The Long-Term Effect of Improving Early-Life Learning Preparedness on Cognitive Abilities*

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February 2021

Abstract

We estimate the long-term impact of an unanticipated shift in the beginning of the academic year from January to July in 1979 in Indonesia. Using a regression discontinuity design, we find that this policy led to between 0.21–0.28 standard deviation increase in cognitive abilities 30 years later. We find evidence that the policy increased long-term cognition by improving learning preparedness in early grades, mainly by increasing the absolute age-for-grade upon enrollment. We also find stronger impacts for individuals who had good health during childhood and did not experience early-life nutrition deficits. Our results provide novel evidence on the long-term impact of improving school readiness on cognition in a low-income country.

Keywords: Age-for-grade, cognition, Indonesia, regression discontinuity
JEL Classification: I25, J24, I28, O15

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*We thank Sonia Bhalotra, Thomas Cornelissen, Rasyad Parinduri, Smriti Sharma, Ian Walker and seminar and conference participants at The Graduate Institute Geneva, UNU-WIDER, Lancaster University, NEUDC 2019, RES Conference 2019, PAA Annual Meeting 2018, Nordic Conference in Development Economics 2018, 4th Workshop on Labour and Family Economics, German Economic Association Conference 2019, and 2nd Scottish Development Workshop for comments. Arya Gaduh would like to thank the UNU-WIDER Visiting Scholars Program. A previous draft of this paper was entitled "The Long Term Effects of Delayed School-going Age: Evidence from Indonesia."

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1 Introduction

As societies age, understanding the lifetime determinants of cognitive skills becomes increasingly important. Declining cognitive abilities, a natural effect of aging, negatively affect labor productivity, savings and retirement planning, and can lead to premature death (Banks et al., 2010; Agarwal and Mazumder, 2013). They also increase demand for health services, which impose significant costs on public health and social security systems (Christensen et al., 2009; World Health Organization, ed, 2015). Addressing this issue would be an important part of government response to shifting demographic compositions, especially in low- and middle-income countries (LMICs) where life expectancy is expected to grow fastest amidst limited social security investments (United Nations, 2020).

Early life investments, whose returns accumulate over a lifetime, can potentially help delay cognitive decline. Since skills are self-productive, having stronger foundational skills earlier increases the productivity of skills developed later in life (Cunha et al., 2006). There is growing empirical evidence of the long-term impact of early childhood interventions on education, labor market participation and wages, health and asset ownership (Currie and Vogl, 2013; Nores and Barnett, 2010; Almond et al., 2018). However, there is little evidence on the persistent impact of early life investments on cognitive skills well into adulthood, especially in low-income countries.

In this paper, we use a unique policy experiment in Indonesia in the 1970s to study how improving learning preparedness can have a long-term impact on cognitive functions. In the middle of the 1978 academic year, the government shifted the start of the 1979 academic year from January to July (Tempo, 1978a; Parinduri, 2014). While schools were open as usual, teachers did not introduce new materials and were asked to reprise existing ones during the extra six months. We use this academic-calendar shift (ACS) to estimate the long-term impact on cognitive functions that capture memory, fluid intelligence, and numeracy from the *Indonesia Family Life Survey* (IFLS). Using a regression discontinuity (RD) design, we find a positive impact of this shift across six measures of memory, fluid intelligence, and numeracy, namely that a one-year delay in grade progression leads to between 0.21 and 0.28 standard deviation (SD) increase in cognitive abilities in middle age.

We show that these ACS impacts improve middle-age cognitive functioning by improving *early-life* learning preparedness. First, we only find these improvements in cognitive functioning for individuals who were part of the school system. Moreover, we find that the policy primarily worked like an early-life intervention, improving outcomes for those who were affected in early grades. Consistent with the literature on early childhood development and the potential complementarity of health and education (see Glewwe and Miguel, 2007, for a review), we also find that the long-term impact on cognitive functions is driven by those with good health and nutrition in early childhood.

We further examine two channels from ACS to preparedness: curriculum repetition and improved absolute maturity. First, since teachers were asked to reprise existing materials, this curriculum repetition could improve the preparedness of affected students. Second, as an unintended consequence, ACS led to a discontinuity in the age-for-grade between children who entered the school system in the 1978 and 1979 academic year. Cohorts who were in school in 1978 would, on average, be six months older than cohorts who entered school later. In the context of Indonesia (whose basic education system was relatively underdeveloped in 1978), increased absolute maturity upon school entry could help less-prepared children absorb the curriculum better. We find evidence consistent with the latter hypothesis. Given the self-productivity of skill formation, this early-life advantage likely persisted into later life through the human capital accumulation process (Cunha and Heckman, 2008).

We contribute to the existing literature in the following ways. First, we contribute to the nascent literature examining the determinants of cognitive functioning in a low-income setting.[1] Normal cognitive aging — non-pathological, age-associated cognitive decline — usually starts in middle age (Singh-Manoux et al., 2012). However, we have little causal evidence of the long-term impacts of early-life circumstances on cognitive abilities among middle-aged and older adults, primarily due to endogeneity concerns arising from unobservable factors (such as inherent ability) that may be correlated with both cognition and early-life circumstances. A notable exception is Banks and Mazzonna (2012) who use changes in compulsory education rules in England to find that an additional year of schooling had a positive long-term effect on memory for males. Our study provides evidence of long-term impacts on a broader set of cognitive indicators in an LMIC.

Second, we also contribute to the literature investigating the impacts of school starting age. Multiple studies have examined to role of delaying entry into kindergarten or primary school on various outcomes. However, these studies largely rely on birth dates and the age-cutoff for entry into school to provide exogenous variation in the age of entry (Bedard and Dhuey, 2006; McEwan and Shapiro, 2008; Fredriksson and Öckert, 2014), and cannot disentangle the effects of absolute maturity from those of relative maturity. Our policy shift provides a unique opportunity to plausibly disentangle these effects. Using a difference-in-difference strategy we find that while being relatively younger in class has adverse effects, an increase in the absolute age-for-grade conferred substantial benefits. Furthermore, by focusing on long-term outcomes, we add to the limited literature looking at long-term impacts of maturity in school on adult wages (Fredriksson and Öckert, 2014; Røed-Larsen and Solli, 2017; Bedard and Dhuey, 2012), mental health and teenage pregnancy (Black et al., 2011), and university attendance (Bedard and Dhuey, 2006).

The rest of the paper is organized in the following sections. Section 2 describes the context and details of the policy experiment of interest. Section 3 provides information on the data and the measures of cognitive abilities. The estimation strategy is outlined in Section 4 and the results

[1] Understanding the determinants of cognitive functioning is particularly relevant for LMICs as recent research shows a robust two-way relationship between poverty and cognitive abilities. On the one hand, poverty can tax cognitive abilities. On the other, declining cognitive abilities can reduce labor productivity and lead individuals to make poor economic decisions that trap them in poverty (Haushofer and Fehr, 2014; Mani et al., 2013; World Bank, 2014).

are presented in Section 5. Finally, we discuss potential pathways in Section 6 and Section 7 concludes.

2 Context and the Policy Experiment

2.1 Indonesian Education in the 1970s and 1980s

The state of education in Indonesia in the early 1970s was poor. Only around 60 percent of its population was literate, with an 80 percent literacy rate among young adults (15–24 years old) (Hill, 1996, p.208). Fiscal constraints from the economic turmoil in the previous two decades resulted in a significant underinvestment in education. Only 57 percent of primary-school children were in school in 1973 (Beeby, 1979, p.27) and schools often had to turn away children from enrolling due to lack of space (Carpenter, 1972, Table 1).

Windfall from the sharp increase in oil prices in 1973 opened up some fiscal space, allowing the government to increase its investment in education. It began with the massive expansion of the supply of primary schools that prioritized underserved regions. There were 65,569 primary schools in 1972 (Beeby, 1979, p.30). Between 1973 and 1984, the government gradually added a total of close to 133,000 schools through the Presidential Instructions (or *Inpres*) program.[2] Its effect on access to primary school was immediate: the overall gross enrollment ratio for primary schools grew from 86.1 percent in 1973 to 100.4 percent just before the shift in the academic year in 1978 (World Bank, 2020a).

The government also implemented policies to improve other aspects of the education system. Policies to increase the quantity and quality of teachers and learning materials were gradually implemented over time. There were a few policy shifts in late 1970s and early 1980s, to wit: (i) the abolition of fees for public primary schools in 1977 for grades 1–3 and in 1978 for grades 4–6; (ii) the announcement of compulsory primary education in 1984; and (iii) curriculum changes in 1975 and 1984 (Parinduri, 2014; Suradi Hp. et al., 1986).

2.2 The Policy Experiment

The policy experiment we focus on is the sudden 6-month academic-calendar shift (hereafter, ACS) in Indonesia in 1978. Until 1978, an academic year would begin in January and end in December. In July 1978, a newly-appointed Minister of Education and Culture, Daoed Joesoef, decided to move the beginning of the 1979 academic year from January to July. The policy was largely unanticipated: although it had been discussed internally, it took a new minister (in the

[2]These numbers were calculated based on the Inpres documents. About 62,000 primary schools were constructed between 1973 and 1979, with between 6,000 to 15,000 added annually. Between 1979 and 1984, more than 70,000 additional schools were built.

newly-appointed cabinet) to implement the policy (Tempo, 1978b).[3] The policy's stated rationale was to facilitate long and uniform school breaks and improve budgetary coordination by timing the academic and governmental fiscal years closer together (Departemen P dan K, 1978a).

ACS affected students who were enrolled in primary and secondary school in 1978.[4] These students had to stay in the same grade for an extra 6 months. During the six-month extension, no new learning materials (other than for civic education) were introduced (Departemen P dan K, 1978a,b). Instead, the ministry-issued guideline advised teachers to "assess (students') mastery (of the materials)", "intensify arts-and-crafts program", "increase . . . sports, arts, scouting, and other activities" (Departemen P dan K, 1978a, p.8–9). Primary school teachers were advised to review materials "that have not been mastered by students" (Departemen P dan K, 1978b, p. 11).

