Starting Early: Returns on Kindergarten Attendance in Indonesia

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Abstract

Indonesia is a rapidly developing economy, having averaged 5.26% economic growth from 2000 to 2019; over the same time, it has achieved near-universal primary school attendance. However, there are concerns about the quality of Indonesian education, with no improvement in standardized test scores between 2012 and 2022. Early childhood interventions are a critical part of human capital accumulation and skills-building, and the efficacy of interventions such as kindergarten in developing countries like Indonesia is under-studied. Using data from the Indonesian Family Life Survey (IFLS) and Village Potential Statistics (PODES), I examine the effects of kindergarten on educational outcomes in Indonesia, focusing on schooling and cognitive performance. My empirical strategy entails ordinary least-squares (OLS), mother fixed-effects (FE), and instrumental variable (IV) estimation, where my instrument is the number of kindergartens per 10,000 individuals in each locality. I find that kindergarten has a significant positive association with schooling, associated with an additional 1.89 years of education. Additionally, I find evidence that kindergarten's positive association with educational outcomes fades out as time passes, there being little to no evidence of significant positive effects after the conclusion of junior high school. I also find little to no evidence that attending kindergarten has a significant association with performance on cognitive tests–suggesting there is a gap between schooling and skills learned in the classroom. My results motivate a closer look at this gap, as well as exploring the effects of kindergarten attendance on earnings or social outcomes such as delinquency.

Keywords: early childhood education, human capital, kindergarten, development, Indonesia

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1   Introduction

Indonesia is a rapidly developing economy: between 2000 and 2019, it averaged 5.26% economic growth and its post-COVID *economic* recovery is robust. Yet, human capital formation has lagged behind the country's impressive economic performance, resulting in slowing productivity growth. The pandemic hit the Indonesian education system particularly hard: starting in March 2022, Indonesian schools were closed for 21 months. Consequently, students lost approximately 11 months of learning–and poor children were even more negatively affected, widening inequality in learning outcomes (Bank, 2023). As a result of these weaknesses in the educational system, 2022 Programme for International Student Assessment (PISA) scores were lower across the board than 2018 scores; in 2022, Indonesia ranked near the bottom in nearly every indicator among the 80 countries that completed PISA tests (Wijaya et al., 2024).[1] These declines are occurring amidst increases in education spending, student enrollment, and gender parity; clearly, there is a divergence between going to school and learning in school (Afkar et al., 2020).

One solution to this trend of worsening or stagnating educational outcomes might be expanded early childhood interventions: a World Bank report in 2020 proposed compulsory and accessible "two years of quality early childhood education" as one way to address Indonesia's challenges in learning and skills attainment (Afkar et al., 2020). Currently, such interventions are sparse; in this project, I focus on *formal* classroom early childhood programs, referred to as kindergarten. In Indonesia, kindergarten is not compulsory and–the same as in the United States–it is typically completed the year immediately prior to students beginning primary school. Currently and historically, the overwhelming majority of kindergartens are private, suggesting a lack of investment in accessible kindergarten.[2]

Early childhood education is a critical part of the process of human capital accumulation; there is a great deal of evidence that in early childhood, when brains are at their most malleable, human capital investments exhibit higher rates of return than equivalent investments later in life (Cantor et al., 2019). Certainly, there is empirical evidence supporting the efficacy of early childhood interventions in improving human capital and later-life outcomes, particularly for *disadvantaged* children (Duncan et al., 2023).

This invites the question of whether kindergarten has previously been effective in improving educational

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[1]PISA tests are fielded by the OECD and focus on 15-year-old academic performance. Declining test scores in Indonesia predate COVID, as Indonesia saw declines in PISA mathematics scores between 2015 and 2018 as well.

[2]See Figure 2. The current evidence comes from the 2020 World Bank Report referenced above: in 2019-2020, 95.7% of kindergartens were private. In 1990 and 2000 that figure was 95.06% and 96.63%, respectively.

outcomes in Indonesia–an under-studied question that is critical to informing future educational policy. Thus, in this research I examine the effects of kindergarten attendance, with an emphasis on looking at short-, medium-, and long-term effects. I focus on 1) schooling and 2) cognitive performance. I define schooling for the purpose of this paper narrowly as *only* the *completion* of schooling. I conceive of cognitive performance as a proxy for learning. Thus, one can think of the distinction between schooling and cognitive performance as the distinction between a child sitting in a classroom–completing schooling–and a child learning while sitting in the classroom–improving cognitive ability and building skills. To examine how the effects of kindergarten may vary over time, a critical concept in the early childhood intervention literature, I have outcome variables that vary over time, such as elementary school completion vs. junior high school completion, and cognitive tests taken during three different years.

To study long-term effects, it is necessary to have a detailed multi-wave household survey in order to track individuals from pre-early childhood intervention to adulthood. To this end, I employ the Indonesian Family Life Survey (IFLS) to track individuals from 1997 to 2014, while also collecting household- and community-level data. The IFLS is richly detailed, and I use four waves of the survey—from 1997 (approximately when children in my sample are attending kindergarten), 2000 (when they are in elementary school), 2007 (when they are approximately in junior high school), and 2014 (when they are adults who are either in college or have left the educational system). I also use the Village Potential Statistics (PODES) survey to gather data on community-level presence of kindergartens. Merging across waves of the IFLS and narrowing my sample down to individuals who were between 3 and 9 years old in 1997, my sample consists of 3,158 individuals.

I use three empirical methods: ordinary least squares (OLS) estimation, mother fixed-effects (FE), and instrumental variable (IV) estimation. While my OLS results are strongly statistically significant and hold across various robustness checks, my results suffer from omitted variable bias, meaning that I am missing necessary explanatory variables in the model. This bias motivates my use of mother fixed-effects, which allows me to control for all unobserved characteristics of siblings' shared mothers; however, mother fixed-effects places additional restrictions on my sample which results in the loss of a significant portion of the sample. Thus, I supplement these two methods with instrumental variable (IV) estimation, using the number of kindergartens per 10,000 people in each locality in 1990 and 2000 as my two instruments. I find that intuitively and empirically these instruments are strong and valid, so long as I control for the number of primary/junior high/senior high schools in each locality.

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I find strong evidence that kindergarten has a positive association with schooling, although that effect decreases in magnitude and significance as time passes. Specifically, I find significant positive results for kindergarten's effects on total years of education, completion of elementary and junior high schools, school attendance in junior high, and the likelihood of a student continuing after the completion of elementary school. Critically, I find that kindergarten has little to no impact on schooling after junior high school, and no impact on cognitive test performance for any of the three years that individuals completed the test.

The results suggest two phenomena: 1) 'fadeout', the concept that the effects of early childhood intervention fade as time passes and 2) a divergence between completed schooling and learning. These findings provide evidence of the fears outlined above, that while schooling is rapidly improving in Indonesia, there is not an accompanying improvement in learning. Thus, before expanding preschool to improve human capital formation, my results suggest there needs to be more research into the *quality* of existing kindergarten programs. Further research may focus on the effects of kindergarten on earnings, economic mobility, and adult health outcomes.

I begin this paper with a review of the relevant literature and how my research is situated in previous scholarly work on human capital accumulation and early childhood interventions. I then describe my sample and the origins of the data used for this research. Next, I introduce the three empirical methods I have used—OLS, mother fixed-effects, and IV estimation. I then present the results of my analysis, before concluding with a discussion of the interpretations of their results, their significance, and future research related to my question.

2 Literature Review

My research question focuses on one determinant of educational outcomes, kindergarten attendance, placing my research in the literature of human capital investments and early childhood interventions. I will begin my review with a summary of the human capital literature's findings on the determinants of educational outcomes. I will describe two categories of determinants, 1) genetic endowments and 2) human capital investments. I will then lay the groundwork of my theoretical approach, based on the work of Cunha and Heckman (2007), in which childhood is multi-staged and skills learned in one stage dynamically interact with skills learned in another stage. I will then conclude with a review of previous empirical findings regarding the effects of kindergarten/preschool, with a particular focus on how these findings–concentrated

in developed countries–may or may not translate to developing countries such as Indonesia.

2.1 Determinants of Educational Outcomes

Broadly, we can categorize determinants of educational outcomes into two: 1) genetic endowments–often referred to as 'ability'–and 2) human capital investments. In the first category, we can think of genetic endowment as something fixed at birth; it is difficult to measure, but the idea is that something like an individual's intelligence quotient (IQ) is indicative of 'natural talent', *independent* of their lived experiences. There is strong evidence that endowments of talent, ability, or intelligence are positively associated with schooling–one influential estimate of the correlation of intelligence and schooling is 0.5 (Johnson et al., 2005). One study of twins in Australia found that between 50 and 65% of variance in schoolings between sets of twins could be explained by genetic endowments, a finding in line with preceding estimates (Miller et al., 2001). Behrman and Taubman (1989) estimate the share is 90 percent. One longitudinal study of about 70,000 children in the United Kingdom found the correlation between a child's latent, or natural, intelligence and their standardized test scores 5 years later to be 0.81 (Deary et al., 2007).

Studies examining the role of natural endowments in human capital accumulation, however, harbor fundamental flaws. First, they rely on a convoluted causal story; there is evidence that more time in school increases latent intelligence, creating a virtuous cycle. Additionally, the relationship between genetics and schooling is very heterogenous–there is evidence, for example, that as limits on women's opportunity to attain education decreased in the 1970s and 1980s, the relationship between genetics and schooling strengthened as constraints on access to education eased (Herd et al., 2019). The dominance granted genetics by the estimates described above may be wildly divergent in contexts where there are either greater constraints on access to education, or (as described below) the opportunity cost of education is so high as to entirely distort human capital investment decision-making.

A second issue with studying the effects of genetic endowments is that 'latent talent' is very difficult to measure–hence the predominance of twin studies, since twins' genetic variation is significantly easier to identify (Miller et al., 2001). For measuring genetic endowment outside of the twin context, however, some scholars have pointed out that standardized tests of intelligence resemble classroom work, putting into question the ability of these tests to distinguish between the endowments independent of the upbringing of a child and the results of that upbringing (Johnson et al., 2009). Because of these challenges, I do not make

studying genetic endowment a central part of my research.[3]

The second category of determinants of schooling is human capital investment—my research is situated in this category of work. While the genetic endowment of a child is fixed at birth, human capital stock, on the other hand, is anything but fixed, and changes according to investments. We can further divide these investments into two types: 1) human capital investments *within* the household, and 2) human capital investments from *outside* of the household. In this literature review, I focus on investments in early childhood, as my research focuses on one such early childhood intervention–kindergarten. Henceforth, I will refer to human capital investments made *without* the household as early childhood interventions, such as attending Head Start in the United States or kindergarten in Indonesia.

My motivation to focus on early childhood is clear: there is overwhelming scientific evidence that at an early age children's brains are more malleable and receptive to learning skills than at later ages (Cantor et al., 2019; Duncan et al., 2023). In particular, evidence has shown that children's brains are most malleable to new learning for the first five or six years of life–fitting into the early childhood education "window" lasting from birth to the age of 12 (Slegers, 1997). Brain development does not occur just in a classroom–neglect as a child results in delays in brain development: during this early childhood window, a child is undergoing dynamic transformations, with human capital investments both within the household and without the household interacting in powerful ways (Perry and Pollard, 1997). We cannot understand the latter (i.e., the effect of kindergarten), without understanding the former.

The motivation for targeted early childhood programs such as Head Start rely on this interaction; some disadvantaged children are born into situations where neglect occurs–resulting in inequity in brain development and human capital between the advantaged and disadvantaged before a child even enters a classroom. Therefore, human capital investments received in the classroom can compensate for a lack of investments made at household for disadvantaged children–promoting equity and efficiency (Heckman, 2011).

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[3]Mother fixed-effects will ostensibly control for some genetic endowment, albeit imperfectly since my study is not one of twins. While in theory I could go further and incorporate variables collected during a pregnancy (attempting to control for genetic endowments traced back to the "fetal origins hypothesis"), such data would prohibitively restrict my sample size (Almond and Currie, 2011). I do attempt to control for *some* genetic variation by including controls for health as well as visits to healthcare (perhaps a child is naturally sickly), although this is only a preliminary step towards understanding the role of genetics in human capital accumulation in Indonesia.

2.2 Theoretical Approaches to Studying Early Childhood Education

In addition to motivating early childhood interventions, economists have used scientific findings about brain development to create models in which skills learned in early childhood complement skills learned later in childhood or even in adulthood. This dynamic is most importantly explained by the concepts of self productivity and dynamic complementarity posited by Cunha and Heckman (2007). While the dominant assumption of theoretical work prior to 2007 followed Becker (1986) in treating childhood as a single period (with adulthood as a separate, succeeding, period), Cunha and Heckman instead treated childhood as two periods–with skills learned in the one period having a dynamic complementarity with skills in the second (Becker and Tomes, 1986; Cunha and Heckman, 2007).

This dynamic complementarity also applies to two different categories of skills, cognitive and non-cognitive–the former of which I focus on. Cunha and Heckman (2007) outline how cognitive and non-cognitive skills complement each other and focusing strictly on cognitive performance–which studies of Head Start largely do–tells an incomplete story. This is a compelling argument, and motivates further research into my question. If kindergarten adds more years of schooling but does not simultaneously improve learning or cognitive outcomes, then those additional years of schooling could nonetheless be generating more positive social outcomes. This may not help to address concerns about human capital formation and Indonesian students' performance on standardized test scores, but would bode well for kindergarten's societal effects.

