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FOSTERING HUMAN CAPITAL IN THE DEVELOPING WORLD: EVIDENCE FROM VIETNAM

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Introduction

The underlying theme of this PhD dissertation is the analysis of social policy, education system and human capital development in the context of a developing country. The thesis makes use of rich datasets on childhood poverty in Vietnam and consists of two papers, each corresponding to a chapter.

The first chapter, titled "The effects of the Vietnam Hunger Eradication and Poverty Reduction program on schooling", is a joint work with Lorenzo Rocco and Marco Bertoni. We study the effects of an anti-poverty 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 enrolment by about 9 percentage points. 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 coordinating different anti-poverty measures to reduce inefficiency and achieve social goals.

The second chapter, "Paying (and paving) my way: the impacts of extra class participation on primary school pupils in Vietnam", takes a closer look at the issues of transparency and equality within the education system. Extra class, or private tutoring offered by schoolteachers to their own pupils, is a widespread phenomenon in many developing countries. Although the provision of extra class for primary-school pupils is prohibited in Vietnam, it is very common for schoolteachers to run home-based daycare as the cover for informal private tutoring. Using Young Lives school survey of 3,284 pupils in Grade 5, I develop an empirical approach for studying the impacts of extra class on pupil outcomes. I find that teachers grant higher school grades to pupils attending extra class, but extra class attendance does not yield higher scores at standardized achievement tests. I interpret these results as

evidence of opportunistic behavior, whereby teachers exploit their arbitrariness in awarding grades, which count for secondary school admissions, to extract rents. The extent of grade inflation is higher in institutionally underdeveloped settings. Attending extra class also generates a gap in pupils' self-image. These findings provide relevant policy implications to align this informal sector with the country's education system.

Chapter 1:

The effects of the Vietnam Hunger Eradication and Poverty Reduction program on schooling*

Abstract

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 enrolment by about 9 percentage points. 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 coordinating different anti-poverty measures to reduce inefficiency and achieve social goals.

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

JEL codes: H52, H53, I24, I32

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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. Transfer programs 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 a variegated bundle of poverty-relief measures, that includes 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, access to student loans and stipends for gifted children, from preschool 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; van den Berg and Nguyen 2011; World Bank in Viet Nam 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 the unavailability of

[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 (H. T. Pham et al. 2008; Phung et al. 2012; Nguyen and Baulch 2007; Oxfam 2017a). van den Berg and Nguyen 2011; Phan et al. 2017 find that pro-poor policies produced rather undesirable effects, such as increasing inequality. There

data that could permit researchers to trace the beneficiaries of the many provisions of HEPR (van de Walle 2004a; 2004b; Evans and Harkness 2008; Roelen 2010; van den Berg and Nguyen 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 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.

Our findings can be summarized as follows. On the one hand, early treatment (at age 8) increases school enrolment by about 9 percentage points, 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

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is ample evidence of bad targeting and actual misconduct in the management of public interventions (Turk 1999; van de Walle 2004a; 2004b; van den Berg and Nguyen 2011; Richard et al. 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 (van den Berg and Nguyen 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.

evidence on the dynamic formation of human capital, showing that early educational investments are more effective (Cunha and Heckman 2007; 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, which in Vietnam coincides with the critical point when children complete lower secondary school and transit into upper secondary 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 to six 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—outcome of this policy. Second, we provide evidence that the effects of anti-poverty policies on educational outcomes 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 education system in Vietnam and 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 discuss these findings in Section 6. Conclusion follows.

2 Background

2.1 The education system in Vietnam

In Vietnam, the Ministry of Education and Training (MoET) is responsible for all educational matters, except for vocational training, which is under the management of the Ministry of Labor–Invalids and Social Affairs (MOLISA). The management of schools, human resources and financial resources is decentralized with high degree of autonomy at local educational bodies: Departments of Education and Training (DoETs, provincial level) and Bureaus of Education and Training (BoETs, district level).

School resources come from both state budget (90%) and households (10%) (Tran 2014). On average, government expenditures on education is around 15–20% of government total expenditures, 5–6% of total GDP (MoET and GSO 2016). Provinces manage their own educational spending and schools are encouraged to expand non-budgetary sources of funding. "Educational socialization" is a policy first enacted in 1997[5] and meant to reduce the burden of education on the public budget and promote participation of all children in education, in which self-sufficient private schools are established, and schools can collect contributions from pupils and parents (school construction fees, voluntary contribution, medical insurance, class' fund, parental associations' fund, etc).

The general education system consists of three levels: primary school (Grade 1 to 5), lower secondary school (Grade 6 to 9), upper secondary school (Grade 10 to 12). Pupils enroll in grade 1 of primary school at age 6. Most children enrolled in general education attend school on a half-day basis. Public schools lay the foundation for the system, accounting for 94% of the total number of educational institutions, while the private sector represents a modest share with less than 2% of pupils attending non-public school at primary and lower secondary levels (London 2011; Pham 2017).

Since 1991, primary education is compulsory and children shall complete primary education by the time they turn 14. To guarantee that families comply with this provision and that primary education becomes universal, all pupils are exempted from tuition fees in public schools at primary level. The government also acts to guarantee the supply of education facilities and teachers. Despite the effort toward primary universalization, many children with disadvantage backgrounds (for example those are in remote and mountainous areas, from low income families, orphans or children with disabilities)

[5] According to Resolution 90 (1997).

remain out of school and fail to complete the five-year primary cycle because their direct[6] and opportunity costs of education are comparatively large (MoET and GSO 2016).

After universal primary education, the country has started to work on universal provision of lower secondary education for children aged 11–18 since 2001 and of preschool education for children aged 5 since 2009. Aside from the provision of affordable schooling (in terms of the supply of facilities and of the availability of teachers and learning materials), programs aiming to raise awareness of parents are also in place (National Education For All Action Plan 2003–2015).

Families still have to pay a fee to attend pre- or post-primary education. As of 2010, monthly tuition fee at a public school alone ranged 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,[7] compared to a per capita income of poor family of about 200.000 VND.

2.2 The HEPR program and poverty definition

In 1998, as part of the Millennium Development Goals, the Government of Vietnam first passed a set of National Targeted Programs (NTPs) including a program on Hunger Eradication and Poverty Reduction (HEPR) which aimed to improve growth and development in several ways.[8] Formally, the HEPR became the NTP on Hunger Eradication Poverty Reduction and Job Creation during period 2001–2005, the NTP on Poverty Reduction during period 2006–2010, and subsequently the NTP on Sustainable Poverty Reduction during periods 2011–2015 and 2016–2020, but its original purpose was always preserved.

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[6] Although there is no tuition at primary level, the various types of non-tuition fees that pupils must pay, following the "educational socialization" policy, add up to a substantial amount (London 2011; Tran 2014). Moreover, schools with full-day schooling option usually do so at an additional full-day fee. At least 14 types of school associated fees have been identified for primary pupils (Duong 2015).

[7] According to Government Decree 49 (2010) on tuition fee for academic year 2010/2011 to academic year 2014/2015. The exchange rate is roughly 1 USD = 20,000 VND by 01/01/2010, therefore monthly tuition fee is equivalent to 1 to 4 USD in the rural area, and 2 to 10 USD in the urban area.

[8] Since 2001, NTPs have been incorporated into Ten-Year Socio-Economic Development Strategies (SEDS) and Five-Year Socio-Economic Development Plans (SEDP) in order to effectively implement poverty reduction policy and maintain sustainable economic growth.

In fact, the HEPR is an umbrella under which various sub-programs and policies targeting specific goals kicked in over time.[9] Initially, large-scale projects such as investments in infrastructures[10] were core, but starting from 2002, specific policies targeting individuals and households living in poverty were implemented, by providing free health insurance, tuition fee exemption and possibly scholarship for school-age children, vocational training and technology transfer, support to business and production, redistribution of land to farming households or relocation of willing households to new economic zones, as well as access to concessional loans at the Vietnam Bank for Social Policies at low interest rate.[11] Occasionally, poor households also receive cash or in-kind transfers, subject to the availability of central and local resources, the latter of which can also be sold for money.

Preschool to upper secondary pupils from poor households are eligible for additional exemptions or reductions of tuition and school contribution fees, although the extent of reduction varies over time. From 1998, poor children enrolling in public schools receive 50% reduction in tuition fees; from 2005, lower secondary poor pupils get 50% reduction in school contribution fees; from 2010 poor children are fully exempted from tuition fees and school contribution fees, together with monthly subsidy of learning materials and textbooks of 70,000 VND for a maximum number of nine months per academic year. Poor students enrolled in vocational or professional secondary school, college and university are eligible for loans from Vietnam Bank for Social Policies with preferential lending rate.

Other HEPR initiatives seek to provide additional support for the most vulnerable poor groups, namely women, ethnic minorities, the disabled, the elderly, and immediate relief for people affected by natural disasters. Overall, HEPR provides poor households income transfers and further expands their purchasing power by a variety of price subsidies and fee exceptions.

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[9] 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.

[10] These projects focused on building roads, electric grid, clean water and sanitation, healthcare centers, primary and secondary schools in rural, extremely poor communes.

[11] According to National Target Programs, Five-Year Socio-Economic Development Plans 2001–2005, 2006–2010, 2011–2015, 2016–2020. Although these policies were included in the first HEPR in 1998, little was effectively put into action until after 2002. For example, regarding the policy on credit for the poor, the Vietnam Bank for Social Policies was established in 2002 (restructured and split up from the Vietnam Bank for Agriculture and Rural Development), together with Government Decree 78 (2002) on credit for the poor and other policy beneficiaries. Regarding healthcare support at household level, health insurance card and healthcare cost coverage for the poor was realized upon Decision 139 (2002).

Similar to other welfare programs in Vietnam, the HEPR strategy is designed by the central government and relevant ministries, but implemented locally by Provinces, Districts and Communes—the lowest local government level—in order to better reach the poor.[12]

Households' eligibility for HEPR depends on their poverty status. MOLISA 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 income were modified every five years as part of the government's Five-Year Socio-Economic 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.[13] 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 at communal level, using a "bottom up" approach. Each Commune (ward) includes several villages (blocks), whose 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 and submitted to higher administrative levels. The process is repeated annually.

[12] HEPR is funded by a combination of central and local financing, community and international mobilizations, and capital credit.

[13] Before 2000, hunger and poor criteria are defined as rice equivalents at local prices. From 2001 on, poverty line is determined using income criteria.

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[14] and 2,000 YC children between round 2 and round 5. Table 1 presents details on children's age by cohort throughout the study, and Table 2 summarizes general characteristics of Young Lives children and their families when the survey started.

<Table 1>

<Table 2>

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; van den Berg and Nguyen 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 based on the MOLISA criteria for Hunger Eradication & Poverty Reduction in year X", where X includes 2006 and each year

[14] 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.

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), 2013 (round 4) and 2016 (round 5).[15]

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 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 trust with the households in the sample with whom they are expected to work over 15 years, and carry out the interview without making 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.[16]

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

- Control children belong to households that have never been included in the official poor list at any time.[17]
- Treated children at round T belong to households that have entered the official poor list for the first time in round T, and were not previously present in the list.

[15] Data on assignment in the official poor lists is not available in round 1. However, this is not a major limitation. Although major 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 until 2003.

[16] 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 considered "stigmatizing" (Jones and Tran 2010).

[17] For the YC, it means at any time between 2006 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.

As Young Lives is a cohort study, all children treated, say, at round 3 are aged 8 if they belong to the YC and aged 15 if they belong to the OC. In the sequel, we alternatively refer to the round or children age at treatment for ease of interpretation (see Table 1 for round-age mapping for each cohort).

Despite that HEPR was formally launched in 1998, the first interventions explicitly targeting the poor were practically implemented only after 2002. Hence at T = 1 (2002) we can safely assume that no one benefitted from the program. For the OC, we consider two alternative treatment groups, the children treated at age 12 (T = 2) and 15 (T = 3) and for the YC we consider three treatment groups, the children treated at age 8 (T = 3), 12 (T = 4) and 15 (T = 5). Table 3 illustrates the full set of definitions of the treatment and control groups by cohort and age, as well as showing their size in the final sample. By definition, pupils in the control group need to be observed in all rounds, and those treated at round T must be continuously observed at least until then. Over the 15 years of the survey, just around 5% of the sample is lost, making attrition a minor concern for our analysis.

<Table 3>

Importantly, according to our definition the treatment status depends only on whether a household appears in the poor list for the first time at round T, regardless of later permanence or removal. As illustrated in Table 4, between 30 to 50% of pupils treated at round T remains in the list at round T + 1 (3 to 4 years later). Following this feature, our analysis compares pupils according to their first assignment status, regardless of how long they remain in the list. We justify this criterion by noticing that the event of being removed from the list is not an independent treatment, rather it reflects the success of the anti-poverty measures that a household benefitted from since its first inclusion in the list.

<Table 4>

We assign an index of economic resources to all households in the sample and all rounds, which is obtained by replicating the calculation of 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 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 5 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 5>

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 5). 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 groups 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 the poor list in 2016. 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 imperfect targeting in program implementation.

<Figure 1>

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, such as chores and 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. Mean outcomes by treatment group over time are presented in Figure A1 and Figure A2, Appendix.

4 Empirical methodology

Separately for each treatment time T and each cohort (YC, OC), we estimate the following linear model with Ordinary Least Squares (OLS):

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

where Y_{irt} is the outcome for child *i* living in region *r* at round *t*[18]. $Poor_{ir}^{T}$ is a dummy which takes 1 for children whose household first appeared in the official poverty list at round 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, $B1_{irt}$ 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 round fixed effects, respectively. Finally, ε_{irt} is an error term, which we allow to be clustered by child *i*. The $\beta_{T+\tau}$ coefficients[19] 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.

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. In our analysis, we observe children at age 5, age 8, age 12, age 15 and age 19, which correspond to preschool, primary school, lower secondary school, upper secondary school and university.

Identification of program effects in Eq. (1) relies on the common trend assumption. As shown in Figure 1 above, the imperfect targeting of the policy permits us to compare treated and control households that share a large part of the support of the B1 score, thereby strengthening the plausibility of the assumption that they would have followed similar trends had the HEPR not been implemented.

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[18] 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. In addition, given that all pupils of a given cohort are born in the same year, it is equivalent to think of *t* in terms of survey year/round or age.

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

However, imperfect targeting also raises concerns about self-selection into the program. For instance, treated and non-treated households can differ in the strength of their connection with Commune officials, which increases the probability of having access to the benefits of HEPR. The inclusion of child fixed effects protects us against selection issues related with time-invariant factors, and our pre-trends tests help to rule out selection on the basis of past shocks (Ashenfelter dips). However, there may still be a concern that households expecting that a shock will hit them in the near future can try to negotiate their inclusion in the lists with the village head. If this is the case, our estimates of treatment effects will be biased towards zero, as the treatment acts as a cushion to absorb the shock that these households would have experienced had they not been in enrolled the lists.

5 Findings

5.1 Educational achievement

Table 6 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. 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).

<Table 6>

Column (1) shows that the effect of treatment at age 8 is an immediate increase of enrolment by 9 percentage points,[20] which remains present until children are 12 years old (T + 1) and almost disappears once they reach age 15 (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 3 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

[20] 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 preschool. 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.

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; 2008; Heckman and Mosso 2014). Because human capital buildup is most efficient during the initial phase of life cycle, households who get HEPR benefits when their children are aged 8 will find it convenient to increase their investment in schooling. Instead, treated-at-12 and treated-at-15 children are deemed too old for the returns to schooling to compensate for the cost. Once children reach these ages, investment in human capital yields lower return and is therefore not undertaken.

These results are also consistent with the "luxury axiom" hypothesis. As the education of young children is a luxury good to poor households, which displays high income elasticity, the additional resources available thanks to HEPR provisions will be disproportionately spent in education (Basu and Van 1998; Edmonds and Schady 2012).

A further possible explanation is that HEPR recipient households might mis-perceive school enrollment as a condition for remaining in the poor lists, since education is a legal obligation. If so, the children of poor households would stay at school until age 14, and as soon as the legal obligation ends, they abandon. We explore this hypothesis in the next section.

5.2 Robustness checks

5.2.1 Dropout

<Table 7>

As an alternative educational outcome, we examine the effects of HEPR on school dropout. Children being enrolled in the previous round and out of school in the current period are considered dropouts. Otherwise, the outcome dummy is equal to 0 for never-enrolled children and those who are enrolling.

Regression results are in Table 7 and consistent with previous estimates on school enrolment. At age 15, treated children are more likely to leave education. The effects are significant across all cohorts and later treatments are associated with higher tendency to dropout. Since dropout is a one-off event, by definition each student can only drop out once. The estimates in Column (3) suggest that HEPR anticipates school dropout decision (many treated children leave school at age 15), but in the longer run—at age 19—the effect fades away as everyone finishes general education.

5.2.2 Ethnicity

<Table 8>

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 8 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-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.

5.2.3 Region

<Table 9>

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 9 and remain virtually unchanged.

5.2.4 Wealth

<Table 10>

Since households' assets and resources may be affected by participation in HEPR program, the resulting B1 score risks to be a bad control. In this robustness check, households' B1 score is kept fixed at the level unveiling in round 1, while still allowing its coefficient to vary by region and round. Results are presented in Table 10 and fully confirm our baseline. In most columns, the point estimates have larger magnitude than that of the baseline, implying that if anything, our estimates of treatment effects are lower bound in absolute terms.

5.2.5 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 aged between 5 and 15 and the OC is aged between 12 and 19. Table 11 shows estimates for YC children treated at age 8, age 12, age 15, and OC children treated at age 15.[21]

<Table 11>

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 (at age 19 or T + 1), when general education naturally comes to an end.

[21] 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.

5.2.6 Effects by gender

We report results obtained when we estimate Eq. (1) separately for boys and girls in Table 12.

<Table 12>

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. In Table A4, Appendix, we show support evidence that treated YC girls are more likely work once they reach age 15, regardless of the initial age at treatment (see Figure A3, Figure A4 in the Appendix for graphical illustration of heterogeneous effects by gender on enrolment and child work).

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 and improves livelihood 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 13 and Figure 4. We observe that the positive effects of treatment at age 8 on enrolment are stronger among urban children. In fact, regarding urban areas,

this 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 once they reach 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 13>

<Figure 4>

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 secondary schools. 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,[22] 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 short-term 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

[22] Teachers prefer to look for jobs in big cities, shorter instructional time and school year in remote areas (Centre for International Economics 2002).

course, participants will receive a primary vocational certificate. Compared to formal education, VTP is much shorter and supports access into the labor market.

Trainees who are in the official poverty lists[23] get exempted from course and material fees (amount up to 3 million VND or 150 USD), can borrow at low-interest rate from Bank for Social Policies under the category of "poor student" and receive allowances for transportation and accommodation during training.

We hypothesize that VTP can be an attractive option for young people and that among rural children, the effect of fee exemption for vocational training outweighs the effects of other HEPR provisions which instead support enrolment in formal education.

Unfortunately, there is no information about VTP in our data to directly test this hypothesis. However, we can provide some indirect supporting evidence by looking at the effect of HEPR on employment. Given that VTP trainees are expected to get quickly employed, we should find that the effect of HEPR on working activities is stronger in rural areas than in urban areas.

<Table 14>

<Figure 5>

Table 14 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. Figure 5 presents the estimated coefficients graphically. 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 14, treated YC children living in rural area are 8.5–18.1 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 14, Column (4)). This piece of evidence is consistent with the contemporaneous higher school enrolment of this group (see Table 13, 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 gradually phased in, the smaller and statistically insignificant effect of HEPR on work among the OC treated at age 15, together with the lack of evidence on school leaving (see Table 13,

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

Column (9)) for this treatment group, further strengthen our argument that the VTP is partially responsible for the heterogeneous treatment effects between rural and urban areas.

Since the VTP is free for any member of a poor households, the program could also 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 the HEPR on school enrolment between urban and rural areas.[24]

We conclude that the combination of positive and negative effects of HEPR on school enrolment depends on the differential salience of the HEPR in rural and urban areas and the interplay between this program and the supported access to VTP in rural 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 have 9-percentage-point higher likelihood 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.

[24] To test whether treated households disproportionally move to rural areas to benefit from free access to VTP, we look at the subsample of households that originally lived in urban areas and estimate the effect of HEPR on migration to rural areas. Results, reported in Table A5 in the Appendix, clearly suggest that there is no selective migration.

