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ABSTRACT

The Effects of the Vietnam Hunger Eradication and Poverty Reduction Program on Schooling*

This paper studies the effects of the Vietnam Hunger Eradication and Poverty Reduction (HEPR) program on school enrolment, using longitudinal data that span over 15 years and a difference-in-differences research design. We find that early treatment (at age 8) increases children enrolment by about 9 percent. This positive effect disappears by age 15, and is more pronounced in urban areas. In sharp contrast, children receiving treatment later (age 12–15) are more likely to drop out by age 15, especially in rural areas. The decline in enrolment is paralleled by an increase in labor market participation. We interpret these divergent results by age as an unintended effect of another program aimed at fostering vocational training among the 15+ in rural areas. Our findings highlight the importance of integrating different anti-poverty measures to reduce inefficiency and achieve social goals.

JEL Classification: H52, H53, I24, I32

Keywords: child poverty, child education, enrolment, Vietnam, poverty reduction

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1 Introduction

Of the millions of people in poverty, children are the most vulnerable. Poverty alleviation programs aim to improve their conditions and opportunities in the long run through investment in human capital. Cash transfer programs designed to encourage investment in education are increasingly adopted all over the world, especially in developing countries.[1] Whether the transfer comes with conditionality or not, several studies show that such programs have been effective at enhancing child schooling and at reducing child labor[2] (de Brauw et al. 2015; Glewwe and Kassouf 2012; Ravallion and Wodon 2000; Filmer and Schady 2008; Edmonds and Schady 2012).

The Hunger Eradication and Poverty Reduction (HEPR) is the most comprehensive anti-poverty program in Vietnam, the country of interest of this paper. Households eligible for HEPR receive free health insurance, subsidized loans, small transfers in cash and in kind, while their children in school age benefit from tuition fee exemptions, schooling material allowances and access to student loans, from primary school to university. Furthermore, working-age members of the eligible households who reside in rural areas receive a fee waiver to participate in the Vocational Training Program for Rural Workers, a massive campaign of vocational education which involves about one million people each year.

Although several studies investigated the efficiency, the evolution, and the progressive implementation of the program over time (Turk 1999; van de Walle 2004b; Berg and Cuong 2011; The World Bank 2012; Oxfam 2017b), little is known about how HEPR affects child development, and in particular educational outcomes.[3] This gap may at least in part be due to data limitation and

[1] Due to its randomized nature when first launched in 1997, PROGRESA—the conditional cash transfer (CCT) program of Mexico, has been widely studied for its impacts both in short and longer term periods (Gertler 1999; Fernald, Gertler, and Neufeld 2009; Behrman, Parker, and Todd 2011; Attanasio, Meghir, and Santiago 2012; Adhvaryu et al. 2018). Similar programs are Bolsa Escola/Familia in Brazil, Bono de Desarrollo Humano in Ecuador, Food-for-Education in Bangladesh.

[2] de Hoop et al. 2019 provide evidence that a partial subsidy to education in the Philippines increased both school attendance and participation in paid work to cover the shortfall in schooling fees, highlighting that transfer size also matters and insufficient funds may trigger unexpected effects.

[3] Few attempted to evaluate the effect of the welfare policies enacted in Vietnam and attention was devoted to ethnic minorities (Pham et al. 2008; Phung et al. 2012; Nguyen and Baulch 2007; Oxfam 2017a). van de Walle, 2004b, points out that Vietnam's poverty reduction miracle in the last decade of 20th century has little to do with the public policy, due to the inefficiency and disorganization of public administration. Berg and Cuong 2011, and Phan et al. 2017 actually find that pro-poor policies produced rather undesirable effects, such as increasing inequality. There is ample

the complex nature of the program which prevents to distinguish the effects of the various interventions (van de Walle 2004a, 2004b; Evans and Harkness 2008; Roelen 2010; Berg and Cuong 2011).[4]

We estimate the effect of the combined HEPR measures on school enrolment by exploiting Young Lives data. Young Lives is an international study of childhood poverty in four countries (Ethiopia, India, Peru and Vietnam). The Vietnamese sample includes 3,000 children from two birth cohorts, and follows these children for five rounds between 2002 and 2016. The 15-year time span covered by the study perfectly coincides with the implementation of HEPR. Crucially, for each child and each round, Young Lives reports whether child's household is a HEPR beneficiary. The availability of longitudinal data makes it possible to analyze how treatment effects depend on the age when children were first included in the program.

Our identification strategy is based on a Difference-in-Differences (DID) design, which compares how school enrolment evolves over time between children whose households benefitted from HEPR—the treated children—and children whose households were kept out of the program—the control children. As in any DID setting, identification relies on the assumption of parallel trends, which we are able to test for the pre-treatment period. More generally, the parallel trend assumption is likely to hold in our setup because of an unintended feature of HEPR: its bad targeting (see Evans and Harkness 2008; Roelen 2010). Although all families satisfying the poverty criterion set by the Ministry of Labor–Invalids and Social Affairs (MOLISA) are supposed to receive HEPR benefits, it turns out that many non-poor households are mistakenly enlisted in the program, while some poor families are left out. This feature of the program permits us to observe and compare treated and control households with similar economic conditions, strengthening the assumption that they would have followed similar trends had the HEPR not been implemented.

evidence of bad targeting and actual misconduct in the management of public interventions (Evans and Harkness 2008; van de Walle 2004a, 2004b; Turk 1999; Berg and Cuong 2011; UNDP 2009). Furthermore, centrally designed programs were often underfunded, with an average annual coverage as low as $0.22 per person (van de Walle 2004a).

[4] Monetary size of transfers (van de Walle 2004a, 2004b; Evans and Harkness 2008) or report of transfer receipts have often been used as indicators of benefit reception (Berg and Cuong 2011; Roelen 2010). However, only a small share of the benefits provided by poverty alleviation programs comes in the form of cash: direct targeting programs more often provide in-kind transfers, exemptions and subsidies.

Our findings can be summarized as follows. On the one hand, early treatment (at age 8) increases school enrolment by about 9 percent, the treatment effect persists at age 12, but fades away when children reach age 15. This positive effect is mostly concentrated among children residing in urban areas. On the other hand, we find no contemporaneous effect for children first enlisted in the program at age 12. Rather, their enrolment significantly declines when they reach age 15. Similarly, children who firstly receive treatment at age 15 are more likely to drop out. These negative effects are more prevalent among children residing in rural areas.

The heterogeneous effects of HEPR on education by treatment age imply that timing matters, and early investments are more effective than later ones. This is consistent with the available theories and evidence on the dynamic formation of human capital, showing that early educational investments are more effective (Cunha and Heckman 2007; Cunha and Heckman 2008). Moreover, when children grow older, the opportunity cost of schooling is higher (as children reach the minimum working age) and corrective investments, although possible, are less rewarding.

Nonetheless, the theory of skills formation fails to explain the drop in enrolment at age 15. In Vietnam, age 15 coincides with the critical point when children complete middle school and transit into high school. In addition, it is also the minimum age required to legally access the labor market.

We conjecture that the interaction among different programs and welfare schemes is responsible for the drop in enrolment at age 15. We refer particularly to the free access to the Vocational Training Program for Rural Workers (VTP), granted to treated households who reside in rural areas (households outside HEPR pay a significant fee to enroll). The VTP is a large program which provides vocational training to rural workers aged 15 and over. Training lasts three months, after which participants are accompanied on the labor market. We hypothesize that the possibility to attend the VTP pushed many treated offspring in rural regions to leave education at age 15. Moreover, the under-supply of schools in rural areas may explain why in those areas we observe small and insignificant effects of HEPR at earlier ages.

Our contribution to the literature is three-fold. First, we assess the effect of the Vietnamese HEPR program on schooling, thereby shedding light on a very relevant—but so far overlooked—component of this policy. Second, we provide evidence that the effects of education policies for the poor depend on the age at which children are initially treated. Third, we highlight the

importance of considering program-substitution effects for both the design and the evaluation of poverty alleviation policies.

The remainder of the paper unfolds as follows. We describe the institutional arrangements of the HEPR program in Section 2. Our data and empirical methodology are described in Section 3 and 4, respectively. We present our results in Section 5 and suggest their interpretation in Section 6. Conclusion follows.