3 Data and Measurements

3.1 The Data

Our analysis uses the Indonesia Family Life Survey (IFLS), a longitudinal, socio-economic household survey that at the time of its inception in 1993 was representative of 83 percent of the Indonesian population. We use data from its fifth wave (IFLS5) collected in 2014/15, which include measurements of cognitive functions (Strauss et al., 2016). To implement our empirical strategy (see Section 4), we limit the sample to respondents who would have been in primary school at the time of the policy shift (born in January 1966–December 1971) and those who would have entered primary school in the six years following the policy shift (born in January 1972–December 1977). We exclude respondents with a missing month, district of birth, or outcome variables.

3.2 Measures of Cognitive Abilities

The IFLS data contain six measures of cognitive abilities to capture three aspects of cognitive functions: memory, fluid intelligence, and numeracy. To facilitate comparability of interpretation, we standardize each of these variables with the mean and standard errors of the control group, namely those who were not of school-going age and not already enrolled in school in 1978.

Memory. The first set of outcomes consist of two measures of memory, namely, *episodic memory* and *mental inactness*. *Episodic memory*, a critical component for reasoning, is measured through immediate and delayed recall (McArdle et al., 2007). Respondents heard a list of 10 nouns in

[3] In the words of the minister in a 1978 interview, "[ACS] has been prepared for two years. That it was implemented now was only a matter of courage" (Tempo, 1978b, p.16).

[4] Indonesia's pre-tertiary education adopted the 6-3-3 structure: 6 years of primary school followed by 3 years of junior and 3 years of senior secondary schools. ACS did not affect students enrolled in tertiary education since their graduation depends on the total number of credits completed.

Indonesian once and were asked to recall them immediately (immediate recall) and then again after some questions, usually after four minutes (delayed recall). From this we construct a "word recall" variable by adding up the total number of words recalled correctly under immediate and delayed recall phases, and this variable ranges from 0-20. Impairment of episodic memory has been found to be precursor for clinical diagnosis of Alzheimer's disease (Bäckman et al., 2001).

To capture *mental inactness*, IFLS asked respondents to perform two tasks. First, respondents were asked to recall the date and day of the week. From this we construct a "Day/Date recall" variable by summing up correct answers to these questions, and this ranges from 0-2. Second, respondents were asked to perform a "serial sevens" task by successively subtracting 7 (for five times) starting from the number 100. First proposed by Hayman (1942), the task is considered as a measure of attention and mental concentration, and is often used as part of the Mini-Mental State Examination (MMSE). Each subtraction is scored independently, so that an initial mistake does not invalidate all subsequent answers. From this we construct a "serial sevens score" variable by summing up all correct answers which ranges from 0-5.

Fluid Intelligence. IFLS collects two measures of fluid intelligence. First, it collects an abridged version of the Raven's Colored Progressive Matrices (CPM, or Raven's test). The Raven's test is a measure of fluid intelligence where respondents were asked to complete a visual geometric design by choosing the missing piece. There are a total of 8 questions and we use the total number of correct answers as our Raven's test measure.

Second, it also implements the adaptive numerical series task. This task requires respondents to complete a numeric pattern by choosing the missing number. The tasks were arranged in an adaptive format: in the first stage all respondents answered 3 basic questions. Following this, in the second stage they received an additional set of 3 questions whose difficulties depended on the number of basic questions they answered correctly in the first stage.[5] We use the IFLS-provided standardized score or the *W-score* that accounts for the adaptive format for our analysis (Strauss et al., 2016, p. 40).

Numeracy. IFLS collects a basic *numeracy* measure comprising five questions. The first three questions ask respondents to solve basic arithmetic problems involving fractions and decimal points. It is followed by two word problems that test respondents' ability to work with percentages. We use the total number of correct answers as the measure of numeracy skill.

Control Variables. We control for demographic variables that may affect cognition such as gender and age (Lei et al., 2012). We include a Javanese-ethnicity dummy variable to capture whether the individual is part of the plurality ethnic group from the better-developed Inner Islands. To account for early-life conditions, we include eight indicator variables: (i) whether the respondent was born in a village; (ii) whether the mother or father of the respondent had

[5] The IFLS implemented a simplified version of the numerical series task implemented in the 2010 wave of the US Health and Retirement Study (Strauss et al., 2016).

completed secondary education; (iii)-(viii) whether the respondent attended kindergarten, first experienced hunger before the age of 6, first experienced hunger between the ages 6-15, reported having suffered from poor health during childhood, and had access to electricity, and piped water during childhood. These covariates are important as existing literature from both developing and developed countries finds a robust positive association between socioeconomic conditions during childhood and cognitive functioning in later life (Strauss et al., 2018; Luo and Waite, 2005; Singh-Manoux et al., 2005; Case and Paxson, 2008).

The summary statistics for these variables are shown in Table 1. Panel A shows that on average, respondents are able to recall 9 words (immediate and delayed). The majority are able to correctly recall the date and day of the week. The average score on the serial sevens is 3.3, 4.7 on the Raven's matrices. The numerical series test *W-score* ranges between 299-635 with a mean of 514. On average respondents correctly solve 1.5 out of 5 questions on the numerical test. Panel B shows the summary statistics for the control variables. Approximately half of the sample are female and the average age of the sample is 41.6 years. About 45 percent of the sample is Javanese and 71 percent of were born in a village. For 47 percent of the sample, either the mother or the father had completed secondary education, and approximately 20 percent attended kindergarten.

4 Empirical Strategy

We estimate the impact of the shift in the academic calendar on long-term cognitive outcomes using a regression discontinuity (RD) design. The policy change affected those who were in school in 1978. However, school enrollment is potentially endogenous as children may select to enroll or drop out based on (unobserved) ability. Following Parinduri (2014), we use the legal minimum age for primary-school enrollment of 6 years old to determine the treated cohort. To enroll by January 1978, the latest birthday cut-off is 31 December 1971. As such, (most) individuals who where born in and after 1972 will not have experienced ACS. We use the individual's birth month as a way to identify treatment status, and choose January 1972 as the cut-off for our RD estimates.

Despite the stated requirement, the minimum enrollment age policy was not enforced. Many children enrolled in school at either younger or older age. In Figure 1 we plot an indicator of whether the person was enrolled in school in 1978 by quarterly bins of birth dates in a six-year window around the January 1972 threshold. Around the cut-off, only approximately 80 percent of individuals born before January 1972 were in school and about 20 percent of children born immediately after the cut-off were already enrolled in primary school in 1978.

We therefore implement a fuzzy RD design, by estimating the following RD flexible linear parametric specification:

$$Y_{imj} = \beta_0 + \beta_1 E_i + \beta_2 d_i + \beta_3 (d_i \times E_i) + \sum^{L} \beta_l X_{imj} + v_j + \gamma_m + \varepsilon_{imj} \tag{1}$$

where the corresponding first-stage regression is:

$$E_i = \alpha_0 + \alpha_1 T_i + \alpha_2 d_i + \alpha_3 d_i \times T_i + \sum^{L} \alpha_l X_{imj} + v_j + \gamma_m + \eta_{imj} \tag{2}$$

In the above equations Y_{imj} is the outcome of interest, E_i is an indicator variable that takes the value 1 if the individual is enrolled in primary school in 1978 and 0 otherwise, and d_i is the running variable, capturing distance of the individual's birthdate from the cutoff of January 1972 in months. We include an interaction of the running variable and the enrollment indicator ($d_i \times E_i$) to allow the slopes of the linear functions to differ below and above the threshold. Since E_i and $(d_i \times E_i)$ are endogenous, we instrument them with T_i (a dummy variable that takes value 1 if born before Jan 1972) and $(d_i \times T_i)$. Equation 2 shows the first stage equation for E_i. Therefore, the coefficient of interest, β_1 in equation 1, captures the local average treatment effect (LATE). $\mathbf{X_{imj}}$ is the vector of a minimal set of individual control variables discussed in Section 3.[6]

We also included district and month-of-birth fixed effects, i.e., v_j and γ_m respectively. The month-of-birth fixed effects help control for seasonal conditions that may affect long-term cognitive abilities. For example, children born during the rainy season may fall ill (when outbreaks of waterborne diseases are more common) more often during the neonatal period adversely affecting post-natal development and growth. Similarly, the district of birth fixed effects control for differences in access to healthcare and schooling during childhood. The model is estimated using triangular weights for the running variables in order to give more weight to observations near the cutoff and following Lee and Card (2008), and we cluster standard errors at discrete values of the running variable.

For our main estimates, we implemented a 6-year bandwidth on either sides of the cut-off. We based our choice of bandwidth on the fact that primary school in Indonesia is six years and that in the 1970s, a lot of students dropped out of school after primary school. World Development Indicators data show that gross enrollment rate for secondary school in Indonesia in 1978 was 24 percent (World Bank, 2020b). Therefore, we limit the analysis sample to those who would have been in primary school at the time of the intervention (born in 1966–71) and those who would have entered primary school in the six years following the intervention (born in 1972–77). We check the robustness of our results to a wide range on alternative bandwidths.

To investigate the heterogeneous effects of the shift in the academic calendar, we estimate the following model with an interaction term:

$$Y_{imj} = \beta_0 + \beta_1 E_i + \beta_2 (E_i \times X_1) + \beta_3 d_i + \beta_4 (d_i \times E_i) + \sum^{L} \beta_l X_{imj} + v_j + \gamma_m + \varepsilon_{imj} \tag{3}$$

where X_1 is the variable that captures the heterogeneity of interest. For the fuzzy-RD regressions, we add the interaction of the indicator of enrollment in school and the subgroup of interest $(E_i \times X_1)$

[6] We also include a dummy variable to control for non-response in location of birth (village/urban).

as an additional endogenous regressor, and add the interaction of the indicator of eligibility for enrollment in primary school and the subgroup of interest as an additional instrument ($T_i \times X_1$). The coefficient β_2 then captures the relative impact of the policy on the subgroup of interest.

Before moving to the main results, we check the validity of the estimation strategy. First, the results may be biased due to selective attrition (for example, due to death). Figure A1 in the appendix shows the distribution of the analysis sample around the cutoff along with a linear fit. Visually, we do not find the density to differ. Testing this more formally using the method proposed by Cattaneo et al. (2017), we do not find the density to be significantly discontinuous at the cutoff (*p-value*=0.15).

