All I Really Need to Know I Learned In Kindergarten? Evidence from the Philippines[*]

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Abstract

Although there is broad agreement that investments in early childhood education are important, questions about the relative effectiveness of different types of programs persist. We study the effects of two distinct types of kindergarten programs in the Philippines: the Jumpstart kindergarten program, implemented by a local faith-based NGO between 2005 and 2017, and a government-sponsored kindergarten program mandated nation-wide beginning in 2012. Exploiting the timing of the roll-out of these two programs relative to the age different children within a household could enter kindergarten, we find large effects on primary school academic performance due to attending Jumpstart and smaller effects due to attending the government kindergarten. We then examine mediating characteristics that may explain these differential effects. We find that socio-emotional skills such as self-control, openness, and conscientiousness partially mediate our primary results. These findings support other research that highlights the importance of developing socio-emotional skills in the pre-elementary grade levels not only as ends in themselves but also as mediators to better academic performance.

Keywords: Early childhood education, socio-emotional skills, cognitive traits, human development, academic motivation, kindergarten

JEL Codes: I21, I24, I25, I28, O22

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"All I really need to know I learned in kindergarten. All I really need to know about how to live and what to do and how to be I learned in kindergarten. Wisdom was not at the top of the graduate-school mountain, but there in the sandpile at school." - Robert Fulghum (1986) *All I Really Need to Know I Learned in Kindergarten*

1   Introduction

Investments in early childhood education matter. These investments often not only lead to higher educational attainment, and in some cases increased learning, but also enable other positive outcomes. Preparing and motivating young children to learn is fundamental for the success of other economic development policies and programs.[1] An extensive empirical literature suggests that interventions in early childhood—in particular through preschool and kindergarten education programs— can have large, positive, and lasting effects across a diverse set of contexts (Currie 2001; Behrman et al. 2004; Cunha et al. 2006; Heckman 2006; Berlinski et al. 2008; Berlinski et al. 2009; Chetty et al. 2011; Rao et al. 2012; Heckman et al. 2013; Gertler et al. 2014; Araujo et al. 2016; Attenasio et al. 2020).

Although these views are shared broadly by policymakers and researchers, several important caveats exist. First, the effectiveness of early childhood education interventions ultimately hinge on the behavioral response of parents (Das et al. 2013, Heckman et al. 2006). Parents may either be unwilling to send young children to school or may not value early childhood education (Bouguen et al. 2018). Moreover, if these beliefs and the parental response to available preschool or kindergarten programs are positively correlated with lower levels of early childhood investment, then the very children who may need preschool or kindergarten the most may not be enrolled (Blau and Currie 2006; Cornellissen et al. 2016). Second, there is less agreement about the most effective and efficient ways to design interventions to assist disadvantaged children. Across a variety of countries, early childhood education programs may focus on several different factors of human development, such as academic skills, nutrition, physical health, mental health, or social and emotional development (Nores and Barnett 2010). In a randomized trial, Attanasio et al. (2018) find that while the offer to transfer from home-based childcare to childcare centers in Colombia increased nutritional outcomes, movement to the childcare centers had no effect on the development of socio-emotional skills and actually decreased children's cognitive development. Indeed Attanasio et al. (2019) find in the Indian Young Lives data that even successful early childhood investments display some "fade out"

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[1]See a discussion of this idea in the 2018 World Development Report, "Learning to Realize Education's Promise" (World Bank 2018).

and therefore usually need to be sustained and complemented by further interventions in order to increase the likelihood that they display measurable long-term effects. Taken together, it is clear that critical questions persist over the relative effectiveness of different types of early childhood interventions.

In this paper we examine the effectiveness of two distinct kindergarten programs in rural Philippine villages. The first program is the Jumpstart kindergarten program implemented by International Care Ministries (ICM), a faith-based NGO operating in the Philippines.[2] The goal of the Jumpstart kindergarten program is to prepare poor and disadvantaged children for success in the public education system. The second program we evaluate is a government-sponsored kindergarten program that was mandated across the country based on the Kindergarten Education Act, passed in 2011 by the Philippine government. This legislation expanded access to kindergarten education throughout the Philippines and mandated that each incoming first grade student must previously enroll and participate in at least one year of kindergarten. By exploiting variation in the timing of the rollout of these programs relative to the timing at which different children within a household were eligible to enter kindergarten, we can evaluate their respective effects on future educational outcomes using a fixed-effects identification strategy similar to Duflo (2001) and Wydick et al. (2013). Although both kindergarten programs each use existing Philippines DepEd kindergarten curriculum, the Jumpstart program also includes several "enrichments." These "enrichments" include medical checkups, provided lunch and snack at school, free school supplies, an added emphasis on the development of character values, as well as potentially better and more motivated teachers. Comparisons between the government kindergarten program and the Jumpstart program identify the combined effect of these enrichments.[3]

We find evidence of substantial effects of enrollment in ICM's Jumpstart kindergarten program on academic performance in elementary school. In particular, we estimate that relative to children who did not go to kindergarten, children who enrolled in ICM's Jumpstart kindergarten are roughly one and a half to twice as likely to be the top academic performer within their household in both third grade specifically and in all of elementary school in general. By contrast children who enrolled in the government kindergarten program are no more likely be the top academic performer within their household compared to children who did not go to kindergarten.[4]

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[2]This is a different program implemented by the same NGO studied by Bryan et al. (2020).

[3]Section 2 includes additional detail about the specific components of these two kindergarten programs.

[4]These effects on the government kindergarten program are consistent with the effects of a similar program in Norway that mandated kindergarten enrollment (Drange et al. 2016).

These results motivate an investigation into the factors that potentially mediate the outcomes of both Jumpstart and the government kindergarten program. We find evidence of positive and meaningful effects on socio-emotional outcomes such as grit and self-identity, and positive but marginally significant effects on self-control and spirituality among children who attended Jumpstart. Re-categorizing our survey questions to create measures of each of the "Big 5" attributes, we find positive effects on openness and conscientiousness among children who attended Jumpstart. Among children who attended the government kindergarten, we find evidence of positive effects on an alternative set of socio-emotional outcomes, such as peer affiliation, self-control, and agreeableness and positive, but marginally significant, effects on conscientiousness and spirituality.[5] Applying a formal mediation analysis (Preacher and Selig 2012), we find that the effects on our primary outcomes may be, in part, mediated by socio-emotional factors, such as self-control, openness, and conscientiousness. The direct effect of enrollment in the Jumpstart program, however, remains strong across all specifications.

This paper contributes to the literature on the effect of early education interventions in three ways. First, due to the nature of the role-out of ICM's Jumpstart kindergarten program (beginning in 2005) and the Philippine government kindergarten program (beginning nation-wide in 2012) we are able to compare the relative effects of attending these two distinct kindergarten programs. This allows for an important discussion about the relevant counterfactual for children used when estimating program impact. Children from different socio-economic backgrounds may experience different benefits from enrollment in these programs, not only because they possess different characteristics, but also because they would have different early educational experiences in the absence of these programs (Berkes and Bouguen 2018). Our research design allows for not only an estimate of program impact against a "no kindergarten" counterfactual, but also for a comparison of effects between ICM's Jumpstart kindergarten program and the government kindergarten program.

Second, we investigate the extent to which the development of socio-emotional or psychological characteristics mediate any estimated effects of enrollment in either kindergarten program. A growing literature suggests that the development of socio-emotional or psychological characteristics are instrumental in driving future academic, labor market, and adult life outcomes (Heckman 2008; Borghans et al. 2008; Chetty et al. 2011; Heckman et al. 2013; Wydick et al. 2013; Glewwe et al. 2018). Using measures of grit (Duckworth and Quinn 2009), self-control (Duckworth and Gross

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[5]These findings are generally consistent with those found by Kosse et al. (2020) studying the role of social environment on pro-social behavior.

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2014), peer affiliation, positive identity, and the "Big 5" personality characteristics, we aim to understand if any of these factors explain not only the treatment effects of each kindergarten program, but also the difference in impact estimates between ICM's Jumpstart kindergarten program and the Philippine government's kindergarten program.

Finally, results from this research add to the literature comparing the effectiveness of public and private education systems, particularly in developing countries (Andrabi et al. 2008; Tabarrok 2013; Bold et al. 2013; Muralidharan and Sundararaman 2015; Wamalwa and Burns 2018; Romero et al. 2019). Our results indicate that children who enrolled in a private, faith-based kindergarten program—ICM's Jumpstart program—acheived better academic performance in elementary school compared to children who enrolled in a public government-funded kindergarten program. Additionally, school aged children who enrolled in the Jumpstart program are more likely to be currently enrolled in school compared to school aged children who attended a government kindergarten. Despite these findings, both Jumpstart and the government kindergarten program help children place in the top third grade section, but this outcome is not able to differentiate the relative performance of children within the top section.[6] Although there are important considerations relating to external validity and scale-up of a relatively small-scale program implemented by an NGO (Bold et al. 2018), for the children in our data, ICM's Jumpstart kindergarten program displayed larger effects than the government kindergarten program on academic performance in elementary school.

The remainder of the paper is structured as follows. The next section introduces and provides contextual details about both the Jumpstart kindergarten program and the Philippine Kindergarten Education Act. Section three introduces the data, presents the identification strategy we use to estimate effects of enrollment in the two different kindergarten programs, and discusses the approach we use to perform mediation analysis. The fourth section presents the results on primary outcomes, the mediation analysis, and heterogenous effects. Section five discusses robustness checks and the limitations of this analysis. Finally, section six concludes.

2   Kindergarten in the Philippines

The first decade of 21st century in the Philippines saw a nationwide decline in educational outcomes. Despite a global push for universal primary education, in the Philippines the net enrollment rate for primary schools dropped from 96 percent in 2000 to 84 percent in 2005 (DepEd Philippines 2007).

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[6]Placement in the top third grade section is based on performance on the National Achievement Test (NAT). We will discuss these data in more detail in Section 3.

