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**Working Paper**

## Compulsory kindergarten education and early-teenage literacy in the Philippines

ADB Working Paper, No. 1384

**Provided in Cooperation with:**

Asian Development Bank Institute (ADBI), Tokyo

*Suggested Citation:* Abrigo, Michael Ralph M.; Francisco, Kris A. (2023) : Compulsory kindergarten education and early-teenage literacy in the Philippines, ADB Working Paper, No. 1384, Asian Development Bank Institute (ADBI), Tokyo, <https://doi.org/10.56506/HCKS6480>

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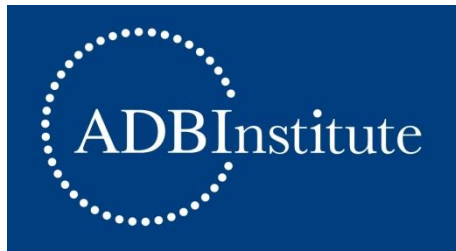
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**ADB Working Paper Series**

**COMPULSORY KINDERGARTEN  
EDUCATION AND EARLY-TEENAGE  
LITERACY IN THE PHILIPPINES**

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Kris A. Francisco

No. 1384  
May 2023

**Asian Development Bank Institute**

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Suggested citation:

Abrigo, M. R. M. and K. A. Francisco. 2023. Compulsory Kindergarten Education and Early-Teenage Literacy in the Philippines. ADBI Working Paper 1384. Tokyo: Asian Development Bank Institute. Available: <https://doi.org/10.56506/HCKS6480>

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The authors are grateful for thoughtful discussion and comments by Daniel Suryadarma, Albert Park, M. Niaz Asadullah, and other participants at the Conference on the Quality of Basic Education in Southeast Asia held at the ISEAS-Yusof Ishak Institute, Singapore on 11 and 12 October 2022. All remaining errors are by the authors.

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**Abstract**

We assessed the impact of compulsory kindergarten education on early-teenage basic and functional literacy skills achievement using a large-scale natural experiment in the Philippines. In 2012, the Philippine government mandated compulsory kindergarten attendance for children aged five years or older prior to enrolling in the country's basic education cycle. This created a nontrivial discontinuity in the propensity of kindergarten school attendance among different cohorts of children, which we exploited in this study. We find that children who were exposed to the policy were no more likely to be able to read, write, and calculate by the age of 11 to 13 years than comparable peers who were not compelled to attend kindergarten. However, those who were exposed to the policy were more likely to reach full functional literacy by early teenage, which is likely due to dynamic complementarities in the formation of skills. While other children were able to eventually catch up with basic literacy skills in later childhood, children who attended kindergarten were more likely to be able to read and write before entering primary school, which allowed them to develop further skills later.

**Keywords:** early-childhood education, literacy

**JEL Classification:** D83, I21, I28

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# 1. INTRODUCTION

Early-childhood interventions are important in the development of children and have important implications in later-life outcomes. Pre-primary school participation, in particular, has been documented as a cost-effective way to improve learning, with impacts extending into adulthood. Although pre-primary school participation has been increasing globally, especially in low- and lower-middle-income countries, achievements have remained uneven, with the most vulnerable more likely to be excluded (UNESCO 2022).

In this study, we assessed the impact of a national policy introduced in 2012 requiring all children to attend at least one year of pre-primary school prior to enrolling in primary school in the Philippines. In 2021, the Philippines was one of only about 40 countries that had made pre-school compulsory and one of only around 80 countries that provided it for free.

We find that children who were exposed to the mandatory kindergarten policy were not more likely to be able to read, write, and calculate by early teenage; however, they are more likely to reach full functional literacy. This impact appears not to be statistically different across children's sex, socioeconomic status, or region of residence, although point estimates for poorer children are generally higher. In order to uncover potential mechanisms for these results, we looked at the basic literacy skills of younger cohorts and found that children who attended kindergarten were more likely to be reading and writing by the start of the primary school cycle. Overall, the evidence we present here support the notion of dynamic complementary in skills formation (Cunha and Heckman 2007), wherein higher-order skills build on more basic skills. While students who did not attend kindergarten were able to catch up in terms of basic reading, writing and arithmetic skills when they reach high school, those who attended kindergarten were more likely to have already developed full functional literacy by acquiring basic literacy skills earlier.

This study bridges several gaps in the literature. First, we employ a large natural experiment to assess the impact of kindergarten education on the achievement of medium-term literacy skills. More specifically, we looked at the impact of compulsory kindergarten education in the Philippines that required children aged five years or older to attend kindergarten prior to enrolling in primary school. This introduced a discontinuity in the probability of kindergarten attendance between those who were required to attend kindergarten and those who were not by sheer virtue of differences in their birth timing, which we exploited in this study. We assessed the impact of this policy on the basic and functional literacy skills achievement of early-teenage children.

By focusing on a universal program, we are able to assess the potential impacts of such early-childhood intervention by schools of more varied quality on a more general population of children, which may be different from those in smaller but more targeted and quality-controlled programs. By employing a natural experiment, we are able to exploit a plausible source of exogenous variation to minimize potential bias from nonrandom selection into kindergarten enrollment. By looking at early-teenage children's outcomes, we provide additional evidence on the later-life impacts of early-childhood interventions, instead of the largely immediate and short-term consequences available in the literature (van Huizen and Plantenga 2018; McCoy et al. 2017; Duncan and Magnuson 2013).

Second, we assessed whether universal kindergarten impacts different subpopulations of children differently. Meta-analytic studies on early-childhood interventions (e.g., van Huizen and Plantenga 2018; Magnuson et al. 2016) have documented that program impacts may vary quite substantially across important dimensions, such as by children's sex, socioeconomic status, and region, which we also explored in this study.

Finally, we add to a small but growing literature documenting the impacts of early-childhood education interventions in developing country contexts. The many positive impacts of pre-school education on socioemotional, motor, and cognitive skills have been recognized in the literature (e.g., Marulis and Neuman 2010; Wick et al. 2017; Nelson and McMaster 2019); however, many of these have been based on experiences in Western, educated, industrialized, rich, and democratic countries. Although early-childhood interventions in developing countries largely follow those implemented in more industrialized economies, their impacts may differ depending on specific country contexts (Rao et al. 2017).

The Philippines is an interesting case study. It has made important progress in expanding access to education. The net enrollment rate in elementary school has traditionally been around 95%, while that for junior and senior high school has shown substantial improvements over the last decade. These successes, in turn, have contributed to the high 97.4% and 89.5% cohort survival rates at elementary and secondary school levels, respectively.

Despite these notable achievements, Filipino students lag behind similar cohorts in other countries. In 2018, the Philippines was at the bottom of the Program for International Student Assessment that tested 15-year-old students in mathematics, science, and reading. These poor results among Filipino students were confirmed in the 2019 Trends in International Mathematics and Science Study, which also showed the Philippines to be a laggard among countries that participated in the international large-scale standardized testing program.

These wake-up calls have come at a time when the Philippine education system is in the process of introducing new innovations, including making kindergarten instruction compulsory, institutionalizing the K+12 basic education program, adopting mother tongue-based multilingual education, strengthening the school-based feeding program, and expanding the country's conditional cash transfer (CCT) for the poor, among many others. This raises questions about whether the interventions that have been introduced thus far are actually effective in delivering good-quality education.