Second, we check the validity of the instrumental variable strategy. The aforementioned Figure 1 shows how well the RD-cutoff works in determining primary school enrollment. The cutoff predicts enrollment well, albeit not perfectly with about 80 (20) percent of the students enrolled in primary school on the left (right) of the RD cutoff. Table 2 shows the estimates of the first-stage regression more formally. We find that children aged 6 and above were 74 percentage points more likely to be enrolled in school in 1978 relative to those younger than 6 years, and this result is not affected by the inclusion of control variables discussed above.

Third, we check if the individuals around the cutoff differ on a number of observed covariates. Since we have several pre-determined covariates, we check for dissimilarities using the method of Johnston and Mas (2018). We first regress each outcome on all the controls to construct an index of predicted outcome. Then we check if this predicted index is discontinuous around the threshold using the following reduced form equation:

$$Y_{imj} = \beta_0 + \beta_1 T_i + \beta_2 d_i + \beta_3 d_i T_i + \varepsilon_{imj} \quad (4)$$

Results in Appendix Table A1 and Figure A2 indicate that the pre-determined covariates are continuous around the threshold.

5 Results

5.1 Baseline Results

To reduce the dimensions of our outcomes, following Kling et al. (2007), we group the six measures into three groups of measures: memory, fluid intelligence, and numeracy. Figure 2 illustrates the long-term impact of the policy on these cognitive outcomes. We plot the mean of these cognitive measures in each quarterly bin against the distance to the cutoff. Also plotted is the local linear fit estimated separately on either side of the discontinuity. As expected, upon moving from younger cohorts (left) to older cohorts (right), we see that there is a decline in cognitive outcomes. However, just at the cutoff all figures show a clear upward jump indicating that the policy induced an increase in long-term cognitive abilities.

We present our LATE estimates for these three measures of cognitive functioning formally in Table 3. All of these outcome measures have been standardized with respect to the control group and therefore, the effects are cited in terms of the standard deviation. To convert these effects into annual effects, we multiply the effect sizes by two. Standard errors are clustered to allow for possible correlations among those born in a particular month in a given year; in Appendix Table A2, we show that inference is robust to the assumption of multiway correlations at month and district of birth (Cameron et al., 2011). Columns 1-3 present estimates without including any control variable and columns 4-6 present estimates with the individual controls.

Our results suggest that a one-year delay leads to between 0.21 and 0.28 standard deviation (SD) increase in cognitive abilities in middle age.[7] These long-term effects are slightly smaller than the short-term effects found in McEwan and Shapiro (2008), who reported a more than 0.3 standard deviation impact of a one-year delay in primary school entry on tests in fourth and eighth grades. Moreover, evidence from a subset of our cognitive measures suggest that ACS primarily increased the *stock* of cognitive abilities rather than slowing down their *rate* of decline in middle age. We incorporate IFLS4 (collected in 2007/8) to construct a panel, albeit only for the two sub-components of memory (word and day/date recall) that were also available in IFLS4. We then take first differences to construct measures of memory decline between 2007/8 (IFLS4) and 2014/5 (IFLS5). Our RD estimates in Appendix Table A4 shows that ACS had no impact on memory declines. Finally, Panel A of Appendix Table A5 explores heterogeneous effects by gender. We do not find evidence for a gender-differentiated ACS impact.

5.2 Robustness Checks

In this section, we investigate whether our results are sensitive to changes in specification. We therefore re-estimate the model using alternative RD bandwidths, outcome measures, and cohort definitions. We find that our results are robust to these modifications.

Alternative bandwidths. We conduct a sensitivity analysis with respect to to the bandwidth by re-estimating our results at alternative bandwidths of 8 and 4 years. Table 4 shows that the effects are significant at these alternative bandwidths and that the magnitudes are similar to those in columns 4-6 of Table 3. As a more intensive check, we re-estimate our results at every interval of 6 months between the bandwidths of 3-9 years. The estimated effects along with the 95 percent confidence intervals are shown in Figure 3. We find that the estimated effects are generally stable over the whole range. As expected, at smaller bandwidths the confidence intervals are slightly larger but become more precise as we increase the sample size.

Indicators of cognition. We check if our results are driven by a particular component of the indices of memory and fluid intelligence by estimating the effects on each component separately in

[7] The corresponding intent-to-treat effects (assuming a sharp RD) are presented in Table A3 in the appendix.

Table A6 of the Appendix. We find that the effects are statistically significant for all components.[8]

Cohort definition. Lastly, we address the concern that the impact of the policy shift maybe affected by changes in the control group. In particular, the average age of the group eligible to enter school in 1979 (born in January 1972–June 1973) would be slightly higher than those who would have been eligible in 1980 (born in July 1973–June 1974) because of the presence of those born in January–June 1972 in the former. We show the results from re-estimating the same model without those born in January–June 1972 in Appendix Table A7. We find that dropping this cohort does not alter the impact of the policy shift, suggesting that the results are not driven by such changes in the control group.

5.3 Identifying the Policy Effect

In this section, we discuss potential threats to our identification strategy. First, we discuss why our results were unrelated to the main education policies in the period, including Indonesia's massive school-building program. Second, we show that our RD estimates captured the ACS effects through the school system, instead of other contemporaneous shocks experienced by affected cohorts. Finally, we address the concern that our findings were the result of how ACS shifted the concurrence of the start of the academic year and the timing of harvest.

***Inpres* school expansions.** Section 2 lists four major education policies that were introduced in 1970s and 1980s: (i) the *Inpres* school building program; (ii) the abolition of school fees in 1977 (for grades 1–3) and 1978 (for grades 4–6); (iii) the announcement of compulsory primary education in 1984; and (iv) the introduction of new curricula in 1975 and 1984. The timing of policies (ii)–(iv) made them irrelevant to our RD identification that relies on the discontinuity between the 1978 and 1979 cohorts because of their uniform effect (or non-effect) on treated and control cohorts around the discontinuity (Parinduri, 2014).

However, the *Inpres* program could affect our identification. The program built an average of around 12,000 primary schools annually between 1973 and 1984.[9] Although the increase in the stock of primary schools was gradual (and somewhat uniform) over time, its sheer size meant that an increase in the number of primary schools in 1979 could plausibly confound our results.[10] However, we do not find this to be the case. Using the school construction data of Duflo (2001), we constructed a "high intensity" indicator variable that is equal to 1 if the program intensity in the individual's district of birth was above the median.[11] If our results were affected by the *Inpres*

[8] Graphical evidence of the effects on each of the six measures is shown in Appendix Figure A3.

[9] The number of schools built per year in the period ranged from 6,000 (in 1973/4 and 1974/5) to 22,600 (in 1982/3) primary schools.

[10] There were 65,569 primary schools in 1972; the additional 47,000 schools built between 1973 and 1978 meant there were around 112,569 schools by 1978. Therefore, the 15,000 primary schools that the government built in 1978/9 represented around 13 percent of the stock of primary schools at the time.

[11] The program targeted regions with low enrollment rates. Therefore, even though Duflo (2001) focused on the

program, they would be heterogenous by program intensity. Panel B of Appendix Table A5 shows no evidence of such heterogeneity.

Contemporaneous policies and shocks. Even though the education policies implemented around the time of our policy experiment were unlikely to affect identification, it is possible that contemporaneous non-education policies (or shocks) that affected treated cohorts may have confounded our RD estimates. As an example, a change in a health policy that affected children who were in the treated cohorts negatively (positively) could have biased our estimates upward (downward).

We show that our results were unlikely to be driven by outside events that were unrelated to schooling. Appendix Table A8 presents the differences in the means of individuals who were never in school that were born before v. after 1972 for each of the cognitive outcomes. The differences in all the outcomes are jointly insignificant. Meanwhile, Appendix Table A9 presents the results of a placebo RD estimation using a reduced form equation (with the 1972 birth-year cutoff) among those who never went to school. The point estimates are not statistically significant, except for a weak negative effect on memory; if anything, they suggest that our RD estimates may have provided the lower bound for the ACS impacts.

The timing of harvest. In predominantly agricultural economies, rural children were often temporarily pulled out of school during harvest time (see, e.g., Beeby, 1979, p. 167). Beginning the academic year around the same time as the harvesting season could disadvantage children who grew up in a farm household. The disadvantages from missing out the early lessons could accumulate throughout the academic year. Had the policy shifted the start of the academic year toward (away from) concurrence with the harvesting season, it could bias the impact attributed to the schooling effect upward (downward).

We argue that ACS did not introduce an upward bias by shifting the start of an academic year from January to July. First, rice was traditionally Indonesia's most important crop (especially in Java). The three main harvest seasons were in February–May, June–September, and October–January (Ellis, 1990). However, about 60 percent of annual output were produced in the main harvest season of February–May, while only about 10 percent was produced in October–January. Hence, ACS moved the start of the academic year away from concurrence with harvest time. Second, the main cash crops are perennial crop such as palm oil, coffee and chocolate (Hill, 1996). Both suggest that the timing of the harvest is unlikely to introduce an upward bias to our estimates.

1973–1978 expansion, we interpret the "high (program) intensity" variable derived from her data as a proxy of regions with low pre-program enrollment rates.

6 Mechanisms

We have demonstrated the long-run, positive impacts of ACS on cognitive abilities; this section investigates its underlying mechanisms. We first show that ACS primarily improved outcomes for those who were affected in early grades. These findings are consistent with the *self-productivity* feature of skill formation, to wit, how a skill developed in one period augments skills developed in later periods (Cunha et al., 2006; Cunha and Heckman, 2008). We then argue that these effects had to do with how ACS increased their *absolute* age-for-grade, consistent with evidence that increased maturity in early grades can help children absorb the curriculum better (Black et al., 2011; Peña, 2017). Given the self-productivity of skill formation, this early-life advantage can persist into later life through the human capital accumulation process. We therefore examine some of the policy's potential pathways toward cognitive advantages later in life.

6.1 ACS as an Early Childhood Intervention

ACS extended the exposure to formal schooling (up to Grade 12) by 6 months. If we assume that responses to ACS were similar across grades, the discontinuity gaps identified by the RD design are the average treatment effects across the population and the RD estimates can be interpreted as the long-term impacts of an additional six months of schooling on cognitive outcomes. However, if treatment effects vary, the RD estimates presented so far are *weighted* average treatment effects, where the weights are proportional to the ex-ante probability that, given an individual's characteristics, his or her running variable is close to the threshold (Lee and Lemieux, 2010). In the latter case, the RD estimates are most informative of the ACS impacts on those who were at Grade 1 in 1978 (closer to the threshold) and less so of their impacts on those in higher grades.