2.3 Empirical Findings of the Effects of Kindergarten

Kindergarten in Indonesia is essentially the equivalent of preschool in the United States; it is not compulsory, with both state-owned and private options (the latter of which is largely responsible for increases in kindergarten attendance, as seen in Figure 2). Empirical studies of the effects of preschool on educational outcomes fall into two categories: 1) randomized control trials (RCTs) and 2) longitudinal studies (Duncan and Magnuson, 2013). My project is the latter, as I track individuals from before they attend kindergarten to until after high school graduation.

I begin my review of the empirical findings on the effects of preschool by examining the US context. Head Start in the United States is the most common subject of preschool studies: it is a program run by the US Federal Government since the 1960s, when it was established as part of the Great Society. Head

Start targets low-income children, so that federal guidelines require 90% of children served by the program come from families below the poverty line (Currie and Thomas, 1993). The other two most popular US programs that have been studied are the Perry Preschool Program and the Abecedarian Project, both of which are intensive and have been implemented at a much smaller scale than the massive Head Start program (Campbell et al., 2002; Heckman et al., 2010). Because the latter two programs are intensive and revolve around very small class sizes, they are favored by scholars employing RCTs (Muennig et al., 2011).[4] On the other hand, most studies of Head Start rely on longitudinal designs.

Studies of Head Start have shown mixed results regarding its effect on later-life outcomes. In 1993, Thomas and Currie (1993)–using mother fixed-effects, just as I do–found that attending Head Start compared to either 1) attending other preschool programs or 2) not attending preschool at all, had some significant effects on test scores for white and Hispanic children, while program participation had no effect for Black children. For that paper, however, they focused only on *short-term* effects: 10 years later, Thomas, Garces, and Currie (2002) revisited Head Start, this time looking at its effects on *long-term* outcomes. Using mother fixed-effects once again, they found evidence that white children who attended Head Start were likelier to complete high school than their siblings who did not–they also found *some* evidence of the same effect for Black children. When looking at the long-term effects of Head Start, Thomas, Garces, and Currie, in particular, found strong evidence for a critical concept in the early childhood literature, "fadeout".

Fadeout is an important concept in the early childhood interventions program; its basic premise is that the positive effects of participating in an early childhood intervention decrease or even approach insignificance as a child ages (Abenavoli, 2019). Fadeout is rooted in Cunha and Heckman's multi-stage approach to childhood; in particular, there is evidence that fadeout occurs only when high-quality interventions are not followed by subsequent high-quality educational experiences (Bailey et al., 2017; Jenkins et al., 2018; Lee and Loeb, 1995).

Fadeout also reveals significant heterogeneity among preschool programs, as high-quality ones are more durable than lower-quality programs. Clearly, fadeout is far from universal; there is even evidence that preschool leads to reduced adulthood delinquency and crime (Barnett, 2008). It is easy to see how this concern directly applies to Indonesia: classroom quality is a primary concern for policymakers as Indonesia has not improved its PISA test scores in recent years despite near-universal completion of primary school (Afkar

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[4]As an example of the intensiveness of the Abecederian Project, instructors create personalized games and curricula for individual children, and children receive instruction for 5 years. Head Start matches neither the personalized nature nor duration of the Project.

et al., 2020). These stagnating test scores may be due to a lack of investments in improving teacher quality, the adoption of learning technologies, and increasing the number of teachers to improve teacher-student ratios. Thus, if later-life investments in education are not being made, then improvements in primary school completion aren't being capitalized, in view of Cunha and Heckman's definition of childhood. Therefore, fadeout–which I analyze by comparing the effects of kindergarten on short- vs. medium- and long-term outcomes–is of prime concern for my research.

Work on preschool in the developing context is limited; this makes sense, as developing countries are less likely to have the comprehensive multi-wave longitudinal household surveys like the Panel Study of Income Dynamics (PSID) that make researching programs like Head Start possible, particularly for the favored research design of family fixed-effects, which requires a large number of households, and detailed within-household data. In this regard, the Indonesian Family Life Survey (for more details, see Section 3) provides a unique opportunity.

There are reasons why these effects of preschool interventions might not translate to developing countries (Dean and Jayachandran, 2020). First, instruction quality might be weaker. Second, the effects of preschool are naturally weighed against the counterfactual–and the counterfactual to early childhood intervention varies widely across contexts.[5] And third, recalling Cunha and Heckman's (2007) conception of childhood, subsequent educational experiences can vary–and thus, through dynamic complementarity of skills across stages of childhood, alter the effects of kindergarten.

There is strong motivation to study kindergarten in developing countries–one estimate is that 200 million children below the age of 5 in developing countries do not reach developmental potential due to a lack of resources (Grantham-McGregor et al., 2007). There is a great deal of evidence that drastic increases in kindergarten attendance over time are driven by those with greater socioeconomic resources–a phenomenon that the Indonesian data supports, with the predominance of private kindergarten. Overall, there is strong evidence that preschool has very strong positive effects—14 studies of developing countries found, on average, significant positive effects (Behrman et al., 2013). These same studies find significant effects of preschool on schooling and educational achievement. There is also significant evidence of fadeout among these studies.

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[5]In the US, for example, studies have shown declining effects for Head Start–this, however, is largely attributable to the improving counterfactual to participation, rather than changes in the characteristics of the program. We can also see this same pattern occurring in Indonesia, as the gap between children who went to kindergarten and children who did not has been narrowing over time in Figure 1.

For more specific estimates of the effects of kindergarten, Dean and Jayachandran (2020) find that, in India, children participating in kindergarten perform 0.8 standard deviations better on cognitive tests than their peers, and that this advantage is persistent even if it decreases over time. They find no impact on socio-emotional development, however. The type of kindergarten a child attends also matters; one study in Ghana found that children who attended *private* kindergarten performed better than their peers who did not attend kindergarten as well as their peers who went to *public* kindergarten (Pesando et al., 2020).

One notable study of preschool in Uruguay, by Berlinski et al. (2008), closely mirrors my research design. The authors found significant positive effects of preschool participation using family fixed-effects; by the age of 15, for example, children who participated in kindergarten completed an extra 0.8 years of education. They focused on grade repetition–i.e., students failing a grade and having to stay there to try again–as the transmission channel for the effects of preschool. Grade repetition, on the other hand, is rare in Indonesia–and it does not help to resolve the Indonesian tension between outcomes in schooling and outcomes in learning and skills (Berlinski et al., 2008).

3 Data

In this section, I describe the data and sample I use for my research. I begin by discussing my two sources of data–the Indonesian Family Life Survey (IFLS) and Village Potential Statistics (PODES) before I proceed to describing my sample and its construction.

3.1 Indonesian Family Life Survey

My main source of data is the Indonesian Family Life Survey (IFLS), a longitudinal household and community survey fielded through five waves from 1993 to 2014 by the RAND corporation. I use it for the entirety of my individual- and household-level data, as well as for the majority of my community-level data. The availability of the IFLS–a free, public dataset–makes Indonesia relatively unique among developing countries for having a detailed longitudinal household survey, which is necessary to study the medium- and long-term effects of early childhood interventions.

The original wave of the IFLS, fielded in 1993 and 1994, consists of 7,200 households and is representative of about 83% of Indonesia's population at the time (Serrato and Melnick, 1995). In each of the successive waves, the survey has attempted to re-interview the same households (with high rates of success

in recontacting across the years), in order to create panel data. In the fifth wave of the IFLS, fielded in 2014, 16,204 households and 50,148 individuals were interviewed (Strauss et al., 2016). I discuss how I merged data from the survey's different waves and ensuing attrition in greater detail in Appendix B.

I use the IFLS to track children from when they were young in 1997 to when they are adults in 2014. I focus on data from 1997 because that is the most recent wave of the IFLS in which kindergarten-age respondents will be at least 18 years old in the most up-to-date survey wave, fielded in 2014. Some of my covariates take the form of panel data–namely in the repeated observations of household-level variables such as household per capita expenditure and size of household, or community-level variables such as the number of elementary schools per 10,000 people in each kecamatan, i.e., subdistrict.[6]

3.2 Village Potential Statistics

As comprehensive as the IFLS is, its community survey only began to inquire after the presence of kinder-gartens in a community in 2014: therefore, in order to construct my instrument–the number of kindergartens per 10,000 individuals in each kecamatan, I needed to supplement the IFLS with another data source.

The Village Potential Statistics (PODES) dataset is immensely powerful; PODES data is published every year, dating back to 1983. It is fielded by Indonesia's Office of Statistics (BPS) and is available only by purchase. As the price of purchasing PODES is dependent on the number of variables taken from the survey, I limit my use to the two variables I need: the presence of kindergartens and village population. I also limit the data to only the years I particularly need–1990 and 2000. My research focuses on children who were kindergarten-aged during the 1990s; by collecting data that bookend the period of interest, I attempt to maximize data coverage.

PODES is comprehensive and representative of Indonesia in its scope, with data from approximately 65,000 villages. Therefore, I sum the number of kindergartens (public, private, and total) and population by kecamatan and year to find the number of kindergartens per 10,000 individuals in each kecamatan for 1990 and 2000. I then merge this data with my IFLS data on each household's kecamatan in 1997.[7] A visualization of these variables can be found in Figure 2.

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[6]For example, my independent variable–a dummy variable of whether an individual attended kindergarten–does not require repeated observation, nor do the *outcomes* I am interested in, i.e., school/grade completion or years of education completed.

[7]Since the PODES data is comprehensive, there is no attrition when merging the PODES data onto the IFLS data. This approach of merging IFLS data with selected variables from PODES is not novel and has been implemented in the literature (Shrestha and Nursamsu, 2021).

3.3 Sample Construction and Description

The starting place for my sample is all children *individually* interviewed in the 'child' book of the 1997 wave of the IFLS: the 'child book' consists of respondents less than 15 years old and 10,356 individuals completed this questionnaire.[8] I then add an age restriction, keeping only the children who completed the survey and were between 3 and 9 years old in 1997. With this restriction, I was left with 5,258 individuals. I added an age restriction to ensure that only children who were of kindergarten age in the 1990s (per the years of my instrument) were included in the data.[9]

Starting with children interviewed in 1997 restricts my sample more than if I had simply looked at children's adult interviews, limiting my variables to their household qualities and individual outcomes.[10] Yet, beginning with the childhood individual variables is necessary: I need to get a sense of the pre-kindergarten (or roughly pre-kindergarten) individual characteristics of the children, characteristics such as their health and how often parents took them to the doctor.[11]

Another component of the household survey, in addition to individual interviews, is the household roster–which I use to connect family members to one another and across survey waves. The roster comprehensively documents each member of the household and their basic characteristics–such as their educational attainment and intra-household relationships. I use the rosters to link the 5,258 children from above to 1) their mother and, by extension, 2) their siblings.[12] I then link the household roster to more detailed household survey observations, such as whether a household has electricity, the size of the household, and the household total expenditure. Finally, I merge these households with the IFLS community survey, which contains information of community characteristics such as population and the number of elementary schools in a community. By replacing communities that were not surveyed with appropriate averages, I ensure there

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[8]This is Book 5 of IFLS2 – individuals over 15 years old are interviewed in Books 3A and 3B.

[9]To exercise tighter control over age, I broke down individuals in my sample into 4 birth cohorts, those aged 3-4, 5-6, 7-8, 9-10. In terms of the number of households these individuals belonged to, I began with 5,171 unique households and, with the age restriction implemented, I was left with 3,488 unique households.

[10]For comparison, 20,529 individuals were individually interviewed in the adult survey in the 1997 wave, which was much longer and detailed than the children's individual survey. These individuals were from 7.538 unique households.

[11]I conduct robustness checks where I relax these specifications to test my hypotheses with fewer covariates and a larger sample size. This narrow sample specification is particularly problematic for my fixed-effects model, a challenge I discuss in greater detail in Section 4.2 and Appendix C.

[12]There is no attrition in this step; children who are individually interviewed are all featured on a household roster, inclusion in the former being a much more stringent restriction than inclusion in the latter.

is no attrition with this step.[13]

Thus far, I have individual, household, and community data for approximately when individuals went to kindergarten; however, in order to evaluate medium- and long-term outcomes, I need to match these early childhood observations with later-life observations. First, I incorporate household and community data from 2000 and 2007 in order to absorb exogenous shocks to a child's situation as they grow. For example, I continue to track household expenditure levels and size over time. From 1997 to 2000, among the 5,258 children in my sample, only 80 were lost to attrition (1.5% attrition rate for this step). From 2000 to 2007, only 130 were lost to attrition (2.5% attrition rate for this step). These attrition rates are very low because I am relying only on the very broad restriction that individuals appear on a household roster–which I use to merge household data onto individual observations.

Finally, I need the educational outcomes of individuals, which are taken from the 2014 wave of the survey–when all of the individuals are over the age of 18. Therefore, for this restriction, I require that all individuals in my sample were interviewed *individually* in 2014.[14] From 2007 to 2014, 1,357 were lost to attrition (26.8% attrition rate)–resulting in 3,691 individuals. For context, 36,391 individuals (aged 15+) were interviewed individually in 2014.