The rest of the paper is organized as follows. In the next section, we provide a historical overview of the kindergarten policy and attendance in the Philippines. This is followed in Sections 3 and 4 by discussions on the data and empirical strategy we employed in our analyses. We discuss the results in Section 5. Finally, in Section 6, we conclude with a summary and some policy implications.

## **2. COMPULSORY KINDERGARTEN EDUCATION IN THE PHILIPPINES**

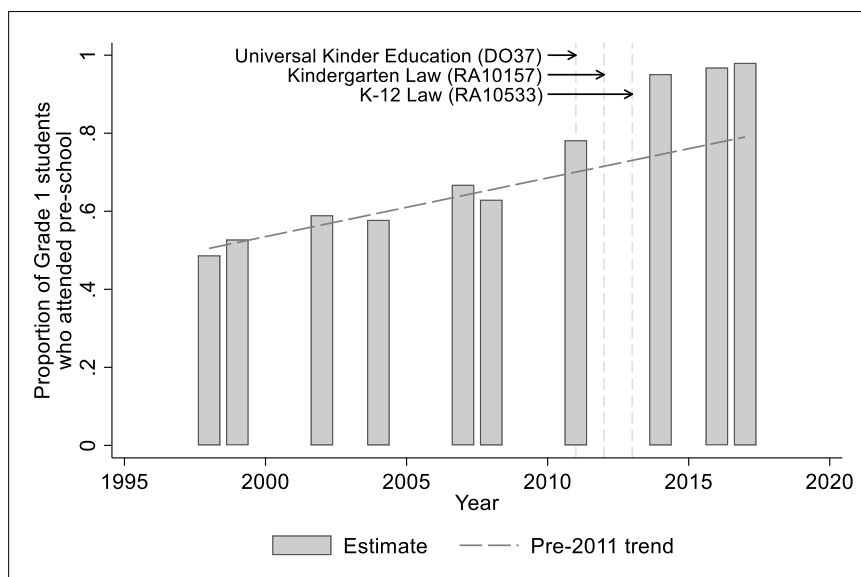
The importance of early-childhood education has long been recognized in the Philippines, with the national government mandating local governments to operate and maintain free public nursery and kindergarten schools as early as 1974. However, it was not until January 2012 that kindergarten education was institutionalized as part of the Philippine basic education system and made a prerequisite for Grade 1 attendance under the Kindergarten Education Act (KEA). In 2011, in preparation for the possible

enactment of the KEA, the Department of Education implemented a universal kindergarten education program. The KEA was partially implemented in the 2011–2012 school year, and eventually fully adopted in the subsequent school cycle. The KEA mandate was further reiterated in the 2013 Enhanced Basic Education Act or the K-to-12 Law.

Under the KEA and the K-to-12 Law, kindergarten education includes one year of preparatory education for children aged five years or older. The standardized kindergarten curriculum emphasizes integrative and interactive teaching-learning strategies and child-centered learning experiences through play-based activities with the goal of preparing children for the content-based curriculum starting in Grade 1.

Prior to the KEA, kindergarten enrollment in the Philippines had been increasing but at a rather slow pace (Figure 1). In 1998, about one in every two Grade 1 pupils had kindergarten education. This ratio had increased to two in every three Grade 1 pupils by 2007. The KEA has accelerated kindergarten education among incoming Grade 1 pupils. In 2011, a year prior to the full implementation of the KEA, about four in every five Grade 1 pupils had kindergarten education. By 2017, virtually all Grade 1 students had attended kindergarten school.

**Figure 1: Kindergarten Attendance among Grade 1 Students in the Philippines: 1998–2017**



Source: Authors' estimates based on APIS data, various years.

Table 1 shows that children from households of a lower socioeconomic status appear to have been more greatly affected by the compulsory kindergarten attendance. In 2007, only half of Grade 1 students from households with, at best, primary school-educated members attended kindergarten, compared with about three in every four Grade 1 students from households with college-educated members. Kindergarten attendance rates were lowest in Mindanao and in the Visayas, which historically also record the highest poverty incidence rates in the country. But by 2017, kindergarten attendance among Grade 1 pupils had become more or less comparable across socioeconomic classes and regions at close to universal attendance.



**Table 1: Kindergarten Attendance among Grade 1 Students (%) by Characteristics**

	1998	2007	2011	2017
All Grade 1	48.7	66.8	78.2	98.0
Sex				
Male	48.8	66.2	77.1	97.6
Female	48.6	67.4	79.5	98.5
Highest education level in household				
Primary	32.4	50.2	67.4	99.8
Secondary	47.9	67.5	78.7	97.8
Tertiary	62.6	77.0	83.8	97.8
Broad region of residence				
NCR	72.7	83.2	87.4	96.3
Luzon, others	54.1	71.3	81.8	99.6
Visayas	41.9	64.3	79.9	96.1
Mindanao	38.6	55.2	67.9	97.7

Source: Authors' estimates based on APIS data, various years.

### 3. DATA

We employ the 2008, 2013, and 2019 rounds of the Philippine Functional Literacy, Education and Mass Media Survey (FLEMMS). The FLEMMS is a nationally representative household-based survey conducted by the Philippine Statistics Authority (formerly the National Statistics Office) to assess the literacy status, educational skills qualifications, and mass media exposure of Filipinos.

There are five literacy levels in the FLEMMS. Those at Level 0 can neither read nor write. Subsequent levels increase with literacy skills: can read and write (Level 1), and compute (Level 2), and comprehend (Level 3). Basic literacy status (Level 1) is provided for all individuals aged six years or older, while functional literacy status (Level 2 or higher) is available for all respondents aged ten years or older. In the FLEMMS, high school graduates are automatically tagged as functionally literate (Level 4).

Basic literacy status is assessed in the FLEMMS by directly asking a household representative, usually a knowledgeable adult, whether a household member aged six years or older “[c]an... read and write a simple message in any language or dialect.” All household members aged ten years or older are provided with self-administered questionnaires to assess their functional literacy. Simple numeracy and basic reading comprehension skills are measured based on three questions that have remained essentially unchanged over survey rounds. Literacy levels are assessed based on these questions regardless of the representative members’ respective appreciation of their other household members’ basic literacy status.

We are interested in the literacy levels of cohorts aged 11 to 16 years, roughly corresponding to junior high school level in the K-to-12 curriculum, at the time of each survey round. Since the KEA was only fully implemented in 2012, those aged 11 to 16 years in the 2008 and 2013 FLEMMS were not compelled to attend kindergarten. The 2019 FLEMMS, on the other hand, included cohorts who were born after 2006, corresponding to those aged five years or younger in 2011, and required to attend kindergarten prior to Grade 1, and those born before 2006 who would have already

attended primary school regardless of kindergarten attendance when the KEA was first implemented.

The literacy levels of cohorts aged 11 to 16 years in the 2008, 2013, and 2019 FLEMMS are summarized in Table 2. During the study period, Level 1 and Level 2 literacy rates among those aged 11 to 16 years in our sample increased by 3.9 and 4.9 percentage points, respectively. However, Level 3 literacy rates increased by only 2.5 percentage points over the same period. When disaggregated by age, sex, and household socioeconomic status, older, female, and more affluent children had higher literacy rates on average than other early teens.