2 The HEPR program and poverty definition

In 1998, as part of the Millennium Development Goals, the Government of Vietnam first passed the National Targeted Programs (NTPs) and National program on Hunger Eradication & Poverty Reduction (HEPR) which aim to improve growth and development in several ways.[5] In fact, the HEPR is an umbrella under which various sub-programs and policies targeting specific goals kicked in over time. Initially, large-scale projects such as investments in infrastructures were the core of the program, but starting from 2002 some policies also began to target individuals and households living in poverty, by providing free health insurance, tuition fee exemption from kindergarten to university, as well as access to concessional loans at the Social Policy Bank with low interest rate.[6] As of 2010, monthly tuition fee at a public school alone falls between 20,000 VND and 80,000 VND per student in the rural area, or between 40,000 VND and 200,000 VND per student in the urban area, amounted to at least 10–80% of per capita income of poor family eligible for HEPR (see Table A1 in the Appendix). Similar to other welfare programs in Vietnam, the HEPR strategy is designed by the central government, but implemented locally by provinces, districts and communes—the lowest local government level—in order to better reach the poor.[7]

Households' eligibility for HEPR depends on their poverty status. MOLISA (the Ministry of Labor–Invalids and Social Affairs) establishes a national poverty line and households whose resources are below the national poverty line are tagged as poor and eligible for the program. Both the poverty line and the list of economic resources which enter in the measurement of household

[5] Its broad aims concern the development of infrastructure, production, education, medical support, credit and training for the poor, the promotion of agriculture – forestry – fishery, the provision of training to the HEPR staff, the promotion of sedentary residence and new economic zones, and the support of ethnic minorities.

[6] National Target Programs, Five-year National Development Goals 2001–2005, 2006–2010, 2011–2015.

[7] HEPR is funded by a combination of central and local financing.

income were modified every five years as part of the government's Five-Year Socioeconomic Development Plan (see Table A1 in the Appendix for more details). These criteria could vary by region, residence and minority status and were partly open to the discretion of the officials in charge of assessing the poverty status. In the latest period (2016–2020), a single and comprehensive grading form (the so-called "B1" form) was introduced, in an effort to harmonize the process throughout the country and make it fairer and more objective. In addition to income, this new form adopts a multidimensional poverty assessment approach which takes into consideration several indicators of health, education, sanitation, and housing size and facilities.

The formal status of poverty is ascertained every year by communes. Each commune (ward) includes several villages (blocks). Every year, the village head drafts a list of poor and near-poor households within his village, based on his own observational experience and villagers' candidature. A local team (Commune Committee) consisting of anti-poverty officials talks with the village head to get an overview of the socioeconomic condition of households in the village. They then visit each household to better assess their livelihood and determine whether the household meets or not the MOLISA criteria for being defined as poor. Next, a list of poor households is published for review, should there be any feedback/disagreement from the village representatives. Finally, the official list of poor households is publicly made available on the village's announcing board. The process is repeated annually.

3 Data

Our analysis is based on the Young Lives Study, a cross-national harmonized longitudinal survey that follows 3,000 children in Ethiopia, India, Peru and Vietnam. It tracks the development of these children across survey rounds (Round 1 in 2002; Round 2 in 2006; Round 3 in 2009; Round 4 in 2013; Round 5 in 2016), with the aim of investigating causes and consequences of poverty during childhood, and drawing relevant policy implications. Young Lives follows two birth cohorts: an Old Cohort—OC, aged 8 at the first Round and followed up until age 22 – and a Young Cohort—YC, aged 1 at round 1 and followed up until age 15. We use the Vietnamese sample of 3,000 children, including 1,000 children from the OC between round 1 and round 4[8] and 2,000 YC

[8] In Round 5, when the OC is aged 22, many of them move out and have their own family. To capture the changes in family composition and livelihood, the questionnaire used for the OC in this round is very different from previous rounds as well as from the one used for the YC. Several variables that we use in the analysis are not available for the OC in Round 5. Therefore, for this cohort we only include in the analysis information collected in the first four rounds.

children between round 1 and round 5. Table 1 summarizes general characteristics of Young Lives children and their families when the survey started.

<Table 1>

One of the main improvements of this study with respect to previous investigations of the effectiveness of anti-poverty programs in Vietnam (van de Walle 2004a, 2004b; Evans and Harkness 2008; Roelen 2010; Berg and Cuong 2011) is that we know with precision whether a family is treated or not, that is, we know if it is included in the Commune official poverty list and thus eligible to receive HEPR benefits.

The list of poor households enrolled in the HEPR program is revised every year by the Commune Committee. Households falling into poverty status will enter the list, while others will get out of the list as they succeed at improving their wealth during the period they spend in the program. This means that the "poor" status of families is not fixed during the study period.

Starting from round 2 in 2006 the Young Lives survey asked households in every round whether they were "included in list of poor households created by Commune Committee on the MOLISA criteria for Hunger Eradication & Poverty Reduction in year X", where X includes 2006 and each year in period 2009–2016. To minimize recall bias, we only use information on poor status for year 2006 (data collection for round 2), 2009 (round 3), 2012 (round 4) and 2016 (round 5).[9]

There are two reasons why we are confident that the answer to this question is reliable and subject to minor measurement error. First, the wording of the question makes it clear that the poor list is the one created following the MOLISA criteria for the purpose of receiving HEPR provisions. Once it is classified as poor, each family receives a "Certification of Poor" from the Commune Committee and must show this certification when it applies for and receives benefits from pro-poor policies. This clarification ensures that respondents do not get confused with other welfare programs from which the household might also benefit. Second, as Young Lives specifically focuses on poverty and inequality, it is crucial for the field teams to keep close contact and build

[9] Data on enrolment in the official poor lists is not available in round 1. However, this is not a major limitation. Although the anti-poverty interventions targeted to poor households can be tracked back as far as 2002, the same year of round 1, there were delays in the phasing-in of the program that make it unlikely that anything relevant had happened at least by 2003. Therefore, we can safely consider 2002 as a pre-treatment period.

trust with the households in the sample with whom they are expected to work over 15 years, and tactically carry out the interview without making the people feel judged on the basis of their responses. Therefore, it is unlikely that families feel embarrassed by their poor status and deceive the answer.[10]

In detail, in our analysis we define treatment and control groups as follows:

- Control households have never been included in the official poor list at any time.[11]
- Households treated at time T have entered the official poor list for the first time at T, and were not previously present in the list.

Table 2 illustrates the full set of definitions of the treatment and control groups, depending on T, as well as showing their size in the final sample. Following these definitions, each analysis of treatment at a certain T takes into consideration only subjects that belong to either the corresponding treatment group or the control group, and excludes those who first entered the list at any time other than T.

<Table 2>

Importantly, according to our definition the treatment status depends only on whether a household appears in the poor list for the first time at time T, regardless of later permanence or removal. As illustrated in Table 3, between 25 to 50% of households treated at time T remains in the list in the following period (3 to 4 years later).

<Table 3>

We assign an index of economic resources to all households in the sample and all rounds, which is obtained by replicating the calculation the B1 form described in Section 2. We therefore refer to the index as "B1 score" hereinafter. Data for most of the items used in the B1 form are readily

[10] The list of poor people is also publicly announced to the whole village, giving respondents less incentive to lie. In addition, the idea of being poor is quite common in Vietnam, and HEPR reception is not generally associated with social stigma or shame. In a survey about strengths and weaknesses of key social protection programs it was noted that the HEPR "lacked concrete mechanisms for monitoring the gender impacts of policies and strategies" but it is not "stigmatizing" (Jones 2010).

[11] For the YC, it means at any time between 2002 and 2016, while for the OC it means any time between 2002 and 2012. We do not take into consideration the poor status of OC children in 2016 (round 5) since the analysis does not make use of information from this round.

available in the household's questionnaire of Young Lives, and only few approximations are needed (see the Appendix for detailed description of the B1 form and data processing). The B1 score is then standardized by round to get rid of time effects.

Table 4 summarizes the statistics of the B1 score and the main household characteristics by treatment status. Panel A is for the YC and Panel B for the OC. Each variable is measured at the age of first treatment for treated subjects and at age 8 for the controls.