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 income effect induced by HEPR 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 provides 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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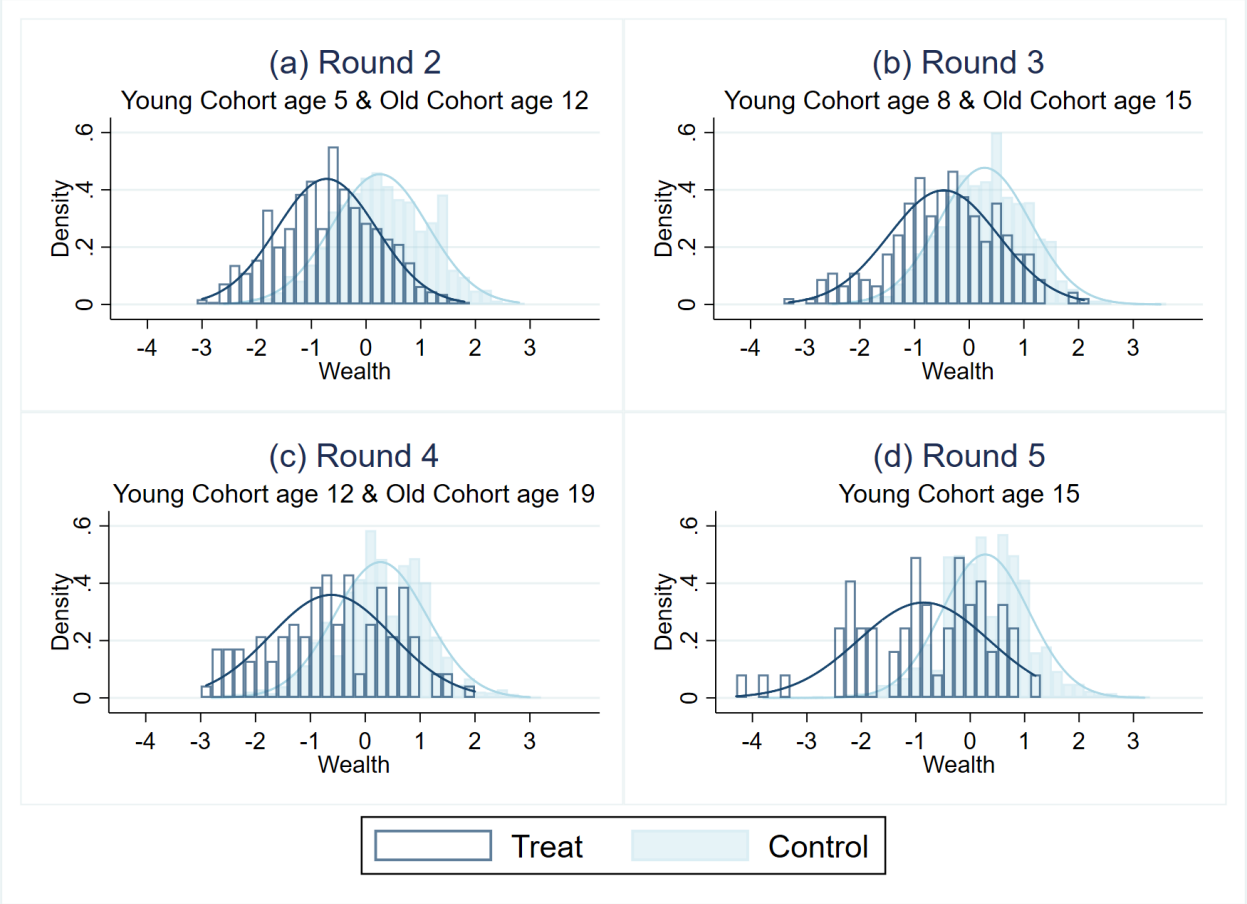
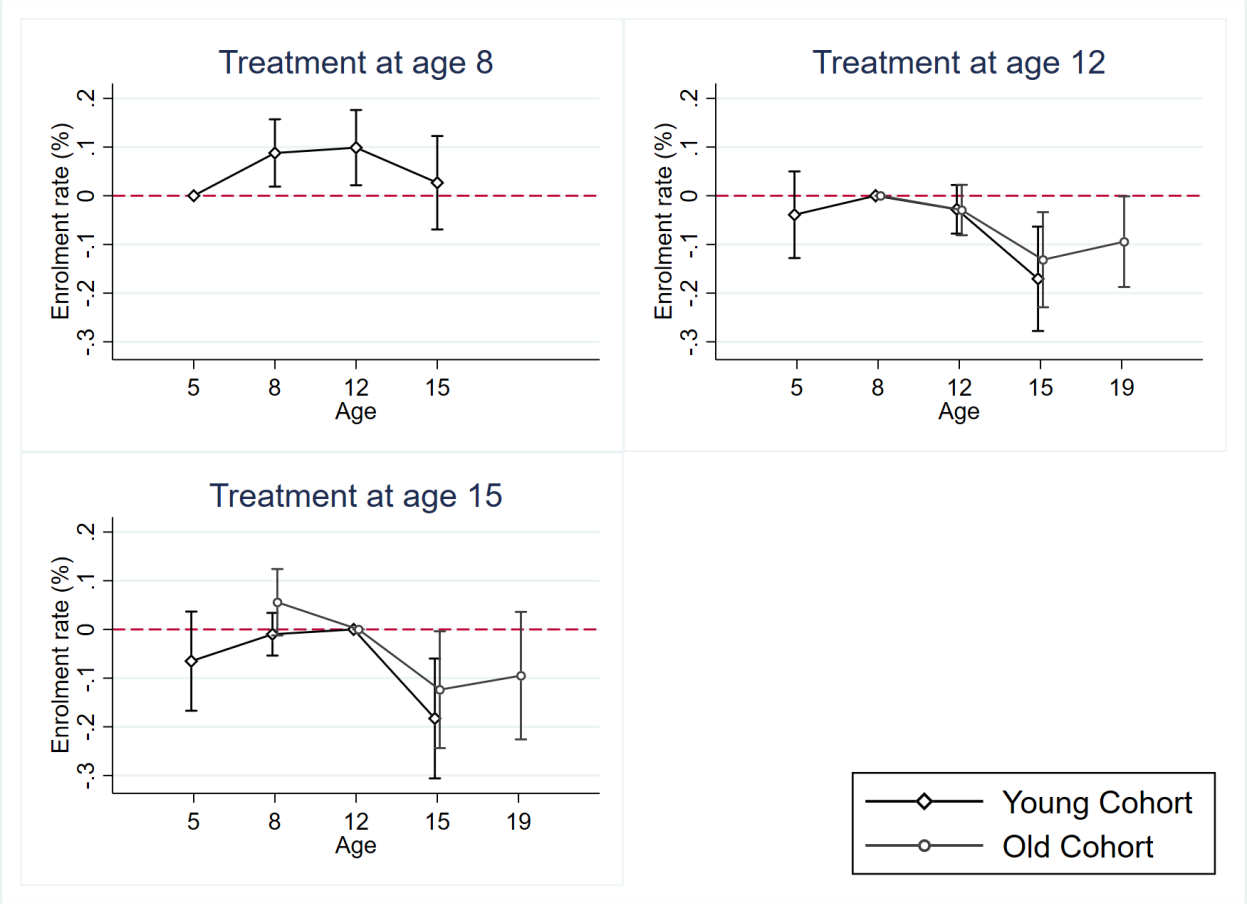
Figures and tables

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



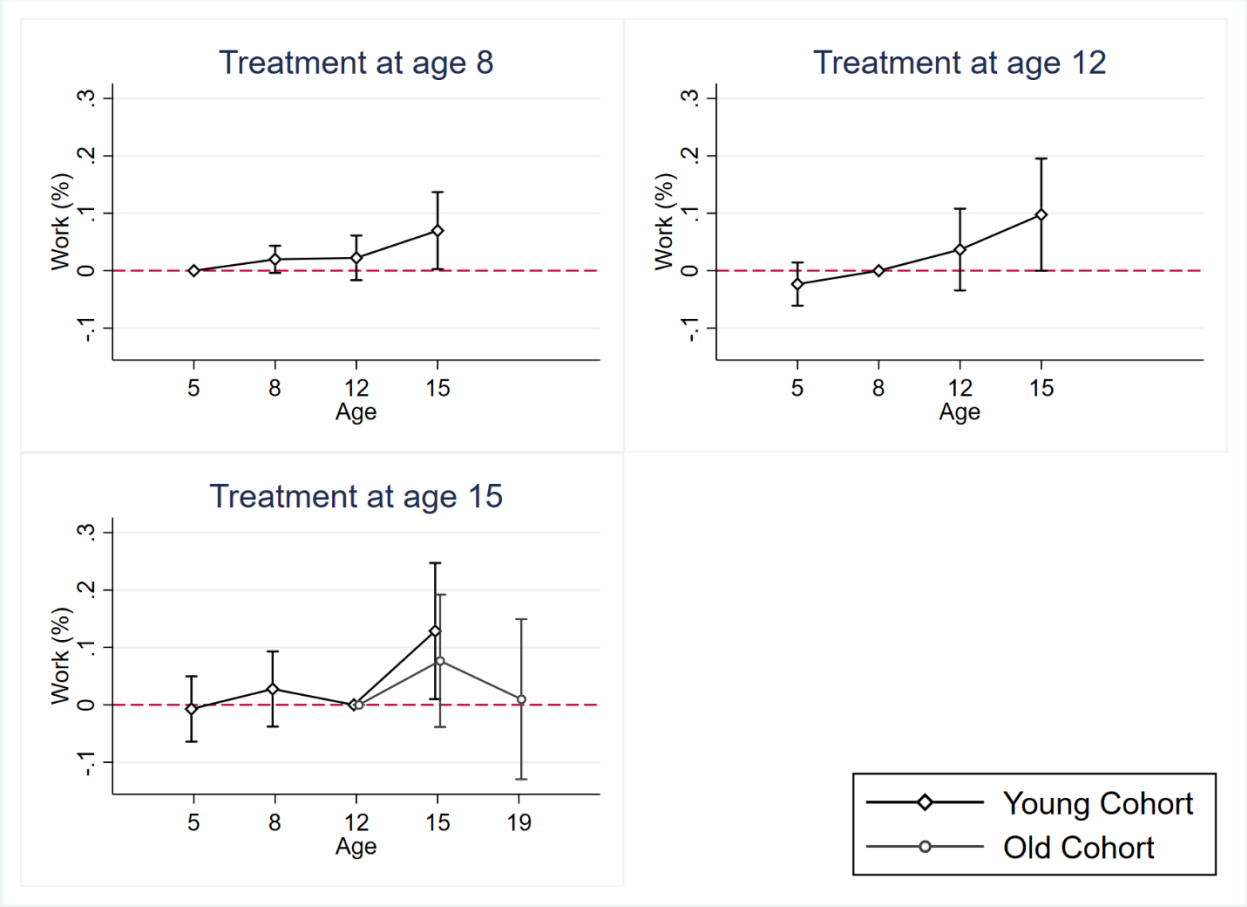
Notes: B1 score is the index measuring socioeconomic status of households (see Section 2), standardized by round.

Figure 2. Treatment effects on enrolment (with 95% C.I.)



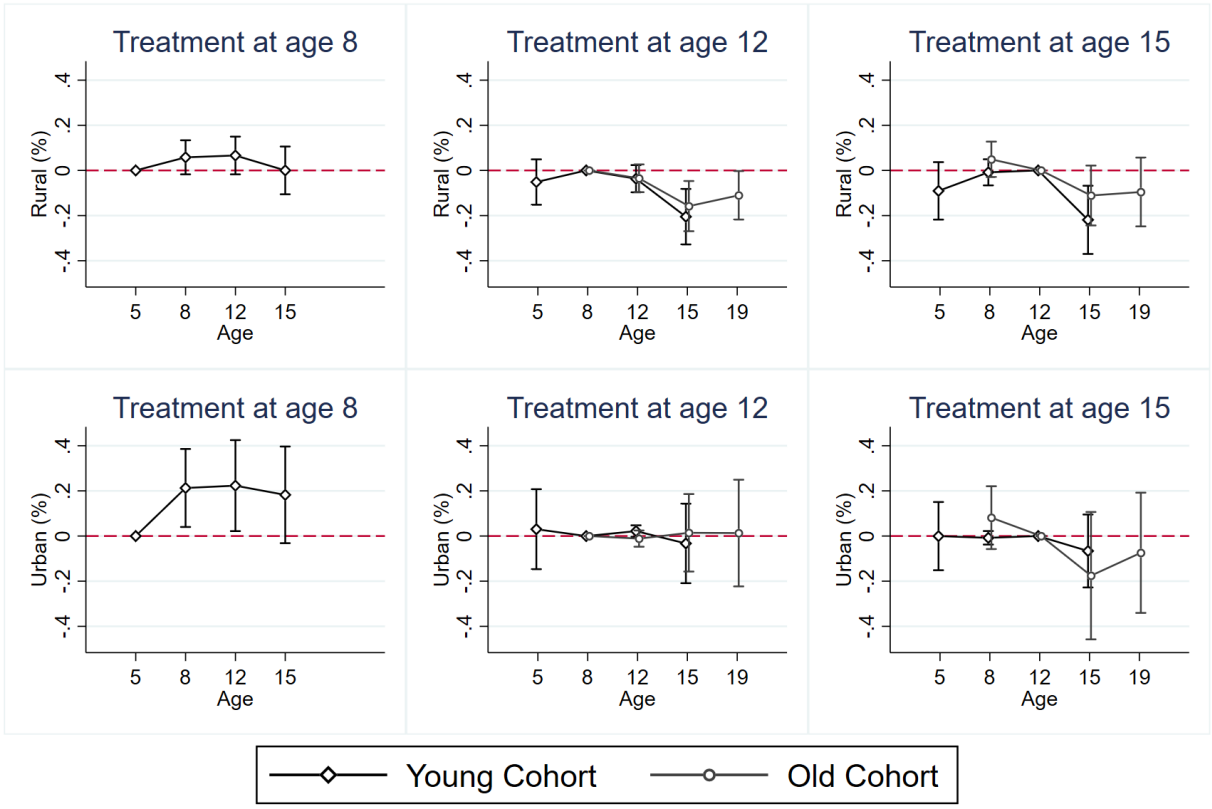
Notes: The figure illustrates differential changes over time in enrolment rate between the treated and the control group. The horizontal dash line corresponds to the trend of controlled children net of time effects as well as treatment effect at T − 1. The three panels show treatment effects for three treatment groups: children treated at age 8, children treated at age 12, and children treated at age 15 (estimated coefficients are reported in Table 6, respectively in Column (1), Columns (2) & (3), and Columns (4) & (5)).

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



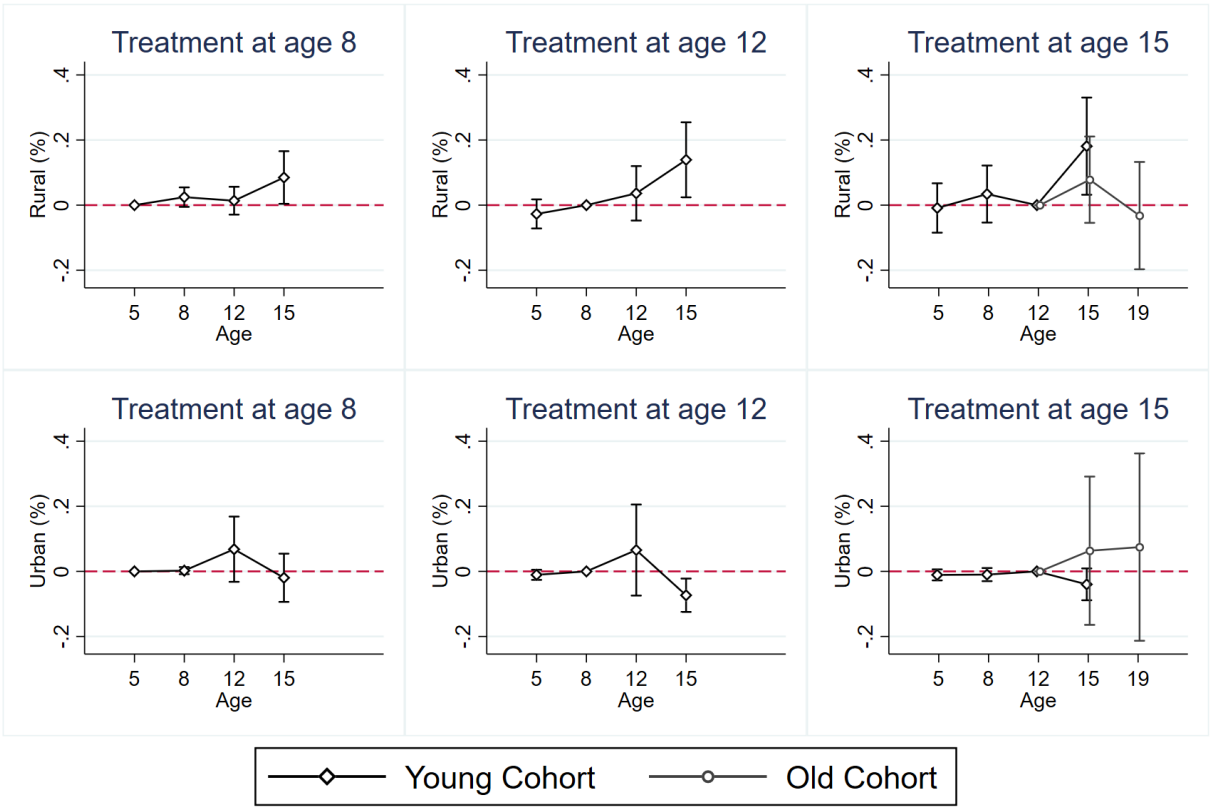
Notes: The figure illustrates differential changes over time in child work incidence between the treated and the control group. The horizontal dash line corresponds to the trend of controlled children net of time effects as well as treatment effect at T − 1. The three panels show treatment effects for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

Figure 4. Heterogeneous treatment effects on enrolment, by living area (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 upper three panels show treatment effects for children in rural areas, and the lower three panels for children in urban areas (estimated coefficients are reported in Table 13).

Figure 5. Heterogeneous treatment effects on child work, by living area (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 upper three panels show treatment effects for children in rural areas, and the lower three panels for children in urban areas (estimated coefficients are reported in Table 14).

Table 1. Children's age over the Young Lives Study's timeline by cohort

		Round 1	**Round 2**	**Round 3**	**Round 4**	**Round 5**	**Obs.**
	Year	2002	2006	2009	2013	2016	
Young Cohort	Age	1	5	8	12	15	2000
Old Cohort	Age	8	12	15	19	22	1000

Table 2. 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 3. 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	X	-	155
	Treated at age 12	O	O	X	X	-	84
	Treated at age 15	O	O	O	X	-	61
	Control	O	O	O	O	-	1,212
Old Cohort	Round			Round 2	Round 3	Round 4	
	Treated at age 12	-	-	X	X	X	174
	Treated at age 15	-	-	O	X	X	70
	Control	-	-	O	O	O	641

Notes: O is for pre-treatment (or no treated) and X for post-treatment.

Table 4. Persistence in the poor list

		Poor at age 12	Poor at age 15	Poor at age 19
Young Cohort	Treated at age 8	33.55%	29.68%	
	Treated at age 12		40.48%	
Old Cohort	Treated at age 12		50.59%	31.36%
	Treated at age 15			35.82%

Notes: Persistence in the poor list is demonstrated through share of treatment group that is still in the list in the next periods.

Table 5. 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 Upper secondary 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 6. Treatment effects on enrolment, by cohort and age at first treatment (baseline)

Dependent: Enrolment	(1) age 8 YC	(2) age 12 YC	(3) age 12 OC	(4) age 15 YC	(5) age 15 OC
T − 3				-0.065	
				(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 7. Treatment effects on dropout, by cohort and age at first treatment

Dependent: Dropout	(1) age 8 YC	(2) age 12 YC	(3) age 12 OC	(4) age 15 YC	(5) age 15 OC
T − 3				0.014	
				(0.020)	
T − 2		-0.004		0.005	-0.038
		(0.013)		(0.023)	(0.033)
T	-0.005	0.018	0.022	0.197***	0.084
	(0.007)	(0.026)	(0.024)	(0.066)	(0.075)
T + 1	0.002	0.152***	0.090*		-0.101
	(0.014)	(0.054)	(0.047)		(0.077)
T + 2	0.074**		-0.107**		
	-0.005		(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 8. 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 9. 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 10. 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				-0.067	
				(0.050)	
T − 2		-0.050		-0.008	0.054
		(0.045)		(0.018)	(0.035)
T	0.106***	-0.041	-0.029	-0.172***	-0.153**
	(0.035)	(0.026)	(0.025)	(0.060)	(0.061)
T + 1	0.102***	-0.174***	-0.167***		-0.123*
	(0.039)	(0.055)	(0.049)		(0.066)
T + 2	0.043		-0.124***		
	(0.050)		(0.047)		
Observations	5,455	5,186	3,056	5,090	2,711
Number of id	1,377	1,308	795	1,285	706

Notes: All regressions include individual fixed-effects, round dummies, interactions of B1 score at round 1 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 child work, by cohort and age at first treatment (baseline)

Dependent: 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 12. Treatment effects on enrolment, by gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent:	age 8	age 8	age 12	age 12	age 12	age 12	age 15	age 15	age 15	age 15
Enrolment	YC	YC	YC	YC	OC	OC	YC	YC	OC	OC
	girl	boy	girl	boy	girl	boy	girl	boy	girl	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 13. Treatment effects on enrolment, by living area

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent:	age 8	age 8	age 12	age 12	age 12	age 12	age 15	age 15	age 15	age 15
Enrolment	YC	YC	YC	YC	OC	OC	YC	YC	OC	OC
	rural	urban	rural	urban	rural	urban	rural	urban	rural	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 14. Treatment effects on child work, by living 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.