**Table 2: Literacy Distribution (%) among Early Teens by Year and Characteristics**

	Basic Literacy			Functional Literacy					
	At least Level 1			At least Level 2			At least Level 3		
	2008	2013	2019	2008	2013	2019	2008	2013	2019
All sample	90.5	93.5	94.4	84.4	87.7	89.3	60.0	59.8	62.5
Age group									
11–13	88.1	91.9	92.9	80.9	84.4	86.2	55.2	54.3	58.8
14–16	92.9	95.1	96.0	88.0	91.1	92.5	65.1	65.6	66.2
Sex									
Male	88.0	91.9	92.9	81.3	85.7	87.5	55.3	56.5	59.3
Female	93.1	95.1	96.0	87.7	89.7	91.3	65.1	63.2	65.9
Highest education level in household									
Primary	70.5	78.3	76.0	58.7	68.6	67.2	32.5	34.2	39.3
Secondary	91.1	93.7	94.7	84.8	87.1	89.0	58.1	56.9	61.2
Tertiary	96.2	97.0	97.1	92.3	93.0	93.5	72.1	69.3	68.4
Broad region of residence									
NCR	95.6	97.1	97.3	90.9	90.1	93.7	74.5	65.6	69.8
Luzon, others	92.8	94.5	95.0	87.2	90.3	90.1	65.6	66.6	65.5
Visayas	87.1	91.9	93.3	79.5	83.3	86.4	52.0	53.2	58.3
Mindanao	87.1	91.8	93.5	80.8	85.3	88.7	51.2	51.8	59.2

Source: Authors' estimates based on FLEMMS data, various years.

## 4. EMPIRICAL STRATEGY

In order to estimate the impact of kindergarten education on literacy rates among early teens, we employ a standard difference-in-differences (DID) strategy by estimating the following linear regression model:

$$y_{iat} = \tau \cdot I(a \leq 13) \cdot I(t = 2019) + X_{it}\beta + \mu_t + \kappa_a + \gamma + \varepsilon_{iat}, \quad (1)$$

where individuals aged  $a$  in period  $t$  are indexed by  $i = 1, 2, \dots, N_t$ . The variable  $y$  corresponds to one of three possible dummy variables indicating literacy level attainment: Level 1, 2, or 3. The row vector  $X$  is individual and household characteristics that we control in the model, and  $\beta$  is a conformable vector of regression coefficients. The parameters  $\mu_t$  and  $\kappa_a$  are period- and age-fixed effects, respectively. The variable  $\varepsilon$  is the usual regression model residual. Our parameter of interest is  $\tau$ , which captures the impact of compulsory kindergarten education on the literacy level attainment of early teens.

In the above empirical model, we compare the average literacy rates of those aged 11 to 13 years with older cohorts aged 14 to 16 years over the study period. As mentioned in the previous section, those aged 11 to 13 years in the 2019 FLEMMS are required to attend kindergarten prior to Grade 1 unlike other cohorts in our sample, which allows us to estimate the impact of kindergarten education in a DID framework. This strategy relies critically on a parallel trends assumption, which requires that the difference in the time trends of the literacy rates of the two age groups should be constant over time had the compulsory kindergarten education mandate in the KEA not been introduced. That is, without any intervention the time trends of literacy rates of the two age groups should evolve parallel to each other.

Figure 2 plots literacy rates by level, survey year, and age for cohorts aged 11 to 16 years in our sample. Visual inspection suggests that the change in age-specific literacy rates across years had been more or less constant for cohorts not compelled to attend kindergarten school. However, there may be important deviations in some age groups, suggesting the existence of preexisting trends, but these need to be investigated more formally since such variations may not actually be systematic and may also come from randomness in the sample.

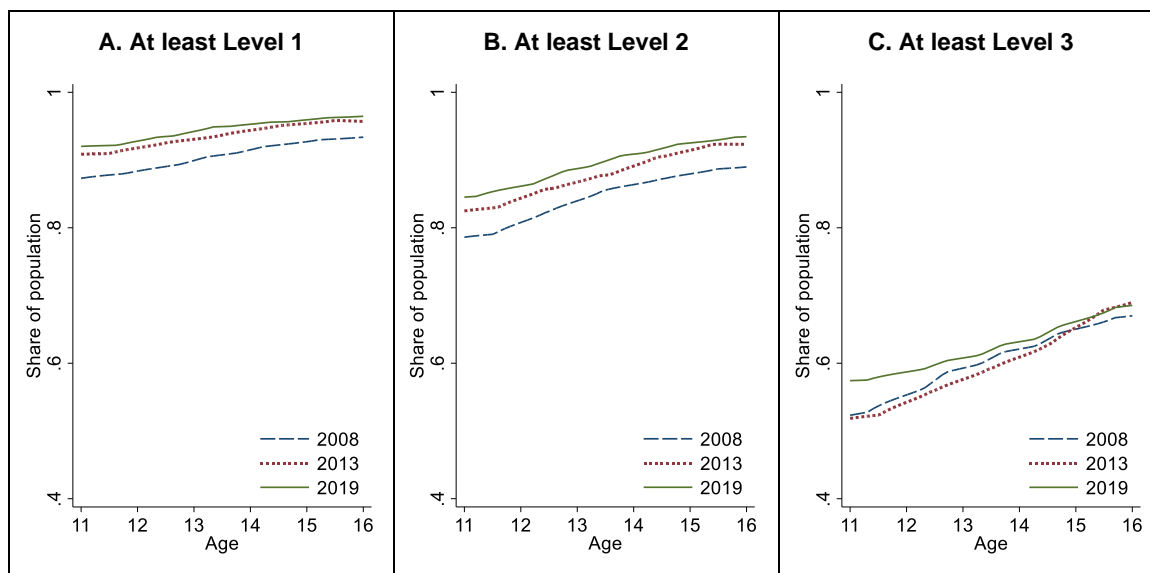
While the parallel trends assumption cannot be empirically tested, we conduct several standard falsification tests to assess whether this assumption is likely to hold in our case. In addition, we limit our main analyses to the available early-teenage sample aged 11 to 16 years to ensure that our observations are more or less exposed to similar household and schooling conditions.

The mandated compulsory kindergarten education among incoming Grade 1 pupils starting in the 2012–2013 school year introduced a discontinuity in the propensity of kindergarten attendance among different cohorts, particularly those born before 2006, who would have been old enough to attend basic education without going through kindergarten at the time of the policy change, and those born starting in 2006, who are compelled to attend kindergarten in order to progress through the basic education system. In such natural experiments, we expect that the distribution of characteristics among those near enough but on either side of the assignment threshold, in this case at the age of 14 in 2019, should be balanced. Covariate balance should also be true for cohorts aged 11 to 16 years in the earlier FLEMMS rounds since they were not covered by the compulsory kindergarten policy in the KEA. We confirm this expectation using household characteristics in Appendix Table A.

By limiting our sample to those aged 11 to 16 years, we also sidestep the issue of the automatic assignment of Level 4 literacy to high school graduates since this age group is likely to be still attending high school at the time of each survey round. In our FLEMMS sample, less than 5% have finished high school, and they tend to be from older cohorts. At worst, this will likely introduce a downward bias to our impact estimates as a result of the automatic tagging of these high school graduates as functionally literate regardless of their true literacy status.