<Table 4>

On average, treated households have lower B1 score. Treated children come from larger families with higher number of dependents in the household (mean comparison between treated and control groups at treated age is not shown in Table 4). Their families are also more likely to engage in agricultural production (managing large land plot for annual trees, owning more cattle) and belong to ethnic minority groups. In comparison with the control group, housing quality (building material and living space) of the treatment group is much lower, together with limited access to clean water and electricity. Enlisted households also own strictly fewer assets (TV, vehicles, fridge, phone, etc.) and have lower education attainment.

As discussed in Section 2, according to the HEPR regulations our B1 score should predict very well assignment to treatment in 2015. Because it provides a comprehensive description of households' economic condition, it should be a reasonably good predictor also in the previous rounds. Figure 1 reports the distribution of B1 score by treatment, separately for each round. While it is true that treated households have lower B1 scores on average, the two distributions share the same support. This is a clear evidence of bad targeting in program implementation.

In the next part, we evaluate the effects of HEPR on the two outcomes: current enrolment status (the child is enrolled in current academic year by the time of interview) and child work (the child reports doing paid job outside the household, or spends more than 4 hours daily carrying out family tasks, e.g. chores, engaging in farming or family business). Descriptive statistics for the outcome variables by round in the control group are reported in Table A2 in the Appendix.

4 Empirical methodology

We estimate the following linear model with Ordinary Least Squares (OLS):

$$Y_{irt} = \alpha_i + \sum_{\substack{\tau=-3 \\ \tau\neq-1}}^{2} \beta_{T+\tau} Poor_{ir}^{T} \times D_{T+\tau} + \gamma_t + \delta_{rt} B1_{irt} + \varepsilon_{irt} \qquad (1)$$

where Y_{irt} is the outcome for child *i* living in region r[12] in year *t*. $Poor_{ir}^{T}$ is a time-fixed dummy which takes 1 for children whose household first appeared in the official poverty list at time T and 0 if they were never included in the list; and $D_{T+\tau}$ are dummies equal to 1 when t = T+τ, that is, τ periods (survey rounds) after T, and to 0 otherwise. The omitted dummy is the one for t = T-1. In addition, B1 is the B1 score discussed in Section 3, whose effect is allowed to vary by region and by time, while α_i and γ_t are child and time fixed effects, respectively. Finally, ε_{irt} is an error term, that we allow to be clustered by child *i*. The $\beta_{T+\tau}$ coefficients[13] capture the differential enrolment rate at time T+τ of treated children compared to control children. When τ takes on negative values, the β_{T-2}, β_{T-3} coefficients indicate any effects of HEPR on child outcomes before the intervention takes place and provide a test for pre-treatment parallel trends between the treatment and the control group.

We run separate regressions for each cohort. However, as illustrated in Table 3, in rounds 3, 4 and 5 YC kids are as old as OC kids in rounds 1, 2 and 3, respectively. We take advantage of this overlap to compare the effects of the treatment at given ages across the two cohorts.

5 Findings

5.1 Educational achievement

Table 5 reports HEPR effects on enrolment by age of treatment and cohort (see Figure 2 for graphical illustration). Column (1) reports the effect of treatment at age 8 for YC.[14] Columns (2) and (3) report treatment effects at age 12 for YC and OC, respectively. Likewise, the effects of treatment at age 15 for YC and OC are displayed in Columns (4) and (5).

[12] To minimize the potential bias due to selective migration, unless otherwise stated we assign families to the regions in which they were residing in round 1.

[13] Notice that the set of coefficients $\beta_{T+\tau}$—corresponding to treatment effects at T+τ—varies depending on the timing of treatment T.

[14] In 2002, when the OC was aged 8, HEPR had not been implemented yet.

<Table 5>

Column (1) shows that the effect of treatment at age 8 is an immediate increase of enrolment by 9 percentage points,[15] which remains present until children reach age 12 (T+1) and almost disappears once they are 15 years old (point estimate at T+2 is 0.027 and statistically insignificant).

In Columns (2) and (3) we see that receiving treatment at age 12 does not appear to affect enrolment. If anything, treated children are less likely to be in school, but the impact is small and insignificant. However, treated individuals have a strong tendency to be out of school 4 years later, at T+1. Result in Column (2) implies a decline of 17.1 percentage points in enrolment at age 15 for YC children. This effect is equal to 13.1 percentage points for OC children—see Column (3). The negative treatment effect remains significant after 7 years, at T+2, when OC is 19 years old. The coefficient at T-2 on YC is not significantly different from zero, implying that the parallel trend assumption is not rejected in the pre-treatment period, a reassuring result for our identification strategy.

Consistently with the pattern above, children of YC and OC treated at age 15 are 18.3 and 12.4 percentage points less likely to be at school, respectively. No statistically significant difference between treated and control children remains at age 19. Finally, we do not observe any evidence against the parallel trend assumption, as pre-treatment effects on school enrolment at T-3 and T-2 for both cohorts are not statistically significant.

The heterogeneity in the effects is in part consistent with the dynamic models of human capital formation (Cunha and Heckman 2007; Cunha and Heckman 2008; Heckman et al. 2014). Because human capital buildup is most efficient during the initial phase of life cycle, households would perceive tuition exemption and educational related benefits at age 8 as great chances to invest in schooling. Instead, treated-at-12 and treated-at-15 children are deemed too old for the marginal benefit to compensate for the marginal cost of schooling. Once children reach these ages, investment in human capital yields lower return, generating economical inefficiency, and is therefore not undertaken even when incentivized.

[15] The omitted coefficient β_{T-1} corresponds to pre-treatment round, in this case when the YC were 5 years old and supposed to be in kindergarten. There are no estimates for β_{T-3} and β_{T-2} in this column. By construction, these coefficients refer to when the Young Cohort was not born yet and was 1-year-old, respectively. Therefore, we cannot test for parallel pre-treatment trends in this regression.

All tests we performed failed to reject the parallel trend assumption. While these tests are encouraging, they refer only to the pre-treatment period and nothing can be said for post-treatment, as in all DID analysis. Fortunately, the bad targeting of HEPR creates the proper condition for the parallel trend assumption to hold. Bad targeting implies that we observe both treated and control households *for any given level* of the B1 score, which necessarily are very similar in terms of their current and potential economic conditions. Of course, treated and non-treated households can differ in many other respects that we cannot control, such as the strength of their connection with Commune officials, which increases the probability of having access to the benefits of HEPR. All these issues notwithstanding, households in similar economic conditions are likely to face similar tradeoffs any time they come to decide about their children schooling.

5.2 Robustness checks

5.2.1 Ethnicity

<Table 6>

Part of the HEPR explicitly focuses on improving living condition of ethnic minority communities. For instance, Program 135 was implemented within the HEPR to improve the well-being of people living in critically poor districts in remote areas (Northern mountainous area, Central Highlands, islands) and ethnic minorities (for more details, see reports and discussions of UNDP, Oxfam, CEMA). Since the eligibility criteria and provisions of these programs are different from other components of HEPR, pooling children of different ethnicities can be questionable. To address this concern, in Table 6 we report estimation results when we exclude ethnic minority children from the baseline sample and use only Kinh (the ethnic majority) children.

The treatment effect for treated at age 8 children is almost unchanged, with an increase of 8.2 and 9.8 percentage point in enrolment at age 8 and age 12, respectively. The remaining results are in line with the baseline. The one novelty worth mentioning is that the negative impact at age 15 seems to persist until age 19 for OC children who were treated either at age 12 or age 15.

5.2.2 Region

<Table 7>

Over time, Young Lives children and their family might move across regions. In the baseline analysis we use region of origin—that is, the one observed in round 1—to avoid issues related with

endogenous mobility. Here, region of current residence is used instead of region of origin. This means that region fixed-effects can be estimated separately from individual fixed effects and are thus included in Eq. (1). Results are reported in Table 7 and remain virtually unchanged.

5.2.3 Wealth

<Table 8>

Since households' assets and resources may be affected by participation to HEPR program, the resulting B1 score risks to be a bad control. In this robustness check, for each treatment at time T, households' B1 score is kept fixed at the level prevailing at time T. Results are presented in Table 8 and fully confirm our baseline.