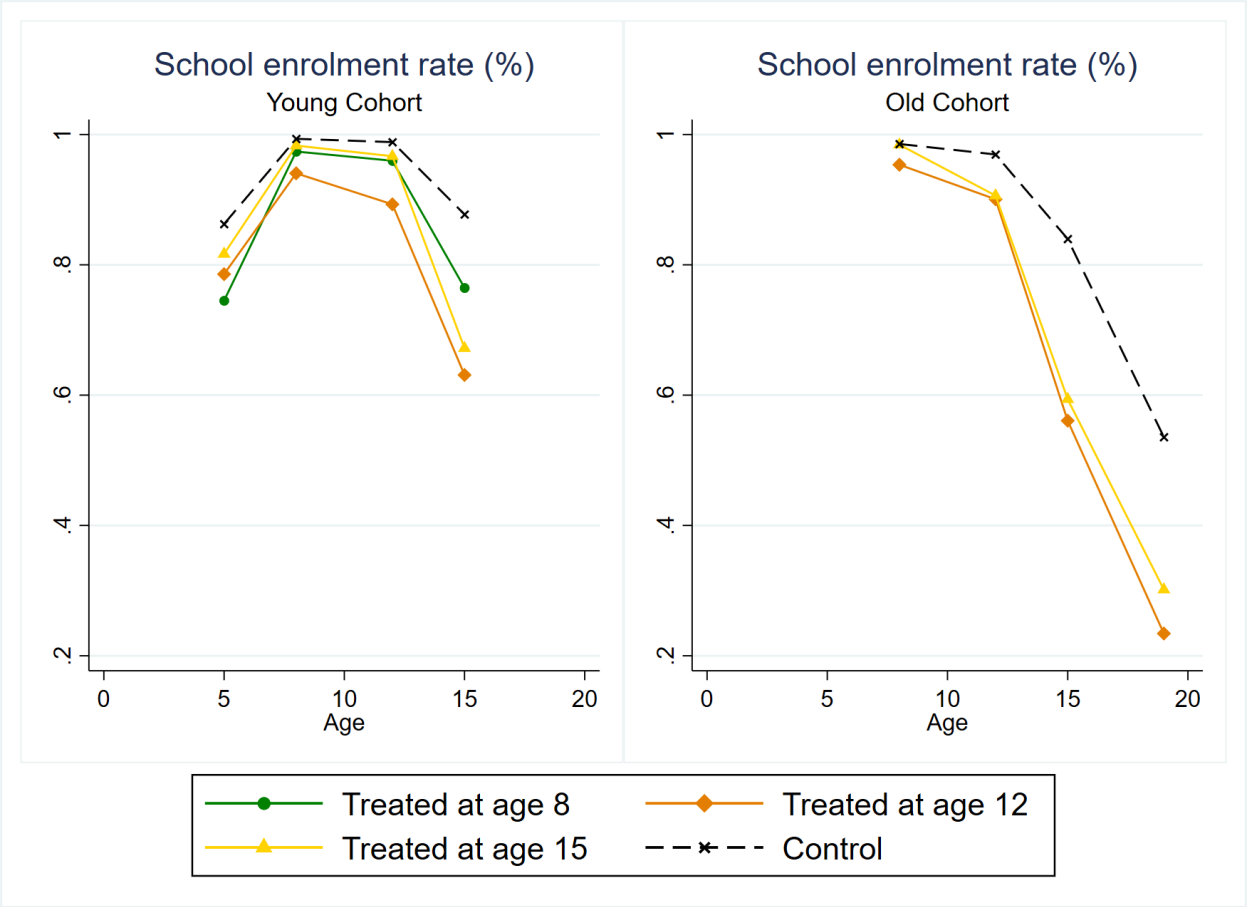
Appendix

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.

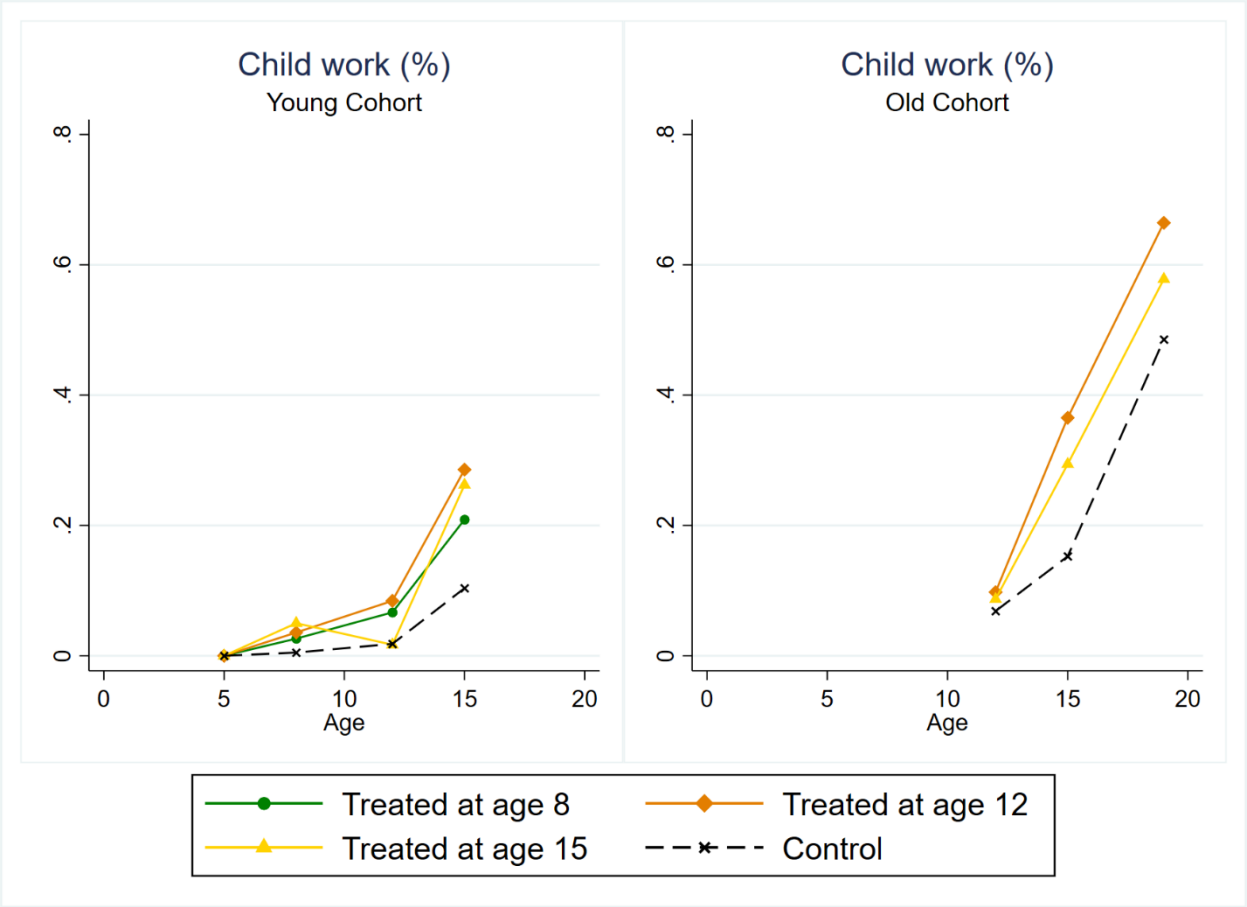
To approximate monthly electricity consumption (kWh) in each round, we divide the household's report on average monthly electricity bill (thousand VND) by the electricity price quote using information from Vietnam Electricity Group—EVN—in the same period. To identify the total area of land and water 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 4). Households will be classified as poor if they have a total score below 140 (170) point and live in a rural (urban) area.

Figure A1. School enrolment rate across treatment groups and cohort over time



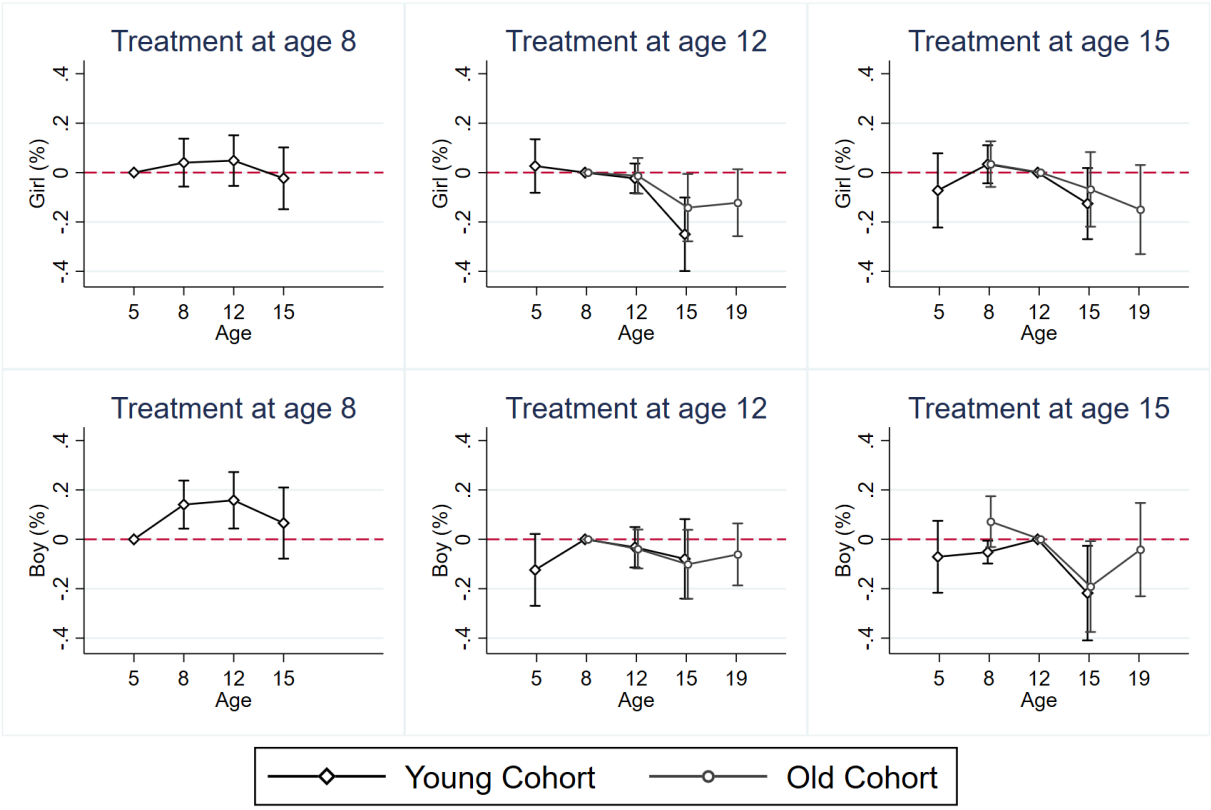
Notes: The figure illustrates mean of raw outcome in enrolment among treatment groups. Enrolment takes value 1 if the child is enrolled in current academic year by the time of interview, and 0 otherwise. Mean outcome variable of control groups is reported in Table A2.

Figure A2. Working activity across treatment groups and cohort over time



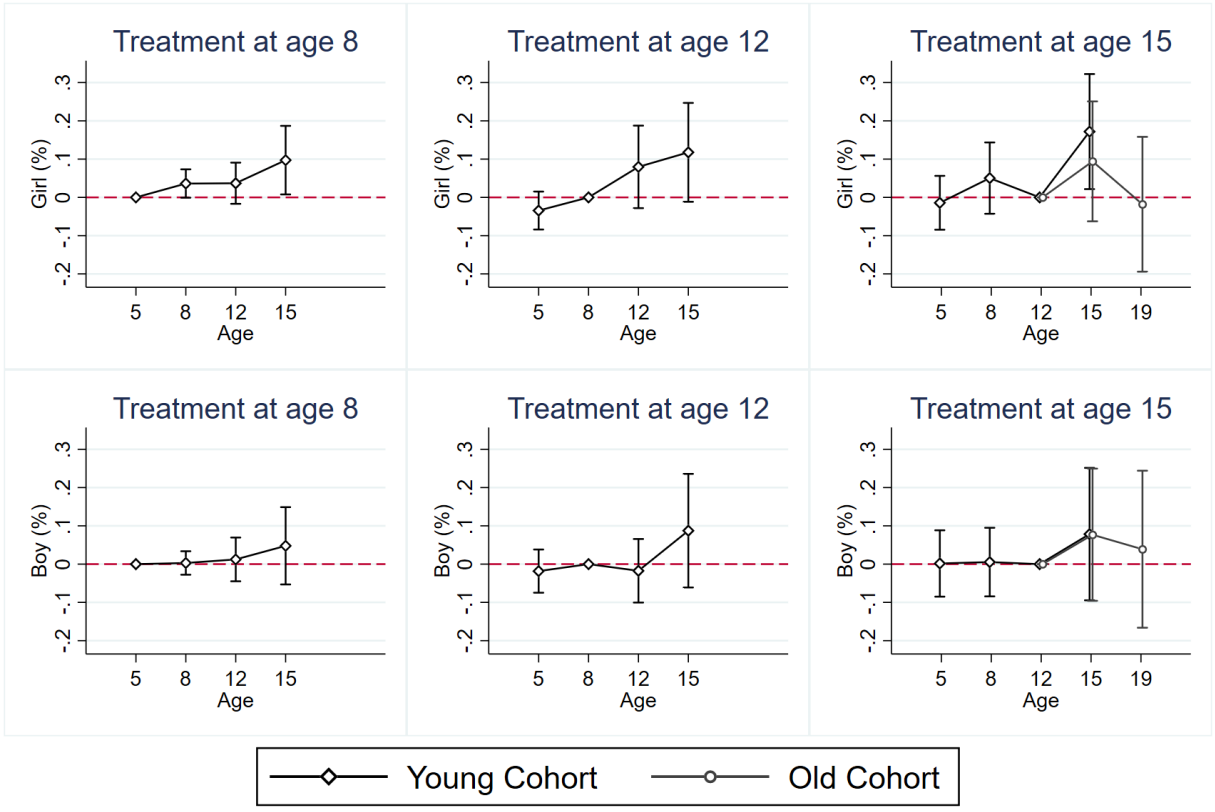
Notes: The figure illustrates mean of raw outcome in child work among treatment groups. Child works if he or she reports doing paid work outside or spending more than 4 hours daily on family tasks. Mean outcome variable of control groups is reported in Table A2.

Figure A3. Heterogeneous treatment effects on enrolment, by gender (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 upper three panels show treatment effects for girls, and the lower three panels for boys (estimated coefficients are reported in Table 11).

Figure A4. Heterogeneous treatment effects on child work, by gender (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 upper three panels show treatment effects for girls, and the lower three panels for boys (estimated coefficients are reported in Table A4).

Table A1. National poverty line definitions by MOLISA

Period	Category	Rural		Urban	Unit (per capita)
		Rural 1	Rural 2		
Before 2000	Hungry	13	13	15	kg of rice
	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: Before 2000, in order to combat hunger, hungry and poor criteria are defined as rice equivalents at local prices. 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. In practice, the nation criteria are subject to adjustments at provincial level to reflect differences in cost of living and available resources across provinces.

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.

Table A4. Treatment effects on child work, by gender

Dependent: Work	(1) age 8 YC girl	(2) age 8 YC boy	(3) age 12 YC girl	(4) age 12 YC boy	(5) age 15 YC girl	(6) age 15 YC boy	(7) age 15 OC girl	(8) age 15 OC boy
T − 3					-0.014	0.002		
					(0.036)	(0.044)		
T − 2			-0.034	-0.018	0.050	0.005		
			(0.025)	(0.029)	(0.047)	(0.046)		
T	0.036*	0.003	0.080	-0.017	0.172**	0.078	0.094	0.077
	(0.019)	(0.016)	(0.055)	(0.042)	(0.077)	(0.088)	(0.080)	(0.088)
T + 1	0.037	0.012	0.118*	0.087			-0.018	0.039
	(0.027)	(0.029)	(0.066)	(0.076)			(0.090)	(0.104)
T + 2	0.097**	0.048						
	(0.046)	(0.051)						
Observations	2,592	2,772	2,466	2,629	2,409	2,589	1,078	1,013
Number of id	670	715	636	677	623	667	366	349

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 A5. Treatment effects on migration, by cohort and age at first treatment

Dependent: Migration	(1) age 8 YC	(2) age 12 YC	(3) age 12 OC	(4) age 15 YC	(5) age 15 OC
T − 4				0.000	
				(0.001)	
T − 3		-0.002		-0.006	
		(0.002)		(0.006)	
T − 2	0.008	-0.012		0.001	0.001
	(0.008)	(0.012)		(0.001)	(0.001)
T	0.009	-0.004	-0.001	-0.000	0.000
	(0.009)	(0.004)	(0.001)	(0.000)	(0.001)
T + 1	0.007	-0.005	-0.000		0.005
	(0.007)	(0.005)	(0.001)		(0.008)
T + 2	0.006		0.009		
	(0.006)		(0.010)		
Observations	1,300	1,218	569	1,218	503
Number of id	265	248	145	248	127

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.

Chapter 2:

Paying (and paving) my way: the impacts of extra class participation on primary school pupils in Vietnam*

Abstract

Extra class, or private tutoring offered by schoolteachers to their own pupils, is a widespread phenomenon in many developing countries. This paper examines the effectiveness of extra class on pupils' learning at primary schools in Vietnam. I find that teachers grant higher school grades to pupils attending extra class, but extra class attendance does not yield higher scores at standardized achievement tests. I interpret these results as evidence of opportunistic behavior, whereby teachers exploit their arbitrariness in awarding grades, which count for secondary school admissions, to extract rents. The extent of grade inflation is higher in institutionally underdeveloped settings. Attending extra class also generates a gap in pupils' self-image. These findings provide relevant policy implications to align this informal sector with the country's education system.

Keywords: extra class, shadow education, human capital, rent extraction, Vietnam

JEL codes: D72, I24, I31, O11

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1 Introduction

Lack of transparency in public sector is a familiar topic in daily life of people in Vietnam. Paying bribes to traffic polices, tax authorities, custom officers to grease and facilitate basic tasks or services is often unavoidable (Bai et al. 2017). The press occasionally reports fraudulent settlements of revenues and expenditures in the education sector, resulting in school constructions that deviate from approved designs and fail to meet quality standards; cheating in teacher selection and recruitment process; mark manipulation of students who are relatives of educational officials (Transparency International, 2011). A widespread practice that might get prone to corruption is *extra class* tutoring, an informal educational activity in which children pay for afterschool tutoring provided by their own schoolteachers. In this arrangement, teachers can leverage on their position in formal education, which grants them the power to let pupils progress to higher grades, to extract unduly rents from their activity of private tutoring. Despite the prevalence and sizable market of this sector,[1] our understanding about its impacts on human capital is rather limited.

At primary schools, a so-called "homeroom teacher" is in charge of all major subjects (Math, Vietnamese, sciences and morality). Provision of extra class for primary-school pupils is prohibited. It is very common, however, for schoolteachers to run home-based daycare as the cover for informal private tutoring (Ha et al. 2005).[2] Bray (1999) and Stevenson & Baker (1992) coined the term "shadow education" to imply that extra class is an informal and dependent reflection of the public education system, subsequently receiving less inspection and monitoring. As each class is taught by a single teacher throughout the whole academic year, weak institutional settings and lack of teacher accountability might increase the discretionary power of the teacher. This issue can be severe in an education system where grades matter, raising two concerns. First, it leaves room for bribery if teachers blackmail parents or favor extra-class attendees. Second, the presence of extra class would interfere with the quality of public school if teachers shift their focus and effort from their teaching at school into their side job (for example, by deliberately instructing part of the official curriculum only during extra classes).

