Experimental Evidence on Early Intervention: The Impact of Full-day Kindergarten

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Abstract

Nearly all school-age children in the United States attend kindergarten, and approximately three-quarters of kindergarten students are in full-day classrooms. While there have been dramatic increases in provision of and participation in full-day kindergarten, there is little evidence on the impact and cost-effectiveness of such programs and policies, particularly as compared to other types of investments in early childhood. Employing data from districts assigning students to kindergarten settings by lottery, I test the impact of full- versus half-day assignment on students' literacy skills at the end of the kindergarten year, generating the first evidence based on random assignment of children to kindergarten type. The results indicate that full-day assignment has a substantial, positive effect (0.31 standard deviations) when comparing students across treatment conditions within the same school. In particular, I find that Hispanic students realize large full-day kindergarten effects (0.70 s.d.), and notably this impact is statistically different than that experienced by students who are not Hispanic. Students who enter kindergarten with low literacy skills also experience particularly large gains. These heterogeneous treatment effects have implications for narrowing or closing the achievement gap early in formal schooling, and in fact the impact for Hispanic students constitutes approximately 70 percent of the control group's end-of-kindergarten ethnicity gap. Using rough cost measures, a simple cost-effectiveness analysis suggests a range of effect sizes from 0.07–0.20 s.d. per thousand dollars of spending, which exceeds similar calculations from experimental evidence on other early investments. Given the positive evidence on program effects, I discuss implications of the study design and findings for policy, including targeted versus universal provision of full-day kindergarten.

JEL No. C93, I24, I28, J13

I. INTRODUCTION

The early childhood years have garnered much policy interest as a critical developmental period and opportunity for early intervention, potentially remediating cognitive and noncognitive skill gaps prior to formal schooling and generating high returns on investment in the form of private and social benefits over the life cycle. Several papers have established the early emergence of achievement gaps by race/ethnicity and socioeconomic status (Fryer & Levitt 2004, Fryer & Levitt 2006, Lee & Burkam 2002, Murnane, Willett, Bub & McCartney 2006, Princiotta, Flanagan & Germino Hausken 2006). While there is some dispute about the magnitude of these gaps—and whether and how they can be explained by student characteristics and family background—there is consistent evidence that test-score gaps by race and socioeconomic status are already sizable at the end of the kindergarten year. In addition, these gaps persist and grow throughout the primary grades.

There is also a growing literature on the importance of kindergarten—both individual skills demonstrated as early as kindergarten and the quality of kindergarten contexts—in predicting later academic and labor market success (Chetty, Friedman, Hilger, Saez, Schanzenbach & Yagan 2011, Duncan, Dowsett, Claessens, Magnuson, Huston, Klebanov, Pagani, Feinstein, Engel, Brooks-Gunn, Sexton & Duckworth 2007). For these reasons, and due to policymaker interest in interventions to improve student achievement early in formal schooling in the presence of accountability and standardized testing in early grades, full-day kindergarten sits firmly in a broader policy discourse about early childhood programming. Policymakers have seized on full-day kindergarten as a relatively inexpensive and readily maneuverable policy lever through which to extend—and potentially enhance—children's early education exposure. The number of kindergarteners in full-day settings has increased dramatically over the last two decades, but there is no rigorous evidence to support full-day kindergarten as an effective early childhood intervention.

As the early childhood policy conversation seizes on the quality versus quantity debate, this paper contributes important new, experimental evidence on the efficacy of a structural

intervention that does not explicitly target quality improvements. Moreover, the evidence informs the decision about how to spend the marginal dollar of investment in early childhood under constraints, and demonstrates whether children are as responsive to intervention later in the early childhood years as they are at younger ages. The motivating research question is whether or not full-day kindergarten students outperform their half-day kindergarten peers as measured by literacy skills at the end of the kindergarten year with consideration of treatment effect heterogeneity and gap-closing effects. In presenting the evidence on the causal impact of full-day kindergarten, the paper proceeds as follows. The next section discusses further the rationale for studying this question as well as previous, related research. Section 3 details the experiment, including the design, data, and empirical strategy. In Section 4, I summarize the results and Section 5 concludes with discussion of the implications of the experimental findings.

II. MOTIVATION

Investment in early childhood, as Nobel laureate James Heckman argues, is one of the rare public policy options in which there are no equity-efficiency tradeoffs. Investing early and in disadvantaged children is both equitable and socially efficient (Heckman & Masterov 2007). The notion that "skills beget skills" describes the role that foundational skill development plays in supporting and augmenting subsequent human capital investments throughout the life cycle, rendering them more efficacious (Heckman 2000, Heckman & Lochner 2000). Moreover, investments in early childhood have a longer time horizon over which to realize their benefits. As early childhood program and policies seek to compensate for the impoverished developmental environments from which many disadvantaged children come, these interventions address equity concerns while often also generating returns to the individual and society in excess of their costs (Currie 2001). The societal costs associated with inadequate early education are not fully internalized by parents making private decisions about investment in their children's early schooling, supporting the argument prevention early

rather than remediation later (Currie 2001).

The developmental literature has coalesced around the notion that children experience declining developmental plasticity and thus early investments—by altering cognitive and social skill development when the brain is most malleable and able to adapt its functioning—are more likely to substantially and permanently affect long-term life chances (Shonkoff & Phillips 2000, Knudsen, Heckman, Cameron & Shonkoff 2006). Because of the declining capacity for the brain to reorganize and adapt, interventions in the early years have a greater likelihood of placing children on a positive developmental trajectory. As Knudsen et al. (2006) summarize, "Central to these principles are the findings that early experiences have a uniquely powerful influence on the development of cognitive and social skills and on brain architecture and neurochemistry, that both skill development and brain maturation are hierarchical processes in which higher level functions depend on, and build on, lower level functions, and that the capacity for change in the foundations of human skill development and neural circuitry is highest earlier in life and decreases over time" (10155). From a physiological perspective, early childhood is a critical period for brain development and therefore constitutes an important time for policies and programs to intervene in disadvantaged children's lives.

There is also a growing body of empirical evidence that early childhood programs reap long-term effects for participants, generating substantial private and social returns that far outweigh the program costs. Long-term evidence from the Abecedarian Project, Perry Preschool Project, Head Start, and the Project STAR class-size reduction intervention all suggests that interventions in the preschool and early school years can have substantial effects on schooling attainment, labor market success, and other measures of health and well-being into adulthood (Chetty et al. 2011, Deming 2009, Schweinhart, Montie, Xiang, Barnett, Belfield & Nores 2005). Improvements in life chances include better health and higher rates of college-going (Campbell, Conti, Heckman, Moon, Pinto, Pungello & Pan 2014, Dynarski, Hyman & Schanzenbach 2013). While these "existence proofs" have garnered significant

attention for early childhood, little is known about whether programs can be implemented effectively at scale, whether programs should be targeted or universally provided, and at what age programs are most effective (i.e., whether early childhood interventions exhibit diminishing returns in age).