In 2005, only 70 percent of students successfully completed primary school (DepEd Philippines 2015).

Both local NGOs as well as the Philippine government exhibited growing concern over these trends. In response, ICM initiated the Jumpstart kindergarten program in 2005 with the goal of improving the preparation of children for elementary school education. Jumpstart began with four kindergarten centers in 2005, growing in subsequent years to 75 schools. Several years later, in 2011, the Philippine government passed the Kindergarten Education Act (No. 10157) that mandated kindergarten education as an integral part of the country's basic education system, and the government began to quickly phase-in kindergarten access across the country. In 2012 the government legislated that all public schools provide kindergarten to local students.

2.1 ICM's Jumpstart Program

The core objectives for Jumpstart are informed by the Millennium Development Goals "Education for All" objective. This guides the aim of the Jumpstart program to improve early childhood education in rural areas of the Philippines, particularly targeting the most disadvantaged and vulnerable children. ICM designed Jumpstart in line with their faith-based values.[7]

In practice, Jumpstart includes several components. The first is a ten-month kindergarten education program that meets five days per week, similar to any other standard kindergarten program. ICM designed this first component specifically to prepare students for future success in public school. In this vein, ICM carefully instructs Jumpstart teachers to use the existing Philippines DepEd kindergarten curriculum. The second component aims to add "enrichments" over and above the typical Philippine public kindergarten program. These enrichments include medical checkups, provided lunch and snack at school, and free school supplies. The medical checkups include provision of de-worming treatment, standard medical screenings, and height and weight monitoring on a monthly basis. The daily provided lunch and snacks include nutritionally fortified rice and other food including soy protein. The provided school supplies include school uniforms and other necessary classroom infrastructure. Finally, Jumpstart also includes a component for parents. This component is essentially the VHL (Values, Health, and Livelihood) "Transform" program which exists as ICM's core program and includes access to credit services, house visits, and livelihood training.[8]

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[7]The Biblical basis for Jumpstart comes from Jeremiah 29:11 "'For I know the plans I have for you," declares the Lord, "plans to prosper you and not to harm you, plans to give you hope and a future."'

[8]See Bryan et al. (2020) for a detailed discussion and evaluation of ICM's "Transform" VHL Program.

The selection and implementation of Jumpstart follows a strict set of guidelines. An application must be submitted to ICM by a local Protestant pastor. This application must provide (a) evidence of support from the village which includes a signed statement of commitment from at least five members of the local church, (b) a village profile providing information about the need for a kindergarten in the area, and (c) a list of at least 40 potentially eligible kindergarten-aged children. Jumpstart teachers must apply with a referral letter from their local pastor indicating their commitment to early childhood development and education. Additionally, they must be a university graduate with a degree in education and must attend a Philippine Department of Education teacher training program. Children must be at least five years old on or before October 31st of the year they begin kindergarten and come from a low-income family. In practice, families are recommended by their local pastor and parents must sign a commitment letter with ICM. Class sizes are strictly capped at 40 children, however, a few "visitor" children can observe the class but are generally not entitled to the additional school supplies or other provisions.

When Jumpstart began in 2005 it filled a needed gap because support by the Philippine government for kindergarten education was minimal. Before passage of the Kindergarten Education Act in 2011, ICM signed an agreement with the Philippine government that Jumpstart would only fill the gaps in public kindergarten provision. Consistent with this agreement, in 2018 ICM officially ended the Jumpstart program based on an understanding that the public K-12 education system was operating at scale throughout the country. At the time, the Jumpstart program had implemented over 700 kindergarten class cohorts and served about 20,000 children over its history.

2.2 The Kindergarten Education Act

In 2011 the Philippine government passed the Kindergarten Education Act which implemented a "mandated and compulsory" pre-elementary year of kindergarten education. After the 2011-2012 school year, all Filipino children were required to attend kindergarten before enrolling in first grade.

The passage of the act led to positive advances in enrollment in early childhood education. The net enrollment rate in kindergarten jumped from 55 percent in 2010 to 75 percent in 2015 and elementary school completion rates grew from 70 percent in 2005 to 83 percent in 2015 (UNESCO 2015). These changes benefited children from the poorest Philippine households. In 2008, the gross enrollment rate in kindergarten for the poorest 20 percent of the population was 33 percent. In 2013, however, this rate increased to 63 percent (World Bank 2016). Although these figures suggest positive changes in kindergarten and elementary enrollment, the Philippines continues to

lag behind neighboring countries in a number of other educational dimensions. For example, only 53 percent of children from the poorest 20 percent of households attend high school (World Bank 2016). This implies that although disparities in access to early childhood education may have declined, disparities in the quality of early childhood education may still persist.

Although kindergarten enrollment has surged in the Philippines, questions remain over the quality of both Jumpstart and the government kindergarten program relative to both the counterfactual of no kindergarten and to each other. We use the age-eligibility rule for kindergarten in both programs in conjunction with the timing of the introduction of the programs in different villages to identify their respective effects on subsequent academic performance as well as potential mediators that may explain differences in their effects.

3   Empirical Framework

Our data come from a household survey administered in 2017.[9] The questionnaire includes information on kindergarten enrollment, mother characteristics, primary school academic performance, and socio-emotional or psychological characteristics of each child. In total, the data includes 2,692 children in 1,132 households across 93 villages. Because we use an identification strategy using household fixed effects to compare outcomes across siblings, we exclude households with only one child, leaving 2,437 children in 943 households across 88 villages for our analysis.

Every third-grade student in the Philippines takes the National Achievement Test (NAT), which assess student reading ability (in both English and Filipino), mathematics, and science. Based on the results of the NAT, students are placed into different class sections. About 30 of the best-performing students are placed in the top section, then the next 30 students are placed in the following section, and so on.

Although we do not have direct access to NAT results by child, we employ a survey based on within-household comparisons of academic performance of each child as reported by mothers.[10] We ask mothers to identify which of their children was the top academic performer in third grade and elementary school overall, which represent two of our primary outcome variables. The core limitation of these two academic outcomes is that they involve perceptions reported by the mother,

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[9]The survey was designed by a research team at the University of San Francisco and implemented in partnership with ICM.

[10]In 81 percent of cases, the child's mother was the respondent to the survey. If the mother was not available another household member served as the respondent. In 11 percent of cases the respondent was the child's father. In 4 percent of cases the respondent was the child's grandmother. In the remaining 4 percent of cases the respondent was the child's grandfather, aunt, or uncle.

however even to the degree that they are subjective, maternal perceptions of academic ability tend to strongly influence demand for schooling in the future (Jensen 2010).[11] The third outcome variable indicates whether a child was placed in the top section in third grade based on his or her NAT score.[12] Finally, the fourth outcome variable indicates if a child is currently enrolled in school, giving us another objective measure of educational and academic performance.

The maternal questionnaire also includes questions on academic and socio-emotional skills that potentially mediate academic performance in primary school.[13] These questions broadly take the form: "Relative to children his/her age [child i] practices math frequently," or "Relative to others his/her age [child i] is easily discouraged." To which mothers answered using a one through five Likert scale; with one indicating "strongly disagree" and five indicating "strongly agree." These questions aggregate together to create indices measuring academic or scholastic skills and a host of socio-emotional characteristics such as: grit, peer-affiliation, self-control, self-identity, spirituality, and behavior. We also use the same survey questions to generate an alternative set of indices, which approximate the "Big 5" personality characteristics. These characteristics include: openness, conscientiousness, extroversion, agreeableness, and neuroticism. This alternative set of index variables allows for a robustness check to the specific categorization of these questions measuring socio-emotional skills. We use the methods of Kling et al. (2007) to generate standardized indices. In the appendix we show a table which summarizes these variables. They each have a mean of zero and a standard deviation of one.[14]

Table 1 shows summary statistics of our data. Respondents in our data, who are mostly mothers, are on average about 43 years old. Less than half have attended high school and only about 10 percent have attended college. Most are married, stay at home, and participated in ICM's

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[11]It is worthwhile to note here that test scores themselves hold several limitations. First, the latent relationship between the test score and student learning is often unknown (Glewwe 1997; Lang 2010; Jacob and Rothstein 2016). Second, evaluating educational performance on test scores alone will omit potential confounding factors that lead to relatively low test scores despite substantial learning. Our parental-reported measures may more accurately account for these sorts of factors.

[12]A limitation of this variable is that only 78 percent of children have valid responses. We therefore, predict the missing values using a linear regression based on kindergarten enrollment, the within-household indicator of best academic performance in third grade, the child's age, the sex of the child, birth order dummy variables, and household/mother fixed effects.

[13]Each of these questions are listed in the appendix.

[14]We use this observational method to assess the socio-emotional skills of children as it represents the most common and traditional method to measure these concepts (DeRosier, 2014). This method of assessment, however, can be limited by personal bias and cultural specificity (Irava et al. 2019). In our analysis we restrict our core results to only considering comparisons between siblings and control for individual observable characteristics that may bias our estimates if omitted (e.g., age, sex, and birth order). Although no approach to measuring socio-emotional skills is a panacea, alternative and more recently developed methods, such as game-based methods, could be used in future research (DeRosier and Thomas 2018).

"Transform" VHL program. Children in our data are typically teenagers when we administered our survey. Most of those still attending school are in the latter grades of primary school or in secondary school.

By simply comparing means, we can already begin to see the effect of kindergarten enrollment on our main outcome variables. Children who attended Jumpstart are much more likely than children who attended no kindergarten or the government kindergarten to be identified as the best performing child in both third grade and overall in elementary school. Children who attended Jumpstart are much more likely, relative to both children who attended government kindergarten and children who did not attend any kindergarten, to be placed in the top section in third grade. Finally, most children who attended either the Jumpstart or government kindergarten are still enrolled in school, and as seen in Table 1 this is not the case for children who did not attend kindergarten.