Understanding which of these assumptions about the treatment effects was borne out in the data is important, because these two interpretations provide support for different policy prescriptions. If the treatment effects do not decline — or at least remain constant — as an individual's age of exposure (i.e., the distance from the threshold) increases, our results would provide support for using policies that lengthen the exposure to schooling to improve long-term outcomes (e.g., Angrist and Krueger, 1991; Parinduri, 2014). However, if the treatment effects declined with increasing grades of exposure — a hypothesis that is consistent with the empirical evidence that cognitive ability is more malleable earlier in the life cycle (Cunha et al., 2006) — then ACS might have improved long-term cognitive outcomes in manners similar to early childhood interventions.

To distinguish between these two interpretations, we need to develop an empirical strategy that addresses the challenge that arises from the uniform implementation of ACS across grades. Since the ACS was implemented across Grades 1 to 12, we cannot directly identify the ACS impacts at higher grades among school enrollees (e.g., using a difference-in-differences strategy).

However, we may be able to infer the ACS impacts at certain grades that are associated with high drop-out rates. In particular, we can use the low transition rate to secondary school around the study period to estimate the impacts of ACS exposure at Grade 6 among the likely drop-outs.[12]

We use the sharp increase in drop-out rate after primary school to determine the effects of the ACS at Grade 6 in two ways. First, we compare the outcomes of those who left with six years of school in 1979 to those who left with the six years of school in 1978 (before the policy change). If the ACS had an effect at this level we may expect the outcomes for the former group to be systematically better than those for the latter. Columns 1–3 of Appendix Table A10 show that for four out of the six outcome variables, we cannot reject the null hypothesis that the outcomes are identical for the two groups. Further, we also find that differences in all the outcomes are jointly insignificant.

Second, we assess ACS effects at Grade 6 using the RD framework. Given that children started school at age 6 and spent another six years in primary school, those who were older than 12 in 1978 were less likely to have been in school at that time of the ACS. This allows us to estimate the effects of exposure to ACS at Grade 6 using a fuzzy RD framework similar to the one laid out in equations 1-2, where the cutoff is now shifted 6 years prior to January 1966. Thus, the running variable (d_i) now captures the distance in months from a cutoff of January 1966, and the 1978 school enrollment indicator (E_i) is instrumented with a dummy variable, T_i, that takes value 1 if born in or after January 1966.[13] The estimated discontinuity gaps are LATE estimates of ACS exposure at Grade 6 among those who, in the absence of the intervention, would not transition to secondary school.

Unlike in the original experiment, these RD estimates can only partially identify the ACS effects at Grade 6. We treat these estimates as upper bounds of the ACS effects at Grade 6 — for two reasons. First, if a positive ACS effect at Grade 6 induced some students who would have otherwise dropped out to transition to the secondary school and beyond, these LATE estimates would confound the effects of extending Grade 6 by six months with the effects from continuing beyond primary school. Second, in this case, the ACS effects are estimated on a negatively-selected sample (of likely drop-outs at Grade 6). We show later in this section that the ACS effects are larger for students with less educated parents. This implies that the estimates for negatively selected population are likely to be larger than the average effects.

We estimated the effects of ACS at the point of transition to lower-secondary (Grade 6) using a six-year window around the cutoff of January 1966 and report the results in Panel A of Appendix Table A11. Although positive, the coefficient estimates are all imprecisely estimated. Panel B reports the results of a similar exercise for transitions to upper-secondary school (Grade 9), using

[12] According to the World Development Indicators, the rate of progression to secondary school in Indonesia in the 1980s hovered around 56.3 to 60.5 percent. Data were not available for progression to secondary school in Indonesia in the 1970s.

[13] Those born in or after January 1966 woud be less than 12 years old at the time of ACS and hence more likely to have been in primary school relative to those older than 12.

a six-year window around the cutoff of January 1963. We find that the sign for the coefficient on memory is negative. Taken together, we cannot reject that the (upper bound of) the impacts of ACS exposure at later grades are zero.

The Role of Other Early-Life Inputs in Cognitive Development. The rate of return to investments in human capital is higher when made earlier in life (Cunha et al., 2006). Deficiencies in physical and psychosocial inputs in early life, e.g., due to poverty, can be detrimental to children's cognitive development (Fernald et al., 2012; Evans and Schamberg, 2009; Farah et al., 2006; Glewwe and Miguel, 2007). Since cognitive skills are self-productive, input deficiencies prior to schooling can adversely affect preparedness in primary school, leading to learning disadvantages that accumulate over time (World Bank, 2017).

In the following, we consider whether in the long term, ACS potentially complemented or substituted for some of these early-life inputs. We use a set of variables to capture early childhood health and psychosocial inputs. For health, IFLS5 asks retrospective questions on the respondent's general health and experience of hunger during childhood. For the latter, respondents had experienced hunger during childhood were asked for the age range when they were first exposed. We distinguish between exposure before 6 years old (i.e., representing their pre-primary school nutrition environment) and after. Meanwhile, we use parental education as a proxy for psychosocial inputs.

Our results in Table 5 suggest that ACS complemented early-life health inputs, but substituted for psychosocial inputs. Panel A1 suggests that individuals with fair or poor health during childhood benefited less from the ACS. The coefficients on the interaction with poorer health status were negative across all outcomes, although it was only statistically significant for numeracy. Panels A2 and A3 suggest that ACS only become complements for health inputs delivered prior to primary school. Panel A2 shows that ACS were only unable to improve cognitive outcomes for those who first experienced hunger at the age of 0–5 years old, but not after. This finding is consistent with neuroscientific and other evidence showing the persistence of cognitive disadvantages due to malnutrition in the critical first few years of life (Farah et al., 2006; Evans and Schamberg, 2009). It also reinforces the interpretation of ACS as akin to an early childhood intervention. On the other hand, Panel B suggests that ACS tended to improve cognitive outcomes more for those whose parents were less educated. Although the coefficients on the interaction with parental education were negative across the board, it was very small for memory and only statistically significant for numeracy.

6.2 The Roles of Absolute and Relative Maturity

We argue that ACS increased school readiness, among others by increasing maturity (or age-for-grade). By extending each grade by 6 months, ACS provided a rare opportunity to study the impact of not only *relative* age-for-grade (being an older student in the cohort), but also *absolute* age-for grade. Figure 4 illustrates how ACS increased the absolute maturity of the treated cohort. Consider enrollees who entered primary school at 6 years old before and after 1979. Because of the policy, at any given grade beginning with Grade 2, cohorts who were in school 1978 (i.e., the treated group) would have been 6-months more mature compared to cohorts that enrolled in primary school in 1980.

The impact of age-for-grade have traditionally been difficult to disentangle in the school-entry age literature that relies on combining birthdates with cutoff dates for school eligibility. Students who just miss the birthdate cutoff and wait a year to enroll, not only start school at an *absolute* later age, but are also *relatively* older to others in the classroom (see Black et al., 2011).[14] The ACS provided an opportunity to disentangle between these two maturity effects.

Our strategy is as follows. For each academic year, we determine the six-month window around the birthdate cutoff. Next we classify those who missed the cutoff as being "relatively older" and those who made the cutoff (within a period of six months) as being "relatively younger" within their respective cohort. Using the same data we then estimate the the following difference-in-difference equation:

$$Y_{imjt} = \alpha_0 + \alpha_1 Young_{imjt} + \alpha_2 (Young_{imjt} \times Treat_t) + \sum^{L} \alpha_l X_{imjt} + v_j + \gamma_m + \delta_t + \eta_{imjt} \tag{5}$$

where $Young_{imjt}$ is a dummy variable that takes the value 1 for those who are classified as "relatively younger", and $Treat_t$ takes the value 1 for those who would have been exposed to the policy shift (those born before Jan 1972).[15] δ_t , γ_m , and v_j are year of birth, month of birth, and district of birth fixed effects, respectively. X_{imj} are the same set of controls used in equations 1-2 and the error terms (η_{imjt}) are clustered at the birth month-year level. The coefficient α_1 captures the average effect of being relatively younger, and the coefficient of interest, α_2, captures the additional effect from increasing absolute maturity due to the ACS.

The results are reported in Table 6. We find that being relatively younger in a cohort has a significant negative long-term effect on all outcomes. However, relatively younger students who were exposed to the ACS — and hence increased their age-for-grade — experienced large positive gains. These results indicate that at the margin, the six-month increase in absolute maturity led to larger long-term improvements in cognitive outcomes for those who were relatively younger in their cohort, and is consistent with existing evidence on the role of absolute maturity on academic performance (Black et al., 2011; Peña, 2017).

[14] A few papers have attempted to disentangle these effects by randomly allocating students across classes (Cascio and Schanzenbach, 2016), or exploiting spatial/temporal variation in school starting age (Peña, 2017).

[15] We do not use actual enrollment status and are thus estimating ITT effects. We also dropped respondents born in the 6 month window of Jan-June 1972.

6.3 Does Curriculum Repetition Explain the ACS Effects?

One may be concerned that the effects of ACS arose from curriculum repetition during the extra six months, rather than via increased maturity in early grades as we have argued so far. As we describe in Section 2, teachers were given no new materials and instead were asked to revisit materials that had not been mastered by students. For those in the early grades, material repetitions could have been the primary channel to increase their readiness for later grades. Separating the maturity from the curriculum-repetition channel is not straightforward since for most affected individuals, these channels are one and the same. However, we provide suggestive evidence that this channel is less likely to drive our results.

First, we directly compare individuals who experienced an increase in age-for-grade with those who experiencing both the increase in age-for-grade and curriculum repetition. Individuals who just failed to make the enrollment cutoff in 1978 — i.e., those who were born in the first six months of 1972 — would have been more mature when they enrolled in the following year, but would not have enjoyed the addition 6 months of curriculum repetition. We compare this group to those who were born in January–June of 1971 and experienced ACS. Columns 4–6 of Appendix Table A10 find that the two groups do not differ significantly on any of the outcome variables. Assuming that the ACS impact estimates were additive functions of maturity and curriculum repetition, these results suggest that the sole effect of maturity was at least as large as the combined effect of maturity and curriculum repetition.