II.A. Mechanisms

There are a few, entangled mechanisms through which full-day kindergarten specifically may improve students' short- and longer-term outcomes. The education production function, widely used in labor economics to understand the organization and deployment of resources in education, provides a framework for understanding the potential impact of increasing instructional time. As Hanushek (1979) formalizes it, educational output, y, is a function of a number of inputs related to the child, family, household, and school. For any individual child i, these inputs $(x_1, x_2, ..., x_n)$ are both contemporaneous and historical and include any inputs to the child's educational achievement. The educational production function is typically modeled as increasing in any particular x, but at a decreasing rate (i.e., any specific input exhibits diminishing marginal productivity).

When modeling instructional time in this framework, more instructional minutes potentially result in increased achievement, but have a larger impact when the initial instructional minutes are relatively low. When the starting point is higher, the increase in instructional minutes will have a smaller effect on achievement. The magnitude of the impact varies with other inputs as well, so location on the education production possibilities frontier matters for the effect on individual student outcomes. Some argue that full-day kindergarten could situate children at the point of diminishing returns, generating little return because of the excessive demands on young children.

Dating back to the Coleman Report (1960) there is considerable debate about the role of resources in improving children's educational outcomes.[1] The body of evidence is mixed and

[1]See Hanushek 1979, Hedges et al. 1994, and Hanushek 1997 for the evolution of the debate, and the corresponding evidence, on whether resources matter for educational achievement and attainment.

inconclusive, but the quality of the education production function estimates in the literature is also quite varied. The randomized study of Project STAR, an intervention to reduce class size in the early grades, is one of the largest scale and most rigorous tests of the role of increased resources in generating educational output. On average, being in a small class was associated with a 0.22 s.d. effect on test scores (Krueger 2002). Notably, class-size reduction is a resource input that similarly operates on the intensive margin of schooling, and in the case of Project STAR, also occurred early in schooling.

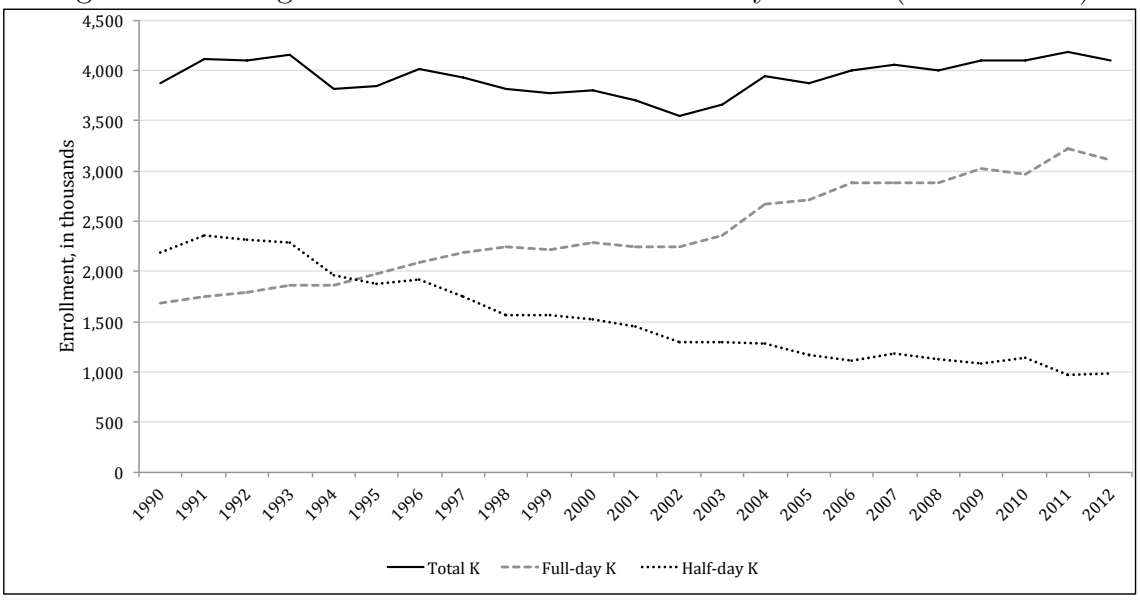
It is important to note that increased instructional time has the additional effect of crowding out other time use activities—a counterfactual condition that may differ by student characteristics, both observed and unobservable—also suggesting the possibility of non-constant treatment effects. Children who would otherwise experience high-quality care environments, enrichment, and time with high-human capital caregivers may be disadvantaged, while children who would otherwise watch television or experience low-quality care would likely benefit from the crowd out induced by full-day kindergarten.

Moreover, full-day kindergarten provides fully subsidized child care in the extended hours, and therefore, constitutes a wealth transfer to parents of full-day kindergarten students. The provision of full-day kindergarten allows parents to adjust to first grade (and beyond) levels of consumption and investment in advance. Decreasing child care costs increases the effective wage which induces both income and substitution effects for marginal parents and could affect parental employment. Finally, children who stay for a full-day of instruction may be more likely to have a nutritious lunch, and possibly a regular nap time, which may influence development apart from the instructional time. Since all of these mechanisms are operating simultaneously, it is not possible to disentangle these effects from the increased input to the education production function. Importantly, these components of the composite effect would likely exist in any implementation of public full-day kindergarten at scale and is a feature shared with many impact evaluations of early childhood programs in the literature, so the combined impact is a relevant estimate for policymakers.

II.B. Previous research

Nearly all students who attend school outside the home participate in kindergarten in the United States and approximately 75 percent of students in kindergarten are in full-day settings (Walston & West 2004, U.S. Department of Commerce). As displayed in Figure 1, less than 20 percent of kindergarten students were in full-day settings in 1970. Full-day kindergarten enrollment exceeded half-day participation for the first time in 1995 and by the 2000-01 academic year, approximately 60 percent of kindergarteners were in full-day classrooms. Of the over four million kindergarten students enrolled in 2012, more than three million attended full-day kindergarten.