A key limitation of this study is that only the highest educational attainment of individuals is recorded in the FLEMMS, and we do not observe actual kindergarten attendance in our sample. As a consequence, we are limited to estimating the intent-to-treat (ITT) effects of compulsory kindergarten education. That is, the study is only able to capture the effects of the policy change on the population who were required to attend kindergarten prior to Grade 1, which does not necessarily reflect the effects of receiving kindergarten instructions. In any case, the average treatment effect as a result of actually attending kindergarten is expected to be larger in magnitude compared with our ITT estimates.

**Figure 2: Literacy Rate by Level and Age**



Source: Authors' estimates based on FLEMMS data, various years.

## 5. RESULTS

### 5.1 Literacy among Early-Teenage Population

Table 3 presents the main results of our analyses based on the sample of children aged 11 to 16 years from the 2013 and 2019 rounds of the FLEMMS. We are interested in the coefficient on the interaction term, which provides the ITT-DID impact estimate of the compulsory kindergarten education on the literacy-level attainment among early-teenage children. Separate estimates are provided for each literacy level up to Level 3. Children are tagged as literate at the specified level if their assessed literacy level is at least as high as the level specified based on their responses to the FLEMMS self-administered questionnaire. For example, a child with an assessed literacy of Level 3 is tagged as literate for Levels 1, 2, and 3.

We progressively add explanatory variables in each set of difference-in-differences models to assess the sensitivity of our estimates to the potential influence of unobserved confounders. We start with a simple DID without controls. Then we add child- and household-specific characteristics that have been documented to influence schooling decisions. Finally, we include area-specific fixed effects to account for potential regional differences in schooling quality, household preferences, etc. that are common across time within regions. Overall, our DID estimates appear to be robust to the inclusion of these additional explanatory variables.

Before turning our attention to our DID estimates, it is worth noting some observations from the estimates in Table 3. First, younger cohorts on average have lower literacy rates across the levels we considered. The discrepancy increases with the literacy level, reaching as high as almost 10 percentage points for Level 3 literacy. Second, literacy rates by level remained practically unchanged between 2013 and 2019, controlling for other factors included in our models.

**Table 3: ITT Estimate of Kindergarten Impact on Literacy Level**

	Functional Literacy									
	Basic Literacy			At least Level 2			At least Level 3			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
1(≤13) x 1(t=2019)	0.002 (0.005)	0.000 (0.005)	0.000 (0.005)	0.003 (0.007)	0.000 (0.007)	0.000 (0.007)	0.000 (0.007)	0.039*** (0.011)	0.035*** (0.011)	0.035*** (0.010)
1(≤13)	-0.032*** (0.004)	-0.021*** (0.004)	-0.021*** (0.004)	-0.067*** (0.006)	-0.052*** (0.006)	-0.053*** (0.006)	-0.112*** (0.009)	-0.095*** (0.008)	-0.095*** (0.008)	-0.097*** (0.008)
1(t=2019)	0.008** (0.004)	0.003 (0.003)	0.005 (0.003)	0.014*** (0.005)	0.010** (0.005)	0.014*** (0.005)	0.006 (0.008)	0.004 (0.008)	0.004 (0.008)	0.012 (0.008)
Child – female		Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes
Household head – employed		Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes
Household head – female		Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes
Household – highest education		Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes
Household – assets		Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes
Household – residence region			Yes			Yes			Yes	Yes
Observations	33,142	33,142	33,142	33,142	33,142	33,142	33,142	33,142	33,142	33,142
Adjusted R-square	0.005	0.052	0.065	0.011	0.052	0.066	0.009	0.040	0.040	0.057
BIC	-1,805	-3,372	-3,684	17,464	16,114	15,760	46,045	45,054	45,054	44,611

Note: \*, \*\*, and \*\*\* signify statistical significance at the 10%, 5%, and 1% alpha levels, respectively. Values in parentheses are heteroskedasticity-robust standard errors clustered at the household level. The sample includes children aged 11 to 16 years in the 2013 and 2019 FLEMMS. BIC – Bayesian information criterion.

Estimates in Table 3 suggest that kindergarten education has no impact on Level 1 and Level 2 literacy for the age group we considered. However, we find a 3.5 percentage point increase in the probability of achieving Level 3 literacy among early-teenage children exposed to the compulsory kindergarten education policy. Compared with the baseline rate of 54.3% among children aged 11 to 13 years (see Table 2), this represents a modest 6.4% increase in the Level 3 literacy rate.

The above intent-to-treat impact estimates capture the effect of the policy regardless of the actual kindergarten attendance of children. A more interesting and policy-relevant parameter would be the impact on children who actually received kindergarten instruction in response to the KEA, i.e., a local average treatment effect (LATE), which we cannot calculate directly due to the structure of the data available to us.

Suppose we have an estimate of the change in kindergarten attendance due to the KEA. Then we can estimate LATE using the Wald estimator

$$\widehat{\tau}_{LATE} = \frac{E[Y_l = 1|Z = 1] - E[Y_l = 1|Z = 0]}{E[K = 1|Z = 1] - E[K = 1|Z = 0]}, \quad (2)$$

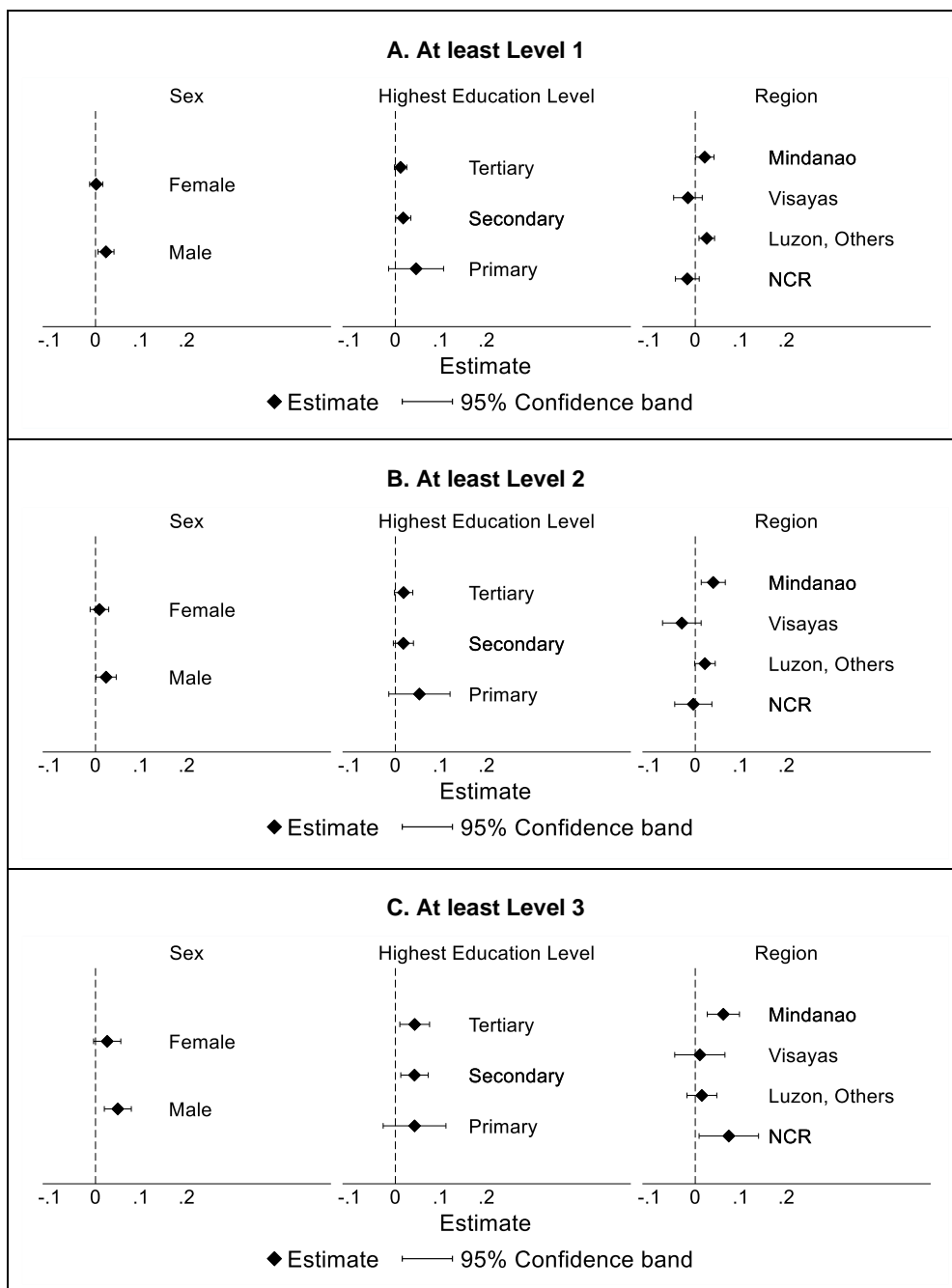
where  $E[Y_l = 1|Z = z]$  and  $E[K = 1|Z = z]$  are, respectively, the Level  $l$  literacy rate and kindergarten attendance propensity conditional on exposure to the compulsory kindergarten policy  $Z$ . The numerator may be proxied by our DID estimates,  $\tau_{DID}$ . On the other hand, the denominator, representing the difference in kindergarten propensity due to the change in policy, may be estimated based on estimates from Figure 1. With an increase in kindergarten attendance of about 10 percentage points, this suggests that the impact on the propensity of children compelled to attend kindergarten to achieve Level 3 literacy is about ten times that of our baseline ITT-DID estimate.