Next, we examine whether the ACS differ by an individual's pre-primary-school academic preparedness to indirectly infer the effects of curriculum repetition. Since teachers repeated *existing* materials during the ACS, improvements that arose from curriculum repetition would have benefited less prepared students more. To test this hypothesis, we interacted the treatment status with whether an individual was enrolled in kindergarten. As shown in Panel C of Appendix Table A5, we do not find heterogeneous ACS impacts by enrollment in kindergarten.

Finally, if curriculum repetition was important, our results would differ by teacher experience. In particular, at the margin, students of inexperienced teachers would benefit more from curriculum repetition. Beeby (1979, p. 64) documented the government stipulation that in newly-built *Inpres* schools, only new teachers (who had never taught before) could be hired until a school was assigned a school principal. Although this rule was not strictly followed, on average these newly-built *Inpres* schools tended to receive less experienced teachers.[16] However, we discuss in Section 5.3 that we do find our results to be heterogeneous with respect to the *Inpres* program intensity.

[16]The policy was implemented to avoid draining some of the older schools of experienced teachers. Beeby (1979, p.66) reported that a small study of more that 50 villages found that "[a] third of the teachers appointed to the SD Inpres schools had more than two years of teaching experience already."

6.4 Pathways to Middle-Age Cognitive Advantages

How did the policy lead to long-term improvements in cognitive functions? We explore two potential pathways: educational attainment and job characteristics. First, Banks and Mazzonna (2012) show that educational attainment improves cognitive functions later in life. The dynamic aspect of human capital accumulation means that the short-term advantages from the ACS would accumulate with increasing education. For example, improved maturity may increase their ability to learn during formal education, hence reducing their likelihood to drop out (Deming and Dynarski, 2008; Whitebread, 2012). The effect would have been especially important given the aforementioned low transition rate from primary to secondary school (despite a high rate of primary school completion) in Indonesia around that period.

Table 7 presents the ACS effects on education attainments. Column 1 of Table 7 indicates that there were no significant effects on the probability of finishing primary school. This is not surprising given the high rates of primary school completion at that time. Furthermore, as discussed in Section 2.2, since Indonesia made primary schooling compulsory in 1984, we maybe underestimating the true ACS effect on primary school completion if the control group (who were between 6 and 12 years old in 1984) were prevented from dropping out of primary school.[17]

However, we find more robust effects for higher levels of schooling. Column 2 of Table 7 shows that the policy increased the likelihood of completing junior high school by 2.8 percentage points, but this is not statistically significant. However, in column 3 we find that policy significantly increased the likelihood of completing senior high school by about 7.3 percentage points (or a 13 percent increase over a control mean of 0.55). These results are similar to those reported by Parinduri (2014).

Second, improved cognitive abilities during adulthood could also be a result of cognitive stimulations at work. Potter et al. (2008) suggest, for example, that conditional on early-life intelligence, jobs that are more intellectually demanding and require greater human interactions are associated with better cognitive outcomes in later life. Relative to blue-collar work, white-collar jobs are expected to involve more complex work entailing mental stimulation. Therefore, if the cumulative ACS effects on human capital resulted in the sorting of treated cohorts into white collar jobs, we might expect them to have better cognitive abilities in adulthood.

The IFLS asks respondents about certain characteristics of their primary jobs such as if the job requires a lot of physical effort, lifting heavy loads, and stooping, kneeling and crouching. The responses were coded on a 1-4 scale (all the time, most of the time, some of the time, none of the time). We use these to construct an indicator for "blue collar work" that take the value 1 if the respondent answered "all the time" or "most of the time" to at least one of these three questions. Results reported in column 4 of Table 7 shows that the policy significantly reduced the probability

[17] The null effect on primary school completion also rules out the concern that the six-month delay may have discouraged students enrolled in 1978 from returning to school in the following academic year. If this were the case, then the treated cohort would be less likely to complete primary school.

of undertaking blue collar work by 7.5 percentage points (or approximately a 10 percent reduction over the control mean of 0.73).

7 Conclusion

A growing body of evidence shows that investments in early childhood can have profound impacts on brain development which can lead to life-long advantages. In this paper, we examined the long term effects of an Indonesian policy change in 1978, that increased school readiness in primary school, on cognitive ability in late adulthood. We find that a one-year increase in the absolute maturity of individuals entering grade 2 led to between 0.21 and 0.28 SD increase in cognitive abilities 30 years later. These results are important as cognitive abilities in middle and late adulthood are strong predictors of physical and mental health, and economic well-being.

Our findings have important implications for aging populations. With rapidly shifting demographics in LMICs, it is imperative to better understand how early life interventions can help protect against cognitive decline in late adulthood. Not only can this help the next generation of seniors live full-filling, productive lives, but also lessen the potential social and economic burden of future illnesses.

Our results suggest dynamic complementarities between health and education. We find that children who faced worse socioeconomic conditions and nutritional status in childhood were less likely to experience the positive effects of the policy shift, indicating that early-life conditions play an important role in determining long-term cognitive functioning through its effect on school readiness. Hence, policies that aim to raise cognitive skills can not be looked at in isolation, and one must take a broader perspective.

Finally, this study is also relevant to the discussion regarding the optimal school starting age, and consequently the optimal age-for-grade. Our evidence is consistent with the hypothesis that the policy improved school readiness by increasing absolute age-for-grade in early grades, which in turn helped children absorb the curriculum better. However, we caution against interpreting our findings as support for a uniform increase in school enrollment age. The benefits to long-term cognitive abilities estimated here must be compared to the costs of delaying school. Publicly provided childcare and/or pre-schools are often unavailable (or are of poor quality) in LMICs. In such contexts spending more time in the home environment could have detrimental effects on the long-term human capital of children from low income households.

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Tables and Figures

Table 1: Summary Statistics

	Mean	SD	Min	Max	Observations
Panel A: Outcomes					
Word recall score	9.14	3.22	2	20	6351
Day/Date recall score	1.63	0.52	0	2	6351
Serial sevens score	3.32	1.61	0	5	6351
Raven's score	4.68	2.07	0	8	6351
Numerical series score	513.67	66.53	299	635	6351
Numeracy test score	1.53	1.40	0	5	6351
Panel B: Control variables					
Age	41.65	3.41	36	48	6351
Female	0.49	0.50	0	1	6351
Javanese	0.45	0.50	0	1	6351
Parental education	0.47	0.50	0	1	6351
Kindergarten	0.19	0.39	0	1	6351
Childhood poor health	0.38	0.49	0	1	6351
Childhood hunger under 5	0.02	0.13	0	1	6351
Childhood hunger 6-15	0.07	0.25	0	1	6351
Had electricity	0.44	0.50	0	1	6351
Had piped water	0.11	0.31	0	1	6351
Place of birth village	0.71	0.45	0	1	6341

Notes: The sample includes respondents born between Jan 1966 - Jan 1978 surveyed under IFLS5. Parental education is an indicator variable that takes value 1 if either the mother or the father of the respondent had completed secondary education. Childhood poor health is a dummy variable for poor health during childhood. Childhood hunger under 5 and childhood hunger 6-15 are dummy variables that take value 1 if the respondent first experienced hunger before the age of 6 or between the ages 6-15, respectively.

Table 2: First Stage

	School Enrollment	School Enrollment
School going age 1978	0.738	0.735
	(0.028)***	(0.027)***
Observations	6286	6286
Controls	No	Yes
Birth Dist. & Month FE	Yes	Yes

Notes: Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table 3: Delayed Schooling and Cognitive Outcomes

	Memory	Fluid Intel.	Numeracy	Memory	Fluid Intel.	Numeracy
	(1)	(2)	(3)	(4)	(5)	(6)
Treated cohort	0.130	0.142	0.136	0.109	0.106	0.114
	(0.045)***	(0.050)***	(0.056)**	(0.044)**	(0.046)**	(0.055)**
Observations	6286	6286	6286	6286	6286	6286
Controls	No	No	No	Yes	Yes	Yes
Birth Dist. & Month FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Outcomes are the mean of the standardized variables for the subvariables following Kling et al. (2007). Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table 4: Delayed Schooling and Cognitive Outcomes: Alternative bandwidths

	Memory	Fluid Intel.	Numeracy
Bandwidth=4 years	0.127	0.098	0.164
	(0.056)**	(0.065)	(0.071)**
Observations	4247	4247	4247
Bandwidth=8 years	0.077	0.106	0.115
	(0.036)**	(0.039)***	(0.041)***
Observations	8589	8589	8589
Controls	Yes	Yes	Yes
Birth Dist. & Month FE	Yes	Yes	Yes

Notes: Outcomes are the mean of the standardized variables for the subvariables following Kling et al. (2007). Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table 5: The Interactions between ACS and Other Early-Life Inputs

	Memory (1)	Fluid Intel. (2)	Numeracy (3)
Panel A: Early-life health environment			
Panel A1: Poor childhood health			
Treated cohort	0.132	0.116	0.170
	(0.044)***	(0.056)**	(0.058)***
Treated cohort × Poor Health	-0.065	-0.027	-0.154
	(0.048)	(0.067)	(0.073)**
Panel A2: First hunger experience between 0–5 years old			
Treated cohort	0.109	0.107	0.120
	(0.045)**	(0.047)**	(0.053)**
Treated cohort × Hunger	-0.208	-0.664	-0.228
	(0.220)	(0.274)**	(0.239)
Panel A3: First hunger experience between 6–15 years old			
Treated cohort	0.104	0.103	0.094
	(0.044)**	(0.048)**	(0.054)*
Treated cohort × Hunger	0.079	0.060	0.385
	(0.114)	(0.154)	(0.169)**
Panel B: Parental Education			
Treated cohort	0.115	0.135	0.183
	(0.049)**	(0.055)**	(0.065)***
Treated cohort × Parental Education	-0.014	-0.061	-0.142
	(0.048)	(0.060)	(0.060)**
Observations	6286	6286	6286
Controls	Yes	Yes	Yes
Birth Dist. & Month FE	Yes	Yes	Yes

Notes: Outcomes are the mean of the standardized variables for the subvariables following Kling et al. (2007). Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table 6: Delayed Schooling, Relative Age, and Cognitive Outcomes