Figure 1: Kindergarten Enrollment of Three to Six-year Olds (in thousands)

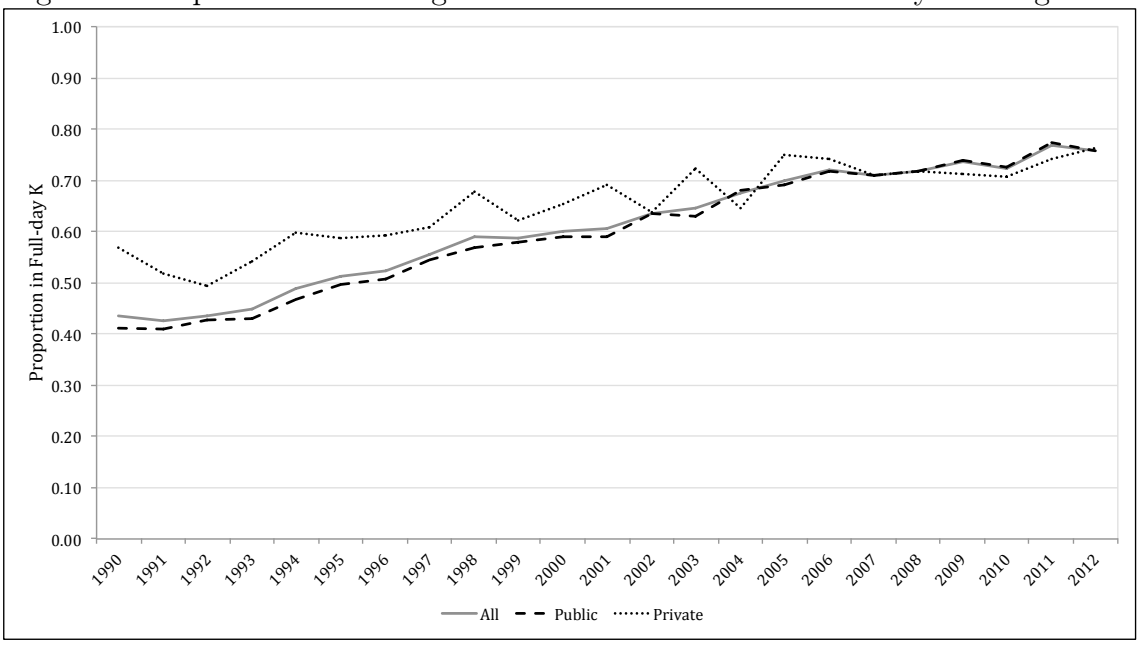


Source: U.S. Department of Commerce, Bureau of the Census, CPS October school enrollment supplement, 1990-2012

While growth in full-day kindergarten participation has occurred in both the public and private sectors, public provision lagged behind private provision through the 1990s. As displayed in Figure 2, three-quarters of kindergarten students are in full-day classrooms in

both private and public settings now, a marked increase particularly for public providers which constitute the vast majority of all kindergartens.

Figure 2: Proportion of Kindergarten Students Enrolled in Full-day Kindergarten



Source: U.S. Department of Commerce, Bureau of the Census, CPS October school enrollment supplement, 1990-2012

Despite its popularity and growing enrollment, research on the benefits of full-day kindergarten, however, is mixed and lacking in rigorous approaches to estimating program impact. Importantly, in this literature full-day kindergarten is compared to half-day kindergarten, rather than no kindergarten attendance at all. This is a notable departure from the literature on other early childhood interventions, particularly pre-kindergarten, and the existing research on kindergarten expansions.

To set the context, a few papers have conducted research relevant to kindergarten that informs the discussion of the full-day kindergarten literature. Two papers explore the impact of kindergarten availability—estimating on the extensive margin—using the plausibly exogenous timing of kindergarten expansions. Cascio (2009) relies on the varied timing of state kindergarten grants to school districts to estimate the long-term effects of kindergarten availability. She finds effects for white children in the form of lower likelihood to drop out

of high school and lower institutionalization rates as adults. She does not find similar effects for blacks–though black children experienced similar increases in public kindergarten enrollment–which is likely due to the crowd out of other early childhood interventions available to disadvantaged populations (Cascio 2009). Dhuey (2011) similarly exploits variation in public kindergarten expansions, though she uses significant increases in kindergarten availability within a state for identification. She finds that Hispanic children and those who live in immigrant households, are of low socioeconomic status, and do not speak English experience benefits from the availability of kindergarten with lower likelihood of being below grade for age and higher wages as adults (Dhuey 2011).

Two recent papers explore the intensive margin, including the quality dimension, of kindergarten participation. Using Project STAR data, researchers find that kindergarten test scores are highly correlated with important, long-term outcomes including college attendance, adult earnings, home ownership, and retirement savings (Chetty, Friedman, Hilger, Saez, Schanzenbach & Yagan 2010). They find that kindergarten intensity, as operationalized by small class size, predicts college attendance. In addition, kindergarten quality measured by teacher experience and peer ability relates to college attendance and higher earnings. While they observe fade out of kindergarten quality effects in test scores, the positive effects on sociocognitive measures remain (Chetty et al. 2010). Fitzpatrick, Grissmer, and Hastedt (2011) capitalize on quasi-randomness in dates of test administration to estimate gains to schooling over the course of the kindergarten and first-grade years. They find that one year of schooling corresponds to 1.2 s.d. on reading tests and 0.9 s.d. on math tests in those early grades, over and above the normal developmental growth children are experiencing. Their results have important implications for thinking about extended school time, particularly early in schooling. While they focus on implications for extending the school year, their work also suggests that substantial learning could take place when doubling the number of instructional hours kindergarten students experience.

The existing literature on full-day kindergarten takes two forms: studies using nationally

representative data and district- and school-level evaluations. In observational studies using the ECLS-K, researchers have found significant differences between full- and half-day kindergarten students on literacy and mathematics assessments at the end of the kindergarten year (Cannon et al. 2006, DeCicca 2007, Lee et al. 2006, Votruba-Drzal et al. 2008). These full-day kindergarten advantages failed to persist, however, over the first-grade year. In one study, marginally significant differences were found in the spring of first grade (Cannon et al. 2006). DeCicca (2007) found significant differences in mathematics and reading in the fall of first grade, but only for white children, which faded but continued to be significant in spring literacy performance. No significant differences were found between full- and half-day kindergarten students in the ECLS-K in third grade (Cannon et al. 2006, Votruba-Drzal et al. 2008) or fifth grade (Votruba-Drzal et al. 2008).

Additional smaller-scale evaluations have supported the ECLS-K findings of short-term outcomes in the kindergarten year, but no significant long-term effects (Zvoch, Reynolds & Parker 2008, Hall-Kenyon, Bingham & Korth 2009). In general, findings on the impact of full-day kindergarten relative to half-day kindergarten suggest some positive associations, particularly in the early schooling years. Results related to the impact of full-day kindergarten over time, or the persistence of these positive findings, are more mixed. All of these studies are still subject to concerns about selection bias in that they cannot fully address endogeneity of student assignment to–or school or district provision of–full-day kindergarten. The present study is the first in the full-day kindergarten literature to use random assignment of students to full- and half-day settings to test the causal impact of full day. The paper also contributes to the broader, growing body of research on investments in early childhood with an internally valid, causal estimate of program impact.