We also performed heterogeneity analyses by interacting our baseline DID specification in (1) with dummy variables representing children's sex, highest educational attainment in the household, or their region of residence. The expectation is that the compulsory kindergarten policy is likely to have more pronounced effects among subpopulations that have lower baseline kindergarten attendance rates. However, the overlapping 95% confidence bands of the DID estimates presented in Figure 3 suggest that the ITT impact estimates do not statistically differ across the subpopulations that we considered despite specific point estimates being higher for children coming from poorer backgrounds, as proxied by the highest education attainment in households.

## 5.2 Falsification Tests

The analyses we have presented rest critically on a parallel trends assumption. That is, the literacy rates of those aged 11 to 13 years and those aged 14 to 16 years should evolve in parallel with each other without the change in kindergarten attendance policy introduced by the KEA. This allows us to ascribe the DID estimate solely to the KEA, and not to any other preexisting or concurrent policies. While the parallel trends assumption cannot be directly ascertained, we performed several falsification tests to establish whether it is likely in our case.

**Figure 3: ITT Estimate of Kindergarten Impact on Literacy Level by Characteristics**



Note: Estimates are calculated using fully interacted difference-in-differences models, controlling for child and household characteristics as enumerated in Table 3. Confidence bands are based on heteroskedasticity-robust standard errors clustered at the household level. The sample includes children aged 11 to 16 years in the 2013 and 2019 FLEMMS.

First, we check the existence of preexisting trends by estimating the same models as in Table 1 but using data for 2008 and 2013. The existence of preexisting trends, as captured by significant DID estimates using pre-policy data, directly negates the existence of parallel trends in the pre-policy phase, and casts doubt on post-policy parallel trends. Estimates presented in Appendix Table B.1 show that we cannot reject the null hypothesis of no preexisting trends for Level 2 and Level 3 literacy. However,

there appears to be a preexisting trend in Level 1 literacy rates, although the estimates are quite small, with trends across age groups narrowing by about 0.3 percentage points per year.

Second, we estimated DID models using a placebo treatment group, in this case the cohort aged 17 to 19 years instead of the original 11 to 13 years. Since these cohorts were all born before 2006, they are not compelled to attend kindergarten. However, those born later had been exposed longer to the K-to-12 policy introduced in 2013, which may also affect literacy levels across cohorts even without compulsory kindergarten schooling. The statistically insignificant DID estimates using an older proxy treatment group presented in Appendix Table B.2 suggest that we can rule out contamination of our baseline estimates from the K-to-12 policy.

Third, we assessed whether the policy has affected related outcomes, which may, in turn, influence early-teenage children's literacy rates. In particular, we look at the impact of the KEA on the presence of everyday readers, writers, and arithmetic users in the household that may serve as role models for children to imitate. Since these adults have not been compelled to attend kindergarten due to the KEA, we expect no impact from the policy on these placebo outcomes, which is confirmed by estimates in Appendix Table B.3.

Finally, we estimate the same DID models, but using an alternative control group, in this case those aged 17 to 19 years instead of the original 14 to 16 years. Under a parallel trends assumption, the estimates based on the original and the alternative control group should be of about the same magnitude. It is straightforward to show that baseline DID estimates in Table 3 and those using the alternative control group in Appendix Table B.4 do not statistically differ from each other, thereby confirming our expectation under a parallel trends scenario.

### 5.3 Sensitivity Analysis

We also assessed the sensitivity of our estimates to the existence of preexisting trends by estimating similar DID models to that in (1), but including linear time trends. In order to do this, we pooled the sample of children aged 11 to 16 years in the 2008, 2013, and 2019 FLEMMS. We provide separate estimates with common and group-specific time trends. The results presented in Table 4 show that the ITT estimates are qualitatively similar to our main estimates summarized in Table 3. We find no impact on Level 1 and Level 2 literacy achievement, but a significant positive and slightly higher impact on Level 3 literacy skills.

Lastly, we relax the assumption of common impact across cohorts by specifying an event study model, instead of the classical DID formulation. More specifically, we allow impact estimates to differ across cohorts by using dummy variables for single-year age groups with age 14 as the reference age instead of the age 11 to 13 years and 14 to 16 years dichotomy in our baseline DID model. As an extension of the classical DID, our event study formulation also relies on a parallel trends assumption.

We provide two sets of event study estimates. The first version controls for child- and household-level characteristics as enumerated in Table 3. The second version also includes sibling fixed effects to control for unobserved characteristics that may be common among children living in the same households, such as parental support.