[1] Extra class makes up a substantial part of household budget (1-5% total expenditures) in Vietnam (Dang 2007).

[2] In Cambodia, extra classes can take place either right at school or at teacher's house. They are "simply a (privatized) continuation of government school classes" and perfectly imitate the school atmosphere in terms of class organization, instructional methods, frequency of groupwork or assignments, learning materials and other class characteristics (Brehm & Silova, 2014).

Using Young Lives (YL) school survey of 3,284 pupils in Grade 5 (last year of primary school), I develop an empirical approach for studying the effectiveness of extra class on pupil outcomes. I find that high-performing pupils or those from wealthier background are more likely to enroll in extra class. Consistent with the figure drawn from national representative data, around one third of pupils in the sample participates in extra class. By contrast, teachers significantly underreport their provision of extra class, suggesting that teachers are not willing to disclose information related to this informal activity.

The richness of the YL data is especially useful for the purposes of this paper. First, the data provides information on the grades awarded by teachers to pupils in Math and Vietnamese, as well as on standardized achievement tests in the same subjects carried out within the YL survey. This will allow me to distinguish between extra-class effects on teacher grades and on standardized achievement test. I shall interpret the difference in the effects in teachers' grading versus standardized tests as rent-extracting behavior by teachers.

Second, the data also provides information about pupils' non-cognitive skills—measured using standard survey instruments within the YL survey—thereby permitting me to provide evidence on the effects of extra class attendance on pupils' academic self-concept (ASC). ASC reflects how a pupil perceive their own level of academic ability, which depends not only on academic performance but also on interactions with the surrounding environment like teachers, parents or peers. ASC can be decomposed into academic self-confidence and academic effort.

Finally, the sampling frame is such that entire classes were surveyed, and class identifiers are recorded in the data. As a result, I can leverage within-class variation in extra class attendance, coupled with rich information on pupils' background, for identification of causal effects.

In the main empirical results, I find evidence that pupils attending extra class receive higher grades at school than their classmates who do not attend extra classes, conditioning on pre-determined competence in the respective subject and controls of individual characteristics and class fixed-effects. On the contrary, extra class seems not to have any real impact on the underlying skills of pupils over time. The better performance at school of extra-class attendees thereby reflects lack of transparency in teaching and grading practice. The baseline model explains for nearly half of total variation in the learning outcomes and the result is robust to a battery of sensitivity tests. To address potential omitted variable bias, I perform an Oster test to examine coefficient stability, taking into consideration the movement of R-squared when controls are added to the regression model (Oster 2019).

In addition, the heterogeneity analysis shows that the positive effect on school grade is mainly driven by extra-class pupils who come from poorer family or live in rural and remote areas. When comparing school type by schooldays duration, the degree of grade inflation is considerably stronger in schools that offer half-day program than in schools with full-day program. These are disadvantaged settings in which there are fewer monitoring mechanisms against (bad) teacher incentives. At the same time, poor households have weaker bargaining power. These results are in line with the theory on misgovernance (Banerjee 1997) which states that agency problems within a public organization are aggravated at lower levels of development and in bureaucracies that deal with poor people. Similar to an agent in a public organization, teachers create pressure on pupils and parents through discriminatory actions and made-up obstacles to increase personal gains. Moreover, since the take-up rate is higher for children in the urban areas or from wealthier families, teachers' rent seeking might turn into a source of inequality exacerbation, geographically and socioeconomically (Smyth 2009; Dawson 2010), rather than helping weak pupils to catch up with their peers (Baker et al. 2001).

Anecdotal evidence suggests that one possible strategy for teacher misbehavior is disclosure of test solutions in advance during extra class, giving extra-class pupils the advantage of insider knowledge (Transparency International, 2011). Second, if teacher behavior aligns strongly with their personal financial interest, they might be more lenient and generous when evaluating the performance of pupils attending extra class, while stricter and more intimidating to those who do not. Unfortunately, I cannot test for teacher discriminatory actions since teacher effort and attitude towards pupils are not observable. I provide indirect evidence of this hypothesis via the effect of extra class on pupils' academic self-perception. The estimated result shows the emergence of a positive gap in confidence level of extra-class attendees in comparison to the other classmates over the course of the academic year.

Using additional longer-term information for a subset of the sample, I discover that children taking extra class in Grade 5 are more likely to get into secondary school at age 15. Together with the lack of impact of extra class on true learning, these results imply that parents pay for extra class mainly over the concern of passing examination and earning qualification. To this end, extra class creates efficiency losses as the education system and the labor market rely on these distorted outcomes to evaluate

pupils, creating the so-called "achievement disease".[3] Pupils from disadvantaged background who cannot afford extra class will likely suffer more in this race.

Although there have been many concerns regarding this controversial education arrangement (see, for example, Transparency International, 2011), to my knowledge this is the first paper to empirically point out the scope for corruption in this "shadow" education market and its consequences on primary-school pupil outcomes in Vietnam. This paper contributes to the cross-theme literature on corruption and education quality,[4] in which teacher incentives are the main issue. The notion of corruption is common in developing world, with rent-extracting behavior by bureaucrats and government officers in power (Fisman, Schulz, and Vig 2014; Olken and Pande 2012; Burgess et al. 2012; Banerjee 1997). Jayachandran (2014) shows that in Nepali schools where private tutoring is offered, teachers strategically refrain from teaching during regular hours, creating negative externality on student achievements where Grade 9 students are more likely to fail the national secondary-school exam.[5]

This paper is also related to the growing literature on shadow education in developing countries (Bray and Lykins 2012; Brehm and Silova 2014). Few studies that look into the effectiveness of extra class show negligible or, at most, very modest impacts on academic achievements of students. Ha et al. (2005); Le & Baulch (2012) find that the hours spent in extra classes are not associated with higher mathematics and vocabulary test scores for 8-year-old children in Vietnam, although it does improve reading ability. Using self-reporting academic performance of Vietnamese students, Dang (2007) instead finds higher probability of falling into excellent academic performance the greater the expenditures on extra class tutoring. The results in my paper reconcile previous studies in that superior performance at school is not necessarily associated with higher level of cognitive skills. In addition,

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[3] Read more: https://vietnamnews.vn/opinion/393014/the-achievement-disease-in-education.html. Directive No. 33 of the Prime Minister in 2006 deals with combating negativities and overcoming the plague of counterfeit achievements in education. In academic year 2006/2007, the Minister of Education and Training (MOET) launched the campaign "Say no to negativities in exams and no to the disease of artificial achievements in education". Decision No. 16 of MOET in 2008 defines *teachers' ethics,* part of which requires teachers to ensure "justice in teaching and education, […] combating the disease of artificial achievements".

[4] Another branch of this literature focuses on systematic cheating (Jacob and Levitt 2003; Bertoni, Brunello, and Rocco 2013) which is also a relevant concern in the education system of Vietnam, but it is out of the scope of this paper.

[55] Absenteeism and low teacher effort, however, might not be a concern in the Vietnam context as teachers are subject to a rigorous evaluation system which relies heavily on their presence and the performance of their pupils. Therefore, teachers have strong incentives not to lower the grades of their pupils, regardless of extra-class status.

the paper investigates the effects of extra class on self-confidence of pupils—a non-cognitive skill that is usually left unexplored when examining pupil outcomes.

The remaining of the paper is as following: Section 2 describes the education system and institutional setting in Vietnam. Section 3 introduces the data, followed by empirical methodology in Section 4. Main findings and robustness checks are presented in Section 5 and Section 6. Section 7 examines longer-term outcomes. Section 8 concludes.

2 The education system and emergence of its shadow in Vietnam

As part of the country's Socio-Economic Development Strategies, Vietnam has achieved universal primary school in the 2000s and since then has been working toward universalization of lower secondary level. The Education for All action plan aims to provide every school-age child with access to affordable schooling. Massive efforts in recruiting teachers and constructing more classrooms were realized through a combination of increased public and private expenditures on education. The call for mobilization of community resources, also known as "socialization of education", reduces the burden on state budget and allows schools to collect various contributions from pupils and parents. At least 14 different types of school contributions are identified, for instance, school construction fees, voluntary contribution, medical insurance, class' fund, parental associations' fund (Duong 2015).[6] One the one hand, it is time to shift the investment focus in the education system from quantity to quality (teacher pedagogical skills, instructional time, curriculum design, school facilities, etc).[7] On the other hand, the common practice of socialization of education makes it customary for people to pay for additional education-related fees, extra class included. To this extend, extra class tutoring gradually emerges beside formal schooling without receiving much questioning or objections from the public and policymakers alike. As in other neighboring countries like China and Cambodia, the industry started to mushroom in Vietnam after the country adopted market economy in the late 1980s, which

[6] In Cambodia, the chronic underfunding issue of the education system also leaves households to pay for several informal educational fees like examination papers, bicycle-parking fee, various miscellaneous costs and fundraising ceremonies, etc. (Brehm and Silova 2014).

[7] In academic year 2010-2011, only 38.9% of primary, 23.1% of lower secondary and 12.8% of upper secondary schools meet national standards.

involves around 31% of primary, 56% of lower secondary and 77% of upper secondary school students, respectively (Dang 2007).[8]

In 2007, the Ministry of Education and Training (MOET) issues a ban on extra class for primary school pupils and those who already go to school on a full-time basis (two shifts per day), exception is for schoolteachers to run home-based daycare *at the request of the family*. A follow-up regulation in 2012 again forbids primary-school teachers from tutoring their own pupils. While these moves signal that the government is aware of this shadow system, regulating effort so far has been minimal and insignificant since they do not have enough resources to resolve the roots of the problem. It is difficult to verify if private tutoring takes place during these daycare sessions at teachers' houses, or if parents' *request* for afterschool childcare is voluntary. The predominant type of extra class at this level of education is after-school tutoring provided by pupils' homeroom teacher with a focus on the same materials in the official school curriculum in Math and Vietnamese, rather than on soft skills or rounded development (for example, English, sport, music).

Demand for extra class stems from various drivers, the first of which is cultural element with an emphasis on success and ranking. Second, deficient public-school system does not meet parental preference for education, due to low quality or limited delivering capacity with a small number of places available. Only small share of children in Vietnam receives full-day schooling, the remaining majority go to school on a half-day basis (either in the morning or the afternoon).[9] Several researches argue that institutional setting of the education system with high-stakes examination contributes to the expansion of extra class, developing and developed countries alike (Jayachandran 2014; Bray 2010).[10] As children progress with their education, competition into the next levels becomes fiercer, creating watershed pressure in public schooling system. School-leaving and/or entrance examinations to transit from one schooling level to another fuel the demand for privatization of education, although

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[8] Primary school consists of Grade 1 to 5, lower secondary school includes Grade 6 to 9, and upper secondary school is Grade 10 to 12. Children are expected to start Grade 1 at age 6 and finish general education by age 18.

[9] Read more: https://www.baodanang.vn/channel/5411/201007/de-cac-truong-tieu-hoc-day-2-buoingay-1998309/. One third of primary schools in Da Nang city (one of the country's biggest urban areas and one among the five surveyed sites in this paper) does not have enough rooms and facilities to deliver full-day schoolings by 2010. Several schools must ration full-day option: in one school with a total of 35 classes, pupils in 27 classes enjoy double-shift study, and those in the remaining 8 classes go to school on a half-day basis (either in the morning or afternoon shift). In other provinces and rural areas, full-day schooling is further limited.

[10] Brehm & Silova (2014): "the scale of private tutoring is the lowest in countries that do not have high-stakes examinations, where public satisfaction with mainstream education is reportedly high, and where teachers are paid comparatively well."

its forms and characteristics might vary across levels. Since 2005, primary-leaving examination has been abolished in Vietnam. However, GPA still plays a role in determining the reputation and quality of the institution that students can get in for secondary education. Third, extra class might be a replacement for afterschool childcare. Even in full-day setting, school usually finishes at 4 pm, while normal office hours last until 5–6 pm and some parents might even work on Saturday. In the perception of parents, extra class is a safe form of childcare in which their kids can also get to work on their homework, while allowing parents to complete their workday.[11] Fourth, extremely low salary of public-school teachers forces them to organize private classes for supplemental income. Public school teachers' salary is determined according to a common salary framework for all public servants, depending on their qualification, experience, position and geographical area. To put things in perspective, in 2011, a teacher with seven years of experience, would expect to receive a monthly salary ranging between 90 and 120 USD.[12] Data from Young Lives sample show that families spend around 3.5 USD per month for extra class per child. Extra class fees make up nearly 50% of educational expenditures and over 4% of household's total non-food expenditures (Table A1, Appendix). In a typical class of 30 pupils, the homeroom teacher would earn an additional income of roughly 105 USD, equivalent to their formal salary. It is therefore interesting to examine whether extra class provides room for extracting behavior by teachers and understand the consequences of extra class on pupil outcomes.

3 Young Lives Data

The analysis in this paper is based on Young Lives (YL) Study in Vietnam, with two datasets: a 15-year longitudinal study of 1,138 children born in 2001—the YL children[13] across five survey rounds (Round 1 in 2002; Round 2 in 2006; Round 3 in 2009; Round 4 in 2013; Round 5 in 2016), and a Primary School Survey in 2011 of 3,284 YL children and their classmates when they are in Grade 5. I primarily rely on the data from school survey to investigate the impacts of extra class on pupils in

[11] Read more: https://vietnamnet.vn/vn/giao-duc/chi-duoc-day-them-moi-tuan-3-buoi-96571.html

[12] Base salary in 2011 for public servants was approximately 40 USD (830,000 VND). At the beginning of their career, a primary school teacher with university training gets a salary coefficient of 2.34, while that for one with college or short-course professional training starts off at 2.10 or 1.86, respectively. As their number of working years accumulate, the salary coefficients slowly progress to the next grades.

[13] The original sample has 2,000 children. However, I limit the analysis to only 1,138 children who were 10 years old and attending Grade 5 during the Primary School Survey.

Grade 5. In the second part of the analysis, I make use of additional information on YL children between Round 2 to Round 5 of the longitudinal study for robustness check and exploring longer-term outcomes. Table A2 in the Appendix summarizes the timeline of data sources used in the analysis and the corresponding sample size and cohort characteristics (age, grade, school level).

The school survey is a spin-off from the longitudinal study and data collection is conducted in schools where there are YL children attending. In each Grade 5 classroom with the presence of at least one YL child, a total of 20 pupils are randomly chosen.[14] The final sample consists of 3,284 Grade 5 pupils, belonging to 176 classes in 92 school sites (both main and satellite sites) with two survey waves: wave 1 at the beginning of the academic year (Oct–Dec 2011) and wave 2 at the end of the academic year (April–May 2012). The survey instruments include data collection at the school, class and pupil level, involving interviews with the principals, homeroom teachers and pupils. In wave 1, the survey asks which pupils to fill in a questionnaire on their family background and schooling. The survey also collects class register and pupil records from homeroom teachers, which provide information on a grade (out of 10) for each child in the first 45-minute tests of the current academic year. These grades are award by teachers and available for tests in Math and Vietnamese—the two core subjects that are generally considered the most important for schooling and exams, especially at primary school level.

Figure 1 presents extra-class participation rate in the sample by subject, gender and living area (Table A3, Appendix shows details at regional level). Pupils take extra class in a specific subject if they answer "Yes" to the question "Do you attend unofficial/non-compulsorily extra classes, whether at school or not?" and spend nonzero hours attending extra class in that subject weekly. In Panel A, consistent with the figure drawn from national representative data, around one third of pupils in the sample participates in extra class. Most pupils who take extra Math also take extra Vietnamese and vice versa, a very small fraction reports attending extra class in just one subject (either Math or Viet). Panel B shows extra class participation separately for boys and girls and finds no gender difference. In Panel C, when living area is taken into consideration, extra class is much more popular in the urban areas, with around half of the urban pupils participating, double the rate in rural areas.

<Figure 1>

[14] Including all YL children in each class and a random selection of their classmates until a total number of 20 pupils is reached. In case there are fewer than 20 pupils in the class, the entire class will be interviewed. In case there are more than 20 YL children in the class, all YL pupils will be interviewed and other non-YL classmates are excluded.

While 35% of the pupils in our sample reports taking extra classes, only 11% of the interviewed homeroom teachers reports providing extra classes to supplement their income (Table A3 and Table A4, Appendix). The underreporting figure suggests that teachers are not willing to be open about this sensitive topic. Perhaps they are afraid that the information would be used against them (for instance, be flagged by local authority), and even though extra class is very common, it better remains an underground activity.

In addition, pupils are administered standardized achievement tests by YL team to assess competences in Math and Vietnamese, both at the beginning and the end of the school year. The tests are constructed in a way that allow the results to be comparable across two survey waves, with scores in wave 1 follow standard normal distributions. Any change in the test scores between wave 1 and wave 2 reflects learning increment in each subject. The final standardized scores are called Young Lives scores (YL scores). Figure 2 illustrates the distribution of YL scores for each wave, and skill gain between the two waves by subject.

<Figure 2>

Furthermore, the survey measures pupils' academic self-concept (ASC), or how pupils perceive their own ability, and their level of academic confidence and effort. Information on ASC is also available for both survey waves. Details about the content and structure of YL assessment tests, how I construct the standardized YL scores, and the ASC scale are described in the Appendix.

Table 1 shows descriptive summary of pupil characteristics by extra-class status. Additional teacher characteristics is presented in Table A4, Appendix. School grades by subject (given by homeroom teacher) are presented both in raw and standardized values. For ease of interpretation, the rest of the analysis makes use of standardized school grades. At the beginning of the school year, pupils who attend extra class appear to perform better, both at school (0.30 standard deviation(SD) higher in both subjects) and on the YL assessment tests (0.40 and 0.33 SD higher in Math and Vietnamese, respectively). They also have higher level of self-perceived confidence and effort. In wave 2, while remain positive, the gaps in YL scores are somewhat reduced (to 0.23 and 0.18 SD). This is not the case, however, for the persistent gap in confidence and effort between extra-class takers and non-takers.

<Table 1>

Selection into extra class is not different across genders. Extra-class takers have fewer siblings, but the margin is rather small and not significant. On average, ethnic minority and pupils from poorer families

are less likely to attend extra classes. Treated children are more likely to have higher educated parents, have more than 10 books at home, live in wealthier families with shorter distance to school. It is worth noticing that a large share (more than one third) of the pupils in the sample is unaware of their parental education, and the incidence is slightly more common among treated children. Pupil's wealth and teacher's wealth are standardized index based on a list of assets and properties owned at home of pupil and teacher. Classroom assets indicates the number of teaching objects and tools available in the classroom out of 12 items. Extra-class takers tend to go to schools with better facilities and larger class size. This is in line with the prevalence of extra class in urban areas or localities with generally wealthier socioeconomic condition.

For YL children, with data in round 2 to 5 of the longitudinal study, I have additional access to information on parental education and employment (up to round 4), child enrolment (up to round 5). The variable on parental education from the longitudinal survey (reported by household head) is more complete and less prone to measurement error than the self-reported answers given by pupils in the school survey. Table 2 presents descriptive statistics for the subsample of YL children.

<Table 2>

4 Empirical Strategy

This paper is interested in examining the impacts of extra class on pupil performance, with two alternative outcomes: standardized teacher grades (given by homeroom teacher) at the beginning of Grade 5, and YL score 2 at the end of Grade 5. I estimate the following model for all Grade 5 pupils:

$$y_{ij} = \alpha + \beta extra_{ij} + \delta ylscore_{ij}^{1} + \boldsymbol{X'}_i \Gamma + \gamma_j + \theta_c + \varepsilon_{ij} \quad (1)$$

where y_{ij} is academic performance in subject *j* (Math or Vietnamese) of pupil *i* from class *c*. $extra_{ij}$ is a dummy variable for pupil *i* to take extra class in subject *j*. $ylscore_{ij}^{1}$ is YL score in wave 1, proxies for pre-determined competence of pupil *i* in subject *j*. $\boldsymbol{X_i}$ is vector of individual controls, γ_j denotes subject fixed-effects and θ_c is class fixed-effects. Since each homeroom teacher is assigned to one single class during each academic year, I do not distinguish between classroom and teacher effects. The error term ε_{ij} is clustered at class level.