	Memory (1)	Fluid Intel. (2)	Numeracy (3)
Younger	-0.146	-0.195	-0.229
	(0.061)**	(0.075)**	(0.091)**
Younger × Treated	0.306	0.386	0.494
	(0.117)***	(0.151)**	(0.178)***
Observations	6086	6086	6086
$\beta(Younger) + \beta(Younger \times T)$	0.16	0.19	0.27
Wald test p-value	0.01	0.02	0.00
Controls	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes

Notes: Outcomes are the mean of the standardized variables for the subvariables following Kling et al. (2007). Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table 7: Pathways: Effects on educational attainment and work

	Education			Work type
	Elementary school	Junior high school	Senior high school	Blue collar work =1
	(1)	(2)	(3)	(4)
Treated cohort	0.008	0.027	0.072	-0.074
	(0.022)	(0.028)	(0.031)**	(0.035)**
Control mean	0.88	0.66	0.46	0.72
Observations	6286	6286	6286	5318
Controls	Yes	Yes	Yes	Yes
Birth Dist. & Month FE	Yes	Yes	Yes	Yes

Notes: Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Figure 1: School Enrollment at the RD Cutoff

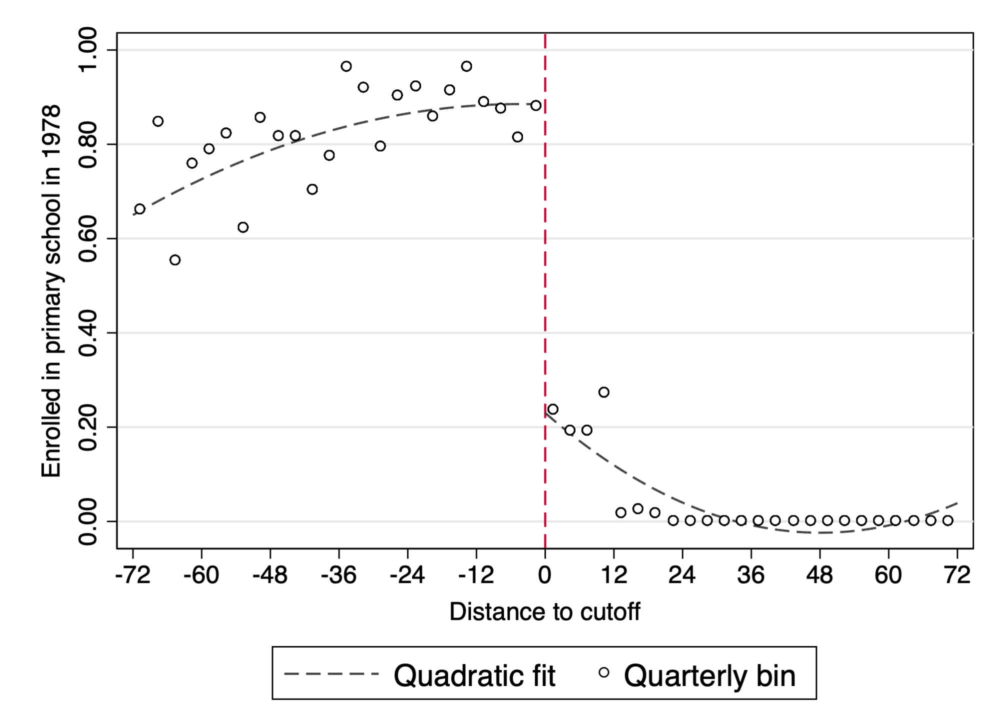
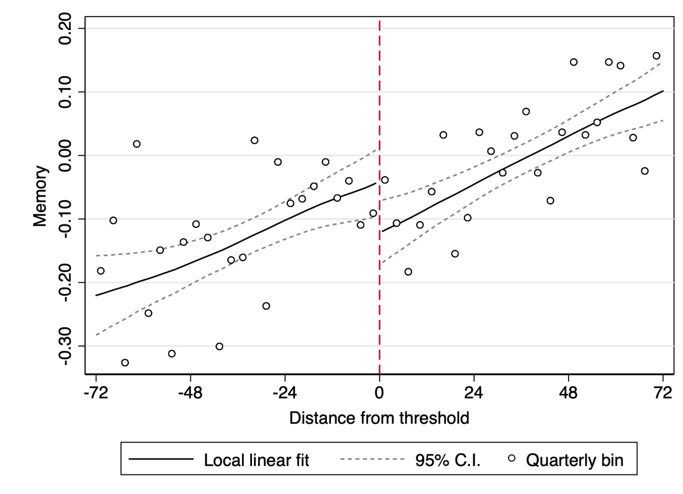
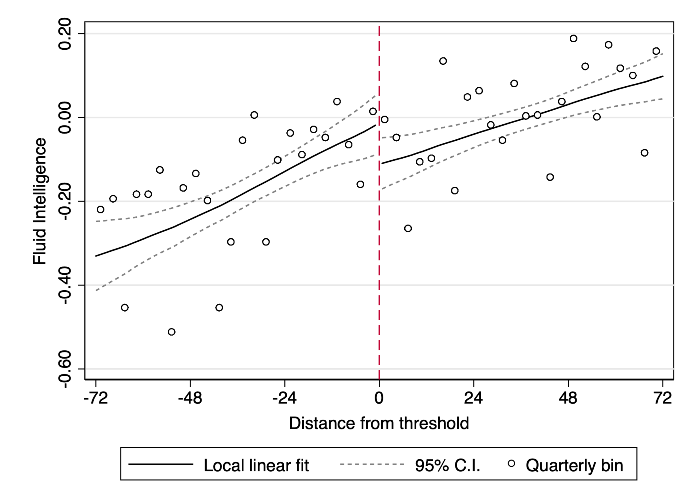


Figure 2: Delayed Schooling Age and Cognition

(a) Memory



(b) Fluid Intelligence



(c) Numeracy

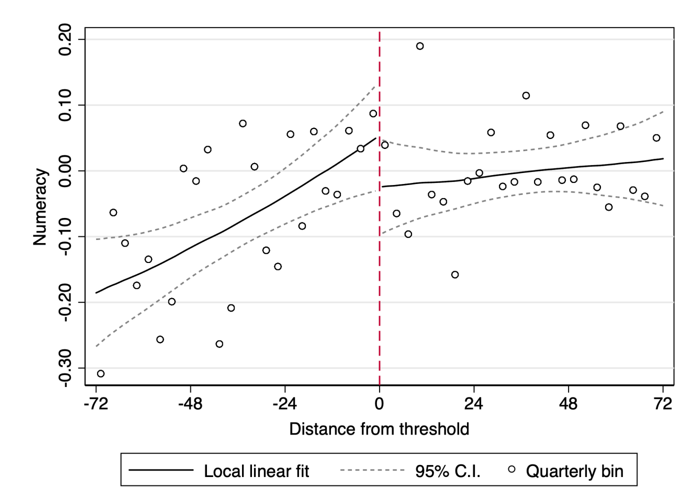
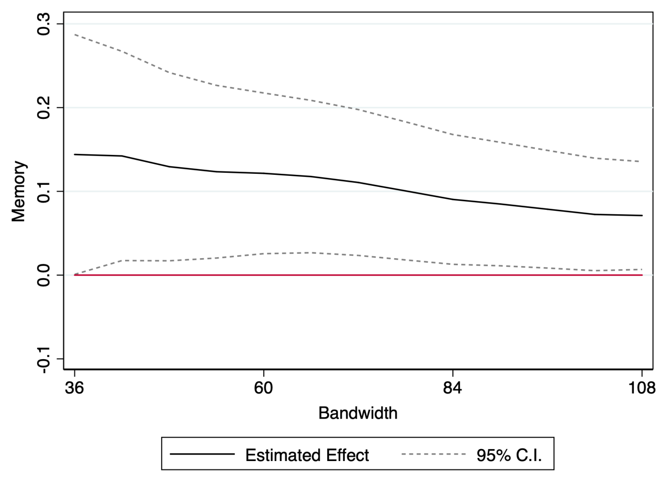
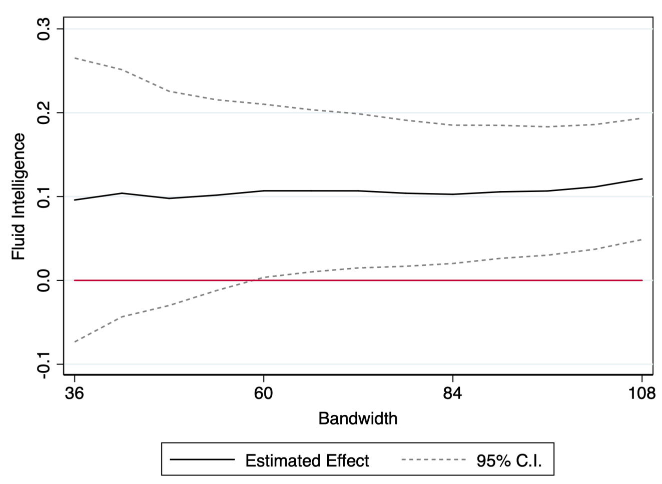