**Table 4: ITT Estimate of Kindergarten Impact on Literacy Level: Pooled Data**

	Functional Literacy								
	Basic Literacy			At least Level 2			At least Level 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$I(a \leq 13) \times I(t=2019)$	0.007 (0.004)	0.007 (0.004)	-0.018 (0.011)	0.001 (0.006)	0.002 (0.006)	-0.004 (0.015)	0.028*** (0.009)	0.027*** (0.009)	0.052** (0.021)
$I(a \leq 13)$	-0.027*** (0.003)	-0.028*** (0.003)	-5.814** (2.568)	-0.053*** (0.004)	-0.053*** (0.004)	-1.332 (3.262)	-0.088*** (0.006)	-0.088*** (0.006)	5.765 (4.580)
$I(t=2019)$	0.015*** (0.003)	-0.026*** (0.007)	-0.014* (0.008)	0.027*** (0.004)	-0.013 (0.009)	-0.010 (0.010)	0.012* (0.006)	0.036*** (0.013)	0.023 (0.016)
Child – female	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household head – employed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household head – female	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household – highest education	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household – assets	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household – residence region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time trend – common		Yes			Yes			Yes	
Time trend – group-specific			Yes			Yes			Yes
Observations	46,180	46,180	46,180	46,180	46,180	46,180	46,180	46,180	46,180
Adjusted R-square	0.074	0.075	0.075	0.077	0.078	0.078	0.066	0.066	0.066
BIC	1,243	1,193	1,198	25,438	25,416	25,426	61,787	61,792	61,801

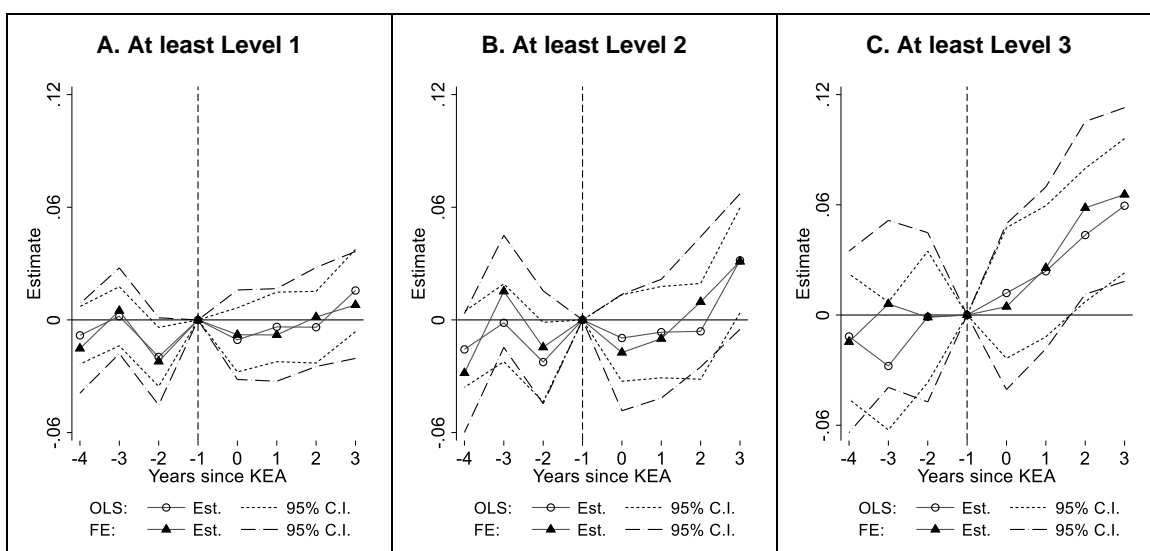
Note: \*, \*\*, and \*\*\* signify statistical significance at the 10%, 5%, and 1% alpha levels, respectively. Values in parentheses are heteroskedasticity-robust standard errors clustered at the household level. The sample includes children aged 11 to 16 years in the 2008, 2013, and 2019 FLEMMS. BIC – Bayesian information criterion.

The results of our event study models presented in Figure 4 are also qualitatively in line with our main estimates. It is noteworthy that estimates for those not compelled to attend kindergarten by the KEA are relatively small in absolute size and largely statistically not significant, bolstering support for our maintained parallel trends assumption. Further, the impact on Level 3 literacy achievement is estimated to be highest among younger cohorts.

### 5.4 Basic Literacy among Grade 1 Students

What could be a possible mechanism to explain why kindergarten education raises Level 3 literacy and not lower levels among early-teenage children? We have already ruled out the modifying effects of sex, socioeconomic status, and region in our heterogeneity analysis. We have also shown that the distribution of household characteristics is similar among children by KEA exposure, and therefore this cannot explain the differences in Level 3 literacy achievement across cohorts.

**Figure 4: ITT Estimate of Kindergarten Impact on Literacy Level by Cohort**



Note: Point estimates and 95% confidence intervals are shown for event study models controlling for child and household characteristics in Table 3 (OLS) and adding sibling fixed effects (FE). Estimates are relative to outcomes among 14-year-old children. The sample includes children aged 11 to 16 years in the 2008, 2013, and 2019 FLEMMS. KEA – Kindergarten Education Act; OLS – ordinary least squares; FE – fixed effects; Est. – estimate; C.I. – confidence interval.

In order to shed light on this conundrum, we look at differences in basic literacy, i.e., Level 1, among Grade 1 pupils by kindergarten attendance. Unlike in our earlier analyses, basic literacy in this case is based on the assessment of a representative household member for children attending Grade 1 at the time of the survey. While we cannot directly estimate inclusion and exclusion errors for those aged below 10 years, we calculated that 95% in our baseline sample of children aged 11 to 16 years were correctly classified by their household member representative, thereby providing some confidence on these personal assessments.

We estimated a linear probability model of achieving Level 1 literacy among Grade 1 pupils using ordinary least squares (OLS) on kindergarten attendance with a battery of child- and household-specific characteristics to control for potential confounders. We also performed a more stringent model using inverse-propensity score reweighting

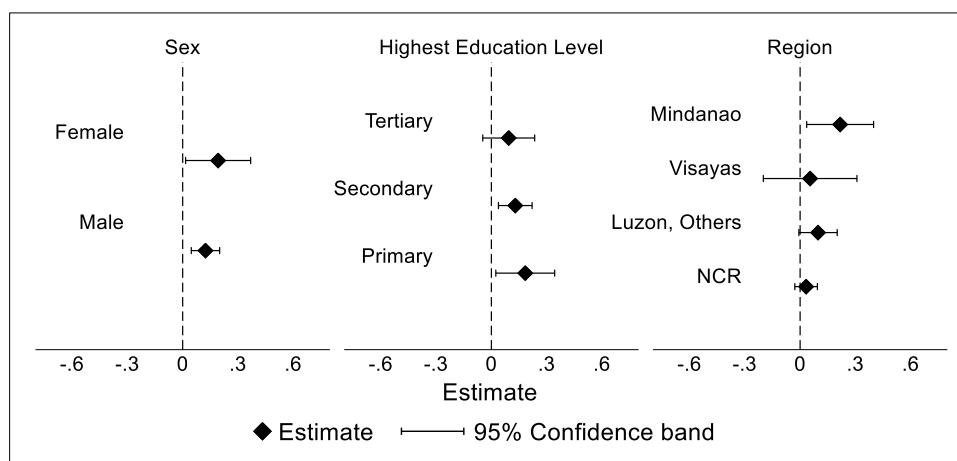
with regression adjustments (IPWRA) as a further refinement. In each of these models, we pooled all children reported to be attending Grade 1 in the 2008, 2013, and 2019 FLEMMS.