5.3 Effects by gender

We report results obtained when we estimate Eq. (1) separately for boys and girls in Table 9.

<Table 9>

Interestingly, the positive effect of HEPR on enrolment holds only for boys (14.1 percentage points increase at age 8 and 15.8 percentage points at age 12), while the corresponding estimates for girls are much smaller and non-significant.

In contrast, for the treated at age 12, the negative effects at age 15 is larger for girls, regardless of the cohort, while for the treated at age 15 the reduction in enrolment is stronger among boys.

The heterogeneous treatment effects by gender reflects gender inequality and cultural perspectives in Vietnam, where the society places more importance on education for males than for females. This story is similar also to other places, for instance in Mexico, where Behrman et al., 2011, observe that older girls (aged 13–15 when treatment began) start working to allow their younger brothers (aged 9–10) to stay in education.

5.4 Work

To complement the analysis on schooling, we estimate the effect of HEPR on work for children in working age. We consider treatment at various ages and investigate whether it affects the labor market participation of children at age 15 and, whenever possible, age 19. The dependent variable is a dummy equal to 1 if the child performs any paid activity on the market, or spends more than 4 hours/day on family tasks or house chores—thereby considering also informal work carried out

in the family. This variable is constructed on the basis of data about time use in the last 7 days, available in Young Lives between round 2 and round 5, when the YC is age between 5 and 15 and the OC is aged between 12 and 19. Table 10 shows estimates for YC children treated at age 8, age 12, age 15, and OC children treated at age 15.[16]

<Table 10>

In the first three columns, there is an increase of 7.0–12.9 percentage points in the probability of working for treated YC children when they reach age 15, regardless of treatment age. Interestingly, effect size is larger the later the treatment. Results in the last column also suggest that the OC children treated at age 15 are more likely to work at the time of treatment. However, the estimates are not statistically significant.

The dynamics in all treatment groups matches that of stronger school-leaving at age 15 mentioned above (see Figure 3). For OC children, treatment effect fades away over time, when formal education naturally comes to an end.

6 Discussion

While the positive effect of HEPR at early ages is expected and consistent with the literature, the negative impact on enrolment at age 15, which holds across cohorts and age of treatment, is unexpected. How can the inclusion in an anti-poverty program which reduces the cost of education push children to drop out of school?

To gain some intuition on the mechanisms behind this result we distinguish between urban and rural areas. Communes are classified as rural or urban according to official administrative system. In the former, the education infrastructure is denser and returns to education are higher, while in the latter the main sector of occupation is agriculture, where the kind of skills learned at school are less relevant (in our sample, the proportion of households involved in agricultural activities is 10 times larger in rural than in urban area, see Table A3 in the Appendix).

Results of this analysis are reported in Table 11. We observe that the positive effects of treatment at age 8 on enrolment are stronger among urban children. In fact, regarding urban areas, this

[16] We do not estimate effects on work for OC children treated at age 12 because time use data for the OC are not available before age 12.

positive effect persists until age 15. Treatment effects in rural areas are instead small in magnitude, not statistically significant, and completely absent by age 15. For treatment at age 12 there is some evidence of a moderate positive contemporaneous effect in urban areas. Conversely, a negative effect emerges for rural children at age 15. Negative effects are also detected among rural children for treatment at age 15. All in all, the program seems to have increased the urban-rural educational gap in Vietnam. This result is opposite to de Brauw et al. 2015's finding on the impact of Bolsa Familia, in which the program apparently reduced urban-rural gap on educational outcomes of Brazilian children.

<Table 11>

We conjecture that this pattern of results depends on two factors. First, unlike in urban areas, schools are not easily accessible in rural areas. Many communes and villages have no local or satellite school, so children must go to a neighboring commune or district center if they wish to study. This is particularly true for lower and upper secondary schools. According to National Committee for Advancement of Women, the majority of students in Northern Uplands, North Central and Mekong Delta spends nearly one hour to walk to school. In Lang Son, a province in Northern Uplands, primary school pupils have to travel more than 6 km every day, crossing rocky routes and mountain streams (Centre for International Economics 2002). The lack of school facilities, coupled with limited supplies of textbook and studying materials, as well as lower teaching quality[17], makes education less appealing in rural area. In this context, even tuition exemptions are probably unable to support children enrolment in rural areas.

Second, since 2009 (round 3), there is another provision interfering with tuition fee exemption in formal education, that is free access to the "Vocational Training for Rural Workers" program (VTP). The VTP aims to improve the quality of rural labor, facilitating industrialization and modernization in agricultural sector and in the rural area through both agricultural and non-agricultural three-month training for rural workers. Each year, the project involves about one million rural workers in working age (15 to 59) and guarantees employment to at least 80 percent of participants. At the end of the course, participants will receive a primary vocational certificate.

[17] Teachers prefer to look for jobs in big cities (Centre for International Economics 2002).

Although the project is opened to all rural workers, it prioritizes people who have contributed to the revolution, people from poor households, near-poor households, ethnic minorities, disabled people and farmland-nationalized farmers. Trainees who are in the official poverty lists[18] get exempted from course and material fees (amount up to 3 million VND or 120 USD), can borrow at low-interest rate from Social Policy Bank under the category of "poor student" and can receive allowances for transportation and accommodation during training.

Upon completion of middle school, at the age of 15, children can access high schools, attend formal vocational schools, or leave education. The duration of the formal vocational programs is about 2–3 years. With its significantly shorter training time and the support for employment, VTP can be an attractive option for young people. Therefore, we hypothesize that, among rural children, the benefits of the fee exemption for vocational training outweighs those of the fee exemption for formal schooling.

Unfortunately, there is no information about VTP in our data to directly test this hypothesis. Given that trainees are expected to get quickly employed, indirect support comes from the evidence that in rural areas treatment effects on children working activities are larger than in urban areas.

<Table 12>

Table 12 reports treatment effects on work for YC children treated at age 8, age 12, age 15 and OC ones treated at age 15, separately by living area. As previously shown in the case of enrolment, the impacts of the HEPR are mostly concentrated among rural children. In Columns (1), (3) and (5) of Table 12, treated YC children living in rural area are 8.5–19 percentage points more likely to carry out work when they are 15 years old. The effects on work only emerge at age 15, despite variation in the timing of treatment. There is no apparent effect on working status of urban children, except a significant decrease of 7.3 percent for the YC children treated at age 12 (see Table 12, Column (4)). This piece of evidence is consistent with the contemporaneous higher school enrolment of this group (see Table 11, Column (4)). While the YC turned 15 in 2016, the OC turned 15 in 2009, exactly the same year when the VTP was put into effect. As the VTP was

[18] VTP obtains this information from local government with the official poverty definition by MOLISA even if it is not under the HEPR umbrella.

gradually phased in, the smaller and not statistically significant effect of HEPR on work among the OC treated at age 15 is understandable.

Since the VTP offers free access to any member of poor households, the program could have induced children to work more and study less through a substitution effect within families. If VTP enhanced parents' employability, they might have charged children with additional tasks at home or in the family business, even before age 15. This possibility provides a further motivation for the different role played by fee exemption on school enrolment between urban and rural areas.

To sum up, the combination of positive and negative effects depends on the differential salience of tuition fee exemptions in rural and urban areas and the interplay between this policy and the supported access to VTP in total areas.

7 Conclusion

This paper evaluates the effects of an anti-poverty set of measures (the HEPR program) in Vietnam on school enrolment. Our investigation adopts a DID design using longitudinal data from the Young Lives survey spanning 15 years. The study contributes to the knowledge on child welfare and protection in Vietnam and highlights the importance of early intervention.

We find in fact heterogeneous effects on schooling depending on the timing of treatment. When treated at age 8, children are 9 percent more likely to enroll. This positive effect lasts until age 12 and disappears by age 15. This result is stronger among boys and children living in urban areas. When the treatment is administered at later ages (age 12 or age 15), we instead observe a reduction in school enrolment by age 15, especially among girls and rural offspring, accompanied by a parallel increase in labor market participation.

While the positive impacts of early intervention on school-enrolling supports the dynamic theory of formation of human capital, implying that early investments are more effective at enhancing educational attainment, the negative effects at age 15 are more puzzling.