Lastly, I require that there are no missing observations for *any* of the covariates used in my non-fixed effects regression specification. This leads to the attrition of 533 individuals, so that my final sample consists of 3,158 individuals (14.4% attrition rate). Summary statistics of my sample for the variables of interest for my sample can be found in Table 1. More information on attrition can be found in the appendix and in Table 2.

[Table 1 here]
[Table 2 here]

Clearly, there is some association between 1) kindergarten and urban/rural status and 2) educational outcomes as well as household wealth or resources. There is also clearly enough variation in my core variables–most of the sample did not attend kindergarten (39% did), and the sample average for completed years of education is 10.97, approximately 1 year short of the completion of high school), while the median number

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[13]Specifically, I am working with 3 community-level variables: elementary schools per 10,000 people, junior high schools per 10,000 people, and senior high schools per 10,000 people (each observed in 1997, 2000, 2007, and 2014). If an individual's community was not interviewed and thus these variables are missing, I replace them with the mean for their kecamatan, conditional on urban/rural status. If there is no data for the kecamatan, I replace their missing observations with the mean for the province, conditional on urban/rural status.

[14]This time as adults, a designation which occurs when an individual is 15 and above for the purposes of the IFLS.

of years of education completed is 12.

To give a sense of how representative the sample is of the whole Indonesian population, 95% of the sample completed elementary school, 81% completed junior high school, and 62% completed senior high school. While historical data is difficult to match, pre-pandemic World Bank Data estimates that Indonesian students average 12.4 years of schooling and 37% of 3-6 years were enrolled in preprimary education in 2018 (Afkar et al., 2020). 2022 OECD data shows that 57.5% of individuals 25-34 years old graduated senior high school, Thus, my sample appears to be broadly aligned with national summary statistics, although it may be that the sample is over-educated relative to the total Indonesian population.

4 Methodology

As discussed in the Section 2, there are significant empirical challenges to isolating the causal effect of early childhood investments on later-life outcomes. One common challenge is selection bias, particularly in Indonesia, where kindergarten is not compulsory and in the 1990s kindergartens were almost entirely private.[15] Clearly, kindergarten included a significant opportunity cost and required a significant investment of resources by a family to pay tuition and physically transport their young child to a classroom when it's not required by law. To meet this challenge, I employ three empirical strategies: ordinary least squares (OLS) estimation, mother fixed-effects (FE), and then instrumental variable (IV) estimation.[16] I believe that selection bias into kindergarten affects the OLS analysis through omitted variable bias–which motivates my use of mother fixed-effects and IV to counter.

4.1 OLS

My starting point is a basic OLS estimation of the effects of kindergarten, using the following model:

$$Y_{if} = \beta_0 + \beta_1 \text{KINDER}_{if} + \beta_2 \mathbf{X}_{if} + \beta_3 \mathbf{Z}_f + \beta_4 \mathbf{K}_f + \epsilon_i \tag{1}$$

where Y_{if} is the outcome variable for individual i in family f, KINDER_{if} is a dummy variable for kindergarten attendance for individual i, $\mathbf{X}_{if}$ is a vector of individual-level variables, $\mathbf{Z}_f$ is a vector of household-level controls, and $\mathbf{K}_f$ is a vector of community-level controls. Our coefficient of interest is β_1, which

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[15]See Figure 2. Based on the PODES data, in 1990, 95.06% of kindergartens were private. That share grew to 96.63% in 2000.

[16]I also use Logit regressions to support my primary analysis, such as to examine selection into attrition and kindergarten; for a description of that method, see Appendix A.

represents the effect of kindergarten attendance on the educational outcome.

Critical to my OLS approach is that I control for the two forces driving household human capital decision-making: 1) household expenditure/resources and 2) the importance placed on education/human capital within the household.[17]

As part of my OLS estimations, I also employ province fixed-effects–as it is clear that there is a heterogenous component of the relationship between kindergarten and education across provinces, as demonstrated in Figure 4.[18] Finally, I use cluster-robust standard errors for the OLS estimations; thus, I cluster standard errors at the kecamatan–the equivalent of a county in Indonesia–level, making my estimates heteroskedastic-robust. As I discuss in Section 5 and Appendix E, I use a variety of post-estimation analysis to ensure my results are robust and the assumptions of OLS hold up under scrutiny.

[Figure 4 here]

4.2   Mother Fixed-Effects

For as many variables I can control in the above OLS model, there are critical household variables that I simply do not have data for–in particular, related to unobservable characteristics such as the emphasis placed on education within a household. Therefore, there will always be omitted variables correlated with *both* kindergarten attendance and the educational outcome variables in the OLS model. I hypothesize this would result in a upwards bias in the estimate of the coefficient for kindergarten attendance, as powerful positive explanatory variables for educational outcomes correlated with kindergarten attendance are omitted: thus, by absorbing these omitted variables into the fixed-effects term, I expect non-biased estimates using the mother fixed-effects model.

This omitted variable bias motivates an oft-employed approach to examining the effects of early child-

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[17]Examples of the former include the natural log of household per-capita expenditure for every available year, tracking household expenditure across the educational career of an individual (this exogenous variable ostensibly absorbs regional economic shocks as well) and whether a household has electricity, also captured over time. Examples of the latter include a mother's or household head's years of completed education, whether an older sibling attended kindergarten, and the number of visits to a doctor by each child. These controls are implemented in conjunction with more basic individual and community controls. For example, in my analyses I control for the number of times a mother has taken an individual child to the doctor in the last few months. This, when weighed against evaluations of the health of the child, may be a proxy for that mother's willingness or desire to invest in the human capital of the child – after all, ensuring health in early childhood has been demonstrated to greatly enhance human capital accumulation (Attanasio et al., 2020).

[18]This province panel variable is more general than the mother identification variable; therefore, while this fixed-effects is less powerful than mother fixed-effects, it also does not restrict my sample–as Figure 5 makes clear, there is sufficient variation in every province so that my 'switching sample' (see below) is the same as my total sample).

hood interventions: family fixed-effects (Currie and Thomas, 1993; Garces et al., 2002).[19] In particular, I employed mother fixed-effects, an approach that is powerful because it immediately controls for *all* household-level variables. FE accomplishes this feat by comparing children of the same mother. The fixed-effects approach estimates the following equation:

$$Y_{if} = \beta_0 + \beta_1 \text{KINDER}_{if} + \beta_2 \mathbf{X}_{if} + \beta_3 \mathbf{M}_{ft} + \mu_f + \epsilon_i \tag{2}$$

Note that the variables constant within the family from equation (1) are replaced with a single fixed effects term, μ_f, which captures *all* mother characteristics–including all those that were not observed as part of the IFLS. Fixed-effects, however, is not a panacea; it only controls for all *time-invariant* characteristics of the mother (rooted in 1997–the initial point of my sample). Therefore, I need to incorporate a vector of time-varying mother/household characteristics, represented by the term $\mathbf{M}_{ft}$.[20] It is possible that in reality a mother could treat their children differently—accordingly I've attempted to control for some pre-kindergarten characteristics that may signal favoritism, focusing on such indicators identified by previous work (Garces et al., 2002).

To avoid the bias of unobserved mother favoritism, I include a variable representing the number of times a parent took a child to the hospital as well as the general health of the child in 1997. This is based on a hypothesis that parents concentrate investment into the child they believe has the best chance of success–so the more often a parent takes a child to the doctor, health held constant, the likelier they may also be to send that child to kindergarten (Duflo and Banerjee, 2011).

It is also possible that there are spillover effects from one child attending kindergarten on successive children–something I attempt to control for by including a dummy variable for whether an older sibling had already attended kindergarten. Lastly, it is possible that a family has a different quantity of resources at its disposal when one child is of kindergarten age than when another child is–something I control for by employing controls for time-varying household characteristics captured by household expenditures. The latter is a particular concern, as fixed-effects controls for all unobserved *time-invariant* characteristics. I alleviate this concern by controlling for age.

Fixed-effects introduces another complication; in order to run a regression for a specific family f, there must be variation in the explanatory variable, introducing a two-step requirement to be included in the fixed-

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[19]The stronger instance of family fixed-effects is the twin studies I described in the Section 2.

[20]These characteristics include economic or health shocks to the mother/household, the household economic status, the household structure (i.e., whether it remains a two-parent household or its size), and community-level shocks such as natural disasters.

effects model specification. These requirements are that 1) each mother has more than one child in our sample and 2) there is variation between a mother's children in kindergarten attendance. This significantly restricts our sample–as seen in the diagnostic graph Figure 5.[21]

[Figure 5 here]

These requirements for my fixed-effects specifications thus distinguish between two types of families already included in my sample: 1) "switching" families and 2) "non-switching families". (Miller et al., 2023) "Switching" families are included in the fixed-effects model and "non-switching" families are not. Often scholars are concerned about selection bias into the "switching" designation. I find the "switching" sample has some puzzling characteristics when compared to the total sample. There is no significant positive selection bias, as the only significant predictor of whether a family is a "switcher" is household size, which I do not find to be significant in explaining educational outcomes. However, selection into kindergarten attendance appears to be a drastically different process–something I discuss in greater detail in Appendix D and Appendix C.

In brief, while household characteristics function similarly in determining kindergarten attendance for switching and non-switching families, individual pre-treatment characteristics–the only category of covariates I can control for in fixed-effects–appear to function in the opposite direction. For example, for non-switching families there is a significant negative effect of whether an older sibling attended kindergarten on selection into kindergarten. Additionally, the switching sample is so small as to render any OLS or IV estimates of the effect of kindergarten restricted to the switching sub-sample insignificant.

4.3 Instrumental Variable (IV) Estimation

Each of my first two empirical approaches–OLS and fixed-effects–have shortcomings. First, OLS merely suggests an association between kindergarten attendance and educational outcomes. Critically, it suffers from omitted variable bias because of a host of unobservable variables. Second, as powerful as fixed-effects is for controlling household factors, it introduces significant sample restriction and selection bias into the switching sample.

This motivates me to use instrumental variable (IV) estimation methods to remedy the bias introduced by the endogenous regressor, kindergarten attendance. I develop two instruments, both taken from the Village

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[21]17.48% of the sample are from households with exactly one child. Only 7% of my total sample qualifies as part of a switching household according to these requirements–meaning there is a sample loss of 93% just for the fixed-effects sample specification.

Potential Statistics (PODES) waves of 1990 and 2000; PODES contains explicit data about the presence of kindergartens in villages–see Section 3.2 for more information. I aggregate population and kindergarten across each kecamatan and then merge based on the kecamatan an individual or household is registered under in the 1997 wave of the IFLS, creating a population-weighted average of kindergartens for each kecamatan. My first instrument consists of total (private + public) kindergartens per 10,000 people in each kecamatan in 1990 and my second instrument is the same measure in 2000.[22] While I use the total number of kindergartens, the data clearly reflects it is private kindergartens driving expansion in the total number of programs between 1990 and 2000, as seen in Figure 2.

[Figure 2 here]

There are different estimators for IV estimation; I employ the Generalized Method of Moments (GMM) estimator. In cases of over-identification, GMM does not reduce multiple instruments into one matrix and therefore is a more efficient estimator. It also gives me greater flexibility in conducting over-identification tests. The employment of an instrument requires a robust defense–both intuitively and empirically. An instrument needs to be predictive of kindergarten attendance while having no impact on educational attainment when relevant factors are controlled for (Levitt, 2002).

4.3.1 Instrument Strength

First, intuitively the two instruments will each be very strong: we would expect that the number of kindergartens in a locality, all else being equal, would have an effect on whether a random student from that locality attends kindergarten. This theoretical expectation is backed up empirically by the results of the first-stage regression, found in Table 3. One of the advantages of GMM is that it employs the two instruments separately; therefore, to demonstrate instrument strength, I regress each instrument on kindergarten attendance separately. While it does appear that the number of kindergartens per 10,000 people in 1990 is a stronger instrument, clearly the corresponding 2000 variable is significantly strong.[23] Since the regressor being complemented by the instrument and the instrument are the same across models–only the outcome variable changing–I only need to check instrument strength once.

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[22]In Appendix F I explore alternative instruments, such as comparing just private to just public kindergartens, taking an average of the 1990 and 2000 measures, and percent change in the presence of kindergartens.

[23]I use a basic F-Test to reject the null hypothesis that either coefficient is equal to 0 in the fully-specified model, each with a p-value of 0.0000. More broadly, the 'rule of thumb' for first-stage regressions is to look for an F-Statistic greater than 10; for the 1990 instrument, the F-Statistic is 136.48 in the fully-specified model, and for the 2000 instrument, it is 121.78.

4.3.2 Instrument Validity

Second, instrument validity requires a more finely-tuned theoretical proof, as well as more detailed empirical evidence. I suspect that these instruments do not exert an effect on educational outcomes *when* other community factors are controlled for. In particular, I control for the number of elementary schools per 10,000 people in each locality in 2000, junior high schools in 2007, and senior high schools in 2014; controlling for the presence of these schools, I theorize, isolates the effect of the supply of kindergartens to only kindergarten attendance. Thus, any effect that the supply of kindergartens has would be mediated through kindergarten attendance. This broad theoretical argument would apply for any of the outcome variables I research in this paper.[24]

For the empirical evidence of validity, I rely on Hansen's J Test, or the over-identifying restrictions test. Two things allow me to run this test: 1) I have two instruments for one endogenous regressor and 2) I employ the GMM estimation method. I then run the over-identifying restrictions test for every model, as the outcome variables alter the calculation of the test's F-Statistic. Overall, the results of this test are promising: I fail to reject the null hypothesis that at least one of the two instruments is not valid for all models except elementary school completion, junior high school completion, school attendance for grades 3-9, and stay-on decision making after grade 6.[25] This largely corroborates the theoretical argument I outlined above.