The main identification challenge in model (1) is the endogeneity of extra class. Parents and pupils who utilize extra class tutoring might differ from those who do not in dimensions that affect both extra-class decision and pupil academic outcomes. For example, in families that care more about

education, parents might spend more time to help their children with homework and at the same time paying for extra classes. Likewise, strongly motivated pupils choose to attend extra class even if they are already at the top of the class.[15] To account for the potential selection into extra class for children with different competences, I control for the scores in the first YL assessment test as a proxy for pre-determined competences when examine how extra class affects school grades. YL scores are measured independently by the survey assessment test and have been validated by experts to make sure that they capture well true underlying skills in Math and Vietnamese of pupils. The questions' wording, level of difficulty and competence areas closely follow the formal curriculum and textbook, making YL score a good measure of pupil underlying skills and free from the teach bias in grading. As such, the coefficient β shows deviation of teacher grade from underlying skills due to extra class participation. This deviation is the sum of i) change in real learning resulting from extra class, ii) grade inflation for pupils who attend extra class, and iii) potentially grade deflation for pupils who do not attend. Since all the variables were measured roughly at the same time in wave 1, it is unlikely that attending extra class has an immediate effect on improving real learning of pupils. Any difference between school grade and real skills must therefore reflect the "corruption" aspect of taking extra class, in other words, the sum of ii) and iii). I suspect that extra-class pupils get to know the exam questions and answers in advance (they are "taught to the test") or be graded by teachers in a more lenient manner, so they earn higher grades than other classmates who are not attending extra class. Alternatively, when the outcome variable is YL score in wave 2, β shows how attending extra class affects the gain in underlying skills over the course of one school year. Extra class participation is assumed to remain unchanged between the two survey waves.

Lastly, I estimate the impact of extra class on primary school pupils' academic self-concept:

$$ASC_i^2 = \alpha + \beta extra_i + \gamma ASC_i^1 + \delta ylscore_i^1 + \boldsymbol{X'}_i\Gamma + \theta_c + \varepsilon_i \qquad (2)$$

in which ASC is academic self-concept score. ASC at the end of the year is a function of extra-class attendance, lagged ASC (wave 1), initial YL scores in Math and Vietnamese. As before, the specification controls for demographic background, class fixed-effects, and the error term is clustered at class level. The treatment dummy, $extra_i$ is equal to 1 for pupils attending extra class in *any* subject, either Math or Vietnamese or both subjects. On the one hand, refusing to participate in extra classes

[15] Table A6 in the Appendix shows logit estimates for determinants of extra-class participation and confirms that underlying skills are positive correlated with extra class participation.

might be associated with negative teacher experience (discrimination) and higher peer pressure for non-takers, causing detrimental effects on their subjective well-being. On the other hand, the additional tutoring workload in the evening can be stressing and tiring for extra-class takers after a long school day. The degree and direction in which extra class stirs the self-image of pupils is therefore ambiguous.

5 Results

5.1 Effect of extra class on school mark

Table 3 reports OLS estimates for regression of Grade 5 pupils' teacher grade on extra class attendance, all columns control for subject fixed-effects. Point estimates are consistent across specifications, attending extra class raises pupil's grade at school by 0.135 to 0.276 standard deviation (SD), all significant at 1% level. The first column simply regresses grade on extra class. After controlling for class fixed-effects, the coefficient of interest reduces slightly from 0.276 to 0.220 in column (2), suggesting that the effect of extra class comes from within- rather than between-class variation. Column (3) adds YL score 1, column (4) further includes individual and household characteristics and parental education. Pre-determined competence is strongly correlate with teacher grade, scoring one SD above the mean in YL test increases teacher grade by more than 0.4 SD. Since both YL scores and grades are measures of pupil learning outcome, this strong and significant correlation is reassuring for my identification strategy in using YL scores as proxy for underlying skills. In column (5), I drop parental education due to large measurement errors of these variables as pupils are not fully aware of their parents' level of education (more than one third of pupils in the sample says that they do not know this information). As expected, these variables are rather uninformative and point estimates in column (5) remain virtually unchanged comparing to column (4). Column (6) includes class-teacher characteristics and school fixed effects. Lastly, column (7) includes additional controls at school-level and commune fixed effects. The results in Table 3 indicates that, controlling for YL score 1, the estimated effect of extra class on teacher grade are highly significant and relatively insensitive to the addition of controls to the regression.

<Table 3>

The specification in column (5) is the preferred model of the analysis, explaining for almost half of the variation in grade (R-squared = 0.472). Extra-class takers receive 0.136 SD higher in teacher grade than their classmates. On average, girls get higher grade than boys. Ethnic minority and pupils with

more than three siblings perform worse at school. Owning more than 10 books at home does not affect school outcome. Children spending more than 15 minutes to get to school also have lower grade in general. Longer travelling time could be due to either living far away from district center or limited transportation option, both of which might indicate lower socioeconomic background. On the contrary, children from wealthier family perform much better at school. Interestingly, the coefficient of wealth on grade is 0.134, nearly equal to the coefficient of extra class. In other word, attending extra class raises grade by an equal amount to the effect of one SD increase in family wealth. Parents with plentiful resources might choose to invest on their children in one way or another, for example by spending more quality time with their kids or helping them learning at home, allowing their kids to use computer with educational softwares, send them to English center or swimming lessons, etc. Since the grade reported is the result of the first test at the beginning of school year, the coefficient of interest shows the immediate effect of taking extra class on school grade, which is comparable to the return through investment of a wealthier family over an extended period. Therefore, the effect magnitude and the instantaneous impact of extra class on pupils' grade is rather questionable. Later in the analysis, I will perform a battery of robustness checks and show that, even without controlling for parental education, inclusion of YL score 1 and other individual and family characteristics is enough to take good care of selection into extra class. To this extent, it is reasonable to interpret the effect on school grade as a lack of transparency in this shadow teaching and learning practice. As discussed in the previous section, the impact of extra class on grade can be attributed to either grade inflation among extra-class attendees or grade deflation among non-attendees. In the rest of the analysis, I refer to this gap in teacher grade as grade inflation.

5.2 Effect of extra class on underlying skills

Following the analysis direction in the previous section, I examine whether attending extra class has any impact on learning outcome of children throughout an academic year. Table 4 reports regression results on YL score 2 or underlying skills of pupils at the end of Grade 5. In the first two columns, pupils attending extra class score significantly higher on the second YL assessment tests. However, the effect quickly disappears as soon as YL score 1 is introduced into the equation. The lagged outcome variable captures pupils' initial or pre-determined competence in material already covered in previous Grades and the beginning of Grade 5, while YL score 2 reflects knowledge related to material taught in Grade 5 curriculum. In our preferred model, column (5), children attending extra class gain 0.036 higher SD in cognitive skills in wave 2, the estimate is not significantly different from zero. The results are very similar and consistent with the baseline when I control for additional class-teacher

characteristics (column 6) and school-principal characteristics (column 7). The impact of extra class on YL score 2 is completely absent. Coupling with the previous argument, this finding provides additional evidence that better performance at school does not goes in hand with a true gain in terms of learning outcome for pupils. So far, we see that attending extra class helps children get better grade at school, but not on learning capacity. Grades at school are evaluated by teachers, therefore subject to teacher bias in grading. In addition, if test is prepared by teacher, it is possible that the test content or material covered in the test is exclusively taught during extra class, bringing insider advantage for pupils who attend these private lessons.

<Table 4>

5.3 Effect of extra class on academic self-concept

Table 5 shows regression results of equation (2) for the effect of attending extra class on pupils' ASC and its components. All columns control for individual and family characteristics and class fixed-effects, as in our preferred model. In addition, YL score 1 in both subjects and lagged outcome variables are also taken into consideration. In each regression, lagged outcome and outcome variables are obtained through the calculation method specified at the top of the table. Outcome variable in the first 4 columns is ASC, outcome in the next 2 columns is academic confidence, and in the last 2 columns is academic effort.

<Table 5>

Attending extra class has a positive effect on ASC among Grade 5 pupils. The point estimates are consistent across specifications and calculation method in columns (1) to (4). The effect size is around 0.1 SD and significant at 5% level in all specifications, aside from column (1) in which ASC score is simple linear transform of pupils' response. The potential ASC difference induced by academic competence are accounted for through the inclusion of YL score 1 in Math and Vietnamese. Nevertheless, ASC score is a *subjective* measure of how pupils *perceive* their own academic ability: two pupils who exert the same level of effort to schooling and achieve comparable educational outcomes might feel differently about their schooling experience. It is possible that pupils who are more ambitious and have a different attitude toward studying (i.e. thriving for better performance) are self-sorted into extra class. Data on outcomes are available in both the baseline and endline waves, allowing the analysis to control for initial ASC of pupils. One SD higher in initial ASC increases year-end ASC by almost 0.4 SD. The coefficients on lagged ASC in all columns are estimated with high precision and all significant at 1% level. Selection bias by self-perception is addressed when lagged ASC score

is controlled for, unless there is heterogeneity in return to ASC, i.e. extra class essentially captures the inherently larger ASC score produced among pupils with higher initial ASC. Table A7 in the Appendix tests for balancing of ASC level at the beginning of Grade 5 and shows no significant difference between the two group of pupils. The impact of extra class on ASC could act as a channel through which teachers might increase their rent extract by enlarging the cost for pupils on mental-health dimension. Parents send their children to (and pay for) extra class not only for the sake of better test results but also to avoid harassments from teachers in the everyday classroom environment.

When ASC score is further decomposed into confidence (columns (4) and (5)) and effort (columns (6) and (7)), it seems that the positive effects of extra class on ASC is entirely driven by higher confidence level. The effect of extra class on pupil confidence at the end of Grade 5 is 0.1 SD, and significant at 10% level. Since the initial levels of ASC and its components are balance across treated and control pupils, the results in Table 5 provide evidence that pupils attending extra class feel more academically confident. This could be a result of either relatively better academic performance or receipt of kinder treatment from their teacher. In a culture that honors hard working and achievement, school grade is an important indicator of success and a favorite subject of discussion. Pupils and their parents can get to be very competitive when it comes to comparing the academic outcomes with other classmates. During parent-teacher meetings, which occur at least once every semester and at year end, the homeroom teacher will publicly give comments on each pupil regarding academic performance and pupil conduct. Parents will also receive a hard copy of full academic transcripts for the whole class, in which pupils are explicated ranked and sorted in descending order based on their GPA. Children attending extra class are rewarded with higher grades at school, which in turn boosts their self-confidence. Extra-class pupils are more likely to answer "I study hard for my tests" and less likely to state that "Most of my classmates are smarter than I am" or "I always do poorly in tests." In contrast, other pupils might feel intimidated by teacher for not paying fees to attend extra class, so they are usually "waiting for the lessons to end" as hours spent at school is not enjoyable. If teacher tend to give non-extra pupils trickier questions on domains that are not taught properly during official class, these pupils would easily "get frightened when asked a question by the teachers."[16]

Interestingly, despite how extra class status affects self-confidence, it does not change a pupil's perception about her/his learning effort (columns (6) and (7)). Even if the ASC is more of a qualitative

[16] Transparency International (2011) reports the story of pupils who are victimized by teachers for refusing to take extra class, making them anxious when go to school.

measure and interpretation of such information calls for caution, this finding is suggestive evidence for discriminatory teacher behavior and draws a distinction between something that is developed in conjunction with the surrounding environment and something that reflects degree of control over one's own decision or action. Self-esteem and confidence depend on interaction with parents, peers and teachers. People *feel* more confidence if they receive recognition for accomplishment and admiration from the others. Self-perception on effort instead characterizes self-belief on own capability in the absence of external constraints,[17] emphasizing on "if I work hard, I *can*" or "I *am able to* […] if permitted."

Another interesting finding in Table 5 is how underlying cognitive skills in Math influences ASC of children to a much larger extent than does Vietnamese. One SD increase in Math skills is associated with 0.165 SD higher in confidence level and 0.083 in effort (columns (6) and (8)).The same improvement in Vietnamese increases confidence and effort by 0.053 and 0.024, respectively, one third the effect size of Math score. This result somehow echoes the consensus preference on Math (and natural sciences) over Literature (and social sciences) in Vietnam.[18] At the university entrance exam, over 60% of students choose fields that involve Math, only 10% or less would select fields that require Literature.[19]

6 Robustness check

In this section, I present a set of robustness checks to support the causal interpretation of the baseline result presented in column (5), Table 3.

6.1 Subsample YL children

In the baseline result with the full sample of Grade 5 pupils, my preferred model controls for basic family characteristics (ethnicity, number of siblings, indicators of book at home, time travel to school and assets owned) but not parental education due to the concern upon large measurement errors of

[17] This argument should hold true at least in the short run. In the long run it is possible that external environments and self-esteem have real consequences on how one behaves and perceives their level of effort.

[18] I also find this preference for Math in our school sample. Among those that take extra class, 1% is for Vietnamese only, 15% is for Math only, and the rest 84% is for both subjects. Good at Math is something to be proud of, while Vietnamese, especially at elementary level, is considered part the mother tongue, so if households know that they must send their kids to extra classes, they may opt for Math only.

[19] MOET 2008–2013. See more: https://www.thongtintuyensinh.vn/Thi-sinh-chon-khoi-thi-dai-hoc-nhu-the-nao_C212_D10039.htm

this variable. Table 6 and Table 7 report the regression results for subsample of YL children, whose precise information on parental background is obtained from the longitudinal household survey. The pattern in Table 6 is very similar to Table 4, with positive impact of extra class on teacher grade, although standard error of most explanatory variables in Table 6 goes up by more than twice as it is Table 4. Take column (4), for example, standard error increases from 0.032 to 0.084 for extra class, 0.015 to 0.031 for YL score 1, 0.065 to 0.153 for ethnic minority. Restricting the regression to YL children reduces the sample size by around two third, which might explain for the less precise estimation. Column (4) shows the effect of extra class on grade after controlling for mother's and father's level of education. Attending extra class raises grade by around 0.149 SD, comparable to the respective baseline effect of 0.135 (Column (4), Table 4). Inclusion of parental education in column (5) does not meaningfully affect the estimated effect of extra class on grade and increases the model's R-squared by a negligible degree, reassuring against the endogeneity of extra class participation in that the observed grade inflation among extra-class takers is not driven by unobservables.

<Table 6>

Table 7 shows the effect of extra class on underlying skills at the end of Grade 5 for YL children, confirming the baseline finding for the full sample that attending extra class does not help improving children cognitive skills at all.

<Table 7>

6.2 Bounds on coefficient of interest using Oster test

The estimates in the previous sections consistently shows the positive effect of attending extra class on grade at school using model (1). As discussed in section 3, this approach may yield unreliable estimates of the coefficient on extra class if, conditioning on YL score 1, parents and pupils in the treated group are still different from the control group in unobservable dimensions. For example, parents who pay for extra class also have more patience and time to help their children with homework. Pupils with more serious attitude toward schooling both go to extra class and spend more time to revise and practice before the test. In this section, I carry out an Oster test to investigate the robustness of my result to omitted variable bias (Oster 2019). Oster's work provides extension to the theory on the link between omitted variable bias and coefficient stability (see, for example, Altonji et al., 2005) and builds a framework to estimate the effect bounds.

The intuition of the idea is that selection on observables are informative about selection on unobservables. By jointly examining coefficient and R-squared movements upon inclusion of controls,

it is possible to diagnose the quality of the controls and recover the relationship between treatment and unobservables, subsequently develop a consistent estimator of the treatment effect. The first key input for the estimator is $\mathbf{R_{max}}$, the hypothetical R-squared in a regression of outcome on treatment and full set of controls (both observable and unobserables). Theoretically, if the outcome variance can be fully explained, the R-squared of such regression will be 1. Empirically, especially in economic research, this is unlikely due to idiosyncratic variation in the outcome and measurement errors. The second key input is $\boldsymbol{\delta}$, the ratio between degree of variation accounted by unobservables and degree of variation explained by observables. A value of $\delta > 1$ means the unobserved controls are more important than their observable counterparts in explaining the treatment effect. When $\delta \in [0, 1]$, the true treatment effect is bounded between values of the β in my preferred specification ($\beta_{controlled}$) and the β estimated from the hypothesis model with R_{max} and δ ($\beta_{bias\text{-}adjusted}$). Following Oster's recommendation, I set $\delta = 1$ and $R_{max} = 0.614$, or 1.3 times the R-squared in the preferred model. The bias-corrected estimate for the regression of teacher grade on extra class attendance is reported in Table 8. In column (6), $\beta_{bias\text{-}adjusted}$ is equal to 0.043, representing the lower bound of treatment effect. Even when selection on unobservables is assumed to be equally large to that of observables, omitted variable cannot explain away my main finding on grade inflation.

<Table 8>

6.3 Intensity of extra class

So far, I examine the effects of extra class using the dummy indicator of participation status. In this subsection, I use an alternative definition that considers extra class attendance at the intensive margin and interact extra class participation with the number of hours spent weekly in extra class. Results are reported in Table 9.

<Table 9>

In columns (1) and (3), I divide extra-class intensity into less than 2 hours per week, 3–4 hours per week and above 5 hours; in columns (2) and (4), extra-class intensity is split between below and above 4 hours per week. In all specifications, the first category is omitted. In the first two columns, similar to the baseline result, extra class participation raises pupils' grade by 0.112–0.134 SD. All interactions between extra class and intensity, however, are not significantly different from zero. This finding suggests that tutoring intensity does not matter and pupils who spend longer hours in extra class do not earn higher grade than those who spend a minimal amount of time there. Columns (3) and (4) fully confirm the baseline result and show that extra classes do not have any effect on underlying skills,

regardless of intensity. All in all, it appears that grade inflation linking to extra class is mostly at the discretion of the homeroom teacher, and the longer time spent in extra class does not translate into knowledge accumulation. Following line of argument, if extra class is purely a means for teachers to collect rents, parents might realize that it is not necessary to enroll their kids into extra class once they pay the bribe. If this is true, the estimated effect on school grade is likely downward biased as some pupils in the control group also get the favorable treatment from their teachers.

6.4 Curricular and non-curricular skills

As mentioned earlier in Section 2, the YL assessment test in Math is a combination of curricular and non-curricular items, and the YL score in Math therefore can be decomposed into curricular and non-curricular Math scores. In Table 10, I redo the analysis using this additional piece of information. If pupils' choice into extra class only concerns over performance at school, their skills in domains that do not directly affect formal achievement will hardly matter or play any important role in the decision-making process.

<Table 10>

Columns (1) to (2) show results for logit regression of extra class attendance on the two components of Math skills, individual and family characteristics with class fixed-effects.[20] As expected, only the curricular-related Math skill is found to correlate with the decision to attend extra class. The coefficients of non-curricular YL score 1 are much smaller comparing to the coefficients of curricular YL score 1 and not statistically significant. Children who are more proficient in Math domains included in school syllabus have higher propensity to pick up extra class, possibly in hope of improving their competitive advantage even further. This who-has-more-craves-for-more tendency does not apply to proficiency in Math domains unrelated to the curriculum, simply because this area of competence does not count on official records.

Columns (3) shows the impacts of extra class on Math grade at school, controlling for the same variables in the preferred model (column (5), Table 3), with YL score 1 in Math broken down into two components. This model produces the same result of the baseline, attending extra class increases Math grade by 0.144 SD, the estimate is significant at 1% level. The degree of grade inflation is

[20] Logit regressions include fixed-effects at different levels. The number of observations varies depending on the nested level, observations are dropped when there is no variation within a cluster (all or no pupils attend extra class).

therefore practically identical for both subjects.[21] Non-curricular Math skill is positively correlated with teacher grade but to a much smaller extend than does curricular skill (0.088 comparing to 0.401). Columns (4) and (5) present the regression results of extra class on curricular Math skill and non-curricular Math skill, respectively. As expected, extra class has no effect on year-end skills while lagged dependents for both types of skills are strong predictors of the outcomes. These findings point toward robustness of extra class's effect and provide additional argument for the link between extra class and preference for superficial schooling achievements.

6.5 Heterogeneity

In this section, I examine the heterogeneous effects of extra class on academic outcomes. Table 11 summaries the heterogeneous treatment effects on achievements, grouped into individual and family factors (Panel A) and school-related factors (Panel B). For the sake of brevity, I present only the estimated coefficients on extra class, separately for two outcomes on teacher grade and YL score 2.

<Table 11>

Individual and family background. The first two columns of Panel A report separate estimates boys and girls and show similar effects on grade for both genders. The point estimate is marginally higher for girls (0.144) than for boys (0.122) but they are not significantly different, suggesting no gender discrimination toward pupils. Columns (3) to (6) of Panel A shows that the effect of extra class on grade is strongly and significantly concentrated on pupils from poor families[22] or living in rural areas (0.265 and 0.187, respective). Such effect almost does not exist (0.007) among treated children from wealthier background. Similarly, extra class has very strong impact in for children attending schools located in poor communes. These are communes listed under P135 program, a national poverty alleviation program targeting ethnic minorities and extremely remote areas. While grade inflation arises in all schools regardless of the communes in which they locate (columns (7) and (8)), the degree of inflation in poor communes is fivefold of that in non-poor communes: 0.429 in comparing to 0.082. In our sample, raw Math grade has a SD of 1.9, so the effect size of 0.429 SD in standardized grade would be equivalent to an increase of nearly 1 point in real term. This sizable

[21] In Table 3, where subject fixed-effects are controlled for, there is also no difference between the two subjects (result not shown in the table).