III. THE FULL-DAY KINDERGARTEN EXPERIMENT: BACKGROUND, DESIGN, DATA, AND EMPIRICAL STRATEGY

The analysis presented in this paper employs data from five school districts in Indiana

that were unable to provide full-day kindergarten to all interested kindergarten students in the 2007–08 academic year. To allocate the oversubscribed slots, these districts used random lotteries to assign entering kindergarten students to either full-day or half-day kindergarten classrooms, creating randomized groups of students in the kindergarten settings. The existence of these lotteries provides a unique opportunity to study rigorously the effects of full-day kindergarten on early literacy skills. Using this randomized assignment of students for identification of the treatment effect, I present findings on both the effect of *being assigned to* and the effect of *attending* full-day kindergarten relative to half-day kindergarten.

III.A. Background

In 2007, the Indiana legislature passed legislation which provided funding to increase access and availability of full-day kindergarten in the 2007–08 school year, with grants targeted directly to school districts and charter schools (Indiana Public Law 234-2007). The stated goal of the bill was to allow, "school corporations [districts] and charter schools to provide full-day kindergarten programs to improve the academic and social development of children in kindergarten." A policy initiative of Governor Mitch Daniels' administration and supported by the Indiana State Board of Education and the Indiana Department of Education, the legislation expanded state grant funds for full-day kindergarten from $8.5 million in the 2006-07 school year to $33.5 million in the 2007-08 school year (Indiana General Assembly 2007). School districts and charter schools, operating as autonomous school districts in the state, were eligible to apply to the state for full-day kindergarten funding.[2] Grant funding was then dispersed to all applicants on a per-pupil basis, allocated based on kindergarten enrollment in the school or district in the 2007-08 school year.

Full-day kindergarten enrollment in the state increased by 20 percentage points from 41 percent of kindergarten students in 2006-07 to 63 percent in 2007-08. The number of school districts and schools offering full-day kindergarten also increased with a 26 percentage-point

[2]"Application" for full-day kindergarten funding consisted of indicating interest to the Indiana Department of Education and doing so by a deadline in order to establish enrollment numbers and make grant allocations.

increase in the number of school districts enrolling more the vast majority of their kindergarten students in full-day settings, and a 21 percentage-point increase in the number of schools with nearly all of their kindergarten students in full-day settings (Lovell, Kochanek, Mathers & Burke 2009).[3] With the increased funding availability from the state, the full-day kindergarten grant became the primary means for supporting full-day kindergarten enrollment, coupled with federal Title I funds, school district general funds, and parent fees in some cases. School district officials indicated that 91 percent of full-day kindergarten students in the state were funded, at least in part, by the state full-day kindergarten grant monies in 2007-08 (Lovell et al. 2009).

Because grants were provided to all interested district and schools and allocated based on kindergarten enrollment, the per-pupil amount was insufficient to provide full-day kindergarten to all students in the 2007-08 school year in many cases. Districts and schools determined the assignment procedures for allocating oversubscribed slots. The study's rigorous approach to estimating the effect of full-day kindergarten takes advantage of the existence of lotteries used to assign these oversubscribed full-day kindergarten slots in five school districts in Indiana. By using the subset of school districts and schools employing random assignment to determine participation in the treatment group, this study is designed to produce meaningful, unbiased estimates of the impact of full-day kindergarten on students' early literacy skills.

Table 1 provides descriptive data on the five participating districts. The districts are varied in their urbanicity and location. They also vary in their composition of minority and disadvantaged students. The two smallest districts–one is a charter school that operates as an autonomous school district–have only one elementary school while the largest districts have over 500 kindergarten students. Notably, the kindergarten enrollment numbers in Table 1 differ from the study sample in that not all students in the districts were assigned to

[3]The definition employed for the purposes of reporting change in school district and school provision is that more than 80 percent of enrolled kindergarten students in the district or school participate in full-day kindergarten.

kindergarten setting by lottery. Within the five school districts, there are 23 participating schools. Table 2 provides a comparison of the study schools and all elementary schools in Indiana that provide kindergarten. The comparison of school characteristics demonstrates that study schools are more often suburban and less likely to be rural or located in a town than other Indiana schools. In addition, study schools have a slightly lower prevalence of Title I eligibility and are less likely to be designated for Title I school-wide. The average student composition in study schools is less disadvantaged and comprised of fewer black students and more Hispanic students than the average Indiana elementary school.

III.B. Design

The design of this study capitalizes on the existence of naturally-occurring experiments in school districts throughout the state to estimate the treatment effect of full-day kindergarten assignment and of actual participation in a full-day kindergarten setting. These experiments exist in those schools and school districts that employed a lottery to determine who attended full- and half-day programs in the 2007-08 school year. Typically, the choice to allocate full-day kindergarten slots in this way resulted from over-subscription for limited slots (i.e., the state full-day kindergarten funding was insufficient to provide full-day kindergarten for all interested students and the school district was unable to support full provision through other funding sources) in these areas.

I identified sites for inclusion in the study through an online, statewide survey of all full-day kindergarten grant applicants administered by the state department of education in summer 2007. I then contacted districts that planned to use lotteries to assign kindergarten slots and verified through extensive telephone follow-up conversations. School districts and schools that appeared to meet the criteria as study sites then completed fact sheets detailing the process by which they assigned students to full- and half-day kindergarten and provided documentation of the lottery procedures. Four districts and one charter school, which operates as an autonomous school district, employed random assignment of students

to kindergarten settings through lotteries.

As displayed in Table 3, the districts generally had more full-day kindergarten slots then half-day openings, but could not accommodate all interested students in those available slots. The student sample included in analyses of lottery district data is presented in Table 3, providing the overall enrollment in full- and half-day kindergarten in the district, according to student records supplied for the study, the assignment of students to full- or half-day kindergarten, and actual participation among those assigned to each setting. The first two columns in Table 3 show the number of students enrolled in full- and half-day kindergarten in the study districts. The middle columns display the number of students assigned to each setting by lottery, and the final two columns show the settings in which those assigned by lottery participated. The differences between the enrollment and assignment numbers are those students who arrive in the district after lottery assignment; the differences between the assignment and participation numbers are treatment crossovers and attriters after assignment.

Table 4 displays compliance with random assignment, which was very high in these districts. Of the 673 students assigned to full-day who enrolled in school in the district, over 99 percent participated in full-day kindergarten. While compliance was high, it is important to note that there were treatment crossovers in both directions, so there are both children assigned to full-day kindergarten who did not take up the treatment as well as those in the control group who ended up in full-day kindergarten classrooms. Of the 434 students assigned to half-day who subsequently enrolled in the district, nearly 98 percent participated in half-day as assigned. Nine students assigned to half-day settings substituted with full-day participation, and five students who were assigned to treatment by lottery instead participated in half-day kindergarten. Lotteries were public, facilitating enforcement of random assignment.