5 Results

I organize my results by outcome variable: each sub-section will include my OLS, fixed-effects, and instrumental variable estimates for the effects of kindergarten on that outcome variable. I have included results and discussion of an analysis of selection into kindergarten attendance in Appendix D.

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[24]Not all kindergartens are the same, and I naturally expect some kindergarten programs to be of a higher quality than others. Neither PODES nor IFLS has data on the *details* of kindergarten programs and their quality, so this would be an area for further research, and one I discuss in Section 6.

[25]Elementary school completion, and school attendance for grades 3-6 do not concern me very much, as there was not enough variance in the outcome to begin with and model results are thus not very significant.

5.1 Years of Education

First, I examine years of education as the outcome variable. All of the individuals in my sample were between 20 and 27 years old by 2014, when the data on years of education completed was collected, ensuring that every individual had the opportunity to finish high school and advance into college before the variable was collected.[26] I employ four model specifications for the OLS and fixed-effects, as can be found in Table 4.[27]

[Table 4 here]

To begin, there is variation in the years of education completed; in my sample, 37.3% of individuals completed exactly, 38.19% of individuals completed fewer, and 34.51% of individuals completed more than 12 years of education. The distribution of years of education completed is nearly identical within the switching sample.[28] Overall, I find significant, positive effects of attending kindergarten. For the OLS specifications, the inclusion of more controls reduces the magnitude of the significant positive effect of kindergarten until kindergarten's effect is estimated to be adding 0.71 years of education, in the fully specified model. Other significant positive predictors of years of education include household wealth and the mother's level of education, while being male has a significant negative effect on the years of education completed. The mother fixed-effects model, however, has unclear and insignificant results: with fixed-effects, I find none of the included covariates exhibit any significant effect on the years of education completed. I suspect that this is because of the bias introduced by the high rates of attrition into the switching sample used to fit the fixed-effects model–something I discuss in greater detail in Appendix C and which a robustness check, discussed in Appendix E.3, confirms.

Next, I employ Instrumental Variable (IV) estimation, the results of which can be found in Table 5. The IV estimates for the effects of kindergarten is greater in magnitude than that for the OLS and fixed-effects models for all specifications. In particular, the fully specified model estimates that kindergarten adds 1.89 years of education–compared to 0.71 years in the fully specified OLS model. Two coefficients are very different in these results: the dummy variables indicating whether a household was urban in 1997 and the

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[26]When focusing on outcomes such as school attendance and stay-on rates, I exclude years of schooling beyond the 14th because, although every individual would have had the opportunity to complete high school and enter into college, the data on college years is incomplete.

[27]I do not include the point estimates of the coefficient for every covariate for conciseness. Also note that I incorporate province fixed-effects in Model (3), alongside the other community controls that are only included in Model (3).

[28]Within the switching sample, 37.56% completed exactly 12 years of education, 36.65% completed fewer than 12 years of education, and 35.79% completed more than 12 years of education.

dummy variable for whether the mother as well as the father are both present in a child's household in 1997. Whereas each of these coefficients were statistically insignificant in the OLS and fixed-effects models, they are positive and significant in the IV estimation.[29]

[Table 3 here]

[Table 5 here]

This increase in the coefficient magnitude from OLS to IV is surprising, and runs counter to my initial hypothesis that omitted variable bias in the OLS model introduced an upward bias because of omitted explanatory variables I would expect to be correlated with both kindergarten attendance and greater educational outcomes. One hypothesis for this increase may be that *accessibility* is a more significant explanatory variable of kindergarten attendance, and by extension schooling, than I would expect. IV exploits the instrument to focus on kindergarten's effect on the portion of the population whose human capital investment decisions were affected by the instrument–the number of kindergartens in an area. Given the number of private kindergartens then, we may imagine a large portion of the population who attended kindergarten would have attended kindergarten even if they had to go to another sub-district; they may have the resources to do so. We can imagine this same population is likelier to complete high school regardless of kindergarten attendance. On the other hand, the portion of the population who would only be attending kindergarten if there were a large number in *their* sub-district would be less wealthy and thus, perhaps, kindergarten would have a greater effect for this subsample. IV, by design, may be more swayed by this latter group, and thus result in higher estimates of the effect of kindergarten. This gap between estimates merits further closer investigation. This hypothesis would apply to other schooling variables as well.

5.2 School Completion

Completed years of education is a long-term outcome, and I can break it up into the completion of the three levels of schooling: elementary, junior high, and senior high school.[30] In this section I analyze the effects of kindergartens where dummy variables indicating whether a child completed each of these levels of schooling are the outcome variables—allowing me to analyze heterogeneity in kindergarten's effects over the life cycle and examine whether there is fade-out. Results of this analysis can be found in Table 6.

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[29]All covariates in this model are the same as the ones I described above; thus, the community controls in this model are the elementary schools per 10,000 people in 2000, junior highs per 10,000 people in 2007, and senior highs per 10,000 people in 2014.

[30]In Indonesia, elementary school ends after the completion of the 6th grade, junior high after the completion of the 9th grade, and senior high after the completion of the 12th grade.

School completion is a slightly challenging outcome to analyze. First, for elementary school completion there is almost no variance–only 5.29% of the sample failed to complete elementary school. I suspect in large part because of this lack of variance in elementary school completion in the sample, the OLS model finds no significant effects of kindergarten on the completion of elementary school. On the other hand, 18.68% failed to complete junior high school and 38.19% failed to complete senior high school.

There is significant heterogeneity across the three model types: OLS, mother fixed-effects, and IV estimation. Focusing on junior and senior high school, the OLS model finds significant positive effects for junior and senior high completion. Similar to the estimates for years of education, fixed-effects once again finds no significant effect of kindergarten attendance. The IV estimates, on the other hand, suggest fadeout; there is a very strong significantly positive effect of kindergarten attendance on the completion of junior high school that 'fades' to insignificance for senior high school. We can focus on even more granular outcomes than school completion by focusing on specific grades and their rates of completion.

Importantly, the estimates for the covariates' coefficients are very similar across the OLS and IV estimates. Household expenditures become increasingly important over the life cycle (particularly the most recent observation in 2007), and the mother's years of education becomes more significant over time as well. This stands in contrast to the declining effects of kindergarten post-junior high found above, and that will also be found below in Figure 6, which focuses on school attendance–the next outcome I will analyze.[31]

5.3 School Attendance and Stay-On Decision

The next, more granular outcome to examine is grade-by-grade school attendance–captured by a dummy indicating whether a child completed every single grade.[32] In Figure 6, I have plotted the attendance rates themselves by grade and then the effects of attending kindergarten by grade. The vertical lines on the attendance rate graph indicate the 6th, 9th, and 12th grades, which are the final grades for elementary, junior high, and senior high school, respectively–note the steep drop-offs in attendance following the conclusion of each grade.

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[31]This difference between junior and senior high suggests that the larger effect of kindergarten on junior high is not merely a function of greater variance in the outcome variable, since we do not see a similarly increased estimate for senior high completion.

[32]I focus on grades 1-14, for reasons described above.

We can make a few observations: first, as I mentioned above, there is nearly no variation in school atten-dance throughout primary school–particularly for children who attend kindergarten. However, the estimated marginal significance of kindergarten's effects throughout grades 1-6 (consisting of elementary school) sug-gests the gap between kindergarten and non-kindergarten children is explained by other factors. The esti-mated effect of kindergarten is greatest in junior high school–for the OLS and IV estimates. After junior high, however, the effects of kindergarten fade to marginal significance once again and become nearly ex-actly zero after high school. We once again have preliminary evidence of fadeout.

Examining school attendance over time has a critical advantage–the sample is kept constant across time. The disadvantage of this approach, however, is that once a child stops attending school, the only possible values for the remaining attendance dummy variables is 0. Thus, I corroborate analyzing school attendance with looking at 'stay-on'; a dummy variable conditional on a student already being in a grade, taking on a value of 1 if they *continue* in school, and 0 if they exit school after that grade.[33] The results of that analysis can be found in Table 7.

[Table 7 here]

These results provide strong evidence of fade-out. The OLS estimates suggest that kindergarten has a strongly significant positive effect on whether a student stays on after the 6th grade, and a weaker effect corresponding to the 9th and 12th grades. The fixed-effects model suggests there is no effect, though there is some very weak suggestion of a negative effect for whether a student stays on after the 12th grade.

Finally, the IV estimates–my primary empirical method–show a very strong significant positive effect for 6th grade and absolutely insignificant effects for staying on after 9th and 12th grades. This corroborates the evidence from the school attendance analysis: kindergarten may not have an effect on completing elementary school, but it may lead children to attend junior high school, after which kindergarten has no *enduring* effect. Interestingly, there is no evidence that the number of schools in a locality has any effect on whether a student stays on–suggesting that supply side forces are not powerful in explaining educational attainment, though this is outside the scope of this research and motivates further work.

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[33]For the stay-on analysis, the sample varies across grades since if a student had dropped out after the 4th grade, say, their assigned value for the 6th grade stay-on variable is missing and not zero. For school attendance by grade, on the other hand, the attendance dummy for 6th grade in this situation would be zero, not missing.

5.4 Cognitive Test Scores

Thus far, my educational outcomes have been different measures of educational attainment; however, as I make clear in Section 1, in Indonesia, student *learning* is not keeping up with rapid advances in school attendance. I am not able to match standardized test scores to individuals in the dataset; however, the IFLS has a book in which interviewees are subject to a basic cognitive test. This cognitive test has two versions: one for children aged 7-14 and the other for respondents aged 15-24. I standardize each individual's test score according to their age group's performance on the test to ensure I am only comparing like-aged individuals. Each test consists of two parts: the first is basic visual recognition of shapes and patterns. An example page of this test from the 15-24 version in 2000 can be seen in Figure 7.

[Figure 7 here]

The second portion consists of basic arithmetic problems. For the 7-14 aged children, these are just math problems while for the 15-24 age group they are more involved word problems. An example page of this test from the 7-14 test in 2000 can be seen in Figure 8.[34]

[Figure 8 here]

I then use individuals' performance on these tests in 2000, 2007, and 2014 to effectively track their cognitive abilities over time.[35] The results of that analysis can be found in Table 8.[36]

[Table 8 here]

These results are mixed; the OLS estimates of the coefficient of kindergarten attendance tell a clear story of fade-out. Kindergarten has a strong positive effect on the 2000 scores, then a still significant and positive, although weaker, effect on 2007 scores, before fading to insignificance for the 2014 scores. The fixed-effects results suggest insignificant effects, and the IV–most perplexingly–also suggest insignificant effects.

The IV estimates tell two very different stories of kindergarten's effect: one for cognitive performance, where kindergarten has no significant effect, and one for educational attainment, where kindergarten has a

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[34]These tests are not perfect and there may be measurement error; however, all covariates except for kindergarten attendance (in particular, the mother's years of education and household expenditure over time) behave very similar for the models with schooling outcomes as for this model with cognitive test performance outcomes, so there is not something systematically biasing results across covariates/explanatory variables.

[35]I create a sub-sample for this analysis, restricting the sub-sample to only those individuals who completed all 3 tests in order to effectively compare tests over-time.

[36]All individuals completed the same cognitive test; therefore, I have added age fixed-effects to account for the differing intercept based on age that we would expect.

very significant positive effect. This divergence would confirm fears outlined in Section 1 that increasing enrollment and school completion may not be translating to better learning and effective human capital formation for students.

We can also look at change in cognitive performance across various years: perhaps kindergarten resulted in increased performance from 2000 to 2007, even if performance remained low. First, we can look at percent change in test performance from 2000 to 2007. For both un-standardized and standardized percent change (standardized according to age), kindergarten has a positive insignificant effect. Second, looking at change from 2007 to 2014, kindergarten's effect's decreases while remaining insignificantly positive, for both standardized and un-standardized change. It is tempting to suggest these results suggest fadeout; however, there was no significant effect of kindergarten to begin with, preventing me from interpreting these results with any significance or weight.

The lack of significant results using IV estimation is made more credible because the IV estimates were so large and statistically significant for the educational attainment variables. If there was some upward bias of the IV method in estimating kindergarten effects, then they would also be at work here–but we see no significant estimate of the effect of kindergarten. This divergence is also critical because the covariates previously demonstrated to be significant and critical to explaining educational attainment–namely household expenditure in 2007 and the mother's years of education–are still very significantly positive in explaining cognitive test performance. I will discuss the interpretation and significance of these results in greater detail in Section 6.

6 Conclusion

In this research, I examine the effects of kindergarten on schooling and cognitive performance. Using data from the Indonesian Family Life Survey (IFLS) and Village Potential Statistics (PODES), I examine the effects of kindergarten on educational outcomes in Indonesia, focusing on schooling and cognitive performance. My empirical strategy entails ordinary least-squares (OLS), mother fixed-effects, and instrumental variable (IV) estimation, where my instrument is the number of kindergartens per 10,000 individuals in each locality.