The urban-rural gradient of these effects helps rationalizing this evidence. On the one hand, the higher availability of education infrastructures in urban areas implies that the tuition fee exemption is more effective in boosting enrolment in urban areas. On the other hand, fee exemption for the Vocational Training Program in place for offspring aged 15 plus in rural areas de-incentivizes

school enrolment for rural children, who may instead find it more attractive to drop out of school and enroll in the much shorter VTP.

The program-substitution effect observed in our analysis is strong evidence of a lack of harmonization among pro-poor schemes, whose goals could at times be in conflict with one another. This result calls for an urge to fully integrate the several social welfare programs in Vietnam, which often overlap in nature, causing inefficiency in both implementation and goal achievement.

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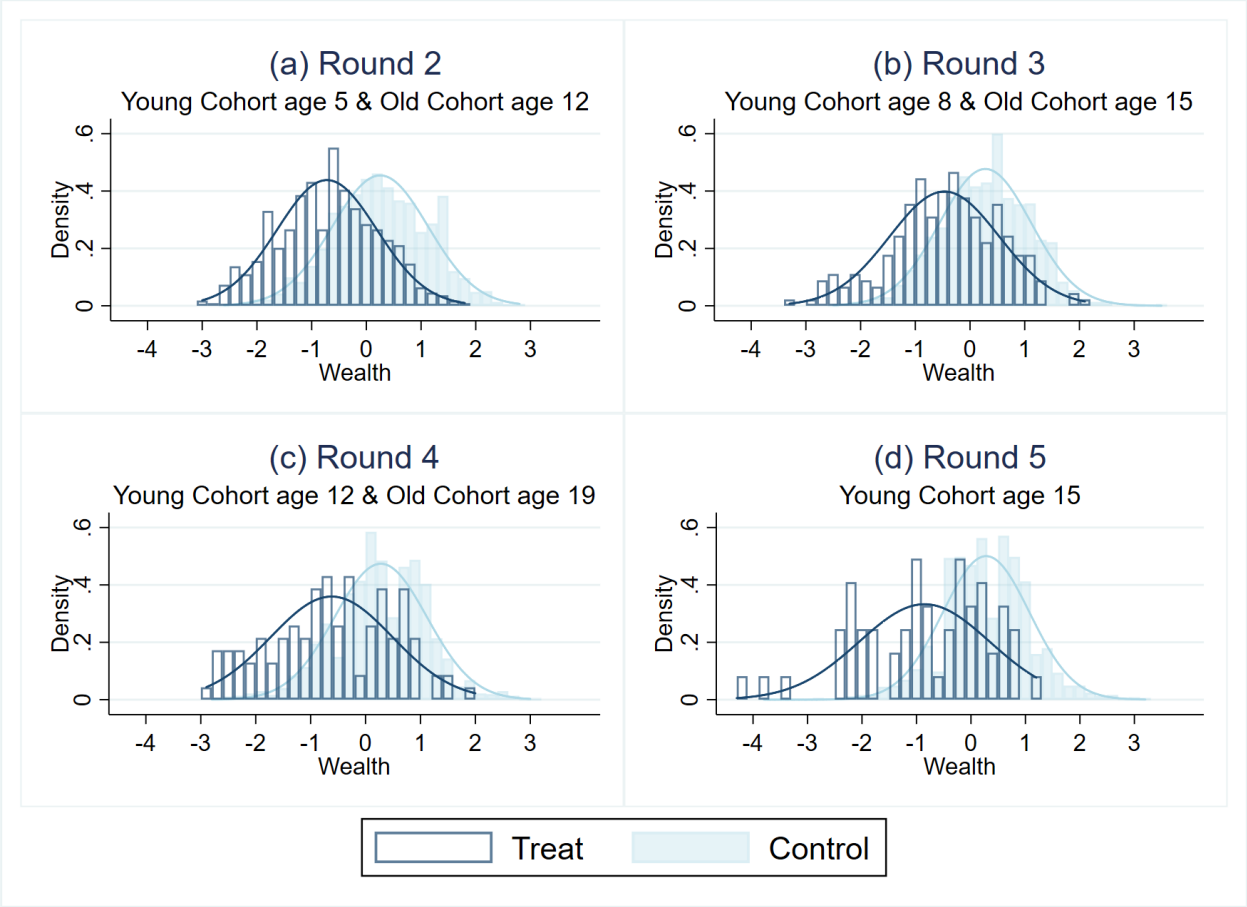
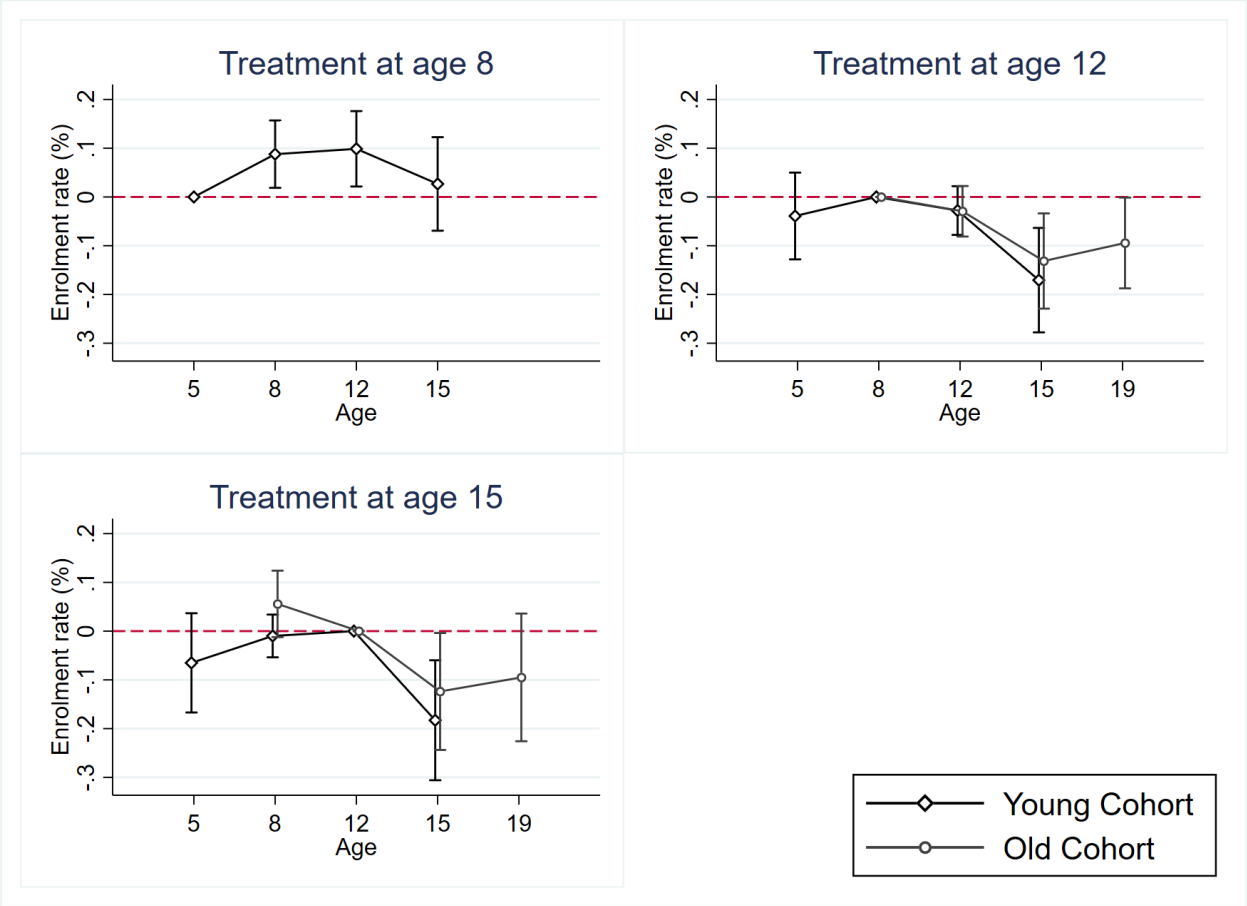
Figures and tables