[22] Cut off point for poor/rich family is at the median of wealth variable.

increase is large enough to move the grade description one level up on a 10-point grading scale:[23] 7 to 8 means Fair to Good, while 8 to 9 means Good to Excellent.

To examine whether there are heterogeneous effects across skill distribution, I divide pupils into low or high skill if their initial YL score is below or above the median and report the results in column (9) and (10). Point estimate for low achievers is 0.148 and for high achievers is 0.100. Although the results provide some suggestive evidence that the degree of grade inflation is larger among former group, the effect is not statistically significant between low and high achievers.[24] In addition, the effect on the latter group is likely capped since skills are highly correlated with school mark, which cannot go beyond 10. This explanation might also help justify the absence of grade inflation for children from richer family in column (4). If children from wealthier background are already at the top and getting the maximum mark, the estimate for this group is likely underestimated. Rich families might invest for their children in ways other than extra class, while low socioeconomic households are more vulnerable and have weaker bargaining power. Extra class participation therefore creates significant added value for the second group but not the first. Moreover, as mentioned before, it is possible that affluent parents, instead of paying for extra class, bribe teachers with gifts and money to protect the interest of their children.

Teacher and school characteristics. The first two columns of Panel B report the effect of extra class by teacher gender. The male : female ratio of primary school teachers in YL school sample is 1 : 3. My regression results reveal that the degree of grade inflation is almost three time higher when it comes to male teachers (0.295) than female teachers (0.092), the difference are significant at 5% level. Looking at teacher qualification, in column (3), I find extra class impact on grade is 0.194 for pupils whose teachers have low qualification (received three-year college or two-year professional training). Unlike their low-qualified colleagues, in column 4, teachers with university or higher degree tend not to favor their pupils based on extra class status. Likewise, columns (5) and (6) suggest that the effect is higher in schools with low-qualified principal (0.202) than in those with high-qualified principal (0.117). The estimated effect in column (5), however, is less precise as less than one fourth of the surveyed schools is led by a college-or-below principal, and it is not statistically different from the effect for schools with a university-or-above principal. The last four columns of Panel B report

[23] Grading is on a scale of 10 where 9–10: Excellent, 8: Good, 7: Fair, 5–6: Poor, 0–4: Failed.

[24] I test for the null hypothesis of no difference between coefficients using Chi-squared statistics following suest command in Stata.

heterogeneity of treatment effect in schools in which pupils attend lessons on a half-day or full-day basic. In half-day setting, each single classroom is usually shared between two classes, one in the morning and another in the afternoon. This arrangement is very common, especially at school sites that have inadequate learning facilities. On the contrary, full-day schooling means school day lasts longer, typically from 7 am to 4 pm, in which pupils often stay at school over lunch (semi-boarding). In columns (7) and (8), full-day option is reported at class level using the answer from homeroom teachers. In columns (9) and (10), full-day option is reported at school level using the answer from school principals. Descriptive summary of full-day option is reported in Table A8, Appendix. Using either piece of information, the effect of extra class is much stronger when no full-day schooling is available: 0.181 in column (7) and 0.248 in column (9), both significant at 1% level. The effect magnitude is three times smaller in schools that provide full-day schooling option (0.083), the difference in heterogeneous effects across schooldays durations is significant at 5% level.

Discussion. A clear pattern emerges in both Panel A and Panel B is the higher extent of grade manipulation in less developed settings, for example schools located in P135 communes and rural areas, or schools without full-day program. Lack of teacher accountability and monitoring mechanisms in such settings raises concern over teaching quality and corruption in education. The effect of extra class on school performance is also stronger for children from poor families, in which parents are more likely to have lower level of education and lack of information to evaluate teacher behaviors. These findings are in line with the theory on misgovernance (Banerjee 1997) in which the teachers play the role of the agent, and the education system is an public organization. Agency problems are intensified when dealing with poor people, in poor areas or underdeveloped institutions, which in turn are highly correlated with the cultural tolerance for corruption. In addition, teachers in these disadvantaged groups generally hold lower qualification (Figure A1, Appendix), which lays heavier financial pressure on them. Teachers graduated from university are more like to secure a position at schools with full-day option and make greater earnings thanks to the combination of higher qualification and double-shift teaching allowance. An average teacher in the YL school sample has seven years of teaching experience, if s/he has finished two-year professional training, the expected teaching salary is around 90 USD per month. Her colleague who has graduated from university with the same level of experience would receive 120 USD, net of other allowances. The reduction in financial pressure might in turn mitigate their incentives for moral hazard. The extended schooling hours also allow more learning materials to be covered at school, reducing the benefit that was previously obtained through extra class. Calculated from detailed data on household expenditures

collected by YL longitudinal study in Round 3 (when YL children are in Grade 3), mean extra-class fee per child per month is around 3.5 USD. In our school sample, the average class size is around 30. Thus, a typical teacher would expect to earn 105 USD from extra class tutoring each month and double their total earnings. If the main reason for teachers to provide extra class is due to low salary, increase teacher salary might mitigate their rent-seeking behavior. Exploiting a randomized experiment from Indonesia, De Ree et al.,(2018) show that teachers are significantly less likely to hold outside jobs or report financial stress when their salary is doubled, although the unconditionality of this policy change is not effective in improving teacher effort or productivity.

Interestingly, the effects on underlying skills at the end of the academic year are null in most specifications, confirming the robustness of the baseline result in Table 6. Exception for treated pupils in poor communes (Column (7), Panel A) and in schools without full-day option (Column (7), Panel B). In these disadvantaged settings, private tutoring might complement its deficient public counterpart and provide useful learning resources for pupils, in terms of both quantity and quality (Dang and Rogers 2008). Still, estimates are marginally significant and the effect magnitudes on skill gain (0.167 in poor communes and 0.095 in half-day schools) is very modest when compare to the degree of grade inflation (0.429 and 0.181, respective).

The lack of impact on true learning implies that parents pay for extra class mainly over the concern of passing examination and earning qualification. To this end, schooling and qualification are mere indicators on which the labor market relies, creating the so-called "achievement disease". Through drilling and exercising, pupils get to find the correct answer without truly understanding the mechanism or concept behind. Such practice might backfire pupil's patience, concentrating ability, and creative thinking, preventing them from solving seemingly new problems (for example, by simply changing the wording of questions) (Bray and Lykins 2012).

While several studies refer to "achievement disease" or "diploma disease" as a fueling source for private tutoring (Bray 2014), such disease is not exclusive from household standpoint. Teachers, school leaders and local authorities might as well have vested interest in polishing pupil achievements. Improved grade point average, low retention rate, reaching the quota for the share of pupils in Good and Excellent categories, etc. can be taken as indicators of quality teaching and decent educational service, earning agents recognition and promotion for their (apparent) contributions. Such manipulation of pupil achievements creates inefficiencies in education and for society (Bertoni, Brunello, and Rocco 2013). To this extent, the existence of extra class is just one manifestation for

the chronic, deep-rooted problem of "achievement disease" and multi-level embezzlement in the education system.

7 Longer-term outcomes

To gain further insights of the impact of extra class in the longer term, I limit the analysis to the sample of YL children and make use of information in other survey rounds from the YL longitudinal study. Two important outcomes that I want to examine are further school enrolment and parental employment.

7.1 School enrolment

Table 12 presents the effects of extra class on school enrolment at age 12 and age 15, for subsample of YL children. A 12-year-old child is expected to attend Grade 7 at lower secondary school. At age 15, students transit from lower to upper secondary school, this is also the minimum age required to enter the labor market. As mentioned earlier in Section 2, admission to upper secondary school depends on results of the entrance examination and previous academic records.

<Table 12>

Column (1) and (3) shows the OLS regression results on enrolment at age 12 and age 15, controlling for initial YL scores in both subjects, individual characteristics and class fixed-effects. In columns (2) and (4), I add parental education and Grade 5 teacher characteristics to the equation and control for commune fixed-effects. All explanatory variables are as of Grade 5 (from the school survey). Since Vietnam is working toward universal education at lower secondary level, nearly all children are in school at age 12 (the constant term is 0.975 in column (1) and 0.916 in column (2)) and extra class has no effect on enrolment at this stage. However, when children reach age 15, those who took extra class are 7.6 percentage point more likely to be enrolled in Grade 10 (column (3)). The estimate in column (4) is very similar to column (3) and significant at 5% level. Extra class participation in Grade 5 increases the probability of upper secondary enrolment by 6.8 percentage point. The estimated result on Grade 10 enrolment is consistent and robust across specifications.

7.2 Parental employment

In rounds 2, 3 and 4 of the YL longitudinal survey, I have additional information on parental employment status, in particular type of employment (unemployed, self-employed, wage-employed or regular salaried employee) and total number of months worked in the last 12 months, separately for

mother and father. These survey rounds correspond to when YL children are in Grade 3, Grade 5 and Grade 7, respectively. Table 13 presents the effects of extra class on parents' number of working months at Grade 7. All columns control for lagged outcomes of mother and father (Grade 3), type of parental employment (Grade 3), parental education, child characteristics and class fixed-effects. The first column shows the effect on total working months of both parents at Grade 7. On average, parents who sent their kid to extra class in Grade 5 work nearly 1.8 months more than those who did not, the effect is significant at 5% level. In column (3) and (3), I separate the working outcome for father and mother. Extra class in Grade 5 is associated with more than one additional month of working for father two years later. The estimate in column (3) also shows some positive effect on mother's total months of working, although to a lesser extent and imprecisely estimated. While I do not observe the extra-class status of children in the subsequent years, it is intuitive to assume that those who have already taken extra class in primary school will continue doing so during secondary education to maintain their competitive edge. The positive effect of extra class on parental employment is likely because they must work harder to afford the increasing cost of education (both public and private) at higher school level. Another possible explanation is parents might send their kids to extra class for reasons other than learning, for example for childcare and preventing them from wasting time on video games or developing risky behaviors. In our sample, half of the pupils attend school on a half-day basic, which mean that they spend just 4 hours/day in classroom (7 am to 11 am or 12 pm to 4 pm). Even in the case of full-day schooling, parents who work as regular employees might find it hard to pick up their children at 4 pm. If the opportunity cost of childcare is very high, extra class tutoring—under the cover of home-based daycare—allows parents to improve their labor market outcome.

<Table 13>

8 Conclusion

Examining the phenomenon of extra class, this empirical paper studies rent-seeking behavior by schoolteachers and impacts of extra class on Grade 5 pupils in Vietnam in short and long terms. I find evidence that pupils attending extra class receive higher grades at school by 0.136 SD. Such grade inflation, however, is not accompanied by a parallel gain on underlying cognitive skills. Extra-class attendees are found to have higher academic self-confidence in comparison to their non-extra classmates. Together, these findings can be interpreted as the proofs of teachers' moral hazard in public school system. The issue is aggravated in disadvantaged settings with lack of monitoring mechanisms, for example in rural areas, in schools that do not have capacity to offer full-day schooling

or for children from lower socioeconomic background, where the degree of grade inflation can be as high as 0.5 SD.

I also perform a battery of robustness checks to support the causal inference of the impacts of extra class on teacher grade. Using additional control of parental education and carrying out Oster test to address omitted variable bias, the results are robust and consistent with the baseline. Extended hours spending in extra class is not associated with larger degree of grade inflation, implying in that treatment effect only depends on whether or not the child pays the extra fee. Alternatively, parents may bribe the teacher in other unobservable ways, making the estimated effect of extra class on teacher grade likely downward biased. Even if extra class attendance has zero productive value on human capital, it yields substantial returns in the longer run. Households pay for extra class in exchange for better education prospects of their children, favorable treatment from schoolteachers and improved labor market outcome for parents.

These findings suggest extra class comes at the costs of both efficiency and equity in education and provide useful implications for the government and educational planner. If teachers can raise the costs for pupils and parents through channels other than schooling outcomes, centralization of national examination might reduce but not sufficiently eliminate the discretionary power of teachers. If teacher low salary is the main reason for them to offer extra class tutoring, the government could solve this issue by setting an efficient wage.

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Figures and Tables

Figure 1. Extra-class participation rate (%)

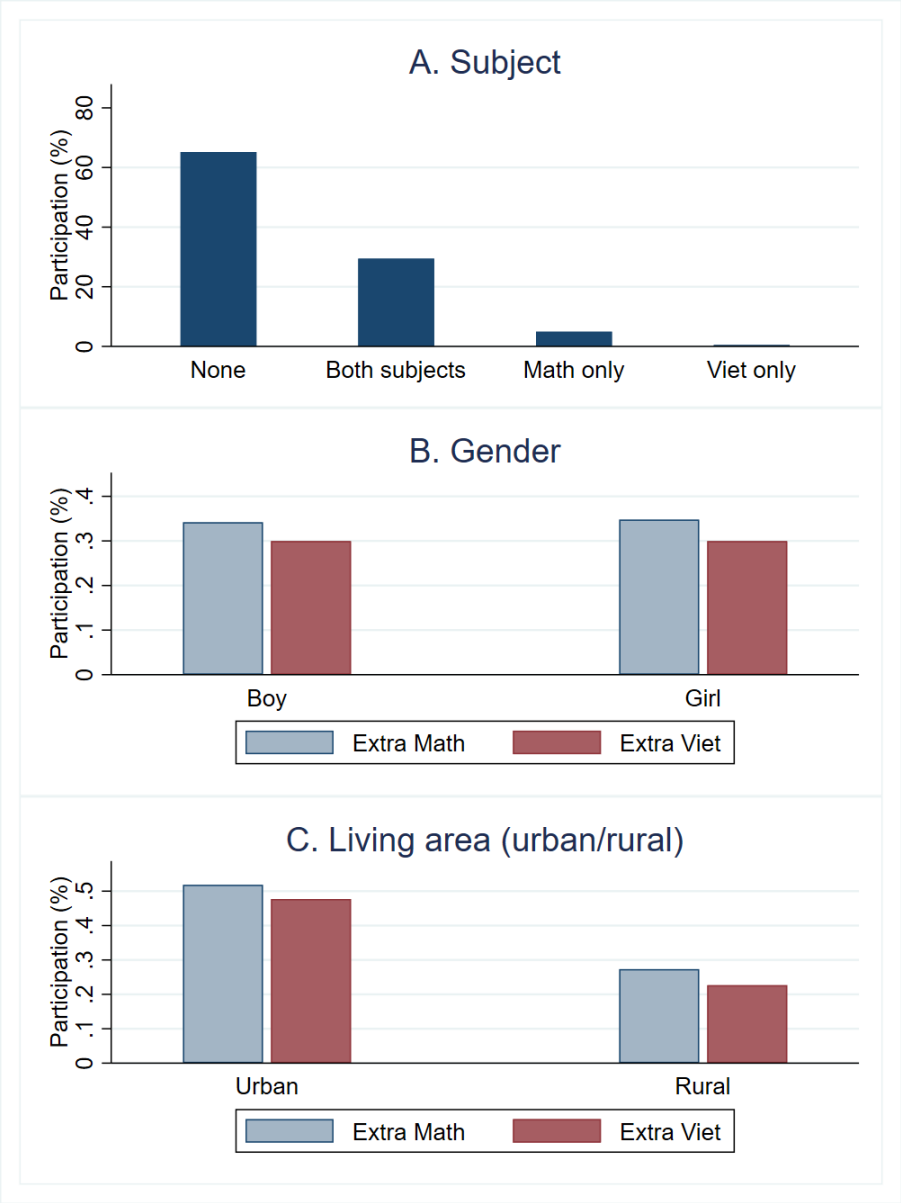

Figure 2. Distribution of YL test scores

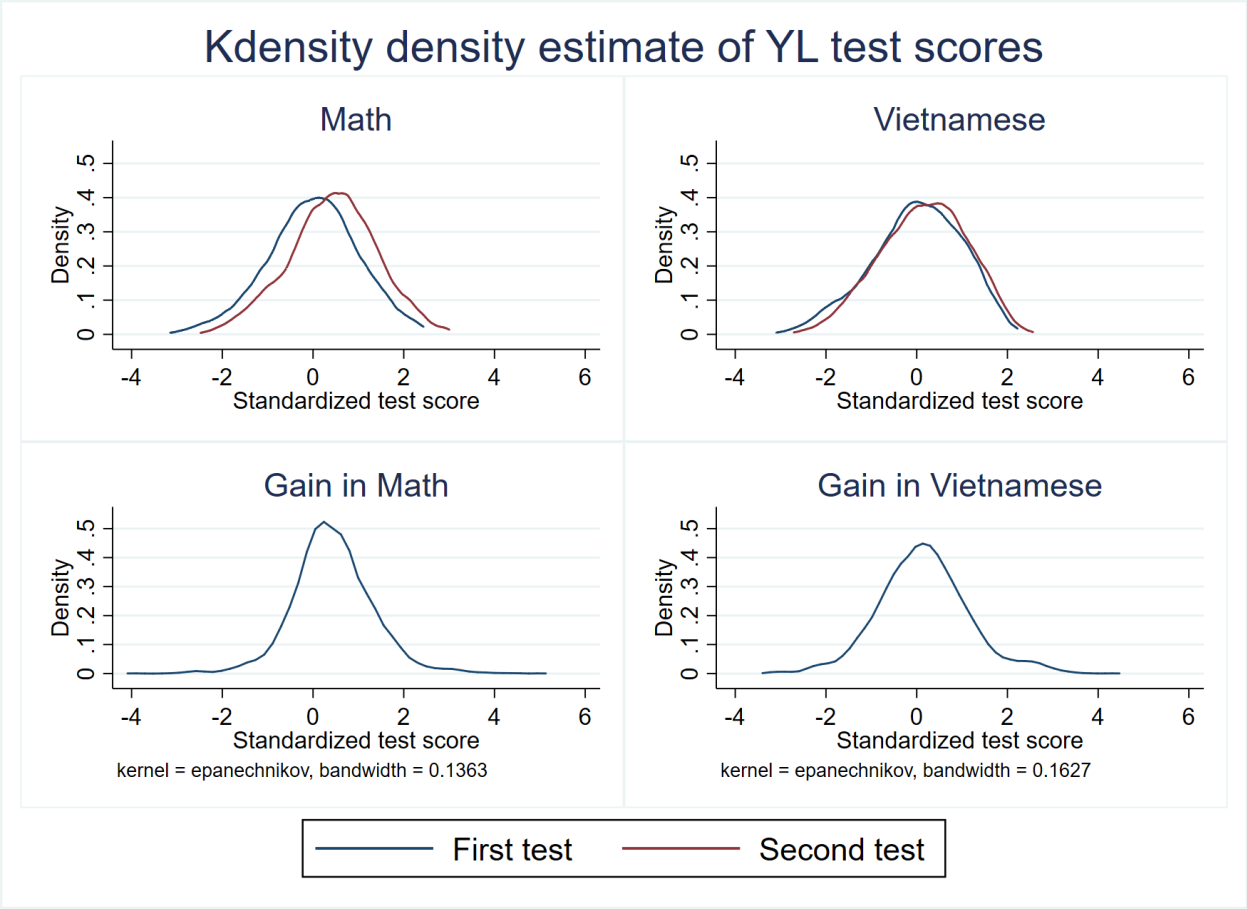

Table 1. Summary of pupil characteristics by extra-class attendance

	Extra-class (1)	None (2)	Diff. (1) – (2)
Baseline (Wave 1)			
School grade in Math (raw)	8.12	7.55	0.57***
School grade in Viet (raw)	8.11	7.62	0.49***
School grade in Math (standardized)	0.20	-0.10	0.30***
School grade in Viet (standardized)	0.20	-0.10	0.30***
Young Lives score in Math	0.26	-0.14	0.40***
Young Lives score in Viet	0.22	-0.11	0.33***
Academic self-concept score	0.10	-0.06	0.16***
Confidence score	0.11	-0.06	0.17***
Effort score	0.06	-0.03	0.09*
Girl	0.47	0.47	0.00
Ethnic minority	0.07	0.15	-0.07***
Number of siblings < 16 years old	0.93	0.99	-0.06
Mother's education			
None	0.17	0.29	-0.12***
Low	0.22	0.24	-0.02
High	0.31	0.20	0.11***
Missing	0.31	0.27	0.03*
Father's education			
None	0.13	0.22	-0.09***
Low	0.16	0.23	-0.08***
High	0.35	0.23	0.12***
Missing	0.36	0.31	0.04*
Have > 10 books at home	1.58	1.39	0.19***
Time traveling to school > 15 minutes	1.40	1.41	-0.01
Wealth	0.37	-0.19	0.57***
Classroom's assets	6.77	6.29	0.48***
Class size	30.56	28.26	2.30***
Observation	1136	2120	3256
Endline (Wave 2)			
Young Lives score in Math	0.59	0.36	0.23***
Young Lives score in Viet	0.27	0.09	0.18***
Academic self-concept score	0.09	-0.10	0.19***
Confidence score	0.13	-0.03	0.16***
Effort score	0.01	-0.14	0.15***
Observation	1119	2060	3179

Notes: ASC, Confidence and Effort scores are calculated using one-parameter logistic model.