Broadly, my results suggest that kindergarten has a positive relationship with educational outcomes, particularly for schooling. Kindergarten is associated with children completing more years of education and

there being a greater likelihood of children attending school in junior high school. This is an important finding and is promising for policymakers in Indonesia who want to continue expanding school attendance beyond primary school.

My results also provide evidence for two interlinked phenomena critical to understanding human capital formulation in Indonesia: 1) 'fadeout' in the effect of kindergarten and 2) divergence between the effects of kindergarten on schooling and learning. First, there is considerable heterogeneity in kindergarten effects over time: in short, kindergarten has a greater impact on short- and medium- term outcomes rather than on long-term ones. I analyze this by breaking down schooling into school completion, school attendance, and the decision to stay-on after the completion of primary, junior high, and senior high school.

Disregarding the elementary school years, for which there is very little variation in outcomes, my results provided robust evidence that kindergarten increased the likelihood a student would attend school post-elementary school and complete junior high. There was also mixed evidence supporting the hypothesis that kindergarten had a positive effect on staying on after the completion of junior high school, and the completion of senior high school. Turning to cognitive performance, my OLS estimates found significant presence of fadeout with significant positive effects of kindergarten on cognitive performance in 2000 and 2007, and an insignificant effect in 2014, while IV estimation found insignificant effects for all three years. These results cumulatively paint a picture where the effects of kindergarten are positive–until approximately junior high school, after which they are insignificant. Figure 6 is an effective visual demonstration of fadeout. These results confirm the extensive evidence of 'fadeout' that previous scholars have found, which are discussed in greater detail in Section 2.

To the second phenomenon, my results regarding the effect of kindergarten on cognitive performance are mixed. I regard the IV results, valid and robust for all three models involving cognitive performance, as more valid than the OLS results that suffer from omitted variable bias. The IV results suggest absolutely no significant effect for any of the 2000, 2007, or 2014 cognitive test performances. In fact, the fixed-effects model finds that kindergarten has a negative effect on the 2014 results, at a 90% confidence level–one of the few significant findings of the fixed-effects model. This is finding parallels major concerns in the Indonesian education system, as there is a clear divergence between schooling and learning occurring–echoing the fears discussed in Section 1 that increasing enrollment numbers in Indonesia were not translating to greater learning and thus not alleviating concerns about human capital formation.

These results cumulatively suggest that there needs to be a closer examination of the quality and ac-

cessibility of kindergarten programs before early childhood education is expanded as a policy to improve human capital formation and, consequently, productivity. That data on kindergarten is unfortunately difficult to capture, the specifics of kindergarten only being asked about in the most recent survey wave of the IFLS, in 2014; however, with a new wave of the IFLS on the horizon, it would be possible to incorporate the 2014 data on kindergarten quality into an analysis on short- and perhaps medium-term outcomes.

There are a great number of ways that this research could be extended and improved. One would be to expand the number and types of outcome variables examined; all of the individuals in my sample were interviewed individually in 2014, so without further attrition I would be able to access variables on labor markets, health, and social outcomes.[37] I could also examine intermediate outcomes; some of my sample were also interviewed in 2000 and 2007, and for those that left school before completing senior high school I could investigate their reasons for doing so, which are asked about in the individual interviews. Additionally, with the new wave of the IFLS being fielded soon (see above), I would be able to incorporate even longer-term outcomes, including–perhaps most promisingly–the role that kindergarten plays in intergenerational economic mobility. The difficulty of these extensions is the attrition of adding another wave of the IFLS to the survey, entailing a trade-off in sample size and sample detail.

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[37]Although due to the informal nature of the Indonesian economy, I would suspect that labor market outcomes harbor measurement error.

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Figures and Tables

Figure 1: Years of Education Over Age, by Kindergarten Attendance
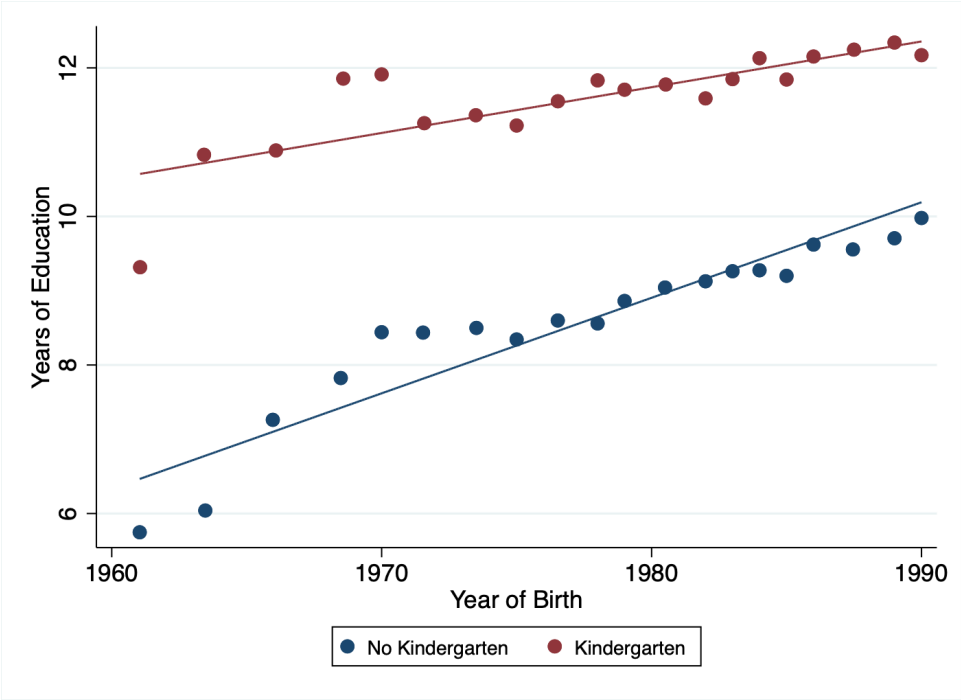
Figure 2: Number of Kindergartens per 10,000 People, 1990 and 2000
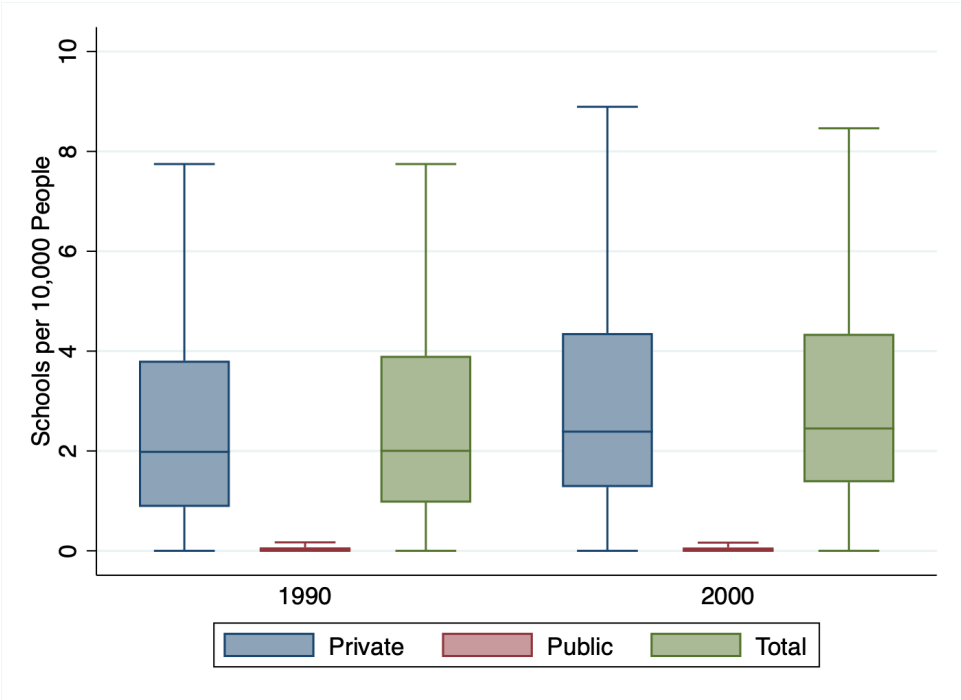
Figure 3: Number of Schools per 10,000 People, by Kabupaten and Year
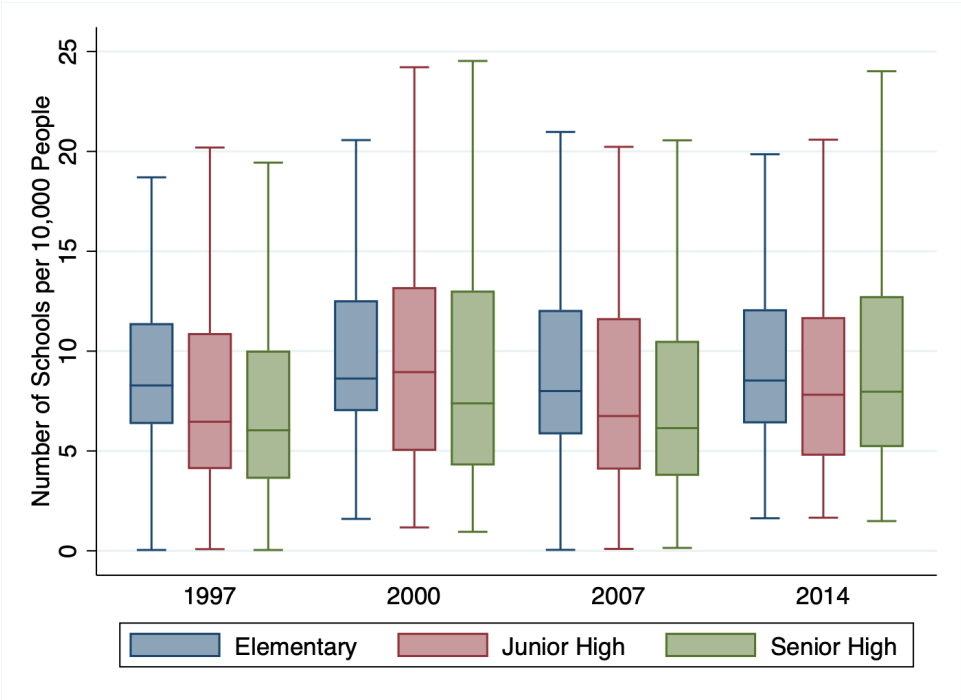
Figure 4: Association between Years of Education and Kindergarten, by Province
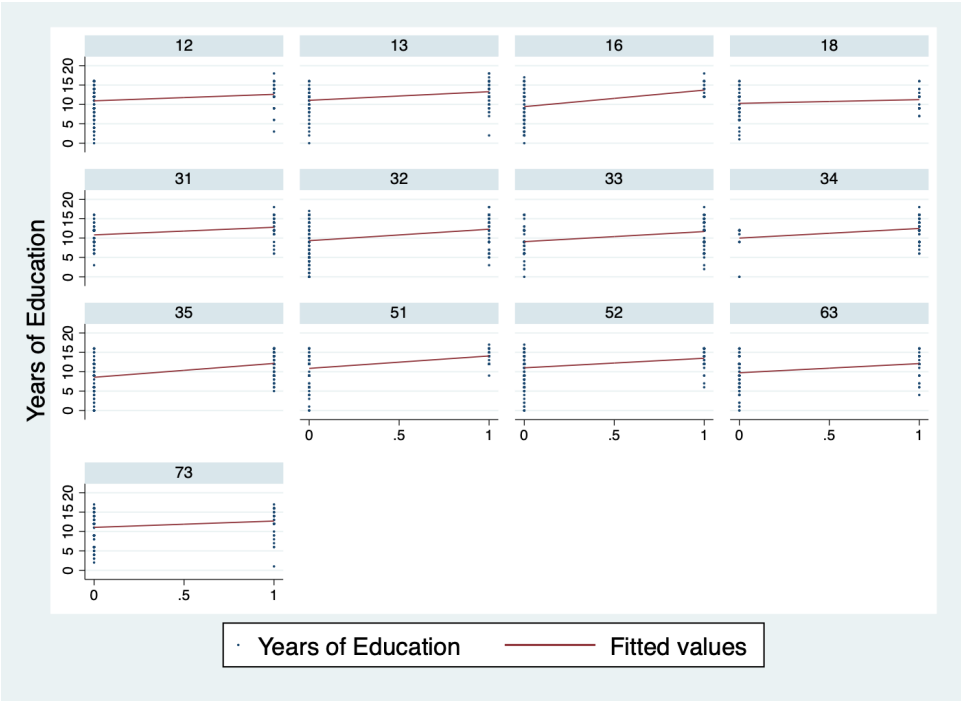
Figure 5: Switching Sample Diagnosis Scatterplot
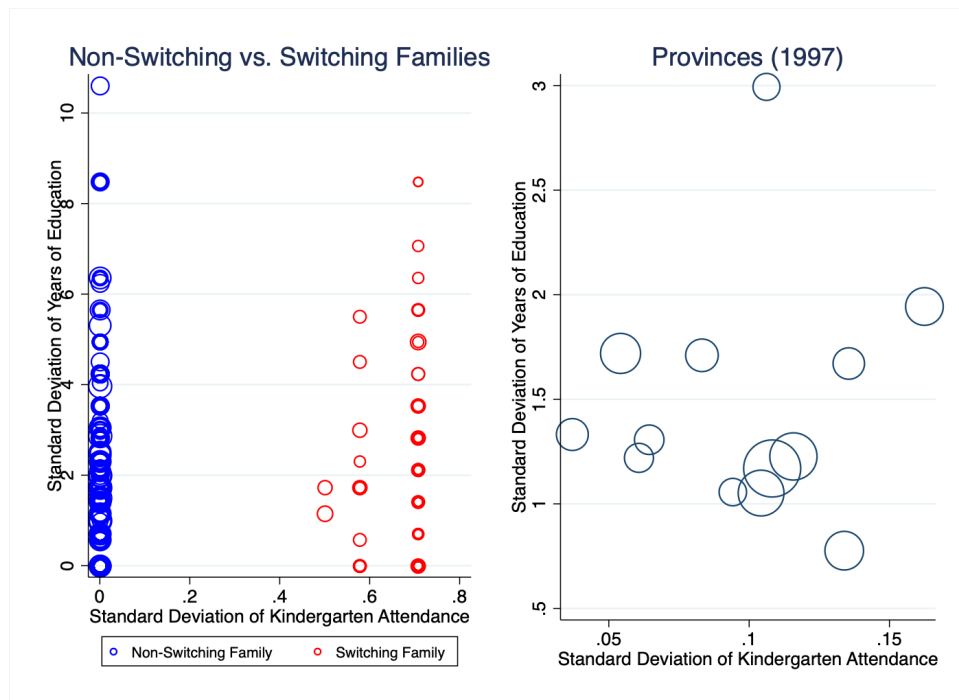
Figure 6: Attendance Rates and Marginal Effects of Kindergarten on Attendance Rates, by Grade
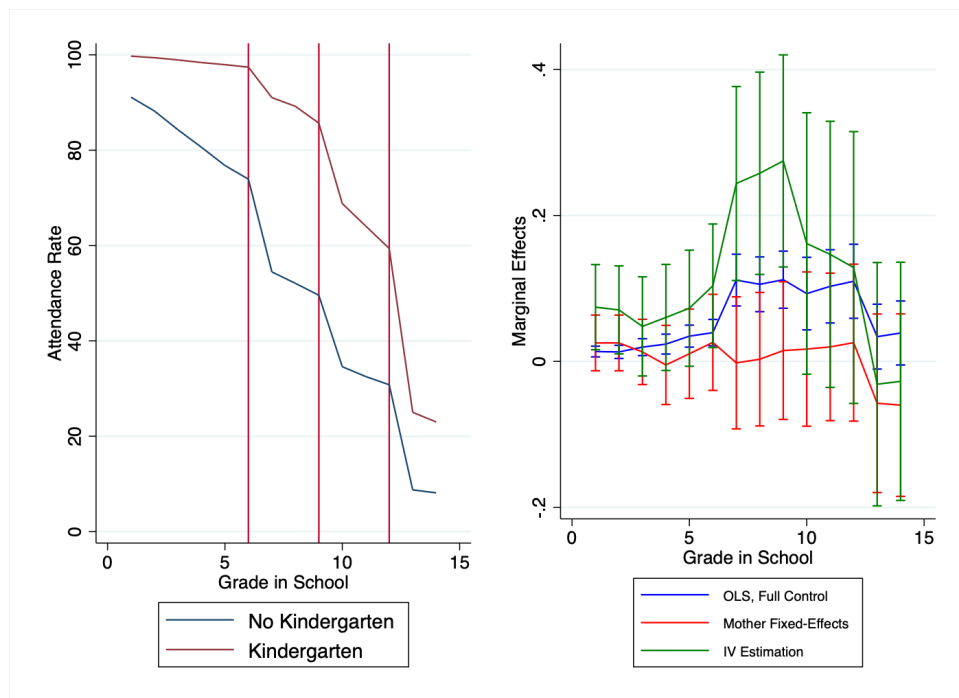
Figure 7: Sample Page from 2000 IFLS Cognitive Test (EK) for Respondents Aged 15-24, Visual
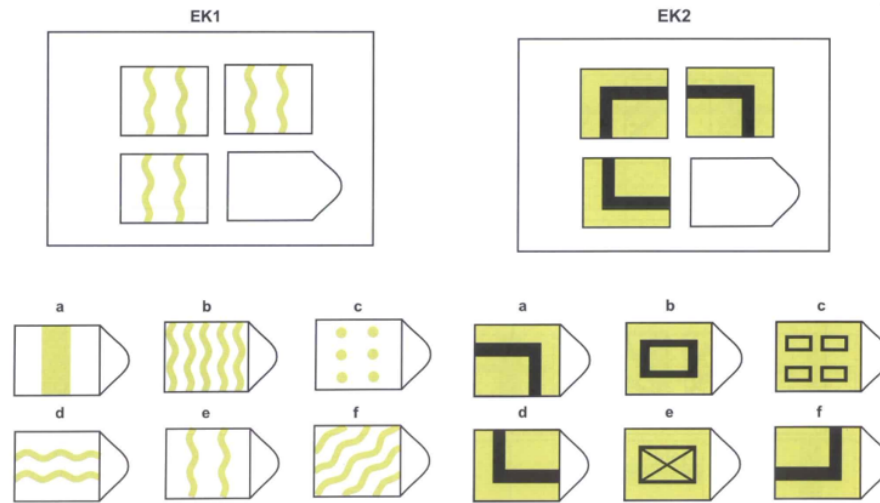
Figure 8: Sample Page from 2000 IFLS Cognitive Test (EK) for Respondents Aged 7-14, Arithmetic