Figure 1. Distribution of B1 score by treatment group and round



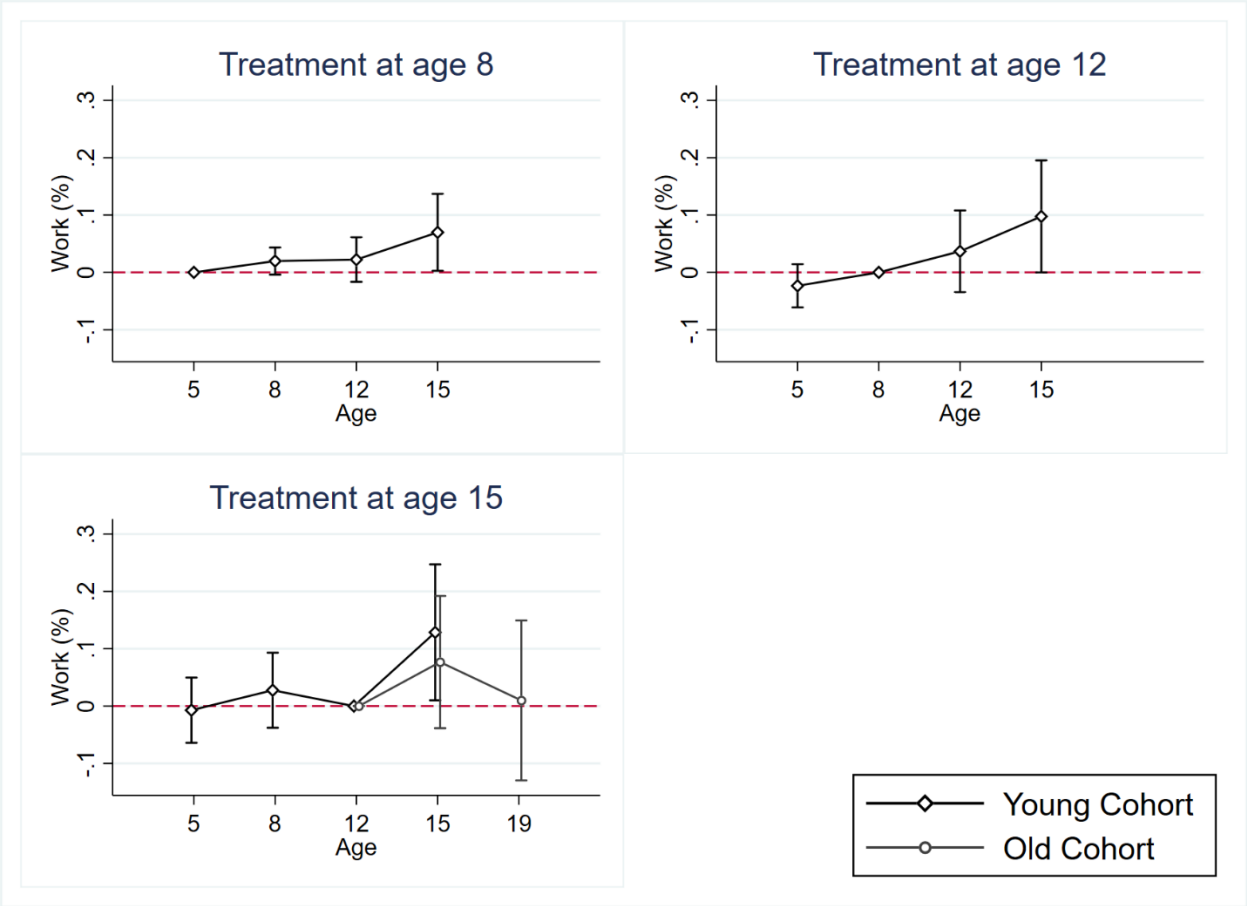
Notes: B1 score is the index measuring socioeconomic status of households (see Section 2), standardized by round.

Figure 2. Effects of treatment on enrolment (with 95% C.I.)



Notes: The figure illustrates differential changes over time in enrolment rate between the treated and the control group. The horizontal dash line corresponds to the trend of controlled children net of time effects as well as treatment effect at T-1. The three panels show treatment effects for three treatment groups: children treated at age 8, children treated at age 12, and children treated at age 15 (estimated coefficients are reported in Table 5, respectively in Column (1), Columns (2) & (3), and Columns (4) & (5)).

Figure 3. Treatment effects on child work (with 95% C.I.)



Notes: The figure illustrates differential changes over time in child work incidence between the treated and the control group. The horizontal dash line corresponds to the trend of controlled children net of time effects as well as treatment effect at T-1. The three panels show treatment effects for three treatment groups: children treated at age 8, children treated at age 12, and children treated at age 15 (estimated coefficients are reported in Table 10, respectively in Column (1), Column (2), and Columns (3) & (4)).

Table 1. Descriptive statistics from round 1 (%), by cohort

	Older	Younger	Total
Child's gender			
Male	50.1	51.35	50.93
Female	49.9	48.65	49.07
Household head's gender			
Male	84	84.25	84.17
Female	16	15.75	15.83
Father's education			
Below primary	28.68	29.92	29.5
Below middle-secondary	35.08	35.36	35.27
Below upper-secondary	20.8	20.08	20.32
Post-secondary, vocational	15.44	14.64	14.91
Mother's education			
Below primary	35.32	33.57	34.15
Below middle-secondary	35.01	36.51	36.01
Below upper-secondary	18.07	18.66	18.47
Post-secondary, vocational	11.6	11.26	11.37
Household size			
2	0.7	0.35	0.47
3	9.9	22.3	18.17
4	36.2	30.4	32.33
5	26.1	17.95	20.67
6	14.8	11.95	12.9
7 or more	12.3	17.05	15.47
Ethnicity			
Kinh (majority)	87.2	85.6	86.13
Minority	12.8	14.4	13.87
Living area			
Urban	20	20	20
Rural	80	80	80
Region			
Northern Uplands	20	20	20
Red River Delta	20	20	20
Central Coast	20	20	20
Southern Central Coast	20	20	20
Mekong River Delta	20	20	20
Observations	1,000	2,000	3,000

Notes: Originally the sample of the 3,000 children selected in Round 1 lived in 20 sentinel sites, which cover 31 communes and belong to 14 districts in five provinces/cities (Lao Cai, Hung Yen, Da Nang, Phu Yen and Ben Tre), each locates in a different region (respectively Northern Uplands, Red River Delta, Central Coast, Southern Central Coast and Mekong River Delta). Over time, Young Lives children and their families move to other regions of the country.

Table 2. Definitions of treatment and control groups, by cohort and age

Cohort		Age 5	Age 8	Age 12	Age 15	Age 19	Obs.
Young Cohort	*Round*	*Round 2*	*Round 3*	*Round 4*	*Round 5*		
	Treated at age 8	O	X	X/O	X/O	-	155
	Treated at age 12	O	O	X	X/O	-	84
	Treated at age 15	O	O	O	X/O	-	61
	Control	O	O	O	O	-	1,212
Old Cohort	*Round*			*Round 2*	*Round 3*	*Round 4*	
	Treated at age 12	-	-	X	X/O	X/O	174
	Treated at age 15	-	-	O	X	X/O	70
	Control	-	-	O	O	O	641

Notes: O is not in the poor list, X is in the poor list.