**Table 5: Kindergarten and Basic Literacy among Grade 1 Pupils**

	OLS		IPWRA	
	(1)	(2)	(3)	(4)
I(Kinder)	0.206*** (0.018)	0.174*** (0.018)	0.155*** (0.018)	0.125*** (0.029)
Child – age	Yes	Yes	Yes	Yes
Child – female		Yes	Yes	Yes
Household head – employed		Yes	Yes	Yes
Household head – female		Yes	Yes	Yes
Household – highest education		Yes	Yes	Yes
Household – assets		Yes	Yes	Yes
Household – residence region			Yes	Yes
Household – with everyday reader	Yes	Yes	Yes	Yes
Household – with everyday writer	Yes	Yes	Yes	Yes
Household – with everyday arithmetic user	Yes	Yes	Yes	Yes
Survey – year	Yes	Yes	Yes	Yes
Observations	9,018	9,006	9,006	9,006
Adjusted R-square	0.028	0.046	0.079	–
BIC	11,330	11,182	10,996	–

Note: \*, \*\*, and \*\*\* signify statistical significance at the 10%, 5%, and 1% alpha levels, respectively. Values in parentheses are heteroskedasticity-robust standard errors clustered at the household level. The sample includes children attending Grade 1 in the 2008, 2013, and 2019 FLEMMS. The IPWRA model weighs observations in the linear probability regression by the inverse of the predicted propensity to attend pre-school prior to attending Grade 1 using child and household characteristics as explanatory variables. OLS – ordinary least squares; IPWRA – Inverse-propensity score reweighting with regression adjustments; BIC – Bayesian information criterion.

**Figure 5: Kindergarten and Basic Literacy among Grade 1 Pupils by Characteristics**



Note: Estimates are calculated using fully interacted linear probability models, controlling for child and household characteristics as enumerated in Table 4, Column 3. Confidence bands are based on heteroskedasticity-robust standard errors clustered at the household level. The sample includes children attending Grade 1 in the 2008, 2013, and 2019 FLEMMS.

The estimates in Table 5 show that Grade 1 pupils who attended kindergarten are more likely to be reported as having basic literacy. The estimates vary quite significantly across specifications, ranging from 12.5 percentage points for the more stringent IPWRA model to 20.6 percentage points for the most basic model in OLS. When disaggregated by children's sex or household characteristics, the estimates of associations presented in Figure 5 have overlapping 95% confidence intervals across subgroups, although point estimates are higher for girls, for children living in poorer households, as proxied by household members' highest educational attainment, and for those living in Mindanao.

When combined with estimates from Table 3, our results appear to suggest the presence of dynamic complementarities in skills formation (Cunha and Heckman 2007). Children who attended kindergarten are more likely to be capable of reading and writing, which are essential skills to unlock higher-level skills, by Grade 1. By early teenage, those who have not attended kindergarten are able to catch up in basic literacy achievement compared with their kindergarten-educated peers. However, the latter were able to expand their skills, further building on their earlier acquired reading and writing skills, thus explaining the difference in the propensity of Level 3 literacy achievement across cohorts.

## 6. CONCLUSION

We employed a large-scale natural experiment requiring children aged five years or older in the Philippines to attend kindergarten prior to enrolling in primary school. This introduced a nontrivial jump in the propensity to attend kindergarten across cohorts of children. Using a difference-in-differences methodology, we find that those who were exposed to the policy were more likely to have a higher functional literacy rate, although they are not more likely to be able to read, write, and calculate in early teenage. We did not find evidence that these results vary by children's sex, socioeconomic background, or region of residence.

By examining basic literacy skills among primary school students, we find indications that our results for teenage children are likely due to kindergarten-educated children being more likely to have acquired reading and writing skills prior to attending primary school. While other children may have caught up eventually in acquiring these basic literacy skills, the kindergarten-educated children were able to further develop additional skills. Overall, these observations support the importance of dynamic complementarities in skills formation (Cunha and Heckman 2007), wherein more complex higher-order skills build on more fundamental, simpler skills.

The encouraging results that we present here may be of interest to policymakers in other developing countries that wish to embark on similar compulsory and free national public pre-primary school programs.

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## APPENDIX

**Appendix Table A: Covariate Balance**

	Age 11–13		Age 14–16		Std. Diff.	Var. Ratio
	Mean	SD	Mean	SD		
<b>A. All sample</b>						
Household wealth index	−0.12	1.07	−0.21	1.09	0.08	0.97
Household head, college-educated (=1)	0.18	0.39	0.17	0.38	0.03	1.05
Household head, employed (=1)	0.11	0.31	0.11	0.31	0.01	1.02
Household head, female (=1)	0.15	0.36	0.14	0.35	0.04	1.08
Household with everyday reader (=1)	0.58	0.49	0.58	0.49	0.00	1.00
Household with everyday writer (=1)	0.14	0.35	0.14	0.35	0.00	1.01
Household with everyday arithmetic user (=1)	0.49	0.50	0.47	0.50	0.02	1.00
<b>B. FLEMMS 2008 sample</b>						
Household wealth index	−0.39	1.22	−0.29	1.21	−0.09	1.02
Household head, college-educated (=1)	0.11	0.31	0.12	0.32	−0.03	0.92
Household head, employed (=1)	0.09	0.29	0.11	0.31	−0.05	0.88
Household head, female (=1)	0.12	0.33	0.14	0.35	−0.05	0.89
Household with everyday reader (=1)	0.66	0.47	0.66	0.47	0.01	0.99
Household with everyday writer (=1)	0.18	0.38	0.18	0.38	0.00	0.99
Household with everyday arithmetic user (=1)	0.40	0.49	0.43	0.49	−0.05	0.98
<b>C. FLEMMS 2013 sample</b>						
Household wealth index	−0.25	1.10	−0.14	1.08	−0.10	1.04
Household head, college-educated (=1)	0.21	0.41	0.23	0.42	−0.06	0.92
Household head, employed (=1)	0.11	0.31	0.10	0.31	0.00	1.01
Household head, female (=1)	0.10	0.30	0.12	0.32	−0.05	0.88
Household with everyday reader (=1)	0.58	0.49	0.59	0.49	−0.02	1.01
Household with everyday writer (=1)	0.13	0.34	0.13	0.33	0.01	1.02
Household with everyday arithmetic user (=1)	0.37	0.48	0.37	0.48	0.00	1.00
<b>D. FLEMMS 2019 sample</b>						
Household wealth index	−0.05	0.96	−0.01	0.96	−0.05	1.01
Household head, college-educated (=1)	0.19	0.39	0.20	0.40	−0.01	0.99
Household head, employed (=1)	0.12	0.33	0.12	0.32	0.01	1.01
Household head, female (=1)	0.18	0.38	0.19	0.39	−0.02	0.96
Household with everyday reader (=1)	0.53	0.50	0.53	0.50	0.01	1.00
Household with everyday writer (=1)	0.12	0.32	0.12	0.33	−0.02	0.97
Household with everyday arithmetic user (=1)	0.59	0.49	0.59	0.49	−0.01	1.01

Note: Std. diff. – standardized difference; Var. ratio – variance ratio.