3.1 Identification Strategy

Simply comparing means may lead to biased estimates of the true effect of enrolling in either Jumpstart or the government kindergarten. Children who attend a specific type of kindergarten (e.g., Jumpstart or government) may differ based on the characteristics of their household (e.g., parental income level and education, socio-economic status, distance from the household to urban centers, etc.) or based on individual characteristics (e.g., age, sex, birth order, etc.) that may be confounded with academic outcomes. Moreover, the placement of programs, and NGO-based programs in particular, may be affected by where they are expected to succeed. To address potential sources of bias we use the following empirical strategies to estimate the effect of enrollment in each of these kindergarten programs. Our first approach is to estimate the following linear regression model using OLS with household fixed effects:

$$Y_{hi} = \beta_0 + \beta_1 Jumpstart_{hi} + \beta_2 Government_{hi} + X'_{hi}\Gamma + \omega_h + \epsilon_{hi} \tag{1}$$

In equation (1) Y_{hi} represents binary variables for each of our four primary outcomes. These primary outcome variables include: (a) a variable indicating if a child is identified as the best performing sibling within their household in third grade, (b) a similar variable indicating if the child is the best performing sibling in elementary school, (c) a variable indicating if a child placed in the top third grade section, and finally (d) and variable indicating if a school-aged child is

currently enrolled in school. The *Jumpstart* variable is a binary variable that indicates if the child attended a Jumpstart kindergarten. Similarly, the *Government* variable is a binary variable that indicates if the child attended a government kindergarten program. X is a vector of child-level control variables, including child age, the sex of the child, and birth order dummy variables. These control variables in particular help to control for any potential reporting or recall bias embedded in our data. Additionally, since our data include children who are not old enough to have completed third grade, we include a binary variable indicating if the child is "too young" (less than 9 years old) for third grade when estimating effects on the first three of the four primary outcome variables. When estimating effects on current enrollment, we include two binary variables. One indicating if the child is "too young" (less than 4 years old) to be enrolled in school and the other indicating if the child is "too old" (greater than 24 years old).[15] Finally, ω_h is a household fixed effect and ϵ_{hi} is the error term.[16] The inclusion of these household fixed effects are important as they difference out issues relating to the endogenous placement of the programs.

Although we are controlling for household fixed effects, enrollment in Jumpstart or government kindergarten may still be endogenous and thus β_1 and β_2 may be biased estimates of the true effect. In particular, parents may make strategic choices about which of their children to send to a particular type of kindergarten. If, for example, parents tend to send more capable children to the Jumpstart kindergarten program, then the estimate of β_1 from equation (1) will be biased upwards. On the other hand, if parents tend to send less capable children to the Jumpstart kindergarten program, the the estimate will be biased downwards. Since it is difficult to reason the magnitude and direction of the potential bias of results estimated from equation (1), a method to address the potential for bias is necessary.

We make use of the fact that both of these kindergarten programs started at different times in different villages and that older children are less likely to attend kindergarten upon the program's first implementation. Using an identification strategy similar to Wydick et al. (2013), we use a vector of dummy variables representing the age of children when the respective two kindergarten programs entered their village to instrument for participation in the programs. Jumpstart started in a small number of villages in 2005 and added additional villages in each subsequent year. We use

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[15]We include these observations in our sample for the purposes of making comparisons between siblings and to allow for comparability between our primary outcomes and the intermediate socio-emotional outcomes. As a robustness test, we re-estimate effects on our primary outcomes with trimmed samples for each outcome and show these results in the appendix. The results are qualitatively similar.

[16]As a robustness test on this specification, we estimate a variant of equation (1) which, instead of household fixed effects, includes village fixed effects with household/mother control variables. These results are shown in the appendix.

a similar strategy to instrument for enrollment in government kindergarten. Specifically we use the age of children when government kindergarten entered their village to instrument for government kindergarten participation. The Philippine government passed the Kindergarten Education Act in 2011 which mandated universal public kindergarten education by 2012. In practice, a number of villages introduced a government-sponsored kindergarten as early as 2008. We use a vector of 11 binary variables that represent if a child is a particular age—zero through 10—when the Jumpstart and government kindergartens entered their village. Specifically we implement this instrumental variable method by estimating the following equations:

$$Jumpstart_{hi} = \sum_{j=1}^{11} I_{ji} + \sum_{g=1}^{11} I_{gi} + X'_{hi}\Pi + \tau_h + \mu_{hi} \tag{2}$$

$$Government_{hi} = \sum_{j=1}^{11} I_{ji} + \sum_{g=1}^{11} I_{gi} + X'_{hi}\Psi + \kappa_h + \eta_{hi} \tag{3}$$

$$Y_{hi} = \delta_0 + \delta_1 \hat{Jumpstart}_{hi} + \delta_3 \hat{Government}_{hi} + X'_{hi}\Xi + \rho_h + \nu_{hi} \tag{4}$$

In equations (2) and (3) I_{ji} and I_{gi} each equal one when a child is a particular age between zero and 10 at the time when the Jumpstart program and the government kindergarten program started in their village, respectively. In equation (4) $\hat{Jumpstart}$ and $\hat{Government}$ are the predicted values from equation (2) and (3).

These instruments are both strongly correlated with program participation and satisfy the exclusion restriction. These instruments are strong since a child's age when a kindergarten is introduced as a local option is the key determinant for kindergarten eligibility and attendance. Of course, instrument strength can be tested directly by calculating an F-statistic testing for the joint significance of all instruments in predicting attendance in Jumpstart ($F - statistic = 78.98$) and government kindergarten ($F - statistic = 25.19$) in the main specifications (Sanderson and Windmeijer 2016).[17] Figure 1 illustrates the variation in enrollment rates across each age at both Jumpstart and government and kindergarten introduction in a village. This vector of instruments satisfy the exclusion restriction because we would not expect the age a child happens to be at the time one of the kindergarten programs enters his or her village to affect child outcomes except through the child's participation in that kindergarten program. In the first stage, each child is assigned a probability of entering either Jumpstart or the government kindergarten based on his

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[17]The first stage estimates for this instrumental approach are shown in the Appendix.

or her age at program entry. For example, if a child is too old for kindergarten when the program entered their village then the instrument exogenously assigns the child a very low probability of entering kindergarten. Conversely if a child is around kindergarten age when the program entered their village, then the instrument exogenously assigns the child a relatively high probability of enrolling in kindergarten. This means that the instrument generates exogenous variation in the probabilities of entering the respective kindergarten programs and should address the potential for parental-choice bias discussed above.

3.2 Mediation Analysis

Although a central motivation for this analysis is to estimate the effects for Filipino children of attending two distinct types of kindergarten, we are also interested in trying to understand what factors mediate the estimated effects on our primary outcomes. In our data we have information on both academic and socio-emotional skills. These are skills that children who attend kindergarten may be more likely to obtain through positive interaction with peers, and especially if socio-emtional development is a specific objective of the kindergarten ethos and programming. For example, the Jumpstart program took special care in aiming to instill positive character values and relational skills through its "enrichments" to the core Philippine kindergarten curriculum.

To assess the extent to which specific academic or socio-emotional skills mediate the primary results we follow the approach of Preacher and Selig (2012). In general, this approach calls for a two-step procedure for estimating the effect of kindergarten enrollment on primary outcomes via potential mediating variables (i.e., the indirect effect) and the effect of kindergarten enrollment on primary outcomes conditional on potential mediating variables (i.e., the direct effect). The first step estimates the following specification:

$$M_{hi} = \alpha_0 + \alpha_1 Jumpstart_{hi} + \alpha_2 Government_{hi} + X'_{hi}\Theta + \psi_h + \xi_{hi} \tag{5}$$

In equation (5) M_{hi} is an index variable measuring either academic or socio-emotional skills of child i in household h. Similar to equation (1), the $Jumpstart$ and $Government$ variables are binary variables indicating if the child attended either a Jumpstart or government kindergarten. X is a vector of child-level control variables, such as child age, the sex of the child, and birth order dummy variables. Finally, ψ_h is a household fixed effect and ξ_{hi} is the error term. Similar to the analysis of effects on primary outcomes, we can instrument for participation in Jumpstart

and the government kindergarten using the age of children when each program first entered the child's village to identify mediation while correcting for the possible bias in OLS estimates. The instrumental variable framework essentially mirrors equations (2) through (4), but instead uses the outcome variables, M_{hi}, which represent potential mediating index variables.[18] The second step estimates and augmented version of equation (1) that includes the potential mediating variables in the specification.

$$Y_{hi} = \gamma_0 + \gamma_1 Jumpstart_{hi} + \gamma_2 Government_{hi} + M'_{hi}\Lambda + X'_{hi}\Delta + \varphi_h + \zeta_{hi} \tag{6}$$

Equation (6) is identical to equation (1) except for that it includes the vector M_{hi} which indicate various potential mediating variables. In the Preacher and Selig (2012) framework the direct effect of Jumpstart attendance on primary outcomes is equal to γ_1, the coefficient on $Jumpstart$ in equation (6). The indirect effect is equal to the product of α_1 in equation (5) and the corresponding coefficient in the vector Λ in equation (6). A corresponding process follows for the direct and indirect effects on government kindergarten enrollment. We again instrument for participation in Jumpstart and the government kindergarten using the age of children when each program first entered the child's village.

