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 equation (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 Treat × Post, Treat × Phasein, Treat, Post and Phasein. While the coefficient for parental schooling indicates the baseline intergenerational persistence of schooling, the coefficient for Treat × Post × Parental Schooling measures how exposure to the reform affects the

---

[14] 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.

intergenerational persistence. The coefficient in Panel C of Table 3 on parental schooling indicates a baseline IGE of about 0.4 years of schooling.[15] 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 3 show the results broken down by child and parental gender pairs. These differences should be interpreted cautiously due to two kinds of sample selection. Father-child pairs are less frequently observed than mother-child pairs because if children reside with a single parent, then this is more likely to be the mother. Second, daughter-parent pairs are less frequently observed than son-parent pairs because daughters are more likely to leave the parental home to live with their in-laws. With these caveats in mind, 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).

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.3, we find these effects to be highly robust against adding a linear cohort trend interacted with a rural indicator.

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

---

[15] For column (1), we measure parental education by the highest level of education obtained by one of the parents. One caveat for our analysis 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. Nevertheless, the magnitude of our IGE 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.

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, 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 and higher earnings might allow the directly affected generation to invest more monetary resources into their children (Bruins, 2017; Calero, 2009).[16] 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.

Main results

We derive the effects of exposure to the reform on investments into children's human capital, based on regression equation (2). In Table 4 we investigate school enrollment, school tuition, and other educational expenditures and, in Table 5 child labor, and in Table 6 health investments.

Column 1 in Table 4 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 8.7 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

---

[16] 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).

primary school is compulsory for everyone in the children's generation.[17] Given very high levels of school attendance at baseline (just above 90% of children of pre-reform parents in treatment regions), the effect size implies that the reform brought school attendance of the children's generation at primary and secondary school to a universal level.

[Insert Table 4 about here]

Reform exposure of at least one parent also increases payments for children's school tuition, by 23.7% of a standard deviation, statistically significant at the 1% level (column 2). A range of school types in Vietnam, such as semi-public and private schools, charge higher tuition fees than traditional public schools, and are thought to be of 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, estimate the tuition equation conditional on enrolment (column 3), and we find that there is still a weakly statistically significant effect of 14% of a standard deviation on tuition fees.

Looking at further types of educational expenditure in the remaining columns of Table 4, 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 (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 regions does not seem to have noticeably increased this practice. The last two column of Table 4 include results on educational expenditures related to the

---

[17] 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.

home rather than school. They show weakly statistically significant effects on children's books and magazines (14.9% of a standard deviation) and toys (17.1% of a standard deviation).

[Insert Table 5 about here]

In Table 5, 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). Column 1 shows no effect of parental reform exposure on the probability that their children work for the household. In column 2, we find that children of parents who were exposed to the reform are also less likely to have a paid job outside of the household, with an effect size of 1.9 percentage points, statistically significant at the 10% level. Finally, as column 3 shows, parental exposure also reduces children's daily hours of paid work, by close to 0.3 hours (statistically significant at the 5% level).

In Online Appendix Table A.4 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 earnings, and nearly half for the hours or work), parental exposure continues to have statistically significant effects.

Health is an important component of human capital and ill-health can hinder school attendance and skill acquisition. In Table 6 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 (14% of a standard deviation, weakly statistically significant). Given that parental smoking is harmful to child health because of second-hand smoke (Adda and Cornaglia, 2010; Cawley and Ruhm, 2011), this reduction can be interpreted as a positive health investment. Our findings are consistent with previous evidence on the tradeoff between household expenditures on food consumption and those on smoking products important in developing countries (Block and Webb, 2009; Wang et al., 2006).

23

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, particularly 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 parts 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. Reassuringly, as we show in Online Appendix Table A.5, parental exposure to the reform has no effect on children's age at the time of the survey.

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 7 we focus on a selection of the outcomes from the previous Tables 4-6. A more comprehensive analysis for all outcomes is in Online Appendix Table A.6. While point estimates show

24

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 1.5-2, and for child labor and hospitalizations to a factor of 3. Furthermore, the effects on total educational spending, child labor, and the health outcomes are statistically significant for boys only. The third row of coefficients in Table 7 reports the differences in the effects between sons and daughters. With the exception of total school related spending, 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.