**Appendix Table B1: Falsification Test: Pretreatment Trend**

	Functional Literacy								
	Basic Literacy			At least Level 2			At least Level 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$I(\alpha \leq 13) \times I(t=2008)$	-0.016** (0.007)	-0.014** (0.006)	-0.015** (0.006)	-0.005 (0.008)	-0.002 (0.008)	-0.003 (0.008)	0.013 (0.012)	0.016 (0.012)	0.014 (0.011)
$I(\alpha \leq 13)$	-0.032*** (0.004)	-0.019*** (0.004)	-0.020*** (0.004)	-0.067*** (0.006)	-0.050*** (0.006)	-0.051*** (0.006)	-0.112*** (0.009)	-0.092*** (0.008)	-0.094*** (0.008)
$I(t=2008)$	-0.022*** (0.004)	-0.018*** (0.004)	-0.014*** (0.004)	-0.031*** (0.006)	-0.024*** (0.006)	-0.019*** (0.006)	-0.004 (0.009)	0.007 (0.009)	0.010 (0.009)
Child – female		Yes	Yes		Yes	Yes		Yes	Yes
Household head – employed		Yes	Yes		Yes	Yes		Yes	Yes
Household head – female		Yes	Yes		Yes	Yes		Yes	Yes
Household – highest education		Yes	Yes		Yes	Yes		Yes	Yes
Household – assets		Yes	Yes		Yes	Yes		Yes	Yes
Household – residence region			Yes			Yes			Yes
Observations	25,434	25,434	25,434	25,434	25,434	25,434	25,434	25,434	25,434
Adjusted R-square	0.009	0.070	0.079	0.012	0.075	0.087	0.012	0.067	0.096
BIC	5,843	4,255	4,167	18,115	16,491	16,311	35,641	34,212	33,566

Note: \*, \*\*, and \*\*\* signify statistical significance at the 10%, 5%, and 1% alpha levels, respectively. Values in parentheses are heteroskedasticity-robust standard errors clustered at the household level. The sample includes children aged 11 to 16 years in the 2008 and 2013 FLEMMS. BIC – Bayesian information criterion.

**Appendix Table B2: Falsification Test: Placebo Treatment Group**

	Functional Literacy								
	Basic Literacy			At least Level 2			At least Level 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$I(a \geq 17) \times I(t=2019)$	-0.003 (0.004)	0.000 (0.004)	0.001 (0.004)	-0.007 (0.006)	-0.003 (0.006)	-0.001 (0.006)	0.004 (0.010)	0.012 (0.010)	0.014 (0.010)
$I(a \geq 17)$	0.014*** (0.004)	0.009** (0.004)	0.008** (0.004)	0.035*** (0.005)	0.026*** (0.005)	0.025*** (0.005)	0.162*** (0.008)	0.146*** (0.008)	0.143*** (0.008)
$I(t=2019)$	0.008** (0.004)	-0.001 (0.003)	0.000 (0.003)	0.014*** (0.005)	0.006 (0.005)	0.009* (0.005)	0.006 (0.008)	0.000 (0.008)	0.008 (0.008)
Child – female		Yes	Yes		Yes	Yes	Yes	Yes	Yes
Household head – employed		Yes	Yes		Yes	Yes	Yes	Yes	Yes
Household head – female		Yes	Yes		Yes	Yes	Yes	Yes	Yes
Household – highest education		Yes	Yes		Yes	Yes	Yes	Yes	Yes
Household – assets		Yes	Yes		Yes	Yes	Yes	Yes	Yes
Household – residence region			Yes			Yes			Yes
Observations	30,624	30,624	30,624	30,624	30,624	30,624	30,624	30,624	30,624
Adjusted R-square	0.001	0.081	0.089	0.004	0.071	0.080	0.035	0.094	0.108
BIC	-14,905	-17,392	-17,531	1,340	-737	-886	35,637	33,745	33,428

Note: \*, \*\*, and \*\*\* signify statistical significance at the 10%, 5%, and 1% alpha levels, respectively. Values in parentheses are heteroskedasticity-robust standard errors clustered at the household level. The sample includes children aged 14 to 19 years in the 2013 and 2019 FLEMMS. BIC – Bayesian information criterion.



**Appendix Table B3: Falsification Test: Placebo Outcomes**

	Functional Literacy								
	Basic Literacy			At least Level 2			At least Level 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
I(≤13) x I(t=2019)	0.012 (0.010)	0.008 (0.010)	0.007 (0.010)	-0.008 (0.007)	-0.010 (0.007)	-0.011 (0.007)	-0.005 (0.010)	-0.010 (0.010)	-0.009 (0.009)
I(≤13)	-0.008 (0.008)	0.011 (0.007)	0.009 (0.007)	0.003 (0.005)	0.009* (0.005)	0.009* (0.005)	-0.002 (0.008)	0.014* (0.007)	0.014* (0.007)
I(t=2019)	-0.061*** (0.009)	-0.062*** (0.009)	-0.066*** (0.009)	-0.002 (0.006)	0.000 (0.006)	0.005 (0.006)	0.222*** (0.009)	0.228*** (0.009)	0.214*** (0.009)
Child – female		Yes	Yes		Yes	Yes		Yes	Yes
Household head – employed		Yes	Yes		Yes	Yes		Yes	Yes
Household head – female		Yes	Yes		Yes	Yes		Yes	Yes
Household – highest education		Yes	Yes		Yes	Yes		Yes	Yes
Household – assets		Yes	Yes		Yes	Yes		Yes	Yes
Household – residence region			Yes			Yes			Yes
Observations	34,698	34,698	34,698	34,698	34,698	34,698	34,698	34,698	34,698
Adjusted R-square	0.003	0.038	0.049	0.000	0.011	0.018	0.045	0.081	0.107
BIC	49,969	48,785	48,555	21,422	21,078	21,009	48,794	47,520	46,691

Note: \*, \*\*, and \*\*\* signify statistical significance at the 10%, 5%, and 1% alpha levels, respectively. Values in parentheses are heteroskedasticity-robust standard errors clustered at the household level. The sample includes children aged 11 to 16 years in the 2013 and 2019 FLEMMS. BIC – Bayesian information criterion.

**Appendix Table B4: Falsification Test: Alternative Comparison Group**

	Basic Literacy			Functional Literacy					
	At least Level 1			At least Level 2			At least Level 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
I(≤13) x I(t=2019)	0.004 (0.005)	0.000 (0.005)	-0.002 (0.005)	0.009 (0.007)	0.003 (0.007)	0.001 (0.007)	0.034*** (0.010)	0.023** (0.010)	0.021** (0.010)
I(≤13)	-0.046*** (0.004)	-0.029*** (0.004)	-0.028*** (0.004)	-0.101*** (0.006)	-0.078*** (0.005)	-0.077*** (0.005)	-0.275*** (0.008)	-0.239*** (0.008)	-0.238*** (0.008)
I(t=2019)	0.006* (0.003)	0.003 (0.003)	0.006* (0.003)	0.008** (0.004)	0.007* (0.004)	0.013*** (0.004)	0.011 (0.007)	0.017** (0.007)	0.026*** (0.007)
Child – female	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household head – employed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household head – female	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household – highest education	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household – assets	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household – residence region			Yes			Yes			Yes
Observations	31,020	31,020	31,020	31,020	31,020	31,020	31,020	31,020	31,020
Adjusted R-square	0.010	0.063	0.075	0.026	0.075	0.088	0.076	0.123	0.140
BIC	-4,284	-5,949	-6,192	12,847	11,297	11,014	37,910	36,350	35,903

Note: \*, \*\*, and \*\*\* signify statistical significance at the 10%, 5%, and 1% alpha levels, respectively. Values in parentheses are heteroskedasticity-robust standard errors clustered at the household level. The sample includes children aged 11–13 and 17–19 years in the 2013 and 2019 FLEMMS. BIC – Bayesian information criterion.