Although the mediation framework is relatively straightforward to implement, causal inference is challenging because the inclusion of the mediating variable M_{hi} can introduce bias into otherwise unbiased causal effect estimates if the potential mediating variable is endogenous (Acharya et al. 2016). We follow Heckman and Pinto (2015), which suggests that in order for mediation analysis to have a credible causal interpretation we need to assume independence between kindergarten enrollment (i.e., treatment status) and any of the potential mediators (i.e., measured inputs) with respect to our primary outcomes conditional on other observed covariates. This is often called the sequential ignorability assumption (Imai et al. 2010; 2011). We implement mediation analysis by making this assumption and support it in two ways: First, we implement unobservable selection and coefficient stability tests of Oster (2017) to test if coefficients on mediators change based on the inclusion of observable controls; if they do not, then Oster (2017) argues that coefficient estimates would be unlikely to change with the inclusion of unobservable variables. Second, similar to Heckman et al. (2013), we identify mediators over a theoretically comprehensive measure of

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[18]Again, as a robustness test on this baseline specification, we estimate a variant of equation (5) using village fixed effects with household/mother control variables instead of household fixed effects. These results are shown in the appendix.

personality, in our context the "Big 5" personality traits. Heckman et al. (2013) argue that using a comprehensive set of personality mediators greatly reduces the possibility of a confounding personality variable that would introduce bias in OLS estimation since conceptually they absorb potentially confounding variation in the error term stemming from personality characteristics.[19]

4 Results

In this section we discuss two sets of results. First, the primary results show the impact of Jumpstart and the Philippine government kindergarten program on primary school education outcomes. These primary outcomes include: the mother's report of which child performed best in third grade and overall in elementary school, whether the child was placed in the highest ranking section in third grade, and whether "school aged" children are currently enrolled in school. We also discuss heterogeneity in these primary effects in two dimensions: (i) by the sex of children, and (ii) by the age of children. The later provides some preliminary insight about whether the government kindergarten program has improved since first implemented. Second, the mediation results show how effects of the programs may occur through the development of different personality traits within the context of the program. In the first stage we show how attending either the Jumpstart or government kindergarten program affects socio-emotional outcomes. We use two sets of socio-emotional variables. The first is a core set of indices that includes grit, self-control, peer affiliation, self-identity, behavior, and spirituality. The second is an alternative set of indices that represent the "Big 5" personality characteristics: openness, conscientiousness, extroversion, agreeableness, and neuroticism. We also use indices that broadly measure current and future academic skills. In the second stage we show how much these potential mediating characteristics influence our primary outcome variables. Finally, we discuss both the direct and the indirect effect, through measured inputs, of attending each of these kindergarten programs.

4.1 Primary Results

What is the effect of enrollment in Jumpstart or the government kindergarten program on academic outcomes in primary school? To answer this question, we estimate the household fixed-effect regression—shown in equation (1)—and the instrumental variables regressions—shown in equations (2) through (4). We report the results from these regressions in Table 2.

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[19]This general mediation analysis approach is similar to the methods used by Kosse et al. 2020.

The first two columns show the effect of enrollment in Jumpstart and government kindergarten on performing best in third grade, relative to siblings within the household who did not attend kindergarten. The results are both statistically significant and of large magnitude. As reported in Table 1, the probability that a child who did not attend any kindergarten is the best performing sibling in third grade is about 27 percent. The coefficient on our OLS estimate is 0.282 ($p < 0.01$), and the coefficient from our IV estimate is 0.259 ($p < 0.01$). As in the subsequent columns, IV estimates are slightly lower than OLS estimates. This could reflect a desire by parents to enroll higher ability children in the Jumpstart program, a bias corrected by the IV estimations. These estimates thus suggest that enrolling in Jumpstart roughly doubles the probability that a child is identified by the mother as the top performer among all siblings in third grade. Additionally, there no evidence of any meaningful effect of enrollment in government kindergarten on the probability that a child is the best performing sibling in third grade. A test of equality of effects between enrollment in Jumpstart and the government kindergarten shows that the difference is highly statistically significant.

Results for the effect on performing best in elementary school, shown in columns (3) and (4) are similar. Again as reported in Table 1, the probability that a child who did not attend any kindergarten is the best performing sibling in elementary school is about 29 percent. This means that OLS and IV estimates suggest that enrolling in Jumpstart increases the probability of a child being the best performing sibling by roughly 56 percent. Again, there is no evidence of any meaningful effect of enrollment in government kindergarten on the probability that a child is the best performing sibling overall in elementary school. Formal tests of equality of these effects show that the difference is highly statistically significant.

Columns (5) and (6) show the effects of enrollment in Jumpstart and government kindergarten on the probability of placing in the top third grade section. Based on the results of the government-administered NAT exam, students are placed into different class sections. About 30 of the best performing students are placed in the top section, then the next 30 students are placed in the following section, and so on. This outcome is likely more objective than the previous two parental-reported outcomes but is a relatively blunt measure of academic performance. This outcome does not differentiate the performance of children within the top section, which is an important detail especially in places without many sections within each grade. Contrary to the previous results, we find economically and statistically significant effects for both children who attended Jumpstart and the government kindergarten program, relative to siblings who did not attend any kindergarten.

As reported in Table 1, children who did not attend any kindergarten realize a probability of being placed in the top third grade section of about 35 percent. OLS and IV estimates suggest that children who enrolled in Jumpstart are about 63 percent more likely to place in the top section in third grade than their siblings who did not attend kindergarten. Similarly, OLS and IV estimates suggest that children who enrolled in government kindergarten are about 50 percent more likely to place in the top section in third grade than their siblings who did not attend kindergarten. A formal test of equality of these effects suggests that these effects are not statistically different from one another.

Finally, columns (7) and (8) report the effects of enrollment in Jumpstart and government kindergarten on current enrollment status for "school-aged" kids. By the definition we use for the present purposes, "school-aged" kids are aged from 4 through 24 years old, which would allow for enrollment through most professional programs. Similar to the results in the first four columns, we find strong evidence of meaningful effects for children who attended Jumpstart and no evidence of meaningful effects for children who attended government kindergarten. As shown in Table 1, only about 60 percent of children who did not attend any kindergarten are currently enrolled in school. Therefore, OLS and IV estimates suggest that school aged children who enrolled in Jumpstart are about 15 percent more likely to be enrolled in school. Additionally, there no evidence of any meaningful effect of enrollment in government kindergarten on current enrollment status of school aged children. Formal tests of equality of the effects between Jumpstart and government kindergarten indicate the difference is statistically significant.

These results lead to two conclusions about the effects of kindergarten education in the Philippines. First, across three of our four outcomes, we find that children who attended the government kindergarten do not perform better than those who did not attend kindergarten. For both the mother's report of academic performance among siblings in third grade and elementary school and current enrollment status of school aged children, only children who enrolled in Jumpstart fair better than kids who did not attend any kindergarten. Second, in one of the four outcomes, we find that the government kindergarten program had a similar effect to the Jumpstart program. Children who attended either the government kindergarten or the Jumpstart program are more likely to be placed within the top third grade section relative to those who did not attend kindergarten. This may be because placement into the upper class is a relatively blunt indicator of academic performance and does not differentiate the performance of children within the top section. Although the outcome indicating if children placed in the top third grade section is likely a more objective mea-

sure of academic performance than parental-reported measures, the estimated effects do not seem to translate into increased levels of future school enrollment—another more objectively identifiable outcome.

One concern regarding our first two outcomes is the mother's subjective report of academic performance among siblings. For this outcome, we asked mothers to list their children in order of academic performance in both third grade specifically and in elementary school in general. Although the presence of the NAT provides parents with some objective information to base their reporting, they may forget their children's scores or grades. It is reassuring, therefore, that the effect of enrollment in Jumpstart on more objective outcomes—such as placing in the top third grade section and current enrollment status—is also positive, of meaningful magnitude, and statistically significant.

4.1.1 Heterogeneity in Primary Effects

These primary effects vary in a number of ways. In this sub-section, we discuss heterogeneity in two dimensions. First, we restrict our sample to only within-sex sibling comparisons. This allows us to estimate differential effects for girls vs. boys. Second, we restrict our sample by age-groups. Specifically, we examine children who are older or younger than 11 at the time of data collection. This allows us to estimate differential effects of these programs over time.

Table A3, in the appendix, reports OLS and IV estimates of primary outcomes separated by sex. Panel A shows effects when we restrict our sample to include both only girls and only households who have at least two girl siblings. This allows us to compare the effects of attending either Jumpstart or the government kindergarten program among only girls. Panel B shows the opposite when we restrict our sample to include both only boys and only households who have at least two boy siblings. The results show that the estimated effects do not vary much at all among only girl siblings vs. only boy siblings. This finding is supported by a recent review of the education literature in lower income countries (Evans and Yuan 2019). Namely, that general educational interventions may be just as beneficial to girls than compared to educational interventions that specifically target girls.

Table A4, in the appendix, reports OLS and IV estimates of primary outcomes separated by age groups. Panel A reports results among children who are 11 years old or older and Panel B reports results among children who are 10 years old or younger at the time of data collection. Children who are less than 10 years old are about prime kindergarten age at the time the Kindergarten

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Education Act was mandated and scaled nationally. This investigation of heterogeneity is driven by questions about whether the effects of these kindergarten programs perhaps improve over time. The government kindergarten program specifically may have suffered from implementation constraints in the early years of the program and may have improved in more recent years. A key challenge we face when implementing these regressions is that in more recent years there are far fewer children who did not attend kindergarten. Therefore, in Table A4, we only examine the effect of being enrolled in Jumpstart vs. not being enrolled in Jumpstart. This comparison effectively pools together children who either attended a government kindergarten or did not attend kindergarten.[20] We find that there is very little difference in the effect of attending Jumpstart vs. no attending Jumpstart over time. If anything, the estimated effects of the benefit of Jumpstart vs. non-Jumpstart kindergarten seem to increase among children who attended kindergarten more recently. Although these results are limited, and future research is needed to examine the longer-run effects of the government kindergarten program in the Philippines, we find no evidence suggesting that the government kindergarten program has improved over time relative to the Jumpstart program.

4.2   Mediation Results

We present the results of the mediation analysis in three parts. First, we show and discuss the results from the first-stage regression that estimates the effect of attending Jumpstart or the government kindergarten on each of our potential mediating variables. Second, we show and discuss the results from the second-stage regression that estimates the direct effect of attending Jumpstart or the government kindergarten conditional on a vector of potential mediating variables. Finally, we discuss the estimated direct and indirect effects of this mediation analysis.