Table 5 provides a comparison of lottery entrants and 158 "latecomers," or those students who did not participate in the lotteries before school began, but subsequently enrolled in

the district and were placed in full- and half-day classrooms. There are 200 latecomers in the data of whom 123 ended up in half-day and 77 in full-day kindergarten. Data on student characteristics is only available for 158 of the 200 latecomers. The latecomers are, in general, a more disadvantaged population. The only statistically significant difference in the two groups is that latecomers are more likely to qualify for free or reduced-price lunch. They are also more often black and Hispanic than the lottery entrants. Latecomers are excluded from the analyses presented in this paper.

Finally, it is important to note that there is study attrition. I treat students who are not observed in the spring literacy skills assessment data as attriters. Attrition rates differ by treatment status, with 61 half-day kindergarten students (14 percent) and 60 full-day kindergarten students (nine percent) missing from the sample at post-test. There are also 46 students who have missing data on the covariates, 21 in half-day kindergarten and 25 in full-day kindergarten. Thirteen students fall into both categories of missingness and attrition. These two issues will be addressed in greater detail in the results section.

III.C. Data

Data for this study were supplied by the five participating districts, with assistance in collecting the data from the Indiana Department of Education. Districts provided three types of files, which were merged to conduct analysis of program impact. Coupled with student-level identified data in the state's data system, these data provide the necessary information to assess immediate program impact for the purposes of this paper as well as follow-up analyses of longer-term outcomes in subsequent academic years.

Files with lottery results include student names, unique student identifiers, and determination of full-day or half-day kindergarten status based on the randomized lottery. School districts also provided administrative records which again include student names and unique student identifiers as well as demographic variables (date of birth, gender, race/ethnicity, and free or reduced-price lunch eligibility). The administrative data include identifiers for

the student's teacher, school, and district and an indicator of whether the student was enrolled in full- or half-day kindergarten in the 2007–08 school year. Finally, the districts supplied assessment records that provide results of a end-of-kindergarten year literacy skills assessment.

As required by their participation in the state-funded full-day kindergarten grant, schools in the study districts administered either the Dynamic Indicators of Basic Early Literacy Skills (DIBELS) or the Indiana Reading Diagnostic Assessment (IRDA), scores from which were standardized within sample for use in the study. These assessment records include student names, unique identifiers, scores, and time frame of administration. Both assessments focus on the five essential components of reading, as detailed by the 2000 National Reading Panel: phonemic awareness, phonics, reading comprehension, fluency, and vocabulary. The assessment subparts administered in the kindergarten year emphasize phonics, phoneme segmentation, and letter recognition. The DIBELS and IRDA are formative assessments intended to inform instructional practice and provide teachers and administrators with data for diagnostic purposes.

Scores from the assessments were standardized within sample to generate comparable z-scores for inclusion in the analyses using the following approach:

$$zscore_{it} = (x_{it} - \mu_t)/\sigma_t \tag{1}$$

where i denotes an individual student and t indicates the test; x is the student score, and μ and σ are the test score mean and standard deviation.

III.D. Empirical Strategy

As previously demonstrated, compliance with treatment assignment was high. Moreover, random assignment appears to have been implemented with fidelity. Table 6 presents the student characteristics of those assigned to full- and half-day classrooms. I conduct the tests of mean differences with both district and school fixed effects, employing heteroskedasticity-

robust standard errors. While the lotteries were conducted at the district level, with individual students randomly assigned to full- or half-day status within schools, I employ school fixed effects in the models of impact to control for school context. This approach leverages only within-school variation in treatment assignment. Because the omission of school fixed effects would confound the full-day kindergarten treatment with school attended, I compare only students in full- and half-day kindergarten settings within the same schools. As displayed in Table 6, the treatment and control groups are balanced on baseline covariates in either specification, and the distributions of the covariates are not jointly different from each other.

Table 7 displays the same tests of baseline balance for the analytic sample, thus restricting those assigned through the lotteries to those with a non-missing post-test score. The sample in Table 7 excludes those who attrited from the sample and are no longer observed in the data in the spring of the kindergarten year. Again, the treatment and control groups included in the analysis are balanced and not jointly different from one another. In addition to documentation and verification of lottery assignment procedures from the participating school districts, the data demonstrate that lotteries were implemented properly as the groups assigned to full- and half-day settings do not look different from each other on observable, baseline characteristics.

The basic ordinary least squares (OLS) model for assessing the impact of full-day kindergarten in lottery sites is:

$$Y_{ik} = \beta_0 + \beta_1 ASSIGN_i + (\beta_2 CH_i) + \gamma_k + \varepsilon_{ik} \tag{2}$$

where Y is the literacy outcome measure for student i in school k. $ASSIGN$ is the treatment indicator variable, which takes a value of zero for assignment to half day and one for assignment to full day, regardless of receipt of treatment, resulting in an intention-to-treat estimate (β_1). CH is a vector of child-level characteristics for each student i, and γ represents the

school fixed effects. In the outcomes model specifications, the child-level characteristics are excluded from one specification and included in one specification for precision. School fixed effects also contribute to precision. Standard errors are heteroskedasticity-robust, clustered at the classroom level.[4]

An analysis accounting for noncompliance with treatment assignment was also conducted in a two-stage least squares (2SLS) framework, employing random assignment to full-day kindergarten as an instrumental variable (IV) for full-day kindergarten participation. To estimate the impact of enrolling in full-day kindergarten, the IV approach adjusts the effects of being assigned to full-day kindergarten via the lottery to account for the proportions of students assigned to full-day kindergarten who instead attend half-day kindergarten and students assigned to half-day kindergarten who instead attend full-day kindergarten. The 2SLS models are as follows:

$$FDK_i = \alpha_0 + \alpha_1 ASSIGN_i + (\alpha_2 CH_i) + \mu_i \tag{3}$$

$$Y_{ik} = \delta_0 + \delta_1 \hat{FDK}_i + (\delta_2 CH_i) + \gamma_k + \varepsilon_{ik} \tag{4}$$

where assignment status ($ASSIGN$) is used to predict actual participation (FDK), and the residuals are employed in the second-stage estimation of treatment impact on outcomes. The IV models produce an estimate of the local average treatment effect, or the LATE, which generalizes to compliers with random assignment. Because of the low frequency of treatment group crossovers, these results do not differ meaningfully from the OLS, or intention-to-treat, results. Results from both the OLS and IV models are included in tables, but the intention-to-treat results are the focus of the discussion.

IV. RESULTS

[4]Because of possible concerns about Huber-White standard errors and poor performance in the presence of the relatively small number of clusters (46 classrooms), I employ the Wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis (Cameron, Gelbach & Miller 2008). Inferences are unchanged.

As described in the empirical strategy, I present results from the OLS models (5) for the outcome of interest, the literacy post-test measure, in Table 8. Model I includes only the indicator variable for full-day kindergarten assignment as a predictor, while Model II also incorporates student characteristics. The models are restricted to the observations included in both specifications, but the results in Model I are robust to inclusion of observations that are missing on the covariates. All models employ school fixed effects and heteroskedasticity-robust standard errors, clustered at the classroom level.