Table 2. Summary of parental background and longer-term outcomes, subsample of YL children

Variable	Obs.	Mean	Std. Dev.	Min	Max
Explanatory variables					
Mother's education					
Primary or below	1,138	0.39	0.49	0	1
Lower secondary	1,138	0.49	0.50	0	1
Upper secondary or higher	1,138	0.12	0.32	0	1
Father's education					
Primary or below	1,138	0.32	0.46	0	1
Lower secondary	1,138	0.46	0.50	0	1
Upper secondary or higher	1,138	0.18	0.38	0	1
Mother's employment					
Unemployed	1,138	0.16	0.36	0	1
Self-employed	1,138	0.59	0.49	0	1
Wage/irregular employed	1,138	0.12	0.33	0	1
Regular salaried	1,138	0.11	0.31	0	1
Father's employment					
Unemployed	1,138	0.03	0.17	0	1
Self-employed	1,138	0.52	0.50	0	1
Wage/irregular employed	1,138	0.28	0.45	0	1
Regular salaried	1,138	0.12	0.32	0	1
Mother working months	1,104	8.23	4.48	0	12
Father working months	1,069	9.56	3.14	0	12
Outcomes					
Mother working months at Grade 7	1,067	8.86	4.60	0	12
Father working months at Grade 7	1,020	9.05	4.14	0	12
Enrolment in Grade 7	1,119	1.00	0.07	0	1
Enrolment in Grade 10	1,119	0.81	0.40	0	1

Notes: All explanatory variables are as in round 2 of the YL longitudinal data, before YL children go to primary school. The outcome variables are measured when YL children attend Grade 7 (round 4) and Grade 10 (round 5).

Table 3. Effect of attending extra class on grade at school

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: Teacher grade						
Extra	0.276***	0.220***	0.161***	0.135***	0.136***	0.141***	0.169***
	(0.072)	(0.055)	(0.044)	(0.042)	(0.041)	(0.040)	(0.041)
YL score 1			0.456***	0.401***	0.402***	0.405***	0.401***
			(0.021)	(0.020)	(0.020)	(0.020)	(0.021)
Girl				0.279***	0.280***	0.282***	0.277***
				(0.028)	(0.028)	(0.028)	(0.029)
Minority				-0.323***	-0.325***	-0.297***	-0.307***
				(0.080)	(0.078)	(0.080)	(0.079)
Siblings							
one				0.037	0.036	0.046*	0.031
				(0.026)	(0.026)	(0.027)	(0.028)
two				0.029	0.027	0.029	0.003
				(0.037)	(0.036)	(0.036)	(0.037)
more than three				-0.092*	-0.098*	-0.106**	-0.093*
				(0.051)	(0.051)	(0.050)	(0.049)
> 10 books at home				0.019	0.017	0.007	0.007
				(0.029)	(0.029)	(0.029)	(0.029)
Time to school > 15'				-0.086***	-0.088***	-0.084***	-0.080***
				(0.028)	(0.028)	(0.029)	(0.029)
Wealth				0.133***	0.134***	0.145***	0.153***
				(0.017)	(0.017)	(0.018)	(0.018)
Constant	-0.102	-0.080	-0.053	-0.361***	-0.319***	0.059	-0.084
	(0.066)	(0.053)	(0.055)	(0.088)	(0.081)	(0.284)	(0.468)
Parental education				X			
Class-teacher characteristics						X	
School-principle characteristics							X
Class FEs		X	X	X	X		
School FEs						X	
Commune FEs							X
Observations	6,504	6,504	6,485	6,309	6,343	6,213	5,949
R-squared	0.017	0.308	0.436	0.472	0.472	0.432	0.421

Notes: All regressions controlled for subject fixed-effects. Parental education includes mother and father level of education (provided by pupil). Class-teacher characteristics include teacher gender, experience, type of contract, qualification, wealth index, class assets, class size, class share of ethnic minority, full-day schooling at class level. School-principle characteristics include principal gender, province of origin, experience, qualification, award, pupil difficulty, school size, school share of ethnic minority, full-day schooling at school level. Standard errors clustered at class level in Columns (2) to (5), clustered at school level in Column (6), clustered at commune level in Column (7), *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Effect of attending extra class on skills at the end of academic year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: YL score 2						
Extra	0.204***	0.110**	0.049	0.034	0.036	0.038	0.053
	(0.076)	(0.047)	(0.036)	(0.037)	(0.036)	(0.037)	(0.039)
YL score 1			0.433***	0.420***	0.420***	0.442***	0.429***
			(0.020)	(0.020)	(0.020)	(0.022)	(0.025)
Girl				0.051**	0.051**	0.060**	0.062**
				(0.025)	(0.025)	(0.026)	(0.027)
Minority				-0.116*	-0.108*	-0.056	-0.058
				(0.060)	(0.058)	(0.057)	(0.061)
Siblings							
one				0.040	0.040	0.059**	0.068**
				(0.025)	(0.025)	(0.025)	(0.026)
two				0.030	0.033	0.005	-0.008
				(0.033)	(0.033)	(0.035)	(0.037)
more than three				-0.054	-0.050	-0.018	0.015
				(0.042)	(0.042)	(0.047)	(0.052)
> 10 books at home				0.003	0.004	-0.020	-0.003
				(0.025)	(0.024)	(0.026)	(0.029)
Time to school > 15'				-0.073***	-0.071***	-0.071***	-0.087***
				(0.022)	(0.022)	(0.024)	(0.028)
Wealth				0.052***	0.051***	0.057***	0.063***
				(0.016)	(0.016)	(0.018)	(0.019)
Constant	0.638***	0.675***	0.700***	0.766***	0.718***	0.960	-0.418
	(0.079)	(0.059)	(0.056)	(0.090)	(0.084)	(0.682)	(0.719)
Parental education				X			
Class-teacher characteristics						X	
School characteristics							X
Class FEs				X	X		
School FEs						X	
Commune FEs							X
Observations	6,352	6,352	6,335	6,159	6,193	6,067	5,809
R-squared	0.030	0.412	0.531	0.535	0.536	0.447	0.418

Notes: All regressions controlled for subject fixed-effects. Parental education includes mother and father level of education (provided by pupil). Class-teacher characteristics include teacher gender, experience, type of contract, qualification, wealth index, class assets, class size, class share of ethnic minority, full-day schooling at class level. School-principle characteristics include principal gender, province of origin, experience, qualification, award, pupil difficulty, school size, school share of ethnic minority, full-day schooling at school level. Standard errors clustered at class level in Columns (2) to (5), clustered at school level in Column (6), clustered at commune level in Column (7), *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Effect of attending extra class on ASC

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Academic Self Concept (ASC)				Confidence		Effort	
Calculation method	sum	PCA	1 pl	2 pl	1 pl	2 pl	1 pl	2 pl
Extra	0.087	0.110**	0.106**	0.116**	0.107*	0.101*	0.056	0.071
	(0.059)	(0.053)	(0.054)	(0.054)	(0.056)	(0.055)	(0.053)	(0.058)
Lagged ASC	0.374***	0.379***	0.389***	0.391***				
	(0.028)	(0.027)	(0.022)	(0.021)				
Lagged confidence					0.337***	0.329***	0.095***	0.116***
					(0.023)	(0.025)	(0.022)	(0.026)
Lagged effort					0.055***	0.048**	0.282***	0.266***
					(0.021)	(0.023)	(0.024)	(0.033)
YL score 1 Math	0.165***	0.150***	0.151***	0.165***	0.152***	0.165***	0.080**	0.083**
	(0.029)	(0.031)	(0.030)	(0.029)	(0.030)	(0.030)	(0.031)	(0.033)
YL score 1 Viet	0.047*	0.068**	0.060**	0.054*	0.064**	0.053**	0.030	0.024
	(0.028)	(0.030)	(0.028)	(0.027)	(0.026)	(0.026)	(0.031)	(0.032)
Girl	0.071**	0.076**	0.065**	0.068**	0.071**	0.062**	0.036	0.041
	(0.032)	(0.032)	(0.033)	(0.032)	(0.030)	(0.030)	(0.039)	(0.043)
Minority	0.043	-0.022	0.037	-0.007	-0.024	-0.031	0.091	0.107
	(0.074)	(0.080)	(0.084)	(0.084)	(0.090)	(0.092)	(0.087)	(0.092)
Siblings								
one	-0.028	-0.016	-0.015	-0.024	-0.027	-0.020	0.001	0.002
	(0.039)	(0.038)	(0.039)	(0.038)	(0.040)	(0.039)	(0.040)	(0.042)
two	-0.054	-0.050	-0.055	-0.068	-0.029	-0.037	-0.065	-0.039
	(0.058)	(0.059)	(0.057)	(0.057)	(0.059)	(0.058)	(0.061)	(0.069)
more than three	-0.028	-0.009	0.008	0.003	0.033	0.033	-0.040	0.018
	(0.083)	(0.075)	(0.070)	(0.071)	(0.073)	(0.075)	(0.071)	(0.079)
> 10 books at home	0.060	0.039	0.034	0.028	-0.005	0.016	0.073	0.076
	(0.042)	(0.041)	(0.040)	(0.040)	(0.041)	(0.041)	(0.046)	(0.050)
Time to school > 15'	-0.095***	-0.085**	-0.101***	-0.101***	-0.060	-0.052	-0.103***	-0.079*
	(0.033)	(0.035)	(0.035)	(0.035)	(0.037)	(0.038)	(0.039)	(0.044)
Wealth	0.036	0.012	0.037	0.015	-0.009	-0.015	0.073**	0.060*
	(0.024)	(0.026)	(0.025)	(0.026)	(0.026)	(0.025)	(0.028)	(0.030)
Constant	-0.117	-0.096	-0.063	-0.040	-0.006	-0.035	-0.120	-0.217
	(0.104)	(0.102)	(0.103)	(0.100)	(0.097)	(0.098)	(0.119)	(0.134)
Observations	3,093	3,082	3,082	3,082	3,082	3,082	3,082	3,082
R-squared	0.311	0.325	0.332	0.339	0.312	0.298	0.250	0.235

Notes: All regressions include individual and family characteristics and class fixed-effects. Standard errors clustered at class level, robust standard errors in parenthesis *** p<0.01, ** p<0.05, * p<0.1. Calculation methods of Academic Self Concept include simple sum of raw score (sum), principle component analysis (PCA), one-parameter (1 pl) and two-parameter logistic model (2 pl). Lagged outcome variables used in each specification are calculated with the same method as for the respective outcome.

Table 6. Effect of attending extra class on grade at school, subsample YL children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: Teacher grade						
Extra	0.236***	0.267***	0.202**	0.149*	0.164*	0.158*	0.176**
	(0.087)	(0.100)	(0.088)	(0.084)	(0.085)	(0.081)	(0.083)
YL score 1			0.395***	0.345***	0.356***	0.382***	0.379***
			(0.034)	(0.031)	(0.032)	(0.028)	(0.026)
Girl				0.287***	0.295***	0.315***	0.311***
				(0.040)	(0.043)	(0.043)	(0.041)
Minority				-0.226	-0.286*	-0.245	-0.303**
				(0.153)	(0.160)	(0.153)	(0.137)
Siblings							
one				-0.009	-0.020	-0.020	-0.035
				(0.045)	(0.043)	(0.041)	(0.045)
two				-0.024	-0.059	-0.087	-0.109*
				(0.069)	(0.070)	(0.066)	(0.063)
more than three				-0.192*	-0.165	-0.228**	-0.207**
				(0.102)	(0.102)	(0.101)	(0.099)
> 10 books at home				-0.004	0.003	-0.006	-0.003
				(0.042)	(0.043)	(0.043)	(0.042)
Time to school > 15'				-0.004	-0.029	-0.075	-0.085
				(0.051)	(0.051)	(0.055)	(0.052)
Wealth				0.065**	0.114***	0.132***	0.138***
				(0.031)	(0.031)	(0.033)	(0.033)
Constant	-0.060	-0.073	-0.050	-0.595***	-0.371***	-0.041	1.013
	(0.081)	(0.065)	(0.069)	(0.128)	(0.125)	(0.402)	(0.723)
Parental education				X			
Class-teacher characteristics						X	
School characteristics							X
Class FEs			X	X	X		
School FEs						X	
Commune FEs							X
Observations	2,241	2,241	2,236	2,066	2,176	2,085	1,999
R-squared	0.014	0.366	0.464	0.518	0.497	0.433	0.423

Notes: All regressions controlled for subject fixed-effects. Parental education includes mother and father level of education (provided by household head). Class-teacher characteristics include teacher gender, experience, type of contract, qualification, wealth index, class assets, class size, class share of ethnic minority, full-day schooling at class level. School-principle characteristics include principal gender, province of origin, experience, qualification, award, pupil difficulty, school size, school share of ethnic minority, full-day schooling at school level. Standard errors clustered at class level in Columns () to (5), clustered at school level in Column (6), clustered at commune level in Column (7), *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Effect of attending extra class on skills at the end of academic year, YL subsample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: YL score 2						
Extra	0.177*	0.143*	0.051	0.033	0.042	0.040	0.074
	(0.103)	(0.081)	(0.062)	(0.066)	(0.065)	(0.066)	(0.067)
YL score 1			0.454***	0.439***	0.448***	0.485***	0.482***
			(0.032)	(0.032)	(0.033)	(0.033)	(0.033)
Girl				0.055	0.062	0.111**	0.118***
				(0.045)	(0.043)	(0.044)	(0.045)
Minority				-0.142	-0.164	-0.084	-0.065
				(0.127)	(0.122)	(0.104)	(0.106)
Siblings							
one				0.048	0.047	0.054	0.075
				(0.048)	(0.046)	(0.046)	(0.048)
two				0.084	0.058	-0.007	-0.038
				(0.065)	(0.063)	(0.065)	(0.064)
more than three				-0.186**	-0.160*	-0.128	-0.121
				(0.090)	(0.090)	(0.091)	(0.091)
> 10 books at home				-0.001	-0.013	-0.036	-0.002
				(0.043)	(0.042)	(0.044)	(0.046)
Time to school > 15'				-0.063*	-0.074**	-0.071*	-0.079*
				(0.037)	(0.036)	(0.039)	(0.041)
Wealth				-0.017	0.007	0.026	0.025
				(0.032)	(0.030)	(0.032)	(0.033)
Constant	0.626***	0.641***	0.669***	0.568***	0.702***	0.679	-0.941
	(0.106)	(0.072)	(0.065)	(0.146)	(0.134)	(0.702)	(0.805)
Parental background				X			
Class-teacher characteristics						X	
School characteristics							X
Class FEs		X	X	X	X		
School FEs						X	
Commune FEs							X
Observations	2,200	2,200	2,197	2,030	2,135	2,044	1,960
R-squared	0.029	0.479	0.590	0.598	0.592	0.489	0.453

Notes: All regressions controlled for subject fixed-effects. Parental education includes mother and father level of education (provided by household head). Class-teacher characteristics include teacher gender, experience, type of contract, qualification, wealth index, class assets, class size, class share of ethnic minority, full-day schooling at class level. School-principle characteristics include principal gender, province of origin, experience, qualification, award, pupil difficulty, school size, school share of ethnic minority, full-day schooling at school level. Standard errors clustered at class level in Columns () to (5), clustered at school level in Column (6), clustered at commune level in Column (7), *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Oster test for stability of coefficient (treatment variable: Extra)

(1)	(2)	(3)	(4)	(5)	(6)
R_{max}	δ	Unrelated controls	$\beta_{uncontrolled}$ $[R^2]$	$\beta_{controlled}$ $[R^2]$	$\beta_{bias-adjusted}$
0.614	1	subject, class	0.237*** [0.313]	0.136*** [0.472]	0.043

Notes: This table shows the validation results for the effect of extra-class participation on test score at school of pupils. Unrelated controls mentioned in Column (3) are included in all regressions. Full controls further add gender, minority, number of siblings, have > 10 books at home, time to school < 15 minutes, wealth index. Column (4) replicates the model in Column (2), Table 3 and shows the estimate for the same sample from Column (5). Column (5) presents the same estimate of the preferred model as in Column (5), Table 3. Column (6) shows $\beta_{bias-adjusted}$, calculated based on R_{max} in Column (1) and δ in Column (2). Standard errors clustered at class level, robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 9. Effects of attending extra class by learning intensity

Dependent variable:	(1) Teacher grade	(2) Teacher grade	(3) YL score 2	(4) YL score 2
Extra	0.134***	0.112***	0.054	0.044
	(0.043)	(0.036)	(0.038)	(0.032)
Extra × intensity 3 hours	-0.063		-0.024	
	(0.057)		(0.057)	
Extra × intensity 4 hours	0.065		0.019	
	(0.055)		(0.057)	
Extra × intensity 5–8 hours	0.019		-0.087	
	(0.060)		(0.058)	
Extra × intensity 4–8 hours		0.068		-0.023
		(0.041)		(0.041)
Constant	-0.329***	-0.330***	0.720***	0.717***
	(0.067)	(0.067)	(0.062)	(0.062)
Observations	6,328	6,328	6,178	6,178
R-squared	0.473	0.472	0.536	0.536

Notes: All regressions use the same preferred specification as in Column (5) of Table 3 and Table 4, with additional control for intensity (hours/week) of extra class. Standard errors clustered at class level, robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 10. Effects on achievements regarding curricular and noncurricular Math skills

Dependent variable	(1) Extra	(2) Extra	(3) Math grade	(4) Curricular YL score 2	(5) Non-curricular YL score 2
Extra			0.144***	0.022	-0.006
			(0.041)	(0.036)	(0.038)
Curricular YL score 1	0.171*	0.552***	0.401***	0.373***	0.183***
	(0.090)	(0.167)	(0.023)	(0.021)	(0.021)
Non-curricular YL score 1	0.066	0.213	0.088***	0.128***	0.161***
	(0.089)	(0.159)	(0.021)	(0.019)	(0.021)
Class FEs	X		X	X	X
Commune FEs		X			
Observations	2,182	800	3,167	3,092	3,092
R-squared			0.491	0.608	0.517

Notes: Columns (1) to (2) show logit estimates of determinants of extra-class participation. Columns (3) to (5) show effects of extra class on different outcomes for whole sample using preferred model. Standard errors clustered at class level, robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 11. Heterogeneous effects on achievements

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A. Individual and family background									
	Child gender		Family		Living area		Poor commune		Skill	
	boy	girl	poor	rich	rural	urban	yes	no	low	high
Dependent variable										
Teacher grade	0.122***	0.144***	0.265***	0.007	0.187***	0.061	0.429***	0.082**	0.148**	0.100*
	(0.044)	(0.043)	(0.055)	(0.058)	(0.061)	(0.049)	(0.119)	(0.039)	(0.058)	(0.055)
YL score 2	0.011	0.027	0.034	0.030	0.059	0.006	0.167*	0.005	0.016	0.025
	(0.046)	(0.046)	(0.049)	(0.048)	(0.053)	(0.045)	(0.091)	(0.040)	(0.050)	(0.043)
	Panel B. Teacher and school									
	Teacher gender		Teacher qualification		Principal qualification		Full-day schooling			
	male	female	low	high	low	high	no	yes	no	yes
Teacher grade	0.295***	0.092**	0.194***	0.057	0.202*	0.117***	0.181***	0.092	0.248***	0.083*
	(0.079)	(0.046)	(0.060)	(0.055)	(0.113)	(0.044)	(0.056)	(0.057)	(0.068)	(0.048)
YL score 2	-0.016	0.051	0.016	0.064	0.113	0.015	0.095*	-0.010	0.065	0.027
	(0.053)	(0.043)	(0.047)	(0.058)	(0.085)	(0.040)	(0.050)	(0.050)	(0.061)	(0.045)

Notes: All regressions include pupil characteristics and class fixed-effects. In Panel A, columns (9) and (10), skill is low/high if YL score 1 is below/above median. In panel B, columns (3) to (6), qualification refers to university degree and up (high) or below (low); full-day schooling is reported by homeroom teacher in columns (7) and (8), and by the principal in the last two columns. Standard errors clustered at class level, robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 12. Effect of extra class on school enrolment, YL subsample

Dependent variable	(1) Enrol Grade 7	(2) Enrol Grade 7	(3) Enrol Grade 10	(4) Enrol Grade 10
Extra (Grade 5)	0.002	0.003	0.076**	0.068**
	(0.003)	(0.002)	(0.038)	(0.033)
YL score 1 Math	0.003	0.005	0.027	0.036*
	(0.006)	(0.004)	(0.021)	(0.020)
YL score 1 Viet	0.002	0.001	0.077***	0.070***
	(0.003)	(0.002)	(0.021)	(0.018)
Girl	0.004	0.000	0.049*	0.035
	(0.005)	(0.004)	(0.025)	(0.022)
Minority	-0.027	-0.031	0.039	0.072
	(0.031)	(0.034)	(0.083)	(0.077)
Siblings				
one	0.004	0.001	-0.010	-0.011
	(0.003)	(0.002)	(0.029)	(0.027)
two	-0.004	-0.003	-0.005	0.007
	(0.009)	(0.010)	(0.042)	(0.040)
three or more	0.013	0.010	-0.099	-0.077
	(0.012)	(0.009)	(0.067)	(0.066)
> 10 books at home	0.009	0.008	-0.026	-0.024
	(0.007)	(0.007)	(0.030)	(0.027)
Time to school > 15'	0.002	-0.001	-0.034	-0.034
	(0.005)	(0.004)	(0.031)	(0.029)
Wealth	0.005	-0.000	0.038**	0.017
	(0.006)	(0.002)	(0.018)	(0.015)
Constant	0.975***	0.916***	0.808***	0.629
	(0.027)	(0.064)	(0.090)	(0.502)
Parental education		X		X
Teacher characteristics		X		X
Class FEs	X		X	
School FEs		X		X
Observations	1,068	994	1,068	994
R-squared	0.111	0.074	0.324	0.310

Notes: Parental education includes mother education, father education. Teacher characteristics includes teacher age, gender, qualification, type of contract, experience, number of weekly teaching periods, parent-teacher meeting frequency. Standard errors clustered at class level in columns (1) and (3), clustered at school level in columns (2) and (4), *** p<0.01, ** p<0.05, * p<0.1.