4.2.1   Mediation First-Stage

As previously noted, the questions in our survey allow us to create several different sets of indices. The core set of indices include variables measuring the following psychological variables: grit, peer affiliation, self-control, and self-identity. This set of indices also include variables measuring interpersonal behavior, spirituality, academic expectations, and academic skills. We can also use the same survey questions and group the questions together differently to generate an alternative set of indices. These indices include variables that measure each of the "Big 5" personality charac-

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[20]As shown in Table 2, with the exception of the effects on being placed in the top third grade section, whether or not a child is enrolled in a government kindergarten or did not attend kindergarten makes little difference among our core primary outcomes.

teristics: openness, conscientiousness, extroversion, agreeableness, and neuroticism. More detailed information about the specific survey questions associated with each index is presented in the appendix.

Table 3 reports results on the psychological attributes from the core set of indices. The first two columns show estimates of attending either the Jumpstart kindergarten or the government kindergarten on the grit index. Both the OLS and IV estimates of attending Jumpstart kindergarten suggest relatively large and statistically significant effects on grit. Since the outcome variable is standardized to have a mean of zero and a standard deviation of one, the coefficient estimates suggest that attending Jumpstart increases grit by roughly 0.14 standard deviations, large enough to have significant effects on task completion (Duckworth and Gross, 2014). Although the effect of attending government kindergarten is smaller and not statistically significant, formal tests fail to reject equality of these two effects. We find similar effects of attending Jumpstart kindergarten on self-control and self-identity. In particular, OLS and IV estimates of Jumpstart attendance suggests that self-control increases by about 0.15 standard deviations and self-identity increases by about 0.20 standard deviations. Again, formal tests fail to reject equality of the Jumpstart effect estimate and the government kindergarten effect estimate. In fact, the estimated effects of attending the government kindergarten on both the peer affiliation index and the self-control index are slightly larger than the effect of attending Jumpstart kindergarten on these indices. In particular, although we find that attending the Jumpstart kindergarten led to no effect on peer affiliation, we find IV estimates suggesting that attending the government kindergarten leads to a 0.16 standard deviation increase in peer-affiliation.

Table 4 shows the estimated effects of attending Jumpstart and the government kindergarten on behavior, spirituality, and two academic skills indexes. The behavior index includes questions about whether the child is obedient to instruction or breaks rules at home and school, whether the child acts like others their age, and whether the child is kind to younger children. The first two columns of Table 4 show that attending either Jumpstart or the government kindergarten does not have any measurable effect on this behavior index. The spirituality index includes questions on whether the child participates in church youth group activities, whether the child asks questions about God, whether the child enjoys attending church, and whether the child gives the family's offering at church. In columns (3) and (4) of Table 4 we find a meaningful effect estimate on spirituality, but one that is only statistically significant at the 0.10 level, due to attending either the Jumpstart or the government kindergarten. This result could be interpreted as increased engagement in church

activities due to attending any sort of kindergarten.

The last four columns of Table 4 report results on two different academic indices. The academic expectations index includes questions about the likelihood a child will finish high school, graduate from university, get a good job, and generally be successful in life. The academic skills index includes questions about how much the child—relative to others their age—reads, practices math, takes interest leaning new languages, achieves highly in school, and is considered smart. IV estimates for the effect of both attending Jumpstart and government kindergarten are relatively large and statistically significant. The estimates in column (6) of Table 4 suggest that attending either Jumpstart or the government kindergarten increases the academic expectations index by roughly 0.23 standard deviations, relative to not attending any kindergarten. The estimates in column (8) suggest that attending either the Jumpstart or government kindergarten increase the academic skills index by about 0.30 standard deviations, again relative to not attending any kindergarten.

These results from our core set of potential mediating variables suggest two intermediate conclusions. First, the results in Table 3 suggest that the Jumpstart program seems to have increased the grit, self-control, and self-identity of the children who attended, relative to children who did not attend any kindergarten. For children who attended the government kindergarten, We find evidence of increased peer affiliation and self control. We fail to reject equality of any of these effects between children who attended Jumpstart and children who attended the government kindergarten. These findings provide new evidence about the production function for personality traits (Pagani et al. 2019), namely, that early childhood education can influence personality traits. Second, in Table 4 we see that both the Jumpstart and government kindergarten programs increased indices measuring future academic expectations and current academic skills. This suggests that some kindergarten, regardless if it was the Jumpstart or government kindergarten, has academic benefits relative to not attending any kindergarten.

We carry out our estimations on an alternative set of indices, where we index responses to our survey questions within the "Big 5" personality traits: openness, conscientiousness, extroversion, agreeableness, and neuroticism. These indices should be viewed as alternatives to the psychological, behavior, and spirituality variables from the core set of indices. We do this for two reasons. First, comparing the results between the core and alternative sets of indices allows for a robustness test of results to different aggregations of the survey questions. If we find similar results across similar types of socio-emotional variables, then this should lend credence to any broad conclusion about the effects of these kindergarten programs. Second, the "Big 5" personality traits have been developed

by psychologists to represent broad dimensions of personality, within which more specific traits are nested (Goldberg, 1993). Following Heckman et al. (2013), we use our "Big 5" personality trait index in order to mitigate the possibility of outside personality traits exhibiting an endogenous influence on our potential mediators.

We report results of the effects of attending Jumpstart or the government kindergarten on the "Big 5" attributes in Table 5. The first two columns report the OLS and IV estimates on the openness index. The OLS and IV estimates suggest that attending Jumpstart increased openness by between 0.18 and 0.23 standard deviations. Although both OLS and IV estimates do not indicate any effect among children who attended the government kindergarten, formal tests fail to reject equality of attending Jumpstart and government kindergarten. Columns (3) and (4) report a similar finding on the conscientiousness index. The OLS and IV estimates suggest that attending Jumpstart increased conscientiousness by about 0.19 standard deviations. Again, we fail to reject equality of Jumpstart and the government kindergarten. In fact, the IV estimate of attending the government kindergarten on conscientiousness is only slightly smaller than the effect of attending the Jumpstart kindergarten, at about 0.17 standard deviations, but is only statistically significant at the 0.10 level. The effects reported in columns (5) through (10) in Table 5 all generally show, with one exception, that attending either the Jumpstart kindergarten or the government kindergarten has effectively no effect on extroversion, agreeableness, and neuroticism. The one exception is the IV estimate for the effect of attending the government kindergarten on agreeableness. This effect suggests a relatively meaningful magnitude, is statistically significant at conventional levels, and mirrors the effect on peer-affiliation reported in Table 4.

Comparing the results between the core and alternative set of socio-emotional indices suggests at least one conclusion for these first-stage mediation results. According to psychologists who study the components of important psychological attributes, attributes such as self-control and grit are often considered as subsets of conscientiousness (John and Srivastava 1999). Given the results reported in Table 3, it may not be too surprising to see a corresponding effect on the conscientiousness index in Table 5.

4.2.2 Mediation Second-Stage

The second-stage of the Preacher and Selig (2012) mediation approach essentially mirrors the main empirical specifications, but includes a vector of indices representing potential mediating factors. In this sub-section we report results separately for the core and alternative sets of indices. Tables

reporting results for each of the second-stage mediation regressions are in appendix Tables A10 through A17. In this sub-section these results are summarized in Figures 2 and 3. In each of these figures, the effects on Jumpstart and government kindergarten attendance are the direct effects of participating in either kindergarten program, relative to not attending kindergarten, conditional on all of the measured potential mediating variables. The effect on each of the index variables are the second-stage mediation effect, which needs to be multiplied with the associated first-stage mediation effects in order to calculate the indirect effect. These indirect effects will be discussed specifically in the next sub-section.

Figure 2 shows the second-stage mediation results using the core set in indices for each of our four primary outcome variables. Each panel represents the IV estimates from the second-stage mediation regression. Panel A shows results for the outcome variable indicating if the child performed best in third grade relative to their siblings. These results indicate a large direct effect due to attending Jumpstart, which persists even conditional on potential mediating index variables. The direct effect on attending government kindergarten remains small and statistically insignificant. The coefficient on each of the index variables is statistically insignificant. The results are about similar for the outcome variable indicating if the child performed best in elementary school relative to their siblings, shown in Panel B. The direct effect of attending Jumpstart remains relatively large, there is no direct effect of attending the government kindergarten, and the effects on each of the index variables are either relatively small or statistically insignificant. The only difference is that in Panel B the effect on the self-control index is now statistically significant at the conventional level of confidence.

Panel C, in Figure 2, shows the second-stage mediation results for the outcome variable indicating if the child placed in the top section in third grade. The direct effects on both Jumpstart and government kindergarten attendance are relatively large and consistent with the primary results shown in Table 2. This indicates that these direct effects persist despite controlling for all of the potential mediating index variables. Additionally, only one of the coefficients on the index variables, the peer affiliation index, is statistically significant at conventional levels and all are relatively small in magnitude. Finally, Panel D, shows the second-stage mediation results for the outcome variable indicating current enrollment status for school-aged children. Similar to the results in Panels A and B, the direct effect on Jumpstart attendance is large and roughly equivalent to the primary results shown in Table 2. One exception is that the effect on the academic skills index appears to be negative in Panel D. This effect, however, is not robust to coefficient stability tests of Oster

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(2017) and should be interpreted with care. Again, along with the finding that the effect estimates on each of the index variables are small or statistically insignificant, this indicates that the direct effect is strong even conditional on each of the potential mediating variables.

The overall pattern, displayed in Figure 2 appears again in Figure 3 which reports results using the alternative set of indices. Again, each panel represents the IV estimates from the second-stage mediation regression. Panel A shows results for the outcome variable indicating if the child performed best in third grade relative to their siblings. Apart from the effect of conscientiousness, similar to Figure 2, these results suggest a large direct effect of attending Jumpstart, no evidence of a direct effect of attending government kindergarten, and small and statistically insignificant effect on each of the index variables. Panel B, which shows results for the outcome indicating if the child performed best in elementary relative to their siblings, also lead to similar findings. Again, the direct effect of attending Jumpstart is large. The only index variable with a statistically significant effect is neuroticism, however similar with the effects on the other index variables, this effect is relatively small.