[Insert Table 7 about here]

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).[18] In the last row of coefficients in Table 7 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 over-compensated these differences leading to post-reform outcomes 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

---

[18] 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.

in Online Appendix Tables A.7-A.10, we show that the results presented in this section are highly robust against allowing for different linear paternal and maternal cohort trends between rural and urban areas.

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 integenerational 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; 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.

Main results

Table 8 presents effects of exposure to the reform on one's parents' old age health outcomes based on regression equation (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.

[Insert Table 8 about here]

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

26

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.

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 9) and *behavioral* mechanisms (Table 10).

With regards to the overall effects in Panel A of Table 9, 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 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.

[Insert Table 9 about here]

Looking at behavioral mechanisms, Table 10 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, there is an increase in the number of children in law (i.e., one's

children being married). 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).

[Insert Table 10 about here]

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.11-A.13), we find the results presented in this section to be highly robust against allowing age effects to differ between rural and urban areas.

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.,

28

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 at., 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

29

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.

References

30

Aaronson, D., Lange, F., & Mazumder, B. (2014). Fertility transitions along the extensive and intensive margins. American Economic Review, 104(11), 3701-24.

Adda, J. and Cornaglia, F., 2010. The effect of bans and taxes on passive smoking. American Economic Journal: Applied Economics, 2(1), 1-32.

Adhvaryu, A., Nyshadham, A., 2016. Endowments at birth and parents' investments in Children. The Economic Journal. 126 (593), 781–820.

Ajayi, K., Ross, P., (Forthcoming). The Effects of Education on Financial Outcomes: Evidence from Kenya. Economic Development and Cultural Change.

Akresh, R., Halim, D., Kleemans, M. 2018. Long-term and intergenerational effects of education: Evidence from school construction in Indonesia. NBER Working Paper 25265.

Anh, T. S., Knodel, J., Lam, D., Friedman, J., 1998. Family size and children's education in Vietnam. Demography. 35 (1), 57–70.

Antman, F.M., 2010. Adult child migration and the health of elderly parents left behind in Mexico. American Economic Review. 100, 205–208.

Assaad, R., Saleh, M., 2018. Does Improved Local Supply of Schooling Enhance Intergenerational Mobility in Education? Evidence from Jordan. World Bank Economic Review. 32(3), 633-655.

Atalay, K., Barrett, G.F., Staneva, A., 2019. The effect of retirement on elderly cognitive functioning. Journal of Health Economics. 66, 37-53.

Attanasio, O. P., 2015. The determinants of human capital formation during the early years of life. Journal of the European Economic Association. 13 (6), 949–997.

Baker, M., Milligan, K., 2016. Boy-girl differences in parental time investments: evidence from three countries. Journal of Human Capital. 10 (4), 399–441.

Banerjee, A. V., Cole, S., Duflo, E., & Linden, L. (2007). Remedying education: Evidence from two randomized experiments in India. *The Quarterly Journal of Economics*, *122*(3), 1235-1264.

Banerjee, Abhijit, X. Meng, N. Qian. 2010. The life cycle model and household savings: micro evidence from urban China.

Barcellos, S. H., Carvalho, L. S., Lleras-Muney, A., 2014. Child gender and parental investments in India: Are boys and girls treated differently? American Economic Journal: Applied Economics. 6 (1), 157–189.

Barnay, T., Juin, S. 2016. Does home care for dependent elderly people improve their mental health? Journal of Health Economics. 45, 149-160.

Basu, K. 1999. Child labor: cause, consequence, and cure, with remarks on international labor standards. Journal of Economic Literature. 37 (3), 1083–1119.

Becker, G. S., Murphy, K. M., Spenkuch, J. L., 2016. The manipulation of children's preferences, old-age support, and investment in children's human capital. Journal of Labor Economics. 34 (S2), S3–S30.

Beegle, K., Dehejia, R., Gatti, R. 2009. Why should we care about child labor?: the education, labor market, and health consequences of child labor. Journal of Human Resources. 44 (4), 871–889.

31

Berniell, L., de la Mata, D., Valdés, N., 2013. Spillovers of health education at school on parents' physical activity. Health Economics. 22, 1004-1020.

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should we trust differences-in- differences estimates? Quarterly Journal of Economics. 119(1), 249–275.

Bhalotra, S., Karlsson, M., Nilsson, T., 2017. Infant health and longevity: Evidence from a historical trial in Sweden. Journal of the European Economic Association. 15, 1101–1157.