Table 13. Effect of attending extra class on parental employment at Grade 7, YL subsample

Dependent variable	(1)	(2)	(3)
	Number of working months in the last 12 months		
	Total	Father	Mother
Extra (Grade 5)	1.757**	1.057**	0.645
	(0.821)	(0.514)	(0.491)
Lagged mother months (Grade 3)	0.351***	0.136*	0.233***
	(0.114)	(0.072)	(0.075)
Lagged father months (Grade 3)	-0.168	-0.062	-0.127*
	(0.113)	(0.071)	(0.071)
Mother employment (Grade 3)			
Self-employed	-1.537	-1.109	-0.426
	(1.486)	(0.906)	(0.906)
Wage/irregular-employed	-1.998	-1.415	-0.565
	(1.672)	(0.915)	(1.033)
Regular salaried	-1.131	-0.638	-0.646
	(1.718)	(1.081)	(1.119)
Father employment (Grade 3)			
Self-employed	4.659	4.188***	0.733
	(2.885)	(1.343)	(1.570)
Wage/irregular-employed	4.132	3.777***	0.648
	(2.850)	(1.325)	(1.518)
Regular salaried	3.356	3.051*	0.707
	(3.223)	(1.573)	(1.761)
Mother education			
Lower secondary	-0.093	0.306	0.584
	(0.492)	(0.431)	(0.441)
Highschool or above	1.901*	-0.024	-0.253
	(1.102)	(0.635)	(0.686)
Father education			
Lower secondary	0.872	0.518	0.609
	(0.621)	(0.406)	(0.401)
Highschool or above	-0.167	0.876*	0.064
	(0.838)	(0.528)	(0.595)
Wealth	0.638	0.345	0.303
	(0.409)	(0.253)	(0.236)
Constant	10.970***	3.278*	7.635***
	(3.483)	(1.666)	(1.797)
Observations	936	951	963
R-squared	0.353	0.272	0.342

Notes: The omitted category for parental employment is unemployed. The omitted category for parental education is below primary. All specifications control for pupil gender, ethnic minority, number of siblings, having more than 10 books at home, time to school more than 15 minutes, class fixed-effects. Standard errors clustered at class level, robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Appendix

Young Lives assessment tests in Math and Vietnamese language

The survey collected data on cognitive and psycho-social competencies of pupils through assessment tests administered in both waves. On the cognitive domain, the School Survey measured children's learning achievement in mathematics and Vietnamese language. Each test consists of 30 grade-appropriate items. In the first wave, the items are predominantly related to competencies in Grade 4's curriculum or below and with similar wording to questions in textbook. The items in the Math assessment test cover geometry, basic arithmetic, algebra, and fractions, while items in the Vietnamese test focus on reading comprehension. To ensure comparability between the two tests at the beginning and the end of the year, a number of questions from the first test are replicated in the second test, allowing results to be linked across waves (15 out of 30 Math items and 11 out of 30 Vietnamese items are repeated). Aside from these *anchor items*, new items specific for Grade 5 are added in the second wave's test.

Following the methodology to generate comparable test scores of the school survey described in Young Lives technical note, I use three-parameter (3 pl) Item Response Theory (IRT) Model to estimate latent trait (or ability) of each pupil, whose relationship with the probability that the pupil will answer correctly item g is given by:

$$P_g(\theta_i) = c_g + \frac{1 - c_g}{1 + exp[-1.7 \cdot a_g \cdot (1 - b_g)]}$$

Where θ_i is the skills or competences of pupil i; a_g, b_g, c_g are parameters of item g, measuring item's degree of discrimination, difficulty and pseudo-guessing, respectively. Answers are coded into True/False (1/0) and missing responses are marked as False. Pupils absent during the test will be dropped off the sample and no IRT score will be generated.

After IRT score generation for Math and Vietnamese in wave 1, fixed item parameters of *anchor items* serve as reference to calibrate the IRT scores for Math and Vietnamese in wave 2. I then standardize the estimated scores for each subject using the mean and standard error of the scores in wave 1, such that the standardized scores have a mean of 0 and a variance of 1 in wave 1 and these moments can shift in wave 2. The difference in skills across the two waves reflects progress or learning increment in respective subject (Math or Vietnamese) over the course of the academic year.

The Math learning assessment test can be further divided into curricular and non-curricular components (not applicable to the Vietnamese test) for both waves. Scores for each Math components are also calculated using the same method as above.

Academic self-concept (ASC)

As part of the YL school survey, the Academic self-concept (ASC) questionnaire includes 20 questions aiming to capture pupils' self-perception about their academic ability.[51] The questionnaire uses 4-point Linkert scale, which is later dichotomized for the sake of analysis, where 0 indicates low esteem and 1 indicates high esteem. From the raw answer, I follow four common psychometric strategies to translate qualitative data into quantitative ASC score (Table A5 in the Appendix shows the ASC questionnaire used in the survey).

In the first method, I simply sum up and standardize the score from the 20 questions for each individual pupil. The second method deploys Principal Component Analysis. Only two components are meaningful, the first of which indicates general level of self-concept and while the second suggests that ASC has two sub-elements: academic confidence and academic effort. I predict the ASC score by retaining the proportion of variation explained by the first component. The third and the fourth method using Item Response Theory, which calculates the difficulty level and level of discrimination of each question, based upon which each pupil's latent trait is measured. "1 pl" and "2 pl" refer to one- and two-parameter logistic model. In combination with the result from PCA, I calculate the score for both ASC and its sub-elements in confidence and effort. Similar to the standardized YL scores in Math and Vietnamese, ASC score in wave 2 reflects relative change to the standardized ASC score in wave 1.

[51] ASC interlinks with academic performance and is also influenced by the environment, i.e. it is formed in conjunction with peers, teachers and parents. The questionnaire is adopted from the ASC questionnaire designed by (Liu, Wang, and Parkins 2005) to assess Singaporean students' academic self-concept. Given the strong influence of cultural factor on how people conceptualize their feelings and believes and place themselves in comparison with the others, this questionnaire seems more appropriate than Western-setting scales for Vietnamese pupils. Validation of the ASC questionnaire for the school survey in Vietnam shows that it comprises of two first-order factors: academic confidence and academic effort, with 9 items each (2 items were removed due to low item loadings). Some items were also found not to load on factor different from designated in the original scale (Yorke 2013).

Figure A1. Teacher qualification by living area, commune and school type

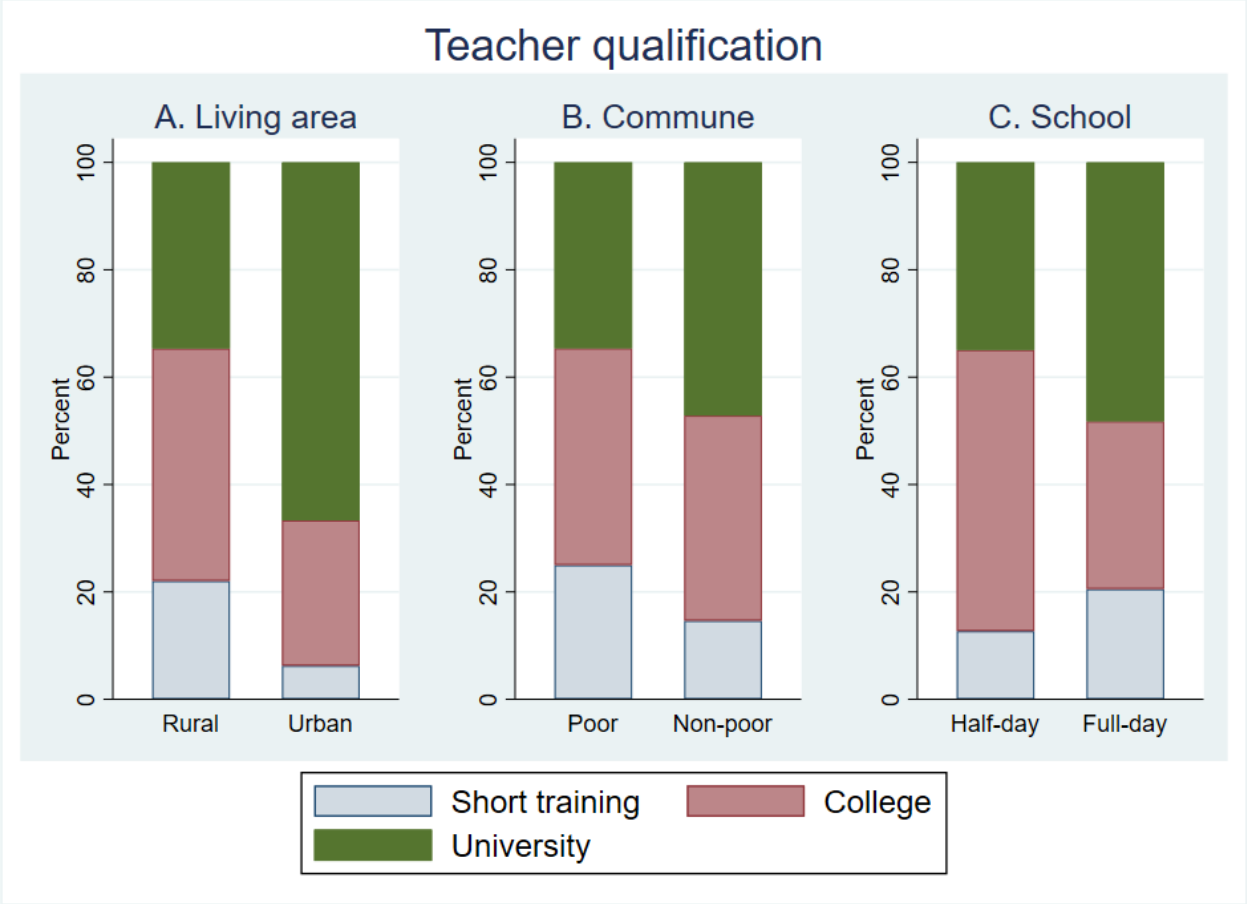

Table A1. Household expenditures (1,000 VND)

	Obs.	Mean	SD. Dev.	Min	Max
Uniform	1,959	212	235	0	2,300
School contribution	1,959	802	2,235	0	63,000
Extra class	1,959	1,087	2,366	0	30,000
of which on YL child	1,292	814	1,458	0	18,000
Book	1,945	397	353	0	3,600
Transport	1,940	33	194	0	4,000
Total educational expenditures	*1,959*	*2,528*	*3,780*	*0*	*70,900*
Housing and renting	1,959	11,786	48,851	0	700,090
Utilities, maintenance	1,959	3,258	4,468	0	75,600
Clothing	1,959	2,265	3,694	0	122,000
Leisure	1,959	398	1,653	0	40,000
Medical	1,959	2,244	7,152	0	160,050
Other (gifts, festival, bribes)	1,959	2,668	6,458	0	152,050
Total nonfood expenditures	**1,959**	**25,146**	**53,514**	**0**	**741,570**

Notes: Household expenditures in the last 12 months as in round 3 of YL longitudinal data, when YL children are in Grade 3. The amount of extra class spending on YL child is calculated by multiplying the total expenditures of extra class with the share spent on YL child (none, less than half, half, more than half or all). Exchange rate: 1,000 VND = 0.05 USD, 1 USD = 20.000 VND.

Table A2. Timeline of data sources in the study

	Round 2	**Round 3**	**School survey**	**Round 4**	**Round 5**
Year	2006	2009	2011 – 2012	2013	2016
Age	5	8	10	12	15
School level	Preschool	Primary	Primary	Lower secondary	Upper secondary
Grade	None	Grade 3	Grade 5	Grade 7	Grade 10
Observation	1,138	1,138	3,284	1,138	1,138
Sample	YL children	YL children	YL children + classmates	YL children	YL children

Table A3. Share of pupils taking extra class by region (%)

Region	Rural	Extra in either Math or Vietnamese		Total
		No	Yes	
Ben Tre	Yes	79.70	20.30	100
Da Nang	No	47.72	52.28	100
Hung Yen	Yes	66.82	33.18	100
Lao Cai	Yes	57.60	42.40	100
Phu Yen	Yes	81.69	18.31	100
Total		65.11	34.89	100

Table A4. Descriptive summary of teacher characteristics

Variable	Obs.	Mean	SD. Dev.	Min	Max
Age	176	39.23	7.55	23	57
Female	177	0.72	0.45	0	1
Ethnicity					
Minority	177	0.02	0.13	0	1
Kinh	177	0.98	0.13	0	1
From the same province	176	0.69	0.46	0	1
Experience (years)	175	17.47	8.12	0	36
Experience in Grade 5 (years)	176	9.43	7.76	0	33
Qualification					
Short training (2 years)	177	0.18	0.38	0	1
College (3 years)	177	0.38	0.49	0	1
University or higher (4 years)	177	0.43	0.50	0	1
Excellent teacher award					
Never	177	0.06	0.23	0	1
At school level	177	0.28	0.45	0	1
At district level	177	0.50	0.50	0	1
At province level	177	0.16	0.37	0	1
Type of contract					
Permanent	177	0.98	0.15	0	1
Temporary	177	0.01	0.11	0	1
Times being evaluated in last academic year	176	7.34	5.74	0	30
Do second job for supplement income	176	0.15	0.36	0	1
Provide private tuition	176	0.11	0.32	0	1
Teacher wealth index	177	-0.07	1.09	-6.27	1.82

Table A5. Academic Self-Concept Questionnaire

		Confidence	Effort
1	I can follow the lessons easily.		x
2	I daydream a lot in class	x	
3	I am able to help my classmates with their schoolwork if permitted.		x
4	I often do my homework without thinking.		
5	If I work hard, I think I can go to the college or university.		x
6	I pay attention to the teachers during lessons.		x
7	Most of my classmates are smarter than I am.	x	
8	My teachers feel that I am poor in my work.		x
9	I am usually interested in my schoolwork	x	
10	I often forget what I have learnt.		x
11	I study hard for my tests.	x	
12	I am willing to do my best to pass all the subjects.		x
13	I get frightened when I am asked a question by the teachers.	x	
14	I often feel like quitting school.	x	
15	I am good in most of my school subjects.		x
16	I am always waiting for the lessons to end.	x	
17	I always do poorly in tests.	x	
18	I do not give up easily when I am faced with a difficult question in my school work		
19	I am able to do better than my friends in most subjects.		x
20	I am not willing to put in more effort in my schoolwork.	x	

Notes: Questions 4 and 18 are removed in the analysis due to low loading on both factors (Yorke, 2013).

Table A6. Logit estimates for determinants of extra-class participation

	(1)	(2)	(3)
Dependent variable		Extra	
YL score 1	0.166***	0.134***	0.153***
	(0.059)	(0.050)	(0.048)
Girl	0.055	0.050	0.031
	(0.105)	(0.094)	(0.091)
Minority	-0.583	-1.758***	-1.880***
	(0.444)	(0.437)	(0.377)
Siblings			
one	-0.071	-0.023	-0.022
	(0.121)	(0.109)	(0.106)
two	-0.170	-0.156	-0.153
	(0.174)	(0.153)	(0.149)
more than three	0.058	-0.067	-0.213
	(0.217)	(0.194)	(0.187)
> 10 books at home	0.366***	0.412***	0.380***
	(0.116)	(0.101)	(0.097)
Time to school > 15'	-0.181	-0.068	-0.017
	(0.116)	(0.102)	(0.098)
Wealth	0.379***	0.403***	0.412***
	(0.072)	(0.064)	(0.062)
Constant	-1.389**	-1.756***	-1.046***
	(0.652)	(0.395)	(0.289)
Class FEs	X		
School FEs		X	
Commune FEs			X
Observations	4,422	5,582	6,013

Notes: Standard errors clustered at class level in column (1), school level in column (2), commune level in column (3), robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table A7. OLS regressions of initial confidence and effort scores on extra-class participation

Dependent variable	(1) Confidence	(2) Confidence	(3) Effort	(4) Effort
Extra	0.055	0.037	0.063	0.031
	(0.051)	(0.053)	(0.053)	(0.053)
YL score 1 Math	0.165***	0.149***	0.136***	0.124***
	(0.030)	(0.030)	(0.031)	(0.030)
YL score 1 Viet	0.240***	0.237***	0.090***	0.061**
	(0.029)	(0.030)	(0.029)	(0.030)
Girl	0.166***	0.157***	0.081**	0.056
	(0.037)	(0.038)	(0.038)	(0.039)
Minority	-0.167**	-0.108	0.027	0.009
	(0.085)	(0.094)	(0.105)	(0.100)
Siblings				
one	0.011	0.004	0.018	0.015
	(0.041)	(0.042)	(0.042)	(0.041)
two	-0.027	-0.028	-0.029	-0.022
	(0.054)	(0.055)	(0.058)	(0.056)
more than three	-0.089	-0.101	-0.038	-0.040
	(0.073)	(0.077)	(0.070)	(0.072)
> 10 books at home	0.021	0.011	0.093**	0.098**
	(0.043)	(0.044)	(0.042)	(0.039)
Time to school > 15'	-0.060	-0.058	-0.070*	-0.062
	(0.037)	(0.038)	(0.041)	(0.039)
Wealth	0.097***	0.093***	0.132***	0.117***
	(0.025)	(0.025)	(0.025)	(0.024)
Constant	-0.181*	-0.153	-0.180*	-0.147
	(0.102)	(0.102)	(0.095)	(0.089)
Observations	3,162	3,162	3,162	3,162
R-squared	0.273	0.256	0.237	0.233

Notes: Outcomes are measured in wave 1 using one-parameter logistic model (columns (1) and (3)) or two-parameter logistic model (columns (2) and (4)). All specifications control for class fixed-effects. Standard errors clustered at class level, robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table A8. Full-day schooling option

	Freq.	Percent	Cum.
Do you attend full-day schooling? (Respondents: pupil)			
No	2,652	40.49	40.49
Yes	3,898	59.51	100
Total	6,550	100	
Do pupils in your class attend full-day schooling? (Respondent: teacher)			
No pupils attend full-day schooling, but it is on offer	1,296	19.73	19.73
All pupils attend full-day schooling	3,196	48.66	68.39
Some pupils attend full-day schooling	238	3.62	72.02
Full-day schooling is not available	1,838	27.98	100
Total	6,568	100	
Does school offer full-day schooling to pupils in Grade 5 (Respondent: principal)			
No	2,344	35.68	35.68
Yes, all pupils	3,352	51.02	86.7
Yes, some pupils	874	13.3	100
Total	6,570	100	