EK13.   49 - 23 = . . .

    a. 25

    b. 26

    c. 27

EK14.   267 + 112 - 189 = . . .

    a. 180

    b. 188

    c. 190

EK15.   (8 + 9)*3 = . . .

    a. 34

    b. 45

    c. 51

EK16.   56/84 = . . .

    a. 4/7

    b. 2/3

    c. 3/4

    d. 5/6

EK17.   1/3 - 1/6 = . . .

    a. 2/3

    b. 1/3

    c. 1/6

    d. 1/9

8

Table 1: Summary Statistics of Variables of Interest

	Full Sample	Urban		Rural	
		Kinder	No Kinder	Kinder	No Kinder
Kindergarten	0.39	1.00	0.00	1.00	0.00
	(0.49)	(0.00)	(0.00)	(0.00)	(0.00)
Years of education (2014)	10.97	12.79	10.79	11.92	9.74
	(3.45)	(2.53)	(3.04)	(2.82)	(3.73)
Completed elementary	0.95	0.99	0.96	0.99	0.90
	(0.22)	(0.08)	(0.20)	(0.11)	(0.30)
Completed junior high	0.81	0.95	0.82	0.92	0.69
	(0.39)	(0.21)	(0.39)	(0.27)	(0.46)
Completed senior high	0.62	0.85	0.61	0.72	0.46
	(0.49)	(0.36)	(0.49)	(0.45)	(0.50)
Two-parent HH	0.91	0.92	0.92	0.89	0.91
	(0.29)	(0.27)	(0.26)	(0.31)	(0.29)
Mom's years of education	5.73	8.68	5.28	6.79	3.98
	(3.97)	(3.75)	(3.32)	(3.92)	(3.28)
HH per-capita expenditure (1997)	12.13	12.42	12.11	12.29	11.94
	(0.69)	(0.74)	(0.65)	(0.71)	(0.62)
HH per-capita expenditure (2000)	12.14	12.47	12.08	12.24	11.96
	(0.65)	(0.66)	(0.62)	(0.62)	(0.59)
HH per-capita expenditure (2007)	12.93	13.27	12.92	12.94	12.76
	(0.69)	(0.68)	(0.62)	(0.70)	(0.66)
Elementaries per 10,000 (2000)	10.71	7.34	7.51	10.58	13.87
	(10.75)	(4.09)	(4.88)	(9.02)	(14.12)
Junior highs per 10,000 (2007)	9.73	6.10	7.02	7.86	13.50
	(15.51)	(5.69)	(7.74)	(6.49)	(21.88)
Senior highs per 10,000 (2014)	9.12	8.71	7.54	10.13	9.59
	(8.97)	(8.01)	(7.58)	(9.28)	(9.74)
Birth cohort	2.50	2.60	2.51	2.58	2.40
	(1.11)	(1.12)	(1.11)	(1.12)	(1.09)
Birth order to mother	1.01	1.00	1.01	1.01	1.01
	(0.08)	(0.07)	(0.07)	(0.09)	(0.09)
Older sibling attended kinder	0.16	0.31	0.08	0.32	0.06
	(0.37)	(0.46)	(0.27)	(0.47)	(0.23)
Healthcare visits	0.20	0.27	0.22	0.23	0.14
	(0.53)	(0.59)	(0.56)	(0.57)	(0.46)
General health status (1997)	1.95	1.92	1.99	1.92	1.95
	(0.44)	(0.49)	(0.41)	(0.45)	(0.42)
Number of Observations	3158	700	571	533	1350

Standard deviations are in parentheses.

Table 2: Summary Statistics of Pre-Treatment Variables by Attrition Status

	In Sample	Missing Obs.	IFLS2 to IFLS3	IFLS3 to IFLS4	IFLS4 to IFLS5
Urban	0.40	0.40	0.50	0.81	0.38
	(0.49)	(0.49)	(0.50)	(0.40)	(0.49)
Two-Parent Household	0.91	0.61	0.81	0.84	0.75
	(0.29)	(0.49)	(0.39)	(0.37)	(0.43)
Mom's Years of Education	5.73	5.92	7.25	9.49	5.67
	(3.97)	(4.07)	(3.92)	(4.60)	(4.09)
Household Expenditure	12.13	12.18	12.34	12.82	12.07
	(0.69)	(0.71)	(0.91)	(0.88)	(0.79)
Birth Order	1.01	1.01	1.38	1.44	1.38
	(0.08)	(0.09)	(0.66)	(0.65)	(0.69)
Healthcare Visits	0.20	0.15	0.16	0.26	0.19
	(0.53)	(0.43)	(0.59)	(0.56)	(0.54)
General Health Status	1.95	1.95	1.95	1.92	1.97
	(0.44)	(0.39)	(0.43)	(0.43)	(0.41)
Number of observations	3158	533	80	130	1357

Standard deviations are in parentheses.

Table 3: Instrumental Variable First-Stage Regression, Kindergarten Attendance as Outcome Variable

	(1)	(1)	(2)	(2)	(3)	(3)	(4)	(4)
Kindergartens/10,000 people (1990)	0.12***		0.06***		0.06***		0.06***	
	(0.00)		(0.01)		(0.01)		(0.01)	
Kindergartens/10,000 people (2000)		0.05***		0.01***		0.01***		0.01***
		(0.00)		(0.00)		(0.00)		(0.00)
Household Controls	NO	NO	YES	YES	YES	YES	YES	YES
Individual Controls	NO	NO	NO	NO	YES	YES	YES	YES
Community Controls	NO	NO	NO	NO	NO	NO	YES	YES
Adjusted R-squared	0.22	0.08	0.40	0.37	0.43	0.41	0.43	0.41
F statistic	1007.01	107.61	207.52	178.45	155.00	134.36	143.14	125.16
Number of observations	3154	3154	3154	3154	3154	3154	3154	3154

Heteroskedastic-robust standard errors are reported in parentheses.

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$

Table 4: OLS and Fixed-Effects Model Results, Years of Education as Outcome Variable

	(1)	(2)	(3)	(4)
Kindergarten	2.36***	0.82***	0.74***	-0.06
	(0.13)	(0.11)	(0.13)	(0.44)
Urban (1997)		-0.30	-0.05	
		(0.23)	(0.26)	
Urban (2000)		0.41*	0.51*	
		(0.23)	(0.26)	
Urban (2007)		0.07	-0.01	
		(0.17)	(0.19)	
Two-parent HH		0.18	0.28	
		(0.19)	(0.21)	
Mom's years of education		0.15***	0.16***	
		(0.02)	(0.02)	
HH per-capita expenditure (1997)		0.15*	0.14	
		(0.09)	(0.10)	
HH per-capita expenditure (2000)		0.30***	0.36***	
		(0.11)	(0.12)	
HH per-capita expenditure (2007)		1.06***	1.14***	
		(0.09)	(0.10)	
Kindergartens/10,000 in kec. (1990)			0.04	
			(0.04)	
Kindergartens/10,000 in kec. (2000)			0.03	
			(0.02)	
Oldest birth cohort			0.10	0.26
			(0.14)	(0.48)
Birth order to mother			0.51	-2.00
			(0.66)	(1.98)
Older sibling attended kinder			0.09	-0.62
			(0.13)	(0.62)
Healthcare visits			-0.00	-0.72
			(0.10)	(0.45)
Very healthy			-1.22	0.67
			(1.12)	(1.59)
Male			-0.26***	0.35
			(0.10)	(0.47)
Mother Fixed-Effects	NO	NO	NO	YES
Adjusted R-squared	0.11	0.34	0.38	0.07
Number of observations	3154	3154	3154	221

Heteroskedastic-robust standard errors are reported in parentheses.