Bharadwaj, P., Lakdawala, L. K. 2013. Discrimination Begins in the Womb: Evidence of Sex-Selective Prenatal Investments. Journal of Human Resources. 48 (1), 71–113.

Björklund, A., Salvanes, Kjell G., 2011. Education and family background: Mechanisms and policies. In Handbook in economics of education, Vol. 3, edited by Erik A. Hanushek, Stephen Machin, and Ludger Woessmann, 201–247. Amsterdam: Elsevier.

Black, S. E., Devereux, P. J., 2011. Recent developments in intergenerational mobility. In Handbook of labor economics, Vol. 4B, edited by Orley Ashenfelter and David Card, 1487–1541. Amsterdam: Elsevier.

Black, S. E., Devereux, P. J., Salvanes, K. G., 2005. Why the apple doesn't fall far: Understanding intergenerational transmission of human capital. American Economic Review. 95 (1), 437–449.

Block, S. and Webb, P., 2009. Up in smoke: tobacco use, expenditure on food, and child malnutrition in developing countries. Economic Development and Cultural Change, 58(1), pp.1-23.

Bloom, D.E., Canning, D., Fink, G., 2011. Implications of population ageing for economic growth. Oxford Review of Economic Policy. 26, 583-612.

Böhme, M.H., Persian, R., Stöhr, T., 2015. Alone but better off? Adult child migration and health of elderly parents in Moldova. Journal of Health Economics. 39, 211-227.

Booth, A. L., Nolen, P., 2012. Gender differences in risk behaviour: Does nurture matter? The Economic Journal 122 (558), F56–F78.

Brenøe, A. A., Lundberg, S., 2018. Gender gaps in the effects of childhood family environment: Do they persist into adulthood? European Economic Review. 109, 42–62.

Brown, P. H., 2006. Parental education and investment in children's human capital in rural China. Economic Development and Cultural Change. 54 (4), 759–789.

Bruins, M., 2017. Women's economic opportunities and the intra-household production of child human capital. Labour Economics. 44, 122–132.

Bullock, J. G., Green, D. P., Ha, S. E., 2010. Yes, But What's the Mechanism? (Don't Expect an Easy Answer). Journal of Personality and Social Psychology. 98 (4), 550–558.

Bütikofer, A., Dalla-Zuanna, A., Salvanes, K. G. 2018. Breaking the Links: Natural Resource Booms and Intergenerational Mobility. NHH Dept. of Economics Discussion Paper No. 19/2018.

Bütikofer, A., Salvanes, K. G. 2020. Disease Control and Inequality Reduction: Evidence from a Tuberculosis Testing and Vaccination Campaign. The Review of Economic Studies. 87 (5), 2087–2125.

Cai, F., Giles, J., O'Keefe, P., Wang, D., 2012. The Elderly and Old Age Support in Rural China. Washington, DC: World Bank.

Calero, C., 2009. Remittances, liquidity constraints and human capital investments in Ecuador. World Development. 37 (6), 1143–1154.

Cameron, L.A., Cobb-Clark, D., 2008. Do coresidency and financial transfers from the children reduce the need for elderly parents to works in developing countries. Journal of Population Economics. 21(4), 1007-1033.

Carneiro, P.M., Lopez Garcia, I., Salvanes, K.G., Tominey, E., 2021. Intergenerational mobility and the timing of parental income. Journal of Political Economy. 129 (3), 757–788.

Carneiro, P., Meghir, C., Parey, M., 2013. Maternal education, home environments and the development of children and adolescents. Journal of the European Economic Association. 11 (1), 123–160.

Case, A., Paxson, C., 2009. Early Life Health and Cognitive Function in Old Age. American Economic Review. 99(2), 104-109.

Cawley, J. and Ruhm, C.J., 2011. The economics of risky health behaviors. In Handbook of health economics (Vol. 2, pp. 95-199). Elsevier.

Chen, Y. and Fang, H., 2018. The long-term consequences of having fewer children in old age: evidence from China's "later, longer, fewer" campaign. NBER Working Paper No. 25041.

Chetty, R., Friedman, J. N., Saez, E., Turner, N., Yagan, D. 2017b. Mobility Report Cards: The Role of Colleges in Intergenerational Mobility. NBER Working Paper No. 23618.

Chetty, R., Grusky, D., Hell, M., Hendren, N., Manduca, R., Narang, J. 2017a. The fading American dream: Trends in absolute income mobility since 1940. Science, 356(6336), 398-406.