The second-stage mediation effects, using the alternative set of index variables shown in Panel C, also show similar results to those reported in Figure 2. Again the direct effect of both Jumpstart and government kindergarten are large, statistically significant, and are consistent with the primary results reported in Table 2. Aside from the effect estimate on the openness index, which is small but statistically significant at the conventional level of confidence, we find no effects on the other index variables. Finally, in Panel D, we show the second-stage mediation results on the outcome variable indicating current enrollment status for school-aged children. These results are consistent with those shown in Figure 2, using the core set of index variables. Similarly, the negative effect of the academic skills index is not robust to the coefficient stability tests of Oster (2017). The direct effect of attending Jumpstart remains strong even when conditioning on all index variables. Additionally, all of the effects of the "Big 5" index variables are small and statistically insignificant at the conventional level of confidence.

4.2.3 Direct and Indirect Effects

As we discussed in the previous sub-section, the direct effects of attending each kindergarten program largely persist even when conditioning on potential mediating variables. More specifically, the effects on Jumpstart and government kindergarten attendance shown in Figures 2 and 3 are qualitatively consistent with the primary effects shown in Table 2. This implies that any poten-

tial indirect effect—calculated by multiplying the corresponding first-stage and second-stage effects together—are relatively small. Indeed this is largely what we see.

Panel A in Table 6 shows the indirect effect of the core set of indices. This set of potential mediating variables includes: grit, peer affiliation, self-control, self-identity, behavior, and spirituality. In most cases, we find that the 95% confidence interval includes zero which indicates a statistically insignificant indirect effect. The one exception is that we find a statistically significant indirect effect on self-control in column (2) of Table 6. This suggests that self-control may have a small mediating effect on academic performance elementary school.

We find similar results in Panel B in Table 6 with the alternative set of indices. This set of potential mediating variables include the "Big 5" attributes: openness, conscientiousness, extroversion, agreeableness, and neuroticism. Again, similar to the results shown in Panel A, we find that in most cases the 95% confidence interval includes zero. There are three exceptions. First, we find evidence of a statistically significant indirect effect of conscientiousness in column (1). This is consistent with the significance of our self-control effect as self-control is an important sub-trait of the "Big 5" trait of contentiousness. This suggests that conscientiousness may have a small mediating effect on academic performance in third grade. Second, we also find evidence of a statistically significant indirect effect of openness in column (3). This suggests that openness may have a small mediating effect on the probability of being placed in the top section in third grade. Finally, we find a statistically significant effect of conscientiousness on current enrollment in column (4).

What explains these results? Results from the first-stage of the mediation analysis in Tables 3 and 5 show evidence of relatively large effects of attending Jumpstart on socio-emotional attributes such as grit, peer affiliation, self-control, openness, and conscientiousness. Each of these effects suggest that attending Jumpstart increases these attributes by between 0.2 and 0.3 standard deviations. When we consider the second-stage of the mediation analysis, however, the effects of these potential mediating index variables have relatively small effects on our primary outcome variables, suggesting that either our indices do not fully capture some important aspects of character traits formed by the Jumpstart program or that there are other large and persistent direct effects from attending the Jumpstart program. This begs the question: What is in the direct effect that makes attending Jumpstart so beneficial even conditional on important socio-emotional attributes that seem to have also improved due to attending Jumpstart?

One plausible explanation is the health components included in the Jumpstart program as enrichments to the standard kindergarten curriculum. These health components include the provision

of de-worming treatment, nutritionally fortified lunch and snacks, and regular medical screening on a monthly basis that monitored each child's physical growth and development. Each of these individual components—de-worming (Miguel and Kremer 2004), school feeding (Alderman and Bundy 2012; Aurino et al. 2019; Chakraborty and Jayaraman 2019), and medical screening (Glewwe et al. 2001; Alderman et al. 2006; Maluccio et al. 2009)—are known to spur positive effects on educational and academic outcomes. Therefore, it is plausible that these components of the Jumpstart program play an important role in driving the persistent and large direct effects.[21] Although this is a plausible explanation, we are ultimately unable to identify these effects quantitatively because the provision of these health components is co-linear with attending Jumpstart, making it impossible to disentangle any of these potentially important indirect effects of the health components from the rest of the Jumpstart program.

5   Limitations and Robustness Checks

In this section we discuss the limitations to our study, the implications of these limitations, and perform robustness checks to our results where possible.

5.1   Outcome Variables

One limitation of our paper are the measures we use as outcome variables. Like most survey data, our data are elicited from the head of household. One weakness with our maternal survey data is that some of our outcomes rely on parental ranking, which does not take into account the size of the differences between siblings. Conceivably there could also be biases that confound behavioral outcomes with academic outcomes, so that if a child behaves better in school as a result of a kindergarten intervention, this could be confused in a parent's mind with better academic outcomes. While institutional data clearly may be advantageous in that it would provide exact measures of scores on grades and national exams, parental data may contain its own advantages. These include a potentially better ability to see the larger and more holistic picture of academic development among children, the ability to account in academic performance for illnesses and other events outside a particular child's control, and a knowledge of other factors that may allow the parent to adjust for other factors (e.g., the challenge of switching schools from a family move,

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[21]A reviewer also points out that it is possible, albeit relatively unlikely, that some of these enrichments lead to an associated income effect. This would occur if the receipt of these enrichments free up financial resources for alternative use.

better or poor teaching quality as reported by children, etc.).

5.2 Village Fixed Effects

We prefer the use of household fixed-effect estimations on our data, especially when used in conjunction with sibling rankings over elementary school academic performance. They control for parenting, many environmental factors common to the household, and to some extent genetic factors, while simultaneously inducing variation in outcomes through the sibling ranking exercise. But if there are intra-household spillovers, the stable-unit treatment values assumption (SUTVA) does not hold, where negative spillovers would bias our estimates upwards and positive spillovers would bias our estimates downwards. To check for this possibility, in Appendix Table A2, we present standard OLS (without fixed effects) and village fixed-effect estimations on the four basic outcomes in our data. These estimations are quite similar to our OLS estimations with household fixed-effects, actually showing slightly higher point estimates for most of the primary outcomes. Estimates using standard OLS give somewhat higher point estimates than the household fixed-effect estimates, but point estimates for village fixed-effect estimates are lower and, while still mostly positive, often statistically insignificant. This is not surprising given the selection method used by ICM for the Jumpstart program, where the most disadvantaged children were chosen for the program, where failing to correct for the level of poverty of the household would cause a downward bias in our estimates.

5.3 Unobservable Selection

Also in the appendix, we present robustness checks using Oster (2017) bounds, which calculates the statistic now known as Oster's δ. Oster's δ represents the ratio of the influence of unobservables to observable control variables that would render a statistically null effect for the variable of concern. A high delta is thus generally considered to represent a result more robust to possible unobserved heterogeneity. A negative result implies that when observable controls (or fixed effects) are added that the magnitude of the coefficient increases, and if this occurs, the result is unlikely to decrease if one were able to include unobservables in the estimation.

In Table A9, in the appendix, we examine the robustness of four of the statistically significant indirect effects reported in Table 6.[22] In columns (1) and (2) of Table A9, we examine robustness

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[22]We do not examine the robustness of the academic skills effect, shown in Panel D of Figures 2 and 3, because the coefficient estimate in the short regression is positive and statistically significant and is negative and statistically significant in the long regression. According to Oster (2017) this result is not robust to observable heterogeneity.

of the indirect effect of conscientiousness on the core outcome indicating if the child performed best in third grade among their siblings. We find a negative value for Oster's δ. We also find a negative value for Oster's δ in columns (3) and (4) when examining the robustness of the indirect effect of self-control on the core outcome indicating if the child performed best in elementary school. These results indicate that the indirect effects of conscientiousness and self-control are highly robust in this context. In columns (5) and (6) we find that Oster's δ is equal to about 0.46. This implies that the indirect effect of openness on being placed in the top section in third grade is less robust to potential unobserved heterogeneity. Finally, in columns (7) and (8) Oster's δ implies that unobservables would need to be about 1.73 times more influential than the relevant observables. According to Oster (2017), this suggests that the indirect effect of conscientiousness on current enrollment is relatively robust to unobserved heterogeneity.

6 Conclusion

Our research examines the relative effects of two types of kindergarten programs that were introduced in the Philippines during the last 15 years. The first program is a faith-based kindergarten, Jumpstart, which was slowly implemented in villages by ICM in 2005 and operated in the country until 2017. The second is a government kindergarten which began to be introduced across the country beginning in 2012.

We find that while the government kindergarten had modest effects overall, the effects of attending Jumpstart were significantly larger. Ordinary least squares estimations with household fixed effects indicate that, relative to children who did not attend kindergarten, children who attended Jumpstart were about twice as likely to be the best academic performer in third grade relative to their siblings. Children enrolled in the government kindergarten were identified as performing no better than children who did not attend kindergarten. Similar results hold for academic performance in elementary school more generally and current school enrollment. These results are robust to instrumental variable estimation that exploits the timing of the village-level rollout of these kindergarten programs relative to the age-eligibility of children for kindergarten enrollment.

We test for mediation and aim to understand through which channels the Jumpstart program affected academic outcomes. We find that Jumpstart increased grit, self-control, and self-identity along with academic skills, where the government kindergarten exhibited a significant effect on peer-affiliation and self-control. Measured in terms of the "Big 5" personality traits, Jumpstart positively

influenced conscientiousness and openness to new experience and the government kindergarten only affected agreeableness. Finally, we find some evidence that the increase in academic achievement caused by Jumpstart was mediated in part by an increase in self-control, conscientiousness, and openness.

These results support the conclusions of previous work carried out in the United States (Heckman et al. 2013) and in Colombia (Attenasio et al. 2018, 2020) that views socio-emotional skills, whether developed at an early age through quality parenting or within the classroom, as important factors to academic success in later years. More specifically, our work supports the results of Arujo et al. (2016), that the quality of teaching at the kindergarten level matters substantially to later childhood academic performance. This research provides added evidence that the development of socio-emotional skills during the pre-school and kindergarten years to be important to subsequent academic achievement and later-life outcomes. Future work should be carried out in the context of low- and middle-income countries to provide external validity to these results.