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$

Table 5: Instrumental Variable Estimation, Years of Education as Outcome Variable

	(1)	(2)	(3)	(4)
Kindergarten	2.33***	2.71***	2.86***	1.96***
	(0.24)	(0.62)	(0.68)	(0.61)
Urban		0.03	0.03	0.35**
		(0.15)	(0.15)	(0.14)
Two-parent HH		0.38*	0.37*	0.40**
		(0.19)	(0.19)	(0.18)
Mom's years of education		0.18***	0.18***	0.20***
		(0.03)	(0.03)	(0.03)
HH per-capita expenditure (1997)		0.16	0.15	0.20**
		(0.10)	(0.10)	(0.09)
HH per-capita expenditure (2000)		0.31***	0.30***	0.37***
		(0.11)	(0.11)	(0.11)
HH per-capita expenditure (2007)		1.15***	1.17***	1.17***
		(0.10)	(0.10)	(0.09)
Oldest birth cohort			-0.02	0.03
			(0.14)	(0.14)
Birth order to mother			0.30	0.36
			(0.71)	(0.71)
Older sibling attended kinder			-0.42**	-0.17
			(0.21)	(0.19)
Healthcare visits			0.03	0.03
			(0.10)	(0.10)
Very healthy			-1.33	-1.28
			(1.21)	(1.16)
Male			-0.20*	-0.24**
			(0.10)	(0.10)
Household Controls	NO	YES	YES	YES
Individual Controls	NO	NO	YES	YES
Community Controls	NO	NO	NO	YES
Number of observations	3154	3154	3154	3154
Adjusted R-squared	0.11	0.32	0.31	0.35

Heteroskedastic-robust standard errors are reported in parentheses.

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$

	Elementary			Junior			Senior		
Kindergarten	0.01*	0.03	0.10**	0.07***	0.01	0.28***	0.10***	0.03	0.12
	(0.01)	(0.03)	(0.04)	(0.02)	(0.05)	(0.08)	(0.02)	(0.05)	(0.10)
Mom's years of education	0.00		-0.00	0.01***		0.00	0.02***		0.02***
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
HH per-capita expenditure (1997)	-0.00		-0.00	0.00		-0.01	0.02		0.02
	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)		(0.01)
HH per-capita expenditure (2000)	0.01		0.00	0.02		0.01	0.04**		0.04**
	(0.01)		(0.01)	(0.01)		(0.01)	(0.02)		(0.02)
HH per-capita expenditure (2007)	0.04***		0.04***	0.08***		0.08***	0.12***		0.12***
	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)		(0.01)
Elementaries per 10,000 (2000)	0.00*		0.00***	0.00***		0.00***	0.00***		0.00***
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
Junior highs per 10,000 (2007)	0.00		0.00	-0.00		-0.00	-0.00		-0.00
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
Senior highs per 10,000 (2014)	0.00		0.00	-0.00		-0.00	-0.00		-0.00
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
Model	OLS	FE	IV	OLS	FE	IV	OLS	FE	IV
Adjusted R-squared	0.08	0.06	0.05	0.18	0.01	0.14	0.26	0.15	0.26
Number of observations	3154	221	3154	3154	221	3154	3154	221	3154

Heteroskedastic-robust standard errors are reported in parentheses.

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$

Table 7: Estimated Effects of Kindergarten Attendance, Stay-On as Outcome Variable

	6th Grade			9th Grade			12th Grade		
Kindergarten	0.06***	-0.06	0.16***	0.04*	0.06	-0.01	0.04*	-0.14	-0.04
	(0.01)	(0.04)	(0.06)	(0.02)	(0.06)	(0.09)	(0.03)	(0.10)	(0.12)
Mom's years of education	0.01***		0.00	0.01***		0.02***	0.02***		0.02***
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.01)
HH per-capita expenditure (1997)	-0.00		-0.01	0.02*		0.02*	0.02		0.02
	(0.01)		(0.01)	(0.01)		(0.01)	(0.02)		(0.02)
HH per-capita expenditure (2000)	0.01		0.00	0.03**		0.03**	0.06***		0.06***
	(0.01)		(0.01)	(0.01)		(0.02)	(0.02)		(0.02)
HH per-capita expenditure (2007)	0.04***		0.04***	0.06***		0.06***	0.15***		0.15***
	(0.01)		(0.01)	(0.01)		(0.01)	(0.02)		(0.02)
Elementaries per 10,000 (2000)	0.00***		0.00***	0.00***		0.00***	0.00*		0.00*
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
Junior highs per 10,000 (2007)	-0.00		-0.00	-0.00*		-0.00*	0.00		0.00
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
Senior highs per 10,000 (2014)	-0.00		-0.00	-0.00		-0.00	0.00		0.00
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
Model	OLS	FE	IV	OLS	FE	IV	OLS	FE	IV
Adjusted R-squared	0.12	0.03	0.10	0.15	0.13	0.15	0.24	0.05	0.24
Number of observations	2987	211	2987	2564	189	2564	1950	140	1950

Heteroskedastic-robust standard errors are reported in parentheses.

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$

Table 8: Estimated Effects of Kindergarten Attendance, Cognitive Test Scores as Outcome Variables

	2000			2007			2014		
Kindergarten	0.17***	0.01	-0.36	0.12**	-0.09	-0.07	0.04	-0.21	-0.13
	(0.05)	(0.12)	(0.28)	(0.05)	(0.26)	(0.30)	(0.05)	(0.28)	(0.26)
Mom's years of education	0.02**		0.03***	0.02***		0.03**	0.04***		0.04***
	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)		(0.01)
HH per-capita expenditure (1997)	0.01		0.03	0.02		0.03	0.05		0.05
	(0.03)		(0.04)	(0.03)		(0.04)	(0.04)		(0.04)
HH per-capita expenditure (2000)	0.02		0.03	0.03		0.03	-0.04		-0.03
	(0.04)		(0.04)	(0.04)		(0.04)	(0.05)		(0.05)
HH per-capita expenditure (2007)	0.11***		0.12***	0.10***		0.10***	0.11***		0.12***
	(0.03)		(0.03)	(0.04)		(0.04)	(0.04)		(0.04)
Elementaries per 10,000 (2000)	-0.00		-0.00	-0.01**		-0.01**	-0.00		-0.00
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
Junior highs per 10,000 (2007)	0.00		0.00	0.00		0.00	0.00**		0.00**
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
Senior highs per 10,000 (2014)	0.00		0.00	-0.00		-0.00	0.00		0.00
	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
Model	OLS	FE	IV	OLS	FE	IV	OLS	FE	IV
Adjusted R-squared	0.14	-0.02	0.10	0.15	0.11	0.14	0.14	0.05	0.14
Number of observations	2117	147	2117	2117	147	2117	2117	147	2117

Heteroskedastic-robust standard errors are reported in parentheses.

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$

Table 9: Means of Controls and Outcomes by Switching Status

	Switching Sample	No Kindergarten	All Kinder	One-Child Sample
Years of education	11.59	9.74	12.98	11.02
Attended kindergarten	0.48	0.06	1.00	0.49
Number of children	2.32	2.70	2.11	1.00
General health status (1997)	1.97	1.97	1.89	1.94
Visits to outpatient care (1997)	0.17	0.17	0.26	0.27
Mom's years of education	6.97	4.98	9.20	6.44
HH expenditure per capita (1997)	12.36	12.06	12.53	12.32
HH expenditure per capita (2000)	12.13	12.11	12.49	12.37
HH expenditure per capita (2007)	12.97	12.87	13.30	12.99
Two-parent HH	0.99	0.91	0.94	0.84

Table 10: Selection into Kindergarten Attendance

	(1)	(2)	(3)
Urban	0.02	0.05	
	(0.03)	(0.21)	
Two-parent HH	0.04	-0.00	
	(0.02)	(0.13)	
HH head's years of education	0.01***	0.01	
	(0.00)	(0.01)	
Mom's years of education	0.02***	-0.00	
	(0.00)	(0.01)	
HH per-capita expenditure (1997)	0.02	-0.02	
	(0.01)	(0.07)	
HH per-capita expenditure (2000)	0.04***	-0.08	
	(0.01)	(0.07)	
HH per-capita expenditure (2007)	0.02	0.05	
	(0.01)	(0.07)	
Oldest birth cohort	0.04**	0.10	0.06
	(0.02)	(0.09)	(0.07)
Birth order to mother	0.12*	-0.27	-0.00
	(0.07)	(0.30)	(0.22)
Older sibling attended kinder	0.20***	-0.40***	-0.40***
	(0.02)	(0.05)	(0.10)
Poorly healthly	-0.02	0.07	0.30**
	(0.02)	(0.13)	(0.15)
Fairly healthy	-0.01	0.33*	0.54***
	(0.03)	(0.18)	(0.18)
Very healthy	0.12	.	0.68**
	(0.15)	(.)	(0.27)
Healthcare visits	-0.01	-0.06	-0.05
	(0.01)	(0.09)	(0.07)
Male	-0.03**	-0.14**	-0.05
	(0.01)	(0.07)	(0.06)
Kindergartens/10,000 in kec. (1990)	0.05***	0.01	
	(0.01)	(0.03)	
Kindergartens/10,000 in kec. (2000)	-0.00	-0.01	
	(0.00)	(0.01)	
Restricted to FE sample	NO	YES	YES
Has mother fixed-effects	NO	NO	YES
Number of observations	3154	220	221

Heteroskedastic-robust standard errors are reported in parentheses.

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$

Appendices

Appendix A   Logit Regression

In some parts of my analysis, I employ logit regression when analyzing a binary outcome–I will briefly explain that empirical method here. This is not a primary empirical method I employ to answer my question, but instead a model I use to analyze selection into kindergarten, attrition, and robustness checks for binary outcomes such as school completion or school attendance.

The logit model differs from OLS in that it restricts the estimated probability from a function to between 0 and 1, improving our ability to interpret model estimates. It differs from the probit model in that it uses a logistic cumulative distribution function (c.d.f.), rather than a standard normal. Additionally, the logit model allows the use of fixed-effects, which I employ to analyze within-household selection. To be clear, when I present coefficients resulting from a logit model, those are the *average* marginal effects (MEs)–calculated after model estimation.

The logit model fits the following model:

$$\Pr(y_i = 1 | \mathbf{x}_i) = \Lambda(\mathbf{x}_i'\beta) \tag{3}$$

where $\Lambda(.)$ is the logistic c.d.f., so that $\Lambda(z) = \frac{e^z}{1+e^z}$.

Appendix B   Sample Attrition

As I discuss thoroughly in Section 3, my sample suffers from attrition as I merge data from across different waves of the Indonesian Family Life Survey (IFLS). In this section, I examine that attrition and check whether it introduces bias into my estimates of kindergarten's effects.[38]

Following the approach of Baulch and Quisumbing (2011), I used the set of covariates employed in the regression analysis as ell as variables collected on the quality of the initial childhood interview in 1997 (Baulch and Quisumbing, 2011).[39] There is variation in the resultant inverse probability weight; the mean

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[38]In this section, when I discuss attrition, I am referring to total attrition, i.e., whether an individual was *ever* lost to attrition–whether that be from merging across waves of the IFLS or from missing observations.

[39]The two interview quality questions were the interviewer evaluation of the interviewed child's attention (on a scale of 1-5) and the interviewer evaluation of the respondent's answers' accuracy. The two are measures are highly correlated (their pairwise correlation is 0.8941) and both have a significant, positive coefficient when predicting total attrition. I had to exclude some covariates because of a lack of variation in attrition; in particular, I excluded province fixed-effects, the two-parent household

weight is 1.07 and the median is 0.993. A basic t-test confirms that the mean weight is not 0, with a 95% confidence interval of (1.06, 1.08).

I applied the inverse probability weight to both the OLS and IV models, focusing on years of education.[40] For the OLS model, my results are not different; the inclusion inverse probability weights slightly raises the point estimate of the effect of kindergarten from 0.66 to 0.65, and makes it slightly less precise with a standard error of 0.15, rather than the previous standard error of 0.14. The IV estimates, on the other hand, are more different; kindergarten's estimated effect on educational attainment is now 1.64, rather than 1.40– and the standard error is unchanged. Comparing the non-weighted model to the weighted models, there are no significant divergences in the coefficients of covariates. Because of my inability to apply inverse probability weights to the fixed-effects model and the absence of a significant difference for the OLS and IV estimates, I do not include inverse probability weights for the main results presented in this paper.

Appendix C   Switching Sample Restriction

As briefly discussed in Section 4.2, the employment of fixed-effects places additional stringent restrictions on my sample–resulting in loss of 93% of my full sample when restricting to only the 'switching' sample employed in the fixed-effects estimation.[41]

I have also conducted basic analysis and diagnosis of the effects of this loss of sample. To begin, I have constructed a diagnosis scatterplot, Figure 5, to visualize selection into the switching sample. This graph allows us to make several observations. Province fixed-effects–which I employ for my OLS estimates–does not result in any attrition, since each group has a non-zero standard deviation in kindergarten attendance. On the other hand, there are many families, denoted by blue circles, with a standard deviation of zero in kindergarten attendance and that are thus removed from the sample. Nonetheless, these families still have a great deal of variance in educational attainment.

I also prepared a table capturing the summary statistics for all variables–the controls as well as outcome

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dummy, the birth order variable, and general health categorical variable. None of these variables were significant predictors of attrition in the first place. On another note, I employ logit regression rather than probit regression to ensure consistency with my analysis of switching and selection into kindergarten–on the other hand, Baulch and Quisimbing employ the latter.