Chetty, R., Hendren, N. 2018a. The Effects of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects. Quarterly Journal of Economics, 133(3), 1107-1162.

Chetty, R., Hendren, N. 2018b. The Effects of Neighborhoods on Intergenerational Mobility II: County Level Estimates. Quarterly Journal of Economics, 133(3), 1163-1228.

Chetty, R., Hendren, N., Jones, M. R., Porter, S. R. 2020. Race and Economic Opportunity in the United States: An Intergenerational Perspective. Quarterly Journal of Economics, 35(2), 711–783.

Chetty, R., Hendren, N., Kline, P., Saez, E. 2014. Where is the land of Opportunity? The Geography of Intergenerational Mobility in the United States. Quarterly Journal of Economics, 129(4), 1553–1623.

Chetty, R., Hendren, N., Lin, F., Majerovitz, J., Scuderi, B., 2016. Childhood environment and gender gaps in adulthood. American Economic Review. 106 (5), 282–288.

Choi, E. J., Hwang, J., 2015. Child gender and parental inputs: No more son preference in Korea? American Economic Review. 105 (5), 638–643.

CPHCSC (Central Population and Housing Census Steering Committee), 2010. The 2009 Vietnam Population and Housing Census: Completed Results. Ha Noi: Statistical Publishing House.

Currie, J. and Vogl, T., 2013. Early-life health and adult circumstance in developing countries. Annual Review of Economics, 5(1), pp.1-36.

Currie, J., Moretti, E., 2003. Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings. Quarterly Journal of Economics. 118 (4), 1495–1532.

Dammert, A. C., de Hoop, J., Mvukiyehe, E., Rosati, F. C., 2018. Effects of public policy on child labor: Current knowledge, gaps, and implications for program design. World Development. 110, 104–123.

Dang, H-A. H., Glewwe, P. W., 2018. Well begun, but aiming higher: A review of Vietnam's education trends in the past 20 years and emerging challenges. Journal of Development Studies. 54 (7), 1171–1195.

Dang, H-A., 2007. The determinants and impact of private tutoring classes in Vietnam. Economics of Education Review. 26 (6): 683–698.

Dang, H-A., Rogers, F. H., 2008. The growing phenomenon of private tutoring: does it deepen human capital, widen inequalities, or waste resources? World Bank Research Observer. 23 (2), 161–200.

De Nardi, M., French, E., Jones, J.B., 2009. Life Expectancy and Old Age Savings. American Economic Review. 99(2), 110-115.

De Neve, J.-W., Fink, G., 2018. Children's education and parental old age survival – Quasi-experimental evidence on the intergenerational effects of human capital investment. Journal of Health Economics. 58, 76–89.

De Neve, J.-W., Harling, G., 2017. Offspring schooling associated with increased parental survival in rural KwaZulu-Natal, South Africa. Social Science & Medicine. 176, 149-157.

De Neve, J.-W., Kawachi, I., 2017. Spillovers between siblings and from offspring to parents are understudied: A review and future directions for research. Social Science & Medicine. 183, 56-61.

Dell, M., Lane, N., Querubin, P., 2018. The Historical State, Local Collective Action, and Economic Development in Vietnam. Econometrica. 86(6), 2083–2121.

Demirel, Merve, Okten, Cagla, 2020. Gender Gap in Intergenerational Educational Persistence: Can Compulsory Schooling Reduce It? IZA Discussion Papers 13362, Institute of Labor Economics (IZA).

Dickson, M., Gregg, P., Robinson, H., 2016. Early, Late or Never? When Does Parental Education Impact Child Outcomes? The Economic Journal. 126 (596), F184-F231.

Duflo, E., 2001. Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment. American Economic Review. 91(4), 795-813.

Edmonds, E. V., Pavcnik, N. 2005. Child labor in the global economy. Journal of Economic Perspectives. 19 (1), 199–220.

Fabrizio, M., Franco, P., 2017. Unhealthy Retirement? Journal of Human Resources. 52(1), 128-151.

Fetter, D.K., Lockwood, L.M., 2018. Government Old-Age Support and Labor Supply: Evidence from the Old Age Assistance Program. American Economic Review. 108(8): 2174-2211.

Figlio, D., Giuliano, P., Özek, U., Sapienza, P. 2019. Long-term orientation and educational performance. *American Economic Journal: Economic Policy*, 11(4), 272-309.