Table 3. Persistence in the poor list

Panel A. Presence in the poor list over time: Pearson's correlation coefficients			
	Poor 2009	Poor 2012	Poor 2016
Poor 2006	0.4439*	0.2693*	0.2600*
Poor 2009		0.3237*	0.2656*
Poor 2012			0.3983*
Panel B. Share of treated-in-year-T household that is still on the list in the next periods			
	Poor 2009	Poor 2012	Poor 2016
Treat 2006	53.07%	35.73%	29.81%
Treat 2009		34.39%	23.61%
Treat 2012			37.17%

*Notes: * Correlation is significant at 0.01 level. Poor status indicates presence of household on the poor list of HEPR in a certain year. Treat status indicates household treated in year T according to our treatment definition.*

Table 4. Descriptive summary of main household characteristics by treatment status

Panel A. Young Cohort

	Treated at age 8			Treated at age 12			Treated at age 15			Controls		
	Mean	Std. dev.	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.	Obs.
B1 score	-0.61	0.96	155	-0.66	1.06	84	-0.85	1.2	61	0.23	0.81	1212
Household size	4.6	1.34	155	4.62	1.28	84	5	1.29	61	4.45	1.08	1212
Number of dependents in the family	2.41	0.97	155	2.39	1.06	84	1.77	1.16	61	2.11	0.86	1212
Highest qualification of household members	0.19	0.49	155	0.12	0.33	84	0.41	0.88	61	0.57	0.97	1212
Working sector of household members	1.53	0.5	155	1.61	0.49	84	1.61	0.49	61	1.72	0.45	1212
Concreted wall material	0.59	0.49	155	0.65	0.48	84	0.59	0.5	61	0.81	0.4	1212
Average living space per capita (m2)	2.03	1.19	155	2.86	1.25	84	2.89	1.23	61	2.54	1.26	1212
Monthly electricity consumption (kWh)	752	1068	155	1544	1452	84	1815	1599	61	1460	2765	1212
Access to safe drinking water	0.15	0.36	155	0.15	0.36	84	0.34	0.48	61	0.21	0.41	1212
Access to sanitation	0.48	0.5	155	0.54	0.5	84	0.79	0.41	61	0.71	0.45	1212
Urban	0.2	0.4	155	0.13	0.34	82	0.21	0.41	61	0.21	0.41	1212
Ethnic minority	0.25	0.44	155	0.37	0.49	84	0.39	0.49	61	0.06	0.24	1212
Child's gender	0.5	0.5	155	0.46	0.5	84	0.48	0.5	61	0.52	0.5	1212
Observations	155			84			61			1212		

Panel B. Old Cohort

	Treated at age 12			Treated at age 15			Controls		
	Mean	Std. dev.	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.	Obs.
B1 score	-0.63	0.86	174	-0.15	1.02	70	0.21	0.93	641
Household size	4.91	1.32	174	4.37	1.28	70	4.79	1.17	641
Number of dependents in the family	2.29	0.97	174	1.39	1.13	70	2.43	0.93	641
Highest qualification of household members	0.17	0.43	174	0.56	0.93	70	0.54	0.9	641
Working sector of household members	1.51	0.5	174	1.64	0.48	70	1.37	0.48	641
Concreted wall material	0.45	0.5	174	0.7	0.46	70	0.64	0.48	641
Average living space per capita (m2)	2.29	1.3	174	2.4	1.29	70	2.66	1.25	641

Monthly electricity consumption (kWh)	345	297	174	835	884	70	757	754	641
Access to safe drinking water	0.08	0.27	174	0.24	0.43	70	0.10	0.30	641
Access to sanitation	0.48	0.5	174	0.63	0.49	70	0.52	0.50	641
Urban	0.2	0.4	174	0.23	0.42	70	0.17	0.38	641
Ethnic minority	0.24	0.43	174	0.17	0.38	70	0.1	0.29	641
Child's gender	0.5	0.5	174	0.54	0.5	70	0.48	0.5	641
Observations	174			70			641		

Notes: each variable is measured at the age of first treatment for treated subjects and at age 8 for the controls. Households with seven or more members are coded as 7. Dependents include children below 15 years old, people above 60 years old, people with disability that are not able to work. Qualification levels include: 0 Lower secondary or below, 1 High school, 2 Vocational training, 3 College, university. Working sector is divided into: 1 Agriculture and 2 Non-agriculture. Gender is coded as 0 female and 1 male.

Table 5. Treatment effects on enrolment, by cohort and age at first treatment (baseline)

Dependent variable: Enrolment	(1) age 8 YC	(2) age 12 YC	(3) age 12 OC	(4) age 15 YC	(5) age 15 OC
T-3				-0.065	
				(0.052)	
T-2		-0.039		-0.010	0.056
		(0.045)		(0.022)	(0.035)
T	0.088**	-0.028	-0.029	-0.183***	-0.124**
	(0.035)	(0.025)	(0.026)	(0.063)	(0.061)
T+1	0.099**	-0.171***	-0.131***		-0.095
	(0.039)	(0.055)	(0.050)		(0.067)
T+2	0.027		-0.094**		
	(0.049)		(0.047)		
Observations	5,469	5,197	3,061	5,101	2,716
Number of id	1,383	1,313	799	1,290	710

*Notes: All regressions include individual fixed-effects, round dummies, interactions of B1 score with region-round. Robust standard errors clustered at individual level in parentheses *** p<0.01, ** p<0.05, * p<0.1.*

Table 6. Treatment effects on enrolment, subsample of ethnic majority (Kinh) children

Dependent: Enrolment	(1) age 8 YC	(2) age 12 YC	(3) age 12 OC	(4) age 15 YC	(5) age 15 OC
T-3				-0.029	
				(0.063)	
T-2		0.011		-0.014	0.039
		(0.052)		(0.009)	(0.034)
T	0.082**	-0.006	-0.010	-0.162**	-0.159**
	(0.041)	(0.018)	(0.023)	(0.073)	(0.068)
T+1	0.098**	-0.153**	-0.142***		-0.146**
	(0.044)	(0.064)	(0.053)		(0.072)
T+2	0.022		-0.110**		
	(0.057)		(0.054)		
Observations	5,031	4,785	2,673	4,719	2,435
Number of id	1,273	1,210	699	1,194	637

*Notes: All regressions include individual fixed-effects, round dummies, interactions of B1 score with region-round. Robust standard errors clustered at individual level in parentheses *** p<0.01, ** p<0.05, * p<0.1.*

Table 7. Treatment effects on enrolment, using current region of residence

Dependent: Enrolment	(1) age 8 YC	(2) age 12 YC	(3) age 12 OC	(4) age 15 YC	(5) age 15 OC
T-3				-0.061	
				(0.053)	
T-2		-0.031		-0.002	0.042
		(0.045)		(0.023)	(0.032)
T	0.083**	-0.026	-0.027	-0.161***	-0.127**
	(0.035)	(0.027)	(0.026)	(0.062)	(0.061)
T+1	0.090**	-0.175***	-0.137***		-0.117*
	(0.039)	(0.055)	(0.049)		(0.068)
T+2	0.026		-0.086*		
	(0.048)		(0.049)		
Observations	5,434	5,159	3,036	5,066	2,695
Number of id	1,383	1,313	799	1,290	710

*Notes: Regions used in these regressions are regions of residence in current round. All regressions include individual fixed-effects, dummies for regions, rounds, interactions of region and round, interactions of region and B1 score, interactions of round and B1 score. Robust standard errors clustered at individual level in parentheses *** p<0.01, ** p<0.05, * p<0.1.*

Table 8. Treatment effects on enrolment, keeping B1 score constant within household

Dependent: Enrolment	(1) age 8 YC	(2) age 12 YC	(3) age 12 OC	(4) age 15 YC	(5) age 15 OC
T-3				-0.062	
				(0.052)	
T-2		-0.044		-0.016	0.047
		(0.044)		(0.022)	(0.032)
T	0.078**	-0.026	-0.026	-0.190***	-0.129**
	(0.035)	(0.024)	(0.026)	(0.063)	(0.061)
T+1	0.071*	-0.159***	-0.139***		-0.111*
	(0.038)	(0.056)	(0.049)		(0.066)
T+2	0.021		-0.121**		
	(0.049)		(0.049)		
Observations	5,422	5,129	3,056	5,089	2,701
Number of id	1,365	1,288	795	1,286	703

*Notes: All regressions include individual fixed-effects, round dummies, interactions of B1 score at treatment (time T) with region-round. Robust standard errors clustered at individual level in parentheses *** p<0.01, ** p<0.05, * p<0.1.*