Figure 3: Effects by varying bandwidths

(a) Memory



(b) Fluid Intelligence



(c) Numeracy

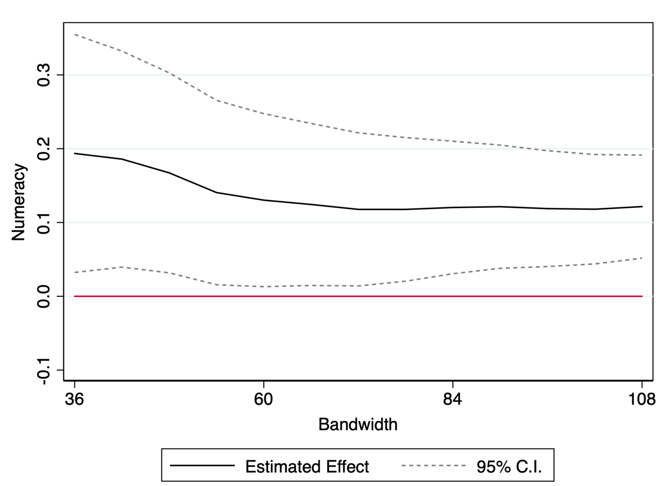
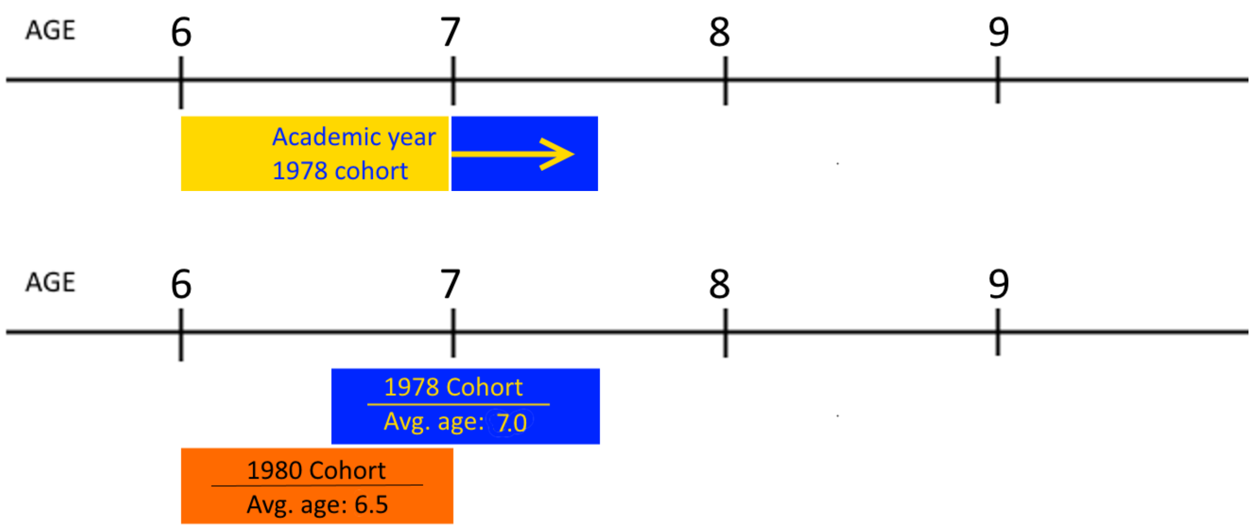


Figure 4: Average Age for Modal Enrollees Before and After 1979



Online Appendix: Not for publication

FIGURE A1: DENSITY

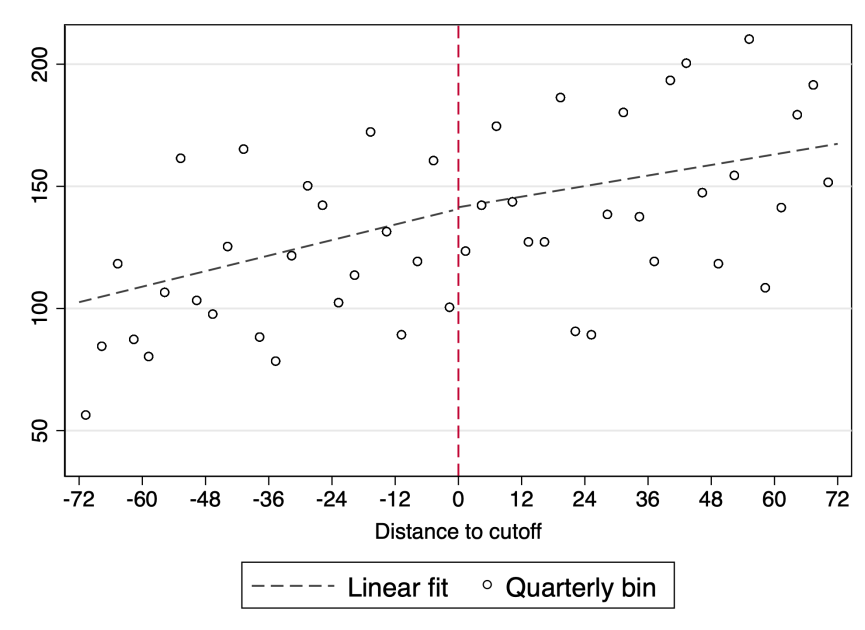
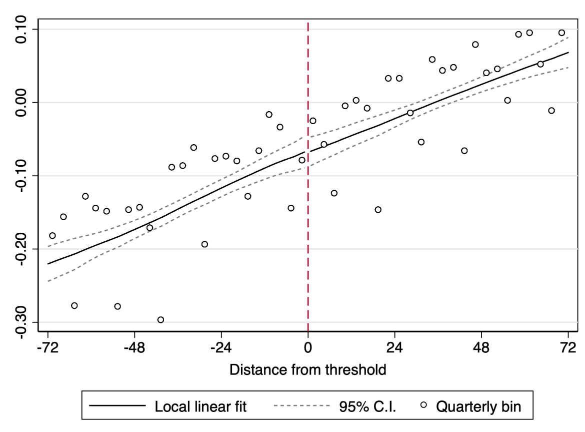
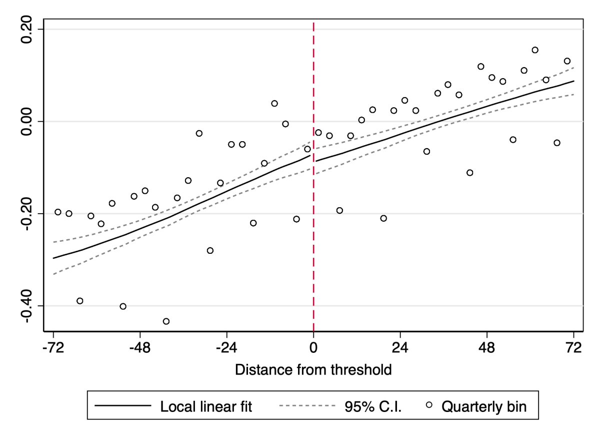


Figure A2: Discontinuity in background characteristics

(a) Memory



(b) Fluid Intel.



(c) Numeracy

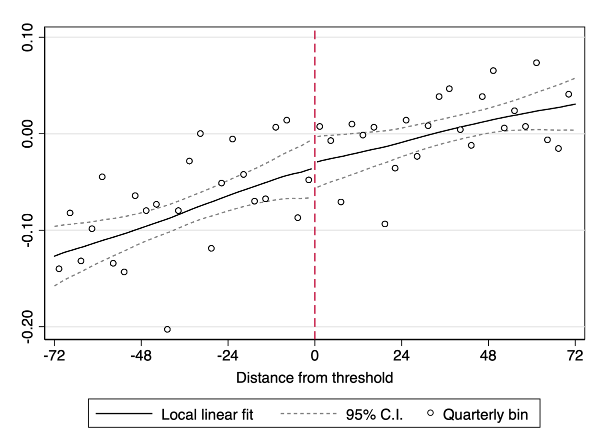
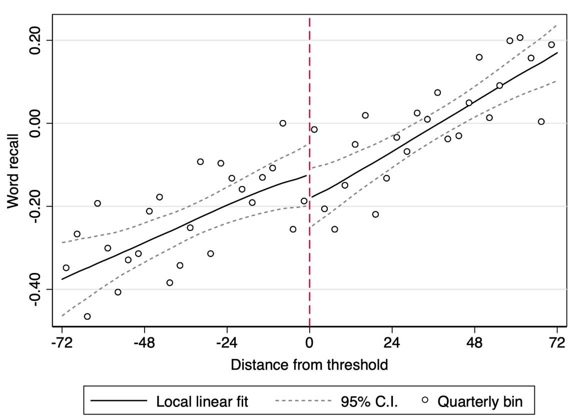
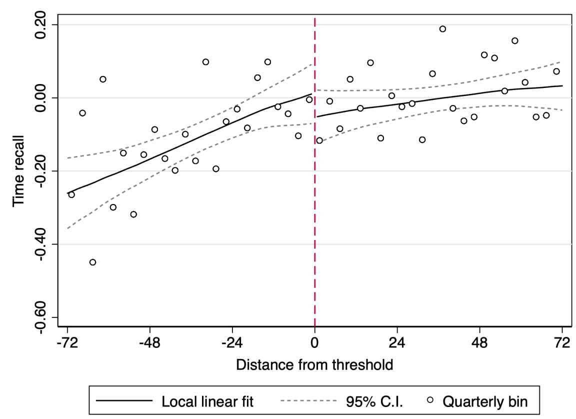