Assignment to full-day kindergarten results in a sizable, statistically significant positive effect (0.31 s.d.) on end-of-kindergarten literacy skills. That finding is unchanged with or without the inclusion of individual student characteristics, which were balanced at baseline. The second-stage results of the IV analysis are included in the Appendix. Not surprisingly, since the LATE estimator essentially inflates the intention-to-treat estimate by the compliance rates, the IV results are larger in magnitude (0.34 s.d.). It is reasonable to consider the intention-to-treat estimate, which does not account for treatment crossovers, as a lower bound estimate of the treatment effect. A naïve estimate of the treatment on the treated, estimated from running equation (5) with full-day participation as the treatment indicator on the same sample of 975 observations, results in a similar estimate of the treatment effect (0.32 s.d.). Not surprisingly, these impact estimates are all comparable as compliance with treatment assignment was very high.

IV.A. Heterogeneity

While the models displayed in Table 8 assume a constant treatment effect across the distribution of students in full-day kindergarten classrooms, descriptive inspection of the data suggests that the treatment effect may differ based on student characteristics. Table 9 presents results from models with the inclusion of interaction terms for poverty by treatment and race by treatment. Exploration of interactions with age and gender did not yield evidence of differential effects. As displayed in Table 9, the interaction of poverty and full-day

assignment is not statistically significant at conventional levels in either specification–with and without the inclusion of covariates–but there is suggestive evidence that the treatment effect may be more heavily concentrated among disadvantaged students. The same pattern appears for the interaction of nonwhite race/ethnicity and full-day kindergarten assignment.

For this reason and for ease of interpretation, I run separate regressions for students who qualify for free or reduced-price lunch, nonwhite students, and Hispanic students. Table 10 provides the OLS results among the subgroups, comparing students in that particular subgroup across treatment status within the same schools. Students who qualify for free or reduced-price lunch assigned to full-day kindergarten outperform their half-day kindergarten peers (0.34 s.d.) though the fully interacted model demonstrates that this treatment effect is not statistically different from the full-day kindergarten effect for non-poor students.

Assignment to full-day kindergarten has a large positive effect (0.52) for nonwhite students, more than half of whom are Hispanic in this sample, as compared to their half-day kindergarten counterparts. The fully interacted model results indicate that the treatment effects for white and nonwhite students may be the same, but it appears that full-day kindergarten impact on early literacy skills may be concentrated more heavily among non-white students. As evidenced in the interaction effects and displayed in the tables, Hispanic students benefit greatly from full-day kindergarten as measured by their end-of-kindergarten literacy skills. They outperform their counterparts assigned to half-day kindergarten by fully two-thirds of a standard deviation.

Exploration of subgroup effects suggest that disadvantaged students benefit greatly from full-day kindergarten, as measured by end-of-year literacy skills. Specifically, students assigned to full day who are eligible for free or reduced-price lunch perform better than poor students assigned to half-day settings. More pronouncedly, nonwhite, Hispanic students assigned to full day make sizable gains relative to their half-day kindergarten counterparts at the end of the kindergarten year. The variation in impact estimates by student characteristics suggests that full-day kindergarten reduces end-of-kindergarten achievement gaps,

particularly between Hispanic and non-Hispanic students. This feature of an early childhood program, improving average outcomes while simultaneously contributing to closing race/ethnicity gaps, is important when considering expanding programs and the student eligibility and assignment policies associated with those expansions.

Because heterogeneity may also exist by student literacy skills at kindergarten entry, Table 11 provides the results of models that include interactions with pre-test quartile. While none of the interactions are statistically significant, and the main full-day assignment effect is relatively stable, the coefficients are suggestive that full-day kindergarten effects may be more pronounced for students who enter kindergarten with low literacy skills.

Table 12 displays the within-quartile treatment effects for each group and demonstrate that while the effect for those in the bottom quartile is not statistically different from the rest of the distribution, that group experiences a large gain in full-day kindergarten relative to their half-day peers. The effects for the other quartiles are similar in magnitude though only the impact within the top quartile is statistically significant. Importantly, the sample sizes for these within-group analyses are not large.

IV.B. Attrition

Because concerns about non-random attrition or missing data may compromise the internal validity of the findings, I run OLS models with attrition and missingness as the outcomes, the results of which are presented in the Appendix. The models include only assignment to full day as a regressor, and are estimated with and without the inclusion of district and school fixed effects. As the results indicate, there is not a significant relationship between random treatment assignment and missingness, except when looking within schools which is likely due to some early attrition in advance of collecting demographic data on all children. [5] Notably, full-day kindergarten assignment is negatively associated with attrition from the

[5]Importantly, the main findings are robust to inclusion of observations with missing data on the covariates in the column I model of Table 8, the specification without the inclusion of student characteristics, limited of course to non-attriters. As presented, the sample is constrained to the same observations available for the estimation of the model in column II of Table 8 (with the inclusion of student characteristics).

sample. For this reason, I investigate attrition in more depth. Table 14 provides descriptive statistics for the full- and half-day kindergarten students who attrited and for whom I have demographic and pre-test data, 51 of 60 full-day kindergarten attriters and 47 of 61 half-day kindergarten attriters. Half-day kindergarten attriters are less likely to be white and more likely to be Hispanic than the full-day kindergarten group. They also have lower literacy pre-test scores, and the distributions of baseline covariates among attriters are jointly different by treatment status. Because the rates of attrition are different with half-day kindergarteners demonstrating a greater propensity to leave and because the students who attrit from the treatment and control groups are different on observed covariates, I employ a bounding technique to generate lower and upper bounds on the treatment effect.

These Manski-style bounds provide lower and upper limits on the treatment effect by trimming the sample (Lee 2002, Lee 2009). I equalize the treatment group participation rate to that of the control group by first dropping the observations with the highest post-test scores, generating a lower bound on the treatment effect. I then drop the lowest scores in the treatment group to manufacture 14 percent attrition among full-day kindergarteners in order to estimate an upper bound on the treatment effect. Table 15 displays the results of running the outcomes models, as previously deployed, on the trimmed samples. The results suggest a lower bound on the treatment effect of 0.25 s.d. and an upper bound of approximately 0.34 s.d., indicating that differential attrition is not driving the impact estimates.

IV. DISCUSSION

The evidence presented in this paper suggests that the overall treatment effect of full-day kindergarten is both positive and large in magnitude, when considering literacy skills at the end of the kindergarten year as the outcome of interest. It also appears that the strongest positive effects of full-day kindergarten assignment are concentrated in specific student subgroups as the treatment effect varies by student characteristics. In particular, the study

sample includes a sizable Hispanic population who realize particularly large gains. The presence of heterogeneous treatment effects is important for thinking about the gap-closing effects of early childhood interventions. The effects for Hispanic students are large and statistically different from the impact on non-Hispanic students, accounting for approximately 70 percent of the end-of-kindergarten literacy skills gap between these two populations. In order for early childhood program participation to close achievement gaps and level school readiness starting points, it must be the case that universal or near-universal interventions have these more pronounced effects among disadvantaged children.