[40]Since these inverse probability weights are calculated at the individual level, they vary within household and between siblings, thus disallowing me from using them in the mother fixed-effects model. If attrition is not significant at the individual level, then I would suspect it is not significant at the household level when switching attrition–which occurs at a significantly higher rate–is accounted for (which I do in Appendix C).

[41]To be transparent about this reduction in sample size, I have ensured that the sample size indicated at the foot of the regression tables with fixed-effects results *only* includes the switching sample.

variables, since all observations in the table are not removed from the sample by the switching restrictoin, as seen in Table 9. There appears to be some selection bias into the switching sample–it appears that the switching sample is more educated than the non-switching sample, for example. I follow this with a logit model, employing my full set of covariates and province fixed-effects. The only significant predictors of switching status at a 95% confidence level are kindergarten attendance (a positive effect), whether a household has two parents (a negative effect), a household expenditures in 1997 (a negative effect), and a household expenditures in 2000 (a positive effect).[42] It makes sense kindergarten has significant positive margin effects; after all, some kindergarten attendance within a household is strictly necessary for switching status. Disregarding the expenditure variables (which appear to counteract one another) and kindergarten attendance, I conduct a joint significance test that the marginal effects for the most important variables for determining educational attainment are all equal to 0–and fail to reject the null hypothesis that none are significant.[43] These results tentatively suggest that the loss of sample–despite its high rate–from the full sample to the switching sample does not bias my fixed-effects results.

On the other hand, the process of selection into kindergarten appears to be biased by this attrition. I run two separate logit regressions, one with only my switching sample and the other only with the non-switching sample. The switching sample logit reveals negative, significant AMEs for the kindergarten spillover variable and the total number of visits to a doctor–an inversion of both my theoretical expectations as well as the empirical findings from both the OLS and IV estimations of selection into kindergarten for the full sample. That comparison can be found in Table 10. Additionally, the non-switching sample reflects strong positive effects for both the household head's years of education as well as the mother's–while the switching sample only shows significance for the latter, albeit with three times as large of a standard error. This is emblematic of the core problem with this sample loss–even if, as my analysis above suggests, there is not a clear bias in switching attrition, the sheer decrease in sample size results in imprecision and loss of efficiency in my estimates that cloud my results. This, in conjunction with the correlated error term issue I discuss later, motivate my use of instrumental variable (IV) estimation.

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[42]Some province dummies are positive significant, though none are significantly negative.

[43]Specifically, I test the AMEs for household head's years of education, mother's years of education, urban (1997, 2000, and 2007), number of kindergartens per kecamatan (1990 and 2000), and kindergarten spillover from older siblings.

Appendix D   Selection into Kindergarten Attendance

To understand the role of kindergarten in human capital accumulation, it is necessary to first understand what drives kindergarten attendance. To understand this process, I used a series of logit regressions, the results of which can be found in Table 10.[44] In this table I present the average marginal effects (AME) associated with each covariate. I included all of the covariates I used in my primary results, found in Section 5. I organized the table into 3 model specifications: (1) full sample, full set of covariates, and no fixed-effects; (2) only the switching sample, full set of covariates, and no fixed-effects; and (3) only the switching sample, individual-level covariates, and fixed effects.[45]

We can make a few interesting observations. First, note the strength of one of my selected instruments–here included only as an ordinary regressor, the number of kindergartens per 10,000 people in each kecamatan in 1990. This confirms the strength of that instrument and motivates its use.[46] However, when restricted to the switching sample, this significance disappears. Additionally, household wealth and education, age, and whether an older sibling attended kindergarten are all positive and significant–although none are positive and significant for the switching sample. In fact, for the switching sample drastically the effect of an older sibling attending kindergarten is drastically changed to a strong negative one. This heterogeneity reflects the bias of small sample size inherent in my employment of fixed-effects and motivates the use of IV estimation.

What is important to note is that the health indicators–insignificant in the OLS models here, as well as each of the OLS, fixed-effects, and IV models presented for educational outcomes–have significant and positive AMEs for the fixed-effects logit regression, when total visits to healthcare is also controlled for. Based on these results, it appears my covariates on the household and community level explain kindergarten attendance well, while my individual-level covariates–within a fixed-effects framework–provide an incomplete, yet suggestive, story of selection into kindergarten attendance.

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[44]For information on the logit regression method, see Appendix A.

[45]Recall that non-individual-level covariates would not be applicable to the fixed-effects model.

[46]When the 1990 variable is excluded, the number of kindergartens per 10,000 people in each kecamatan is similarly positive and statistically significant.

Appendix E   OLS Regression - Post-Estimation Analysis and Robustness Checks

E.1   OLS Assumptions - Homoskedasticity and Zero Conditional Mean of Errors

A natural starting point with analyzing the OLS results is to check 2 critical assumptions of OLS: 1) homoskedastic errors and 2) uncorrelated errors. Homoskedasticity requires that $\text{Var}(u|x) = \sigma^2$ for residual u and explanatory variable x (in all of my models, kindergarten attendance). Then uncorrelated errors require that errors are not correlated with values of the explanatory variable, or equivalently that the conditional mean of u is 0: $\text{E}(u|x) = 0$.

First, using the Breusch-Pagan/Cook-Weisberg test for heteroskedasticity, I reject the null hypothesis that errors exhibit constant variance, or equivalently are homoskedastic, for *every* OLS model. This is not a problem, as heteroskedasticity does not bias our results so long as we ensure that reported standard errors are robust to heteroskedasticity. This does mean that the results of latent-variable models such as logit regression, are biased as they strictly require homoskedasticity. Thus, I do not use those regressions for my main results, and only for robustness checks.

Second, none of the OLS models exhibit any correlation between kindergarten attendance and their respective residuals–the residuals for each model exhibit a pair-wise correlation with kindergarten attendance of 0.0000. Thus, my results do not violate the zero conditional mean assumption of OLS and are not biased in this respect.

E.2   Omitted Variables Bias and Specification Tests

I suspect that my OLS models suffer from omitted variable bias because of unobservable mother characteristics influencing educational attainment. One way to check this is through specification tests, such as Ramsey's RESET test, in which powers of fitted values of the outcome variable are added to the regression (Long and Trivedi, 1992; Ramsey, 1969). Then a joint significance test is conducted to check whether, jointly, these coefficients are equal to 0; if so, then the initial model specification is adequate, and if not then the initial model specification is inadequate and there may be non-linearity or an omitted variable. The Ramsey test leads me to reject the null hypothesis that there are no omitted variables for each model; this finding is what motivates my use of mother fixed-effects, as I theorize unobserved mother characteristics–omitted in the OLS models–have a critical impact on educational outcomes.

There are other ways to examine the adequacy of a model, along the same conceptual lines as the Ramsey test, i.e., fitting a richer model and testing whether the new variables are jointly significant. To further test omitted variable bias, then, I create an exhaustive set of interaction terms, with kindergarten interacted with every one of the covariates in the fully-specified OLS model, and include these as variables alongside the original covariates. I then conduct a basic Wald Test on the null hypothesis that the joint significance of these terms is 0. For every model I reject the null hypothesis, further suggesting that my OLS estimates suffer from omitted variable bias and that the omitted variables are not only interaction or non-linear terms with existing variables.

Finally, I need to check whether kindergarten attendance, my independent variable, is endogenous. At 95% confidence, I reject the null hypothesis that kindergarten attendance is exogenous for all models except those with the following outcome variables: senior completion; school attendance in the 2nd, 3rd, 4th, 5th, 10th, 11th, 12th, 13th, and 14th grades; stay-on after the 6th, 9th, and 12th grade; and cognitive performance in 2000, 2007, and 2014. This does not invalidate my IV approach, as my instruments largely satisfy the two assumptions of IV estimation, but it does strengthen the OLS estimates.

E.3 Switching Sample

Because of worries about bias resulting from nonrandom selection into the switching sample that the mother fixed-effects model uses, I can run the OLS estimation for only the switching to check if selection into switching induces heterogeneity in the effects of kindergarten.[47] As I would predict given the drastic reduction in size from the non-switching sample to the switching sample (the latter is 7% the size of the former), the standard errors are significantly higher for the OLS models restricted to the the switching sample: none of the coefficients for kindergarten attendance are statistically significant, either positively or negatively, for any of the outcome variables. Thus, even if there is not clear bias in selection into the switching sample, the sheer magnitude of the attrition is introducing bias into the fixed-effects estimations.

E.4 Migration

Intra-Indonesia migration between IFLS waves may introduce confounding factors into my analysis, although I do not anticipate it being a significant issue because 1) improved educational opportunities resulting from moving would be absorbed by the schools per 10,000 variable and 2) improved economic opportunities

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[47]See Appendix C for more detail and analysis of selection into the switching sample.

resulting from moving would be absorbed by the household per capita expenditure variable. Both these variables are collected over time, so they are not fixed in one place. Nonetheless, it is worthwhile to investigate whether kindergarten effects substantively differ between the total sample and migrating sub-samples.

For this section, I define migration simply as whether an individual completed the IFLS identifying with a different combination of province, kabupaten, and kecamatan from one wave to another. Further, I break migration into two types: 1) migration that would have occurred while a child was in school, between 1997 and 2007, and 2) migration that occurred between 2007 and 2014, around when a child would be coming-of-age and exiting school. Overall, 61.8% of the sample migrated between 1997 and 2007, and 8.99% of the sample migrated between 2007 and 2014.

The results for the migrating sub-samples are mixed. First, for the sub-sample that migrated between 1997 and 2007, the effect of kindergarten on years of education and junior high school completion is approximately halved, but remains significant positive. Overall, for this sub-sample the estimates of kindergarten's effects tend to be similar as for the main sample, albeit with lower magnitude and insignificance in some cases. Second, for the sub-sample that migrated between 2007 and 2014, the results are similar but mostly of a greater magnitude. The most notable difference is that, unlike for the 1997-2007 migrating sub-sample and the total sample, for the 2007-2014 migrating sub-sample, kindergarten had a significant positive effect on the completion of junior high and senior high, and kindergarten effects actually increased over time, the opposite of fadeout. This motivates further research into the role of migration in educational attainment.

Appendix F   IV Estimation - Post-Estimation Analysis and Robustness Checks

F.1   Treatment Effects IV Model

There are several robustness checks I can conduct for the IV Estimations. First, I can add structure to the IV estimations involving a binary independent variable by employing a latent-variable model in the first-stage regression–creating one type of 'treatment-effects' instrument variable model. Since kindergarten attendance, a binary variable, is the treatment variable for all of my models, I can employ this robustness check to every specified model in the paper. I compare the results of the treatment-effects model to the basic model. Critically, all of the IV estimates exhibit some degree of heteroskedasticity; this does not bias the IV results since I can employ a weighting matrix optimal for heteroskedastic errors. However, for the latent-variable model used in this treatment-effects model, heteroskedastic errors *do* bias estimates. Thus, while

these results are suggestive as a robustness check, they are biased and should be viewed with skepticism.

Comparing the two types of IV models, there are significant differences: most prominently, the treatment-effects models estimates kindergarten has a strong significant negative effect on elementary completion and a significant negative effect on school attendance in the 3rd, 4th, 5th, 6th. Note that kindergarten's effect on none of these outcomes was found to be significant using GMM IV estimation. Other than these outcome variables, the results either matched or were slightly less significant than the results from the GMM IV estimation.

F.2 Switching Sample

Much like I did above, in Appendix E.3, I can run the IV estimation for only the switching sub-sample. The results of this robustness check are largely the same as for the OLS estimates; the standard errors are rendered so large for the coefficient of kindergarten attendance as to make all estimates insignificant, either positively or negatively.

F.3 Alternative Instruments

In this section, I examine the implications of selecting alternative instruments, in particular: 1) focusing exclusively on private kindergartens, 2) focusing exclusively on public kindergartens, and 3) the percent change of the number of kindergartens per 10,000 people in each kecamatan. Unsurprisingly, the IV estimates using only private kindergartens in 1990 and 2000 as instruments were nearly identical to the estimates using the total number of kindergartens–as nearly all kindergartens were private.[48] On the other hand, the IV estimates using only the public kindergartens in 1990 and 2000 as instruments were imprecise and no kindergarten effect for any outcome was statistically significant.[49] The percentage change in kindergarten is a perplexing instrument; in the first-stage regression, it has a significant (with a t-value of -5.21) negative coefficient of -0.0001783. Nevertheless, none of its estimates of kindergarten's effects are statistically significant. I hypothesize this is because increases in kindergartens were heavily concentrated in places with little to no kindergarten to begin with, in 1990.

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[48]There is a 0.9865 correlation between the private and total in 1990 and a 0.9876 correlation in 2000.

[49]Given the sheer paucity of public kindergartens, there is a 0.0997 correlation between public and total in 1990 and a 0.3069 correlation in 2000.

F.4 Migration

See Appendix E.4 for a description of how I treat migration. First, kindergarten's effects are now entirely insignificant for the sub-sample that migrated between 1997 and 2014. Second, the effect of kindergarten is insignificant for every outcome for those that migrated between 2007 and 2014, I hypothesize because of the loss of 91.01% of the total sample when restricting to the 2007-2014 migrating sub-sample. Much like the parallel OLS robustness check, this finding warrants further analysis of the role that migration plays in educational attainment and the effects of kindergarten.