Figure A3: Delayed Schooling Age and Cognitive Functions: Detailed Measures

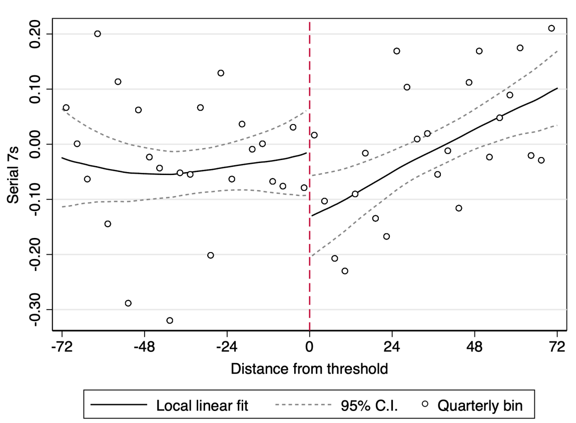
(a) Word Recall



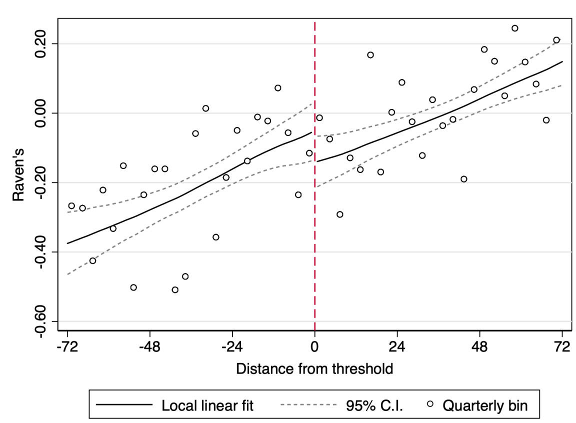
(b) Time Recall



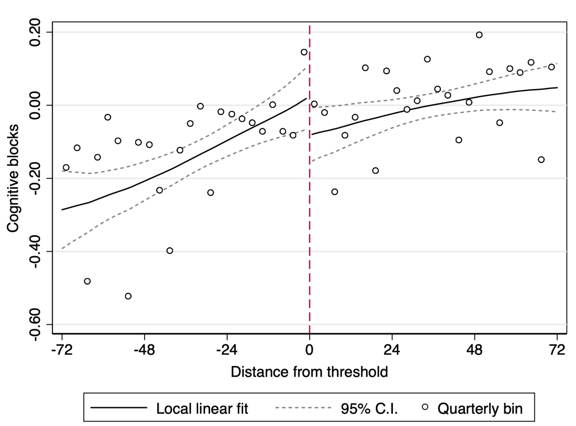
(c) Serial Sevens



(d) Raven's Test



(e) Numerical Series



(f) Arithmetic

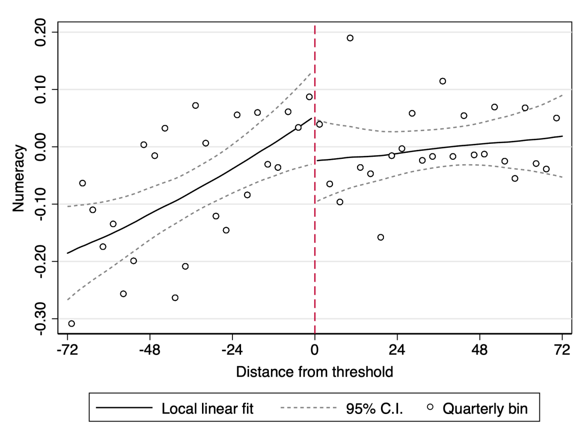


Table A1: Discontinuity in controls: using predicted outcome index

	Memory	Fluid Intel.	Numeracy
	(1)	(2)	(3)
School going age 1978	0.003	0.018	-0.005
	(0.040)	(0.057)	(0.036)
Observations	6286	6286	6286

Notes: Each column reports sharp RD estimates for the predicted outcomes which are constructed by regression the outcomes on all controls (gender, age, indicator variables for month of birth, district of birth, parental education, ethnicity, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood). Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table A2: Delayed Schooling and Cognitive Outcomes: Clustering at district of birth

	Memory	Fluid Intel.	Numeracy
	(1)	(2)	(3)
Treated cohort	0.109	0.106	0.114
	(0.052)**	(0.049)**	(0.058)**
Observations	6286	6286	6286
Controls	Yes	Yes	Yes
Birth Dist. & Month FE	Yes	Yes	Yes

Notes: Outcomes are the mean of the standardized variables for the subvariables following Kling et al. (2007). Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year and district of birth. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table A3: Delayed Schooling and Cognitive Outcomes: ITT Estimates

	Memory (1)	Fluid Intel. (2)	Numeracy (3)	Memory (4)	Fluid Intel. (5)	Numeracy (6)
Treated cohort	0.103	0.103	0.093	0.086	0.077	0.076
	(0.035)***	(0.038)***	(0.045)**	(0.035)**	(0.035)**	(0.044)*
Observations	6286	6286	6286	6286	6286	6286
Controls	No	No	No	Yes	Yes	Yes
Birth Dist. & Month FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Outcomes are the mean of the standardized variables for the subvariables following Kling et al. (2007). Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table A4: Delayed Schooling and Cognitive Outcomes: Change over time

	Memory	
	Word Recall (1)	Day/Date Recall (2)
Treated cohort	-0.091	0.045
	(0.076)	(0.068)
Observations	5303	5303
Controls	Yes	Yes
Birth Dist. & Month FE	Yes	Yes

Notes: Outcomes are the standardized values of the change between IFLS5 and IFLS4. Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year and district of birth. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table A5: Delayed Schooling and Cognitive Outcomes: additional heterogeneity

	Memory	Fluid Intel.	Numeracy
	(1)	(2)	(3)
Panel A: Gender			
Treated cohort	0.125	0.124	0.120
	(0.049)**	(0.053)**	(0.065)*
Treated cohort × Female	-0.033	-0.036	-0.010
	(0.050)	(0.062)	(0.081)
Observations	6286	6286	6286
Panel B: Inpres school-building program			
Treated cohort	0.122	0.121	0.107
	(0.047)***	(0.051)**	(0.064)*
Treated cohort × High intensity	-0.031	-0.034	0.018
	(0.056)	(0.058)	(0.076)
Observations	6229	6229	6229
Panel C: Kindergarten			
Treated cohort	0.113	0.090	0.136
	(0.046)**	(0.048)*	(0.058)**
Treated cohort × Kindergarten	-0.022	0.082	-0.112
	(0.054)	(0.073)	(0.093)
Observations	6286	6286	6286
Controls	Yes	Yes	Yes
Birth Dist. & Month FE	Yes	Yes	Yes

Notes: Outcomes are the mean of the standardized variables for the subvariables following Kling et al. (2007). Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table A6: Delayed Schooling and Cognitive Outcomes: Detailed

	Memory			Fluid Intelligence		Numeracy
	Word Recall	Day/Date Recall	Serial Seven	Raven	Numerical Series	
Without controls	0.099	0.128	0.165	0.136	0.147	0.136
	(0.057)*	(0.066)*	(0.089)*	(0.060)**	(0.061)**	(0.056)**
Observations	6286	6286	6286	6286	6286	6286
With controls	0.080	0.111	0.135	0.101	0.111	0.114
	(0.057)	(0.066)*	(0.086)	(0.057)*	(0.058)*	(0.055)**
Observations	6286	6286	6286	6286	6286	6286
Birth Dist. & Month FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Outcome variables are standardized. Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table A7: Delayed Schooling and Cognitive Outcomes: excluding six months

	Memory	Fluid Intel.	Numeracy	Memory	Fluid Intel.	Numeracy
	(1)	(2)	(3)	(4)	(5)	(6)
Treated cohort	0.123	0.151	0.120	0.105	0.118	0.097
	(0.041)***	(0.053)***	(0.067)*	(0.040)***	(0.049)**	(0.064)
Observations	6021	6021	6021	6021	6021	6021
Controls	No	No	No	Yes	Yes	Yes
Birth Dist. & Month FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Outcomes are the mean of the standardized variables for the subvariables following Kling et al. (2007). Individuals born during Jan-June 1972 are excluded from the analysis. Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table A8: Differences in outcomes among those who never went to school

	Born before 1972 (1)	Born in or after 1972 (2)	Difference (2)-(1)
Word recall score	5.87	6.49	0.63
	(3.44)	(3.25)	
	164	67	
Day/Date recall score	1.15	1.15	-0.00
	(0.54)	(0.53)	
	164	67	
Serial sevens score	1.71	1.61	-0.10
	(1.69)	(1.62)	
	164	67	
Raven's score	2.48	2.87	0.38
	(1.82)	(2.04)	
	164	67	
Numerical series score	422.13	412.75	-9.39
	(94.26)	(87.15)	
	164	67	
Numeracy test score	0.83	0.72	-0.11
	(1.03)	(0.87)	
	164	67	
F-test joint significance			1.12
F-test p-value			0.35

Notes: The sample includes respondents surveyed under IFLS5 who never went to school. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table A9: Differences in outcomes among those who never went to school

	Memory	Fluid Intel.	Numeracy
	(1)	(2)	(3)
School going age 1978	-0.522	-0.122	0.010
	(0.297)*	(0.458)	(0.284)
Observations	229	229	229
Controls	Yes	Yes	Yes
Birth Dist. & Month FE	Yes	Yes	Yes

Notes: Outcomes are the mean of the standardized variables for the subvariables following Kling et al. (2007). Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table A10: Differences in samples

	Left school after 6 years ...			Joined grade 1 in ...		
	in 1979	in 1978	Difference	in 1979	in 1978	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Word recall score	7.78	7.89	-0.11	9.10	9.41	-0.32
	(2.87)	(2.99)		(3.12)	(3.03)	
	68	74		178	189	
Day/Date recall score	1.62	1.36	0.26***	1.59	1.53	0.06
	(0.55)	(0.63)		(0.58)	(0.63)	
	69	77		186	199	
Serial sevens score	3.45	3.39	0.06	3.22	3.10	0.12
	(1.50)	(1.66)		(1.71)	(1.73)	
	69	77		185	198	
Raven's score	3.63	3.77	-0.14	4.78	4.93	-0.15
	(1.92)	(2.23)		(2.12)	(2.04)	
	68	71		176	187	
Numerical series score	505.63	490.97	14.66*	515.39	518.94	-3.55
	(56.18)	(44.72)		(58.26)	(54.39)	
	68	73		178	186	
Numeracy test score	1.09	1.27	-0.18	1.62	1.64	-0.01
	(1.14)	(1.15)		(1.42)	(1.37)	
	68	71		176	187	
F-test joint significance			1.62			0.53
F-test p-value			0.15			0.79

Notes: The sample in column 4 includes respondents who were born between Jan-June 1972 and started grade 1 in 1979. The sample in column 5 includes respondents who were born between Jan-June 1971 and started grade 1 in 1978. */**/*** denotes significant at the 10/5/1 percent significance levels.

Table A11: Estimated effects of ACS at higher grades

	Memory	Fluid Intel.	Numeracy
	(1)	(2)	(3)
Panel A: FRD estimates at grade 6			
Enrolled in 1978	0.237	0.254	0.096
	(0.291)	(0.272)	(0.347)
Observations	5131	5131	5131
Panel B: FRD estimates at grade 9			
Enrolled in 1978	-0.383	0.082	0.194
	(0.466)	(0.318)	(0.454)
Observations	4665	4665	4665

Notes: The analysis in Panel A includes individuals born between 1960-1971, while Panel B includes individuals born between 1957-1968. Outcomes are the mean of the standardized variables for the subvariables following Kling et al. (2007). Controls include gender, age, and indicator variables for ethnicity, being born in a village, parental education, attending kindergarten, having poor health during childhood, first experiencing hunger before age 6, first experiencing hunger between ages 6-15, and having electricity and piped water during childhood. Standard errors clustered by birth month-year. */**/*** denotes significant at the 10/5/1 percent significance levels.