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Table 1: Summary Statistics

Panel A: Household Variables	Mean	Std. Dev.	Obs.
HH income	4,982	4,246	921
IHS HH income[a]	9.00	0.73	921
HH size	6.08	2.36	942
Mother's age	42.73	9.35	943
Mother attended high school	0.48	0.50	943
Mother attended college	0.10	0.30	943
Mother married	0.86	0.34	943
Mother "stay-at-home"	0.58	0.49	943
Mother graduated VHL	0.83	0.38	943

Panel B: Child Variables	Mean	Std. Dev.	Obs.
Child age			
Jumpstart	11.67	2.30	565
Gov't Kindergarten	9.54	2.68	791
No Kindergarten	17.71	4.67	1,081
Child current grade			
Jumpstart	5.86	2.02	544
Gov't Kindergarten	4.18	2.38	774
No Kindergarten	9.57	2.40	647
Sex of Child (1 = Male)			
Jumpstart	0.51	0.50	565
Gov't Kindergarten	0.54	0.50	791
No Kindergarten	0.57	0.49	1,081
Performed best in third grade			
Jumpstart	0.51	0.50	565
Gov't Kindergarten	0.27	0.44	791
No Kindergarten	0.27	0.45	1,081
Performed best in elementary school			
Jumpstart	0.49	0.50	565
Gov't Kindergarten	0.30	0.46	791
No Kindergarten	0.29	0.46	1,081
Placed in top third grade section[b]			
Jumpstart	0.44	0.50	565
Gov't Kindergarten	0.38	0.49	791
No Kindergarten	0.35	0.48	1,081
Child currently enrolled in school			
Jumpstart	0.96	0.19	565
Gov't Kindergarten	0.98	0.15	791
No Kindergarten	0.60	0.49	1,081

Notes: [a]IHS is the inverse hyperbolic sine transformation. This is a "log-like" function that is capable of computing zero values. [b]The variable identifying children placed in the top third grade section includes predicted values for missing information due to item non-response.

Figure 1: Enrollment Rates at Age when Kindergarten Entered Village
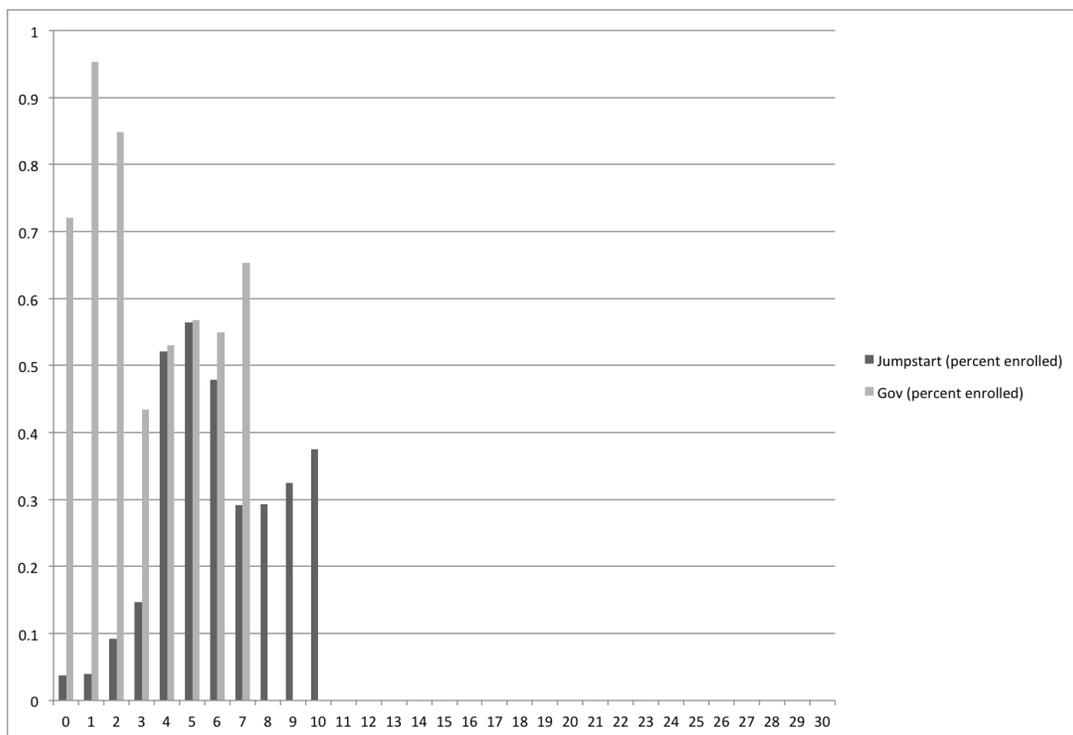
Notes: In this figure the darker bars represent the enrollment rate in Jumpstart kindergarten at each age when the Jumpstart program entered each village. The lighter bars represent the enrollment rate in government kindergarten at each age when the Kindergarten Education Act was passed in 2011.

Table 2: Effects on Primary Outcomes — OLS and IV Estimates

	Performed Best in Third Grade		Performed Best in Elementary		Placed in Top Third Grade Section		Currently Enrolled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Jumpstart	0.282***	0.259***	0.178***	0.165**	0.229***	0.213***	0.112**	0.0865**
	(0.0652)	(0.0615)	(0.0576)	(0.0773)	(0.0505)	(0.0548)	(0.0483)	(0.0421)
Gov't kindergarten	0.00997	-0.0122	-0.00782	-0.0735	0.177***	0.188***	0.0479	-0.0858*
	(0.0506)	(0.0823)	(0.0544)	(0.0881)	(0.0492)	(0.0622)	(0.0519)	(0.0483)
Jumpstart = Gov't test (p-value)	0.000	0.000	0.000	0.007	0.218	0.685	0.018	0.000
Observations	2,437	2,437	2,437	2,437	2,437	2,437	2,437	2,437
R-squared	0.254	0.253	0.185	0.184	0.638	0.638	0.672	0.665
Weak IV test								
Jumpstart (F-stat)		78.08		78.08		78.08		54.72
Gov't kindergarten (F-stat)		25.19		25.19		25.19		24.50
Child controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household/mother fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Results are relative to a child who did not attend kindergarten. Child controls include the child's age, the sex of the child, and birth order dummy variables. In columns (1) through (6) an additional control variable indicates if a child is less than 9 years old. In columns (7) and (8) two additional control variables indicate if the child is less than 4 or over 24 years old. Weak instrument tests report the Sanderson and Windmeijer (2016) F-statistic. In columns (1) through (4) and (7) through (8) standard errors are clustered at the village level. In columns (5) and (6) standard errors are bootstrapped with 1000 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: Effects on Psychological Attributes — OLS and IV Estimates

	Grit index		Peer affiliation index		Self control index		Self identity index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Jumpstart	0.131*	0.141**	0.113	0.0498	0.136**	0.158*	0.184**	0.207**
	(0.0699)	(0.0687)	(0.0716)	(0.0798)	(0.0679)	(0.0839)	(0.0803)	(0.0857)
Gov't kindergarten	0.0763	0.131	0.0180	0.157**	0.0732	0.190**	0.0925	0.0769
	(0.0676)	(0.0805)	(0.0645)	(0.0738)	(0.0557)	(0.0885)	(0.0918)	(0.0978)
Jumpstart = Gov't test (p-value)	0.473	0.902	0.136	0.215	0.334	0.706	0.220	0.178
Observations	2,437	2,437	2,437	2,437	2,437	2,437	2,437	2,437
R-squared	0.795	0.795	0.832	0.829	0.775	0.774	0.749	0.749
Weak IV test								
Jumpstart (F-stat)		78.08		78.08		78.08		78.08
Gov't kindergarten (F-stat)		25.19		25.19		25.19		25.19
Child controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household/mother fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each of the indices are standardized using the technique used by Kling et al. (2007). Results are relative to a child who did not attend kindergarten. Child controls include the child's age, the sex of the child, and birth order dummy variables. Weak instrument tests report the Sanderson and Windmeijer (2016) F-statistic. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Effects on Behavior, Spirituality, and Academic Indices — OLS and IV Estimates

|	Behavior index		Spirituality index		Academic expectations index		Academic skills index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Jumpstart	-0.0162	0.0231	0.0358	0.104*	0.114	0.230***	0.230**	0.305***
	(0.0450)	(0.0520)	(0.0688)	(0.0620)	(0.0927)	(0.0825)	(0.0962)	(0.0812)
Gov't kindergarten	-0.0371	0.0301	0.0721	0.133*	0.106	0.235**	0.152*	0.320***
	(0.0481)	(0.0562)	(0.0488)	(0.0711)	(0.0640)	(0.111)	(0.0853)	(0.112)
Jumpstart = Gov't test (p-value)	0.667	0.910	0.514	0.680	0.921	0.969	0.414	0.905
Observations	2,437	2,437	2,437	2,437	2,437	2,437	2,437	2,437
R-squared	0.898	0.898	0.834	0.834	0.728	0.727	0.639	0.637
Weak IV test								
Jumpstart (F-stat)		78.08		78.08		78.08		78.08
Gov't kindergarten (F-stat)		25.19		25.19		25.19		25.19
Child controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household/mother fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each of the indices are standardized using the technique used by Kling et al. (2007). Results are relative to a child who did not attend kindergarten. Child controls include the child's age, the sex of the child, and birth order dummy variables. Weak instrument tests report the Sanderson and Windmeijer (2016) F-statistic. Standard errors are clustered at the village level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Effects on "Big 5" Attributes — OLS and IV Estimates