Table 9. Treatment effects on enrolment, by gender

Dependent: Enrolment	(1) age 8 YC girl	(2) age 8 YC boy	(3) age 12 YC girl	(4) age 12 YC boy	(5) age 12 OC girl	(6) age 12 OC boy	(7) age 15 YC girl	(8) age 15 YC boy	(9) age 15 OC girl	(10) age 15 OC boy
T-3							-0.072	-0.071		
							(0.076)	(0.074)		
T-2			0.026	-0.124*			0.034	-0.052**	0.034	0.071
			(0.055)	(0.074)			(0.039)	(0.024)	(0.047)	(0.052)
T	0.040	0.141***	-0.023	-0.032	-0.013	-0.039	-0.126*	-0.218**	-0.068	-0.191**
	(0.049)	(0.050)	(0.030)	(0.042)	(0.037)	(0.040)	(0.073)	(0.097)	(0.077)	(0.094)
T+1	0.049	0.158***	-0.250***	-0.079	-0.142**	-0.101			-0.150	-0.042
	(0.052)	(0.058)	(0.076)	(0.082)	(0.069)	(0.071)			(0.092)	(0.096)
T+2	-0.023	0.066			-0.122*	-0.061				
	(0.064)	(0.073)			(0.069)	(0.064)				
Observations	2,635	2,834	2,508	2,689	1,607	1,454	2,454	2,647	1,425	1,291
Number of id	669	714	636	677	413	386	623	667	366	344

*Notes: All regressions include individual fixed-effects, round dummies, interactions of B1 score with region-round. Robust standard errors clustered at individual level in parentheses *** p<0.01, ** p<0.05, * p<0.1.*

Table 10. Treatment effects on child work, by cohort and age at first treatment (baseline)

Dependent variable: Work	(1) age 8 YC	(2) age 12 YC	(3) age 15 YC	(4) age 15 OC
T-3			-0.007	
			(0.029)	
T-2		-0.023	0.028	
		(0.019)	(0.033)	
T	0.020	0.037	0.129**	0.077
	(0.012)	(0.036)	(0.060)	(0.059)
T+1	0.022	0.098*		0.010
	(0.020)	(0.050)		(0.071)
T+2	0.070**			
	(0.034)			
Observations	5,364	5,095	4,998	2,091
Number of id	1,385	1,313	1,290	715

*Notes: All regressions include individual fixed-effects, round dummies, interactions of B1 score with region-round. Robust standard errors clustered at individual level in parentheses *** p<0.01, ** p<0.05, * p<0.1.*

Table 11. Treatment effects on enrolment, by urban or rural area

Dependent: Enrolment	(1) age 8 YC rural	(2) age 8 YC urban	(3) age 12 YC rural	(4) age 12 YC urban	(5) age 12 OC rural	(6) age 12 OC urban	(7) age 15 YC rural	(8) age 15 YC urban	(9) age 15 OC rural	(10) age 15 OC urban
T-3							-0.091	-0.000		
							(0.065)	(0.077)		
T-2			-0.051	0.030			-0.008	-0.008	0.049	0.081
			(0.051)	(0.090)			(0.029)	(0.015)	(0.040)	(0.070)
T	0.058	0.213**	-0.037	0.022*	-0.035	-0.011	-0.219***	-0.066	-0.111	-0.176
	(0.038)	(0.088)	(0.031)	(0.013)	(0.031)	(0.018)	(0.077)	(0.082)	(0.068)	(0.143)
T+1	0.066	0.223**	-0.205***	-0.033	-0.158***	0.014			-0.096	-0.074
	(0.043)	(0.102)	(0.063)	(0.090)	(0.057)	(0.087)			(0.078)	(0.135)
T+2	0.000	0.182*			-0.110**	0.013				
	(0.054)	(0.109)			(0.055)	(0.120)				
Observations	4,276	1,176	4,080	1,100	2,471	560	3,983	1,103	2,192	504
Number of id	1,106	330	1,054	308	659	150	1,031	309	584	135

*Notes: All regressions include individual fixed-effects, round dummies, interactions of B1 score with region-round. Robust standard errors clustered at individual level in parentheses *** p<0.01, ** p<0.05, * p<0.1.*

Table 12. Treatment effects on child work, by urban or rural area

Dependent: Work	(1) age 8 YC rural	(2) age 8 YC urban	(3) age 12 YC rural	(4) age 12 YC urban	(5) age 15 YC rural	(6) age 15 YC urban	(7) age 15 OC rural	(8) age 15 OC urban
T-3					-0.009	-0.011		
					(0.039)	(0.009)		
T-2			-0.027	-0.011	0.034	-0.010		
			(0.023)	(0.008)	(0.045)	(0.010)		
T	0.025	0.002	0.036	0.066	0.181**	-0.040	0.078	0.063
	(0.015)	(0.006)	(0.043)	(0.071)	(0.076)	(0.025)	(0.068)	(0.115)
T+1	0.014	0.068	0.139**	-0.073***			-0.032	0.075
	(0.022)	(0.051)	(0.059)	(0.026)			(0.084)	(0.145)
T+2	0.085**	-0.020						
	(0.041)	(0.038)						
Observations	4,192	1,155	3,999	1,079	3,902	1,081	1,681	388
Number of id	1,107	330	1,053	308	1,030	309	585	136

*Notes: All regressions include individual fixed-effects, round dummies, interactions of B1 score with region-round. Robust standard errors clustered at individual level in parentheses *** p<0.01, ** p<0.05, * p<0.1.*

Appendix

Data approximation for items on the B1 form

The B1 form assesses the following items: number of household members, number of dependents, highest education level in the family, household members work in non-agricultural sector, pension, housing quality, average per capita living space, monthly electricity consumption, water source, toilet condition, main assets (tv, fridge, motorcycle, car, heater, oven, air conditioner, boat), land and property (for living, agriculture, fishery, production), number of cattle, region.

To approximate monthly electricity consumption (kWh) in each round, we divide the household's report on average monthly electricity bill (thousand VND) by the electricity price quote Using information from Vietnam Electricity Group—EVN—in the same period. To identify the total area of land and water surface by use purpose, we use the reported land area for each type of plant then distinguish annual crops from perennial ones (in our data, perennial plants include: avocado, black pepper, coffee, eucalyptus, fruits, guava, mango, orange, pineapple, sugarcane, apple, coconut, plum, longa, litchi, rambutan, cashew, bonsai, rubber, mangosteen, tea) and finally sum them up according to crop types. The household is counted as fish-farming if they report having a water surface for aquaculture. Due to unavailability of information on pension in some rounds (data are collected only for Young Cohort in round 2, round 4, round 5) and relatively small share of population receiving pension (< 10%), we exclude this item from the calculation. In addition, we impute missing data on average living space, electricity, oven, boat in round 1 and average living space, land and property in round 3 using information from round 2. The weight of each item ranges from 0 to 25 points, depending on the region (see Table 3). Households will be classified as poor if they have a total score below 140 (170) point and live in a rural (urban) area.

Table A1. National poverty line definitions by MOLISA

Period	Category	Rural		Urban	Unit (per capita)
		Rural 1	Rural 2		
Before 2000	Poor	15	20	25	kg of rice
2002 – 2005	Poor	80,000	100,000	150,000	VND
2006 – 2010	Poor	200,000		260,000	VND
2011 – 2015	Poor	400,000		500,000	VND
	Near poor	520,000		620,000	VND
2016 – 2020	Poor	700,000		900,000	VND
	Poor	1,000,000 and lack from 3 indexes		1,300,000 and lack from 3 indexes	VND and service index
	Near poor	1,000,000 and lack less than 3 indexes		1,300,000 and lack less than 3 indexes	VND and service index

Notes: The multidimensional approach in 2016 considers 10 measurement indicators of access to services: access to healthcare services, ownership of health insurance, educational level of adults, schooling status of children, housing quality, average per capita housing space, clean water, hygienic toilet, use of telecommunication services, assets used for access to information.

Table A2. Mean outcome variables of control groups over time

	Young Cohort		Old Cohort	
	Enrolment	Work	Enrolment	Work
Age 5	0.86	0.00	-	-
Age 8	0.99	0.01	0.99	-
Age 12	0.99	0.02	0.97	0.07
Age 15	0.88	0.10	0.84	0.15
Age 19	-	-	0.54	0.49

Notes: Enrolment takes value 1 if the child is enrolled in current academic year by the time of interview, and 0 otherwise. Child works if he or she reports doing paid work outside or spending more than 4 hours daily on family tasks.

Table A3. Proportion of households living out of agriculture by living area (%)

Sector	Urban	Rural	Total
Agriculture	4.05	50.06	40.6
Non-agriculture	95.95	49.94	59.4
Total	100	100	100

Notes: Calculation by authors using Young Lives data, pooling together all rounds.