Friedman, E.M., Mare, R.D., 2014. The schooling of offspring and the survival of parents. Demography. 51(4), 1271-1293.

Gimenez-Nadal, J. I., Molina, J. A., 2013. Parents' education as a determinant of educational childcare time. Journal of Population Economics, 26 (2), 719–749.

Glewwe, P., Jacoby, H., 1998. School enrollment and completion in Vietnam: An investigation of recent trends. In D. Dollar, P. Glewwe, and J. Litvack, eds., Household welfare and Vietnam's transition. Washington, D.C.: World Bank.

Glewwe, P., Patrinos, H.A., 1999. The role of the private sector in education in Vietnam: Evidence from the Vietnam Living Standards Survey. World Development. 27(5), 887–902.

Goulart, P., Bedi, A. S. 2008. Child labour and educational success in Portugal. Economics of Education Review. 27, 575–587.

Grépin, K. A., Bharadwaj, P., 2015. Maternal education and child mortality in Zimbabwe. Journal of Health Economics. 44, 97–117.

Hahn, Y., Nuzhat, K., Yang, H.-S., 2018. The effect of female education on marital matches and child health in Bangladesh. Journal of Population Economics. 31 (3), 915–936.

Hanushek, E. A. (2002). Publicly provided education. *Handbook of public economics*, 4, 2045-2141.

Hanushek, E. A., 1992. The trade-off between child quantity and quality. Journal of Political Economy. 100 (1), 84–117.

Heckman, J. J. and G. Karapakula. 2019a. Intergenerational and intragenerational externalities of the Perry Preschool Program. Working Paper 2019-033, Human Capital and Economic Opportunity Global Working Group.

Heckman, J. J., G. Karapakula. 2019b. The Perry Preschoolers at late midlife: A study in design-specific inference. Working Paper 2019-034, Human Capital and Economic Opportunity Global Working Group.

Heckman, J. J., Karapakula, G., 2019. Intergenerational and Intragenerational Externalities of the Perry Preschool Project. NBER Working Paper No. 25889.

Hertz, Tom, Tamara Jayasundera, Patrizio Piraino, Sibel Selcuk, Nicole Smith, and Alina Verashchagina (2007). The Inheritance of Educational Inequality: International Comparisons and Fifty-Year Trends. The B.E. Journal of Economic Analysis & Policy: Vol. 7: Iss. 2 (Advances), Article 10.

Holmlund, H., Lindahl, M., Plug, E., 2011. The causal effect of parent's schooling on children's schooling: A comparison of estimation methods. Journal of Economic Literature, 49 (3), 615–651.

Huang, C., Li, H., Liu, P. W., Zhang, J., 2009. Why Does Spousal Education Matter for Earnings? Assortative Mating and Cross-Productivity. Journal of Labor Economics. 27 (4), 633-652.

Huebener, M., 2018. The Effects of Education on Health: An Intergenerational Perspective. IZA DP No. 11795.

Huebener, M., 2019. Life expectancy and parental education. Social Science & Medicine. 232, 351-365.

ILO (International Labour Organization). 2017. Global estimates of child labour: results and trends, 2012–2016. Geneva: International Labour Office.

Jayachandran, S., Kuziemko, I., 2011. Why do mothers breastfeed girls less than boys? Evidence and implications for child health in India. Quarterly Journal of Economics. 126 (3), 1485–1538.

Keats, A., 2018. Women's schooling, fertility, and child health outcomes: Evidence from Uganda's free primary education program. Journal of Development Economics. 135, 142–159.

Kinsler, J., Pavan, R., 2016. Parental beliefs and investment in children: The distortionary impact of schools. Working Papers 2016-029, Human Capital and Economic Opportunity Working Group, University of Chicago.

Krueger, A. B., Lindahl, M. 2001. Education for Growth: Why and for Whom? Journal of Economic Literature, 39 (4), 1101-1136.

Kuziemko, I., 2014. Human Capital Spillovers in Families: Do Parents Learn from or Lean on Their Children? Journal of Labor Economics. 32(4), 755-786.

Lange, F., & Topel, R. (2006). The social value of education and human capital. *Handbook of the Economics of Education*, 1, 459-509.

Lavy, V., Strauss, J., Thomas, D., De Vreyer, P., 1996. Quality of health care, survival and health outcomes in Ghana. Journal of Health Economics. 15, 333-357.