When considering full-day kindergarten program expansions, one important policy design consideration is that of targeted versus universal provision. Because there are concentrated effects among Hispanic students, the policy response—in the presence of constrained resources—may be to target full-day kindergarten to specific populations of students at kindergarten entry. The study design, in this case, is individual student assignment with treatment administered at the classroom level, so it is important to note the bundling of treatment with the classroom peer group. In this study, students received the treatment of full-day kindergarten in the context of a varied peer group with mixed ability at kindergarten entry. It remains unknown whether these large, positive treatment effects would generalize to a context in which students participated in full-day kindergarten with a homogeneous peer group, particularly a low-performing group at kindergarten entry.

Using rough cost calculations from survey estimates and average elementary and secondary per pupil expenditures, I generate a range of cost-effectiveness estimates of 0.07–0.20 s.d. per thousand dollars of spending. These estimates suggest a rather large return on $1,000 of investment as measured by impact on end-of-kindergarten literacy skills. As other early childhood and early grades interventions (Head Start, Project STAR class size reduction) result in effect sizes on early outcomes within that range for greater cost than $1,000, full-day kindergarten constitutes a promising policy alternative in thinking about early childhood investment. The results also suggest, in a current context, that investments later in

the early childhood years can still generate sizable returns, and that structural interventions that do not focus on quality can be effective.

With all of the attention on early childhood interventions and policy efforts, it is surprising how little is known about which types of interventions produce results for which types of children at which points in the early childhood years. As states increasingly turn to full-day kindergarten as a policy lever, this paper provides the first experimental evidence on its effectiveness and uses recent data to demonstrate that full-day kindergarten is impactful. The presence of concentrated effects for certain subgroups of students, coupled with positive impact for many participants regardless of demographic characteristics and kindergarten entry skills, suggests that universal provision of full-day kindergarten could do much to alleviate early schooling achievement gaps.

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Tables

Table 1: Characteristics of Lottery Study Districts

	Locale Type	% Free or Reduced-Price Lunch	% Minority	Elementary Schools	Elementary Enrollment	Kindergarten Enrollment	Kindergarten Teachers
Lottery Districts							
District 1	City, Small	61	48	7	3076	562	18
District 2	City, Midsize	59	26	1	677	119	4
District 3	Suburb, Large	42	36	4	855	116	5
District 4	Suburb, Large	20	16	11	4719	748	18
District 5	Rural, Distant	19	4	1	464	81	3

Source: U.S. Department of Education's National Center for Education Statistics, Common Core of Data and Indiana Department of Education, Accountability System for Academic Progress.

Table 2: Comparison of Lottery Study Schools and All Indiana Elementary Schools

	Study Schools n=23	All Schools n=1,119
School Characteristics		
Location – % city	34.78	28.14
Location – % rural	21.74	35.75
Location – % suburb	43.48	19.80
Location – % town	0	16.31
% Charter or magnet	4.35	3.40
% Title I eligible	86.96	95.62
% Title I school-wide	10.00	15.33
Average Student Composition		
% Free or reduced-price lunch eligibility	35.06	43.02
% Male	50.83	51.58
% White	76.42	79.42
% Black	3.97	12.88
% Hispanic	14.15	6.33
% Asian	3.81	1.09

Source: National Center for Education Statistics, Common Core of Data & Indiana Department of Education, Accountability System for Academic Progress.
Note: "All Schools" includes all Indiana elementary schools providing kindergarten in the 2007–08 school year.

Table 3: Lottery Study Sample by District

	Enrollment		Assignment		Participation	
	Full day	Half day	Full day	Half day	Full day	Half day
District 1	288	180	245	160	236	154
District 2	43	37	37	15	37	15
District 3	24	72	22	17	23	11
District 4	380	213	363	201	362	200
District 5	19	51	19	50	19	50
Total	754	553	686	443	677	430

Notes: "Enrollment" includes all students in the participating school districts for whom kindergarten enrollment status could be determined and "Assignment" includes those students for whom results of the lottery could be determined. "Participation" includes all students for whom results of the lottery could be determined and kindergarten enrollment status was also available.

Table 4: Compliance with Random Assignment

		Assignment	
		Full day	Half day
Participation	Full day	99.26% (*n*=668)	2.07% (*n*=9)
	Half day	0.74% (*n*=5)	97.93% (*n*=425)

Table 5: Comparison of Lottery Entrants and Latecomers

	Lottery Entrants *n*=1,083	Latecomers *n*=158	p-value
Age (years)	5.649	5.679	0.962
Free or reduced-price lunch eligibility (%)	36.29	60.13	0.001
Male (%)	47.83	48.73	0.477
White (%)	68.14	59.49	0.880
Black (%)	3.32	5.06	0.654
Hispanic (%)	17.45	25.32	0.933

Notes: Unadjusted means presented by group in columns I and II.
Tests of mean differences calculated with district fixed effects and heteroskedastic-consistent standard errors.
F-test of joint significance: F = 5.77, p = 0.000

Table 6: Fidelity of Random Assignment

	Assigned to full day *n*=656	Assigned to half day *n*=414	p-value (district fixed effects)	p-value (school fixed effects)
Age (years)	5.645	5.656	0.919	0.275
Literacy pre-test (standardized score)	0.006	0.013	0.903	0.034
Free or reduced-price lunch eligibility (%)	38.41	33.33	0.083	0.675
Male (%)	49.39	45.65	0.188	0.967
White (%)	67.07	69.32	0.618	0.746
Black (%)	4.11	2.17	0.111	0.677
Hispanic (%)	17.68	17.39	0.784	0.528

Notes: Unadjusted means presented by experimentally assigned group in columns I and II.
Tests of mean differences calculated with fixed effects and heteroskedastic-consistent standard errors.
F-test of joint significance: F = 1.06, p = 0.385

Table 7: Fidelity of Random Assignment: Analytic Sample

	Assigned to full day *n*=605	Assigned to half day *n*=367	p-value (district fixed effects)	p-value (school fixed effects)
Age (years)	5.642	5.653	0.920	0.253
Literacy pre-test (standardized score)	-0.007	0.054	0.345	0.198
Free or reduced-price lunch eligibility (%)	36.53	29.70	0.058	0.503
Male (%)	50.08	45.50	0.128	0.609
White (%)	67.60	73.57	0.149	0.339
Black (%)	4.30	2.45	0.178	0.886
Hispanic (%)	17.02	13.08	0.110	0.139