|	Openness index		Conscientiousness index		Extraversion index		Agreeableness index		Reverse(Neuroticism) index	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV	(9) OLS	(10) IV
Jumpstart	0.181**	0.226**	0.183**	0.192**	0.0590	0.0384	0.0850	0.104	-0.00358	0.00335
	(0.0902)	(0.0935)	(0.0794)	(0.0833)	(0.0836)	(0.0812)	(0.0643)	(0.0702)	(0.0497)	(0.0627)
Gov't kindergarten	0.104	0.127	0.0863	0.170*	0.0309	0.127	0.0231	0.172**	0.0231	0.0917
	(0.0837)	(0.123)	(0.0738)	(0.102)	(0.0648)	(0.0875)	(0.0695)	(0.0803)	(0.0510)	(0.0700)
Jumpstart = Gov't test (p-value)	0.299	0.356	0.249	0.855	0.728	0.371	0.345	0.424	0.581	0.137
Observations	2,437	2,437	2,437	2,437	2,437	2,437	2,437	2,437	2,437	2,437
R-squared	0.677	0.677	0.720	0.720	0.768	0.768	0.784	0.782	0.885	0.885
Weak IV test										
Jumpstart (F-stat)		78.08		78.08		78.08		78.08		78.08
Gov't kindergarten (F-stat)		25.19		25.19		25.19		25.19		25.19
Child controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household/mother fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each of the indices are standardized using the technique used by Kling et al. (2007). Results are relative to a child who did not attend kindergarten. Child controls include the child's age, the sex of the child, and birth order dummy variables. Weak instrument tests report the Sanderson and Windmeijer (2016) F-statistic. Standard errors are clustered at the village level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Figure 2: Second-Stage Mediation Results, Core Indices
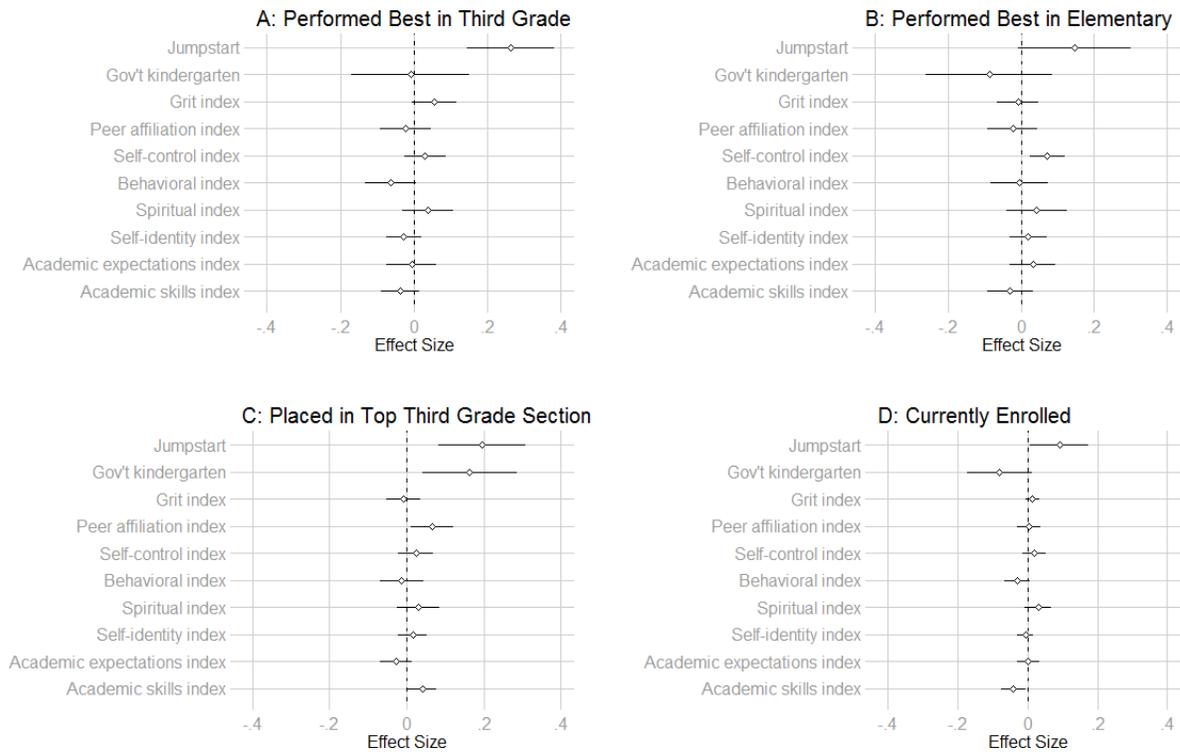
Notes: Each of the indices are standardized using the technique used by Kling et al. (2007). Results are relative to a child who did not attend kindergarten. Controls include the child age, the sex of the child, and birth order dummy variables. Standard errors are clustered at the village level. Tables showing the individual regressions behind these figures are in the appendix.

Figure 3: Second-Stage Mediation Results, "Big 5" Indices
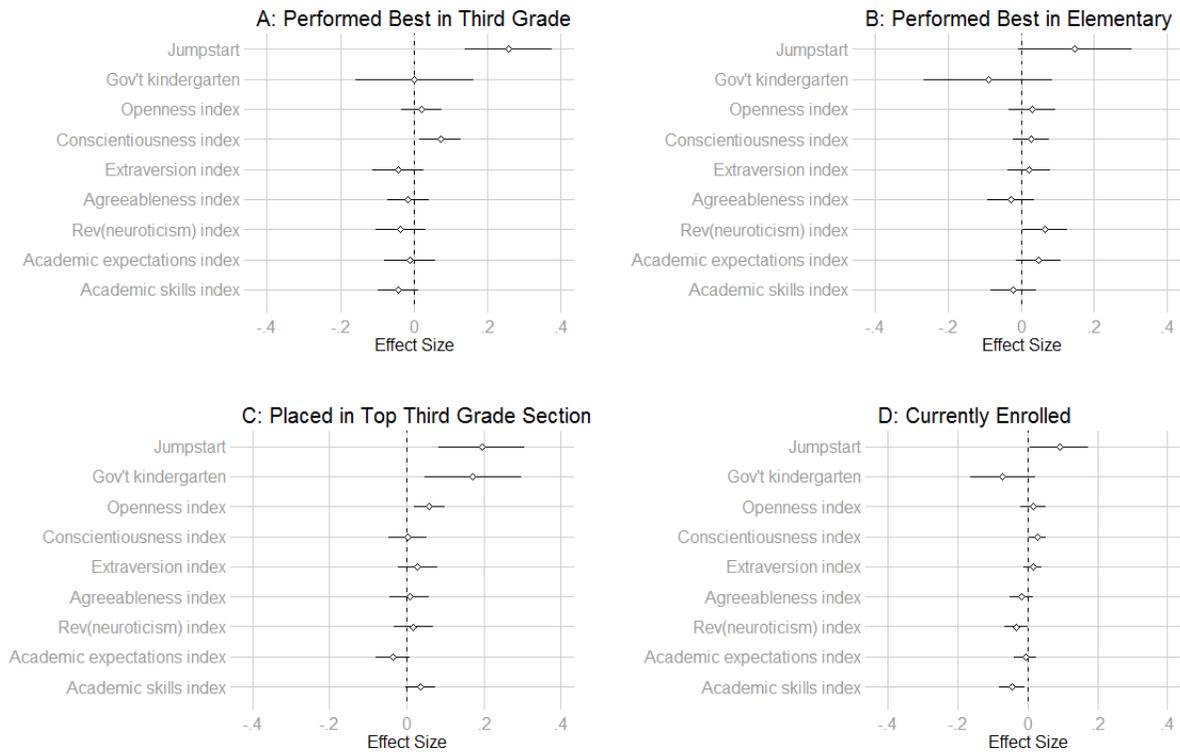
Notes: Each of the indices are standardized using the technique used by Kling et al. (2007). Results are relative to a child who did not attend kindergarten. Controls include the child age, the sex of the child, and birth order dummy variables. Standard errors are clustered at the village level. Tables showing the individual regressions behind these figures are in the appendix.

Table 6: 95% Confidence Intervals of Indirect Effects

	Performed best in third grade (1)	Performed best in elementary (2)	Placed in top section (3)	Currently enrolled (4)
Panel A: Core indices				
Grit index	[-0.001; 0.022]	[-0.011; 0.008]	[-0.007; 0.007]	[-0.001; 0.007]
Peer affiliation index	[-0.015; 0.005]	[-0.017; 0.006]	[-0.003; 0.015]	[-0.005; 0.006]
Self-control index	[-0.005; 0.017]	[0.000; 0.029]	[-0.002; 0.016]	[-0.002; 0.011]
Self-identity index	[-0.019; 0.005]	[-0.007; 0.017]	[-0.004; 0.013]	[-0.007; 0.004]
Behavior index	[-0.009; 0.005]	[-0.005; 0.004]	[-0.003; 0.003]	[-0.005; 0.003]
Spirituality index	[-0.003; 0.019]	[-0.004; 0.019]	[-0.003; 0.012]	[-0.001; 0.010]
Panel B: Alternative "Big 5" indices				
Openness index	[-0.008; 0.021]	[-0.008; 0.026]	[0.002; 0.029]	[-0.005; 0.015]
Conscientiousness index	[0.001; 0.033]	[-0.004; 0.019]	[-0.010; 0.011]	[0.000; 0.014]
Extraversion index	[-0.013; 0.007]	[-0.005; 0.008]	[-0.005; 0.009]	[-0.003; 0.005]
Agreeableness index	[-0.011; 0.005]	[-0.013; 0.005]	[-0.006; 0.008]	[-0.008; 0.002]
Reverse(neuroticism) index	[-0.007; 0.006]	[-0.009; 0.010]	[-0.004; 0.004]	[-0.005; 0.005]
All "Big 5"	[-0.009; 0.043]	[-0.013; 0.036]	[-0.003; 0.037]	[-0.005; 0.023]

Notes: We calculate these confidence intervals using the Monte Carlo approach detailed by Preacher and Selig (2012). Figures showing the distributions of these indirect effects are presented in the appendix.