Lefgren, L., McIntyre, M., 2006. The Relationship between Women's Education and Marriage Outcomes. Journal of Labor Economics. 24 (4), 787-830.

Lindeboom, M., Llena-Nozal, A., van der Klaauw, B., 2009. Parental education and child health: Evidence from a schooling reform. Journal of Health Economics. 28, 109–131.

Lochner, L., & Moretti, E. (2004). The effect of education on crime: Evidence from prison inmates, arrests, and self-reports. American Economic Review, 94(1), 155-189.

Lundborg, P., Majlesi, K., 2018. Intergenerational transmission of human capital: Is it a one-way street? Journal of Health Economics. 57, 206–220.

Lundborg, P., Nilsson, A., Rooth, D-O., 2014. Parental education and offspring outcomes: evidence from the Swedish compulsory schooling reform. American Economic Journal: Applied Economics. 6 (1), 253–278.

Lundborg, P., Nordin, M., Olof-Rooth, D., 2018. The intergenerational transmission of human capital: the role of skills and health. Journal of Population Economics. 31 (4), 1035–1065.

Ma, M., 2019. Does children's education matter for parents' health and cognition? Evidence from China. Journal of Health Economics. 66, 222-240.

Majlesi, K., 2014. Demand for low-skilled labor and parental investment in children's education: Evidence from Mexico. Working Papers 2014:5, Lund University, Department of Economics.

Manuelli, R. E., Seshadri, A. 2014. Human Capital and the Wealth of Nations. American Economic Review. 104 (9), 2736-2762.

Meghir, C., Palme, M., Schnabel, M., 2012. The effect of education policy on crime: An intergenerational perspective. NBER Working Paper No. 18145.

Mazumder, B., Triyana, M., Fernanda Rosales, M., 2019. Social Interventions, Health and Wellbeing: The Long-Term and Intergenerational Effects of a School Construction Program. Working Paper, No. 2019-09, 2019, the Federal Reserve Bank of Chicago.

Meng, X. and Xue, S., 2020. Social networks and mental health outcomes: Chinese rural–urban migrant experience. Journal of population economics, 33(1), pp.155-195.

MOET (Ministry of Education and Training of Vietnam) and JICA (Japan International Cooperation Agency). 2002. Vietnam support program for primary education development: Phase I, final report, Annex 1.

MOET (Ministry of Education and Training), 1998. In-service teacher training program to follow the revised lower secondary education curriculum and revised textbooks. Hanoi: MOET/ADB.

Moretti, E. (2004a). Human capital externalities in cities. *In Handbook of Regional and Urban Economics* (Vol. 4, pp. 2243-2291). Elsevier.

Moretti, E. (2004b). Workers' education, spillovers, and productivity: evidence from plant-level production functions. American Economic Review, 94(3), 656-690.

National Committee for EFA Assessment, 1999. The assessment of education for all: Vietnam, 1990–2000. Hanoi, Vietnam.

Newman, S. J., Holupka, C. S., 2014. Housing affordability and investments in children. Journal of Housing Economics. 24, 89–100.

Nguyen, B.T., Albrecht, J. W., Vroman, S. B., Westbrook, M. D., 2007. A quantile regression decomposition of urban–rural inequality in Vietnam. Journal of Development Economics. 83(2), 466–490.

Nguyen, M.P. and Wilson, A., 2017. How could private healthcare better contribute to healthcare coverage in Vietnam? International journal of health policy and management, 6(6), p.305.

Nguyen, N. N., 2004. Trends in the education sector. In Glewwe, Paul, Agrawal, Nisha; Dollar, David (eds), Economic growth, poverty, and household welfare in Vietnam. Washington, DC: World Bank.

O'Donnell, O., Rosati, F. C., Doorslaer, E. 2005. Health effects of child work: evidence from rural Vietnam. Journal of Population Economics. 18, 437–467.

Oliveira, J. 2016. The value of children: Inter-generational support, fertility, and human capital. Journal of Development Economics. 120, 1-16.

Oreopoulos, P., Page, M. E., Stevens, A. H., 2006. The Intergenerational Effects of Compulsory Schooling. Journal of Labor Economics. 24 (4), 729-760.

Oreopoulos, P., Salvanes K. G. 2011. Priceless: The Nonpecuniary Benefits of Schooling. Journal of Economic Perspectives. 25 (1), 159-184.

Osili, U.O., Long, B.T., 2008. Does female schooling reduce fertility? Evidence from Nigeria. Journal of Development Economics. 87(1), 57-75.

Pencavel, J., 1998. Assortative Mating by Schooling and the Work Behavior of Wives and Husbands. American Economic Review. 88 (2), 326-329.

Piopiunik, M., 2014. Intergenerational transmission of education and mediating channels: Evidence from a compulsory schooling reform in Germany. Scandinavian Journal of Economics. 116 (3), 878–907.

37

Potente, C., Präg, P. and Monden, C.W., 2020. Does Children's Education Affect Parental Health and Mortality? A Regression Discontinuity Approach with Linked Census Data from England and Wales. SocArXiv, https://doi.org/10.31235/osf.io/eah4w.

Rosenzweig, M. R., Zhang, J., 2009. Do population control policies induce more human capital investment? Twins, birth weight and China's "One-Child" policy. Review of Economic Studies. 76 (3), 1149–1174.

Salm, M., 2011. The Effect of Pensions on Longevity: Evidence from Union Army Veterans. The Economic Journal. 121(552), 595-619.

Strauss, J. and Thomas, D., 1998. Health, nutrition, and economic development. Journal of economic literature, 36(2), pp.766-817.

Strauss, J., Thomas, D., 1995. Human resources: Empirical modeling of household and family decisions. In Handbook of development economics, vol. 3, ed. J. Behrman and T. N. Srinivasan, 1883–2023. Amsterdam: Elsevier Science.

Thompson, O., 2018. The determinants of racial differences in parenting practices. Journal of Political Economy. 126 (1), 438–449.

Torssander, J., 2013. From child to parent? The significance of children's education for their parents' longevity. Demography. 50, 637-659.

UNESCO. 2019. EFA Global Monitoring Report 2019: Migration, displacement and education: Building bridges, not walls. Paris: UNESCO.

Wang, H., Sindelar, J.L. and Busch, S.H., 2006. The impact of tobacco expenditure on household consumption patterns in rural China. Social science & medicine, 62(6), pp.1414-1426.

World Bank, 2016. Global Monitoring Report 2015/2016: Development Goals in an Era of Demographic Change. Washington, DC: World Bank.
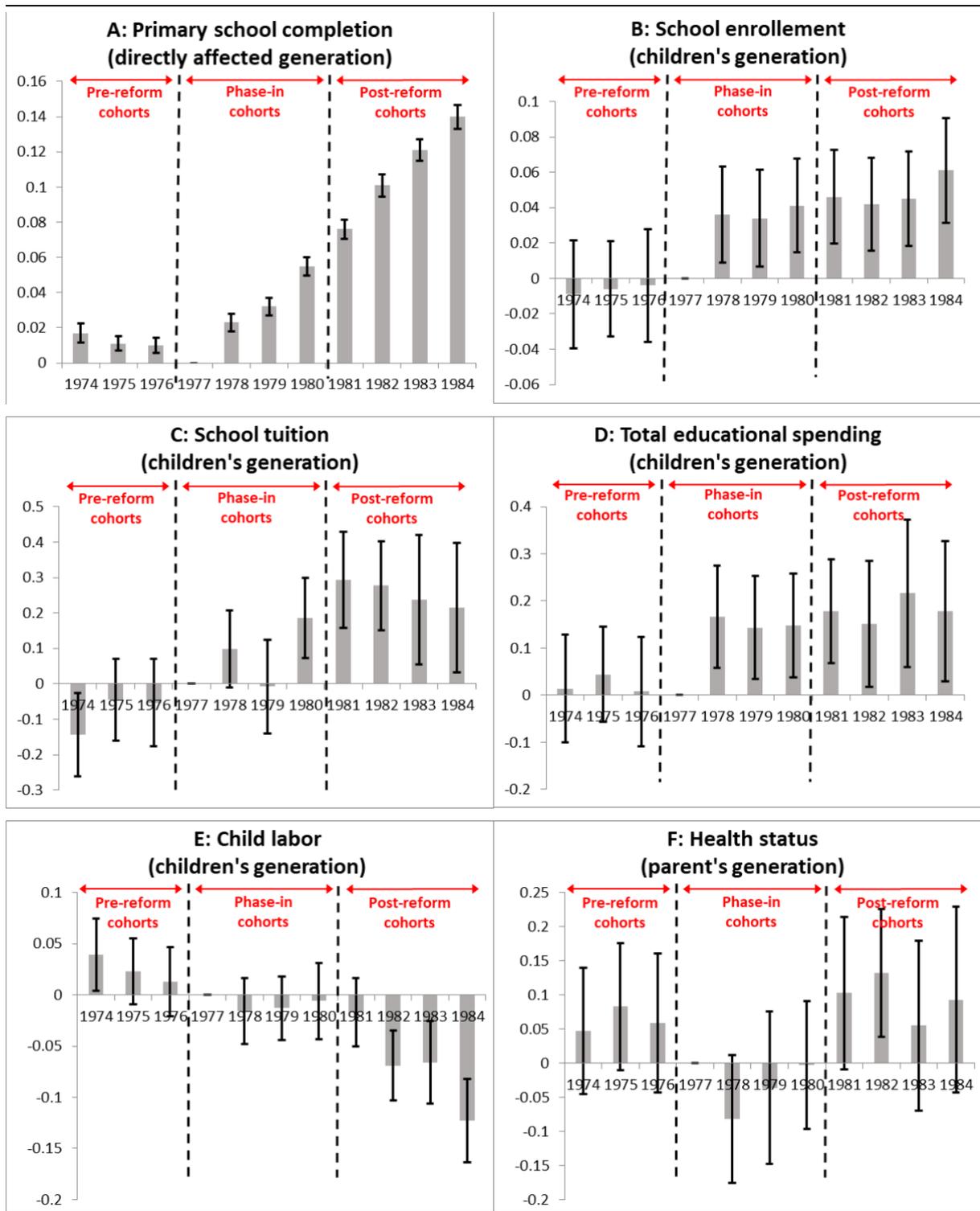
Figures and Tables

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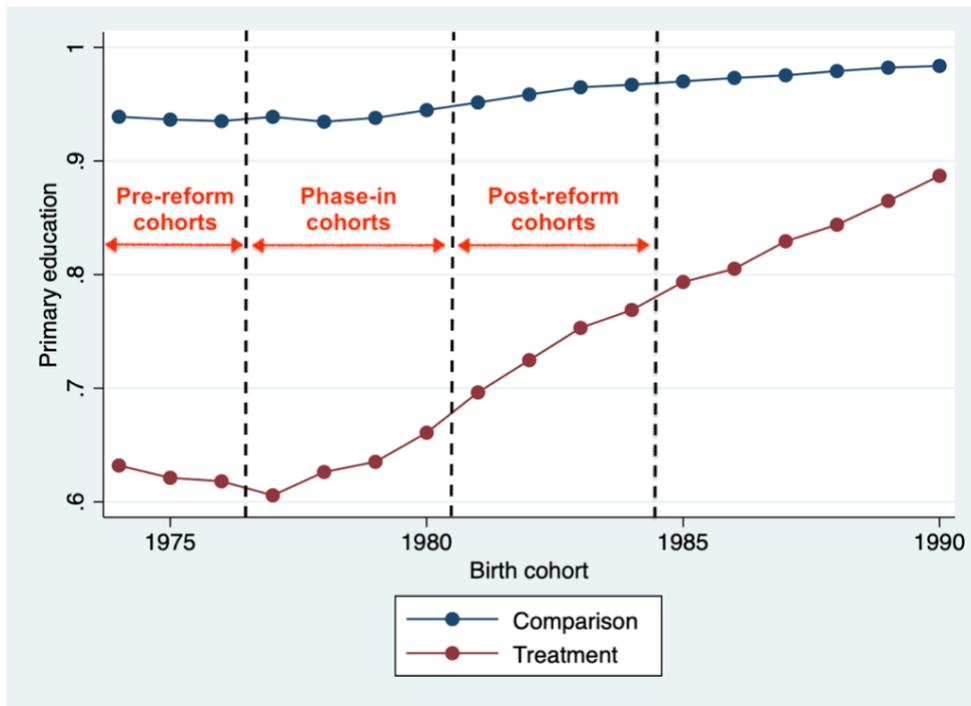
Data source: Population and Housing Census of Vietnam (2009, 15% sample).

Figure 2. Event study graphs

Data source: Population and Housing Census of Vietnam (2009, 15% sample). Vietnam Household Living Standards Survey (2010, 2012 and 2014), and Vietnam Aging Survey (2011).