Notes: Unadjusted means presented by experimentally assigned group in columns I and II.
Tests of mean differences calculated with fixed effects and heteroskedastic-consistent standard errors.
F-test of joint significance: F = 1.37, p = 0.216

Table 8: Intention-to-Treat Results

	Literacy Post-test				
	I	II	III	IV	V
Assignment to full day	.310**	.328**	.309**	.326**	.249**
	(.100)	(.091)	(.101)	(.092)	(.086)
Literacy pre-test	—	—	—	—	.607**
					(.035)
Age, centered	—	.327**	—	.325**	.026
		(.098)		(.099)	(.091)
Age, centered^2	—	-.771**	—	-.738**	-.537**
		(.203)		(.207)	(.150)
Female	—	.184*	—	.189*	.021
		(.071)		(.072)	(.057)
Poverty	—	-.408**	—	-.400**	-.036
		(.077)		(.072)	(.071)
Nonwhite	—	-.153	—	-.137	-.055
		(.100)		(.102)	(.065)
School fixed effects	x	x	x	x	X
Observations	975	975	972	972	972

Notes: Heteroskedastic-consistent standard errors in parentheses, clustered at the classroom level.

Models I and II include the sample that is non-missing on covariates, excluding the pre-test. Models III–V include the sample that is non-missing on all covariates and the pre-test though only model V includes the pre-test in the estimation.

Because Huber-White standard errors rely on large sample asymptotic theory, the Wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis, was employed (Cameron, Gelbach & Miller 2008). Inferences were unchanged.

* $p < 0.05$, ** $p < 0.01$

Table 9: Interacted Intention-to-Treat Results

	Literacy Post-test					
	I	II	III	IV	V	VI
Full day	.274*	.266*	.238*	.243*	.230*	.226*
	(.134)	(.129)	(.118)	(.107)	(.095)	(.090)
Full day*Poverty	.097	.124	—	—	—	—
	(.176)	(.170)				
Poverty	-.529**	-.486**	—	—	—	—
	(.157)	(.162)				
Full day*Nonwhite	—	—	.207	.211	—	—
			(.192)	(.177)		
Nonwhite	—	—	-.404*	-.274	—	—
			(.177)	(.170)		
Full day*Hispanic	—	—	—	—	.466*	.493**
					(.178)	(.180)
Hispanic	—	—	—	—	-.998**	-.887**
					(.104)	(.113)
Controls		x		x		x
School fixed effects	x	x	x	x	x	x
Observations	972	972	972	972	972	972

Notes: Heteroskedastic-consistent standard errors in parentheses, clustered at the classroom level. Controls include centered age, centered age squared, sex, poverty status, and race/ethnicity.
* $p < 0.05$, ** $p < 0.01$

Table 10: Pre-test Quartile Interaction Model Results

	Literacy Post-test							
	I	II	III	IV	V	VI	VII	VIII
Full day	.246*	.258*	.304**	.317**	.316**	.337**	.252*	.271**
	(.100)	(.101)	(.108)	(.099)	(.112)	(.104)	(.105)	(.097)
Full day*Q1	.207	.201	—	—	—	—	—	—
	(.165)	(.169)						
1st quartile	-1.205**	-1.114**	—	—	—	—	—	—
	(.157)	(.137)						
Full day*Q2	—	—	.010	.026	—	—	—	—
			(.121)	(.116)				
2nd quartile	—	—	-.214**	-.219**	—	—	—	—
			(.078)	(.075)				
Full day*Q3	—	—	—	—	.020	-.022	—	—
					(.147)	(.147)		
3rd quartile	—	—	—	—	.189	.125	—	—
					(.120)	(.122)		
Full day*Q4	—	—	—	—	—	—	-.067	-.076
							(.149)	(.154)
4th quartile	—	—	—	—	—	—	1.103**	1.034**
							(.123)	(.129)
Controls		x		x		x		x
School fixed effects	x	x	x	x	x	x	x	x
Observations	972	972	972	972	972	972	972	972

Notes: Heteroskedastic-consistent standard errors in parentheses, clustered at the classroom level.
Controls include centered age, centered age squared, sex, poverty status, and race/ethnicity.
* p < 0.05, ** p < 0.01

Table 11: Pre-test Quartile Subgroup Model Results

	Literacy Post-test							
	Q1		Q2		Q3		Q4	
Assignment to full day	.520**	.513**	.197	.218	.206	.274	.265**	.230*
	(.181)	(.175)	(.183)	(.186)	(.152)	(.145)	(.094)	(.089)
Age, centered	—	-.223	—	.107	—	-.237	—	.591**
		(.187)		(.176)		(.121)		(.184)
Age, centered^2	—	-.355	—	-.659*	—	-.220	—	-.846**
		(.464)		(.251)		(.299)		(.304)
Female	—	.178	—	.010	—	.091	—	-.083
		(.122)		(.107)		(.093)		(.110)
Poverty	—	-.036	—	-.025	—	-.140	—	-.175
		(.757)		(.141)		(.111)		(.135)
Nonwhite	—	-.203	—	-.085	—	-.000	—	.153
		(.179)		(.164)		(.141)		(.120)
Controls		x		x		x		x
School fixed effects	x	x	x	x	x	x	x	x
Observations	243	243	238	238	244	244	247	247

Note: Heteroskedastic-consistent standard errors in parentheses, clustered at the classroom level.

* p < 0.05, ** p < 0.01

Table 12: Subgroup Model Results

	Literacy Post-test					
	Low-income		Nonwhite		Hispanic	
Assignment to full day	.337*	.351**	.524**	.521**	.665**	.608**
	(.133)	(.121)	(.140)	(.129)	(.176)	(.192)
Age, centered	—	.454	—	.432	—	.872**
		(.228)		(.242)		(.295)
Age, centered^2	—	-.671	—	-.356	—	-1.121
		(.333)		(.412)		(.627)
Female	—	.167	—	.309**	—	.155
		(.085)		(.085)		(.134)
Poverty	—	—	—	-.273	—	.113
				(.151)		(.284)
Nonwhite	—	-.311*	—	—	—	—
		(.134)				
Controls		x		x		x
School fixed effects	x	x	x	x	x	x
Observations	330	330	293	293	151	151

Note: Heteroskedastic-consistent standard errors in parentheses, clustered at the classroom level.

* p < 0.05, ** p < 0.01

Table 13: Attrition Summary Statistics

	Full day *n*=51	Half day *n*=47	p-value
Age (years)	5.674	5.685	0.814
Literacy pre-test (standardized score)	0.158	-0.310	0.022
Free or reduced-price lunch eligibility (%)	60.78	61.70	0.816
Male (%)	41.18	46.81	0.531
White (%)	60.78	36.17	0.012
Black (%)	1.96	0	0.325
Hispanic (%)	25.49	51.06	0.009

Note: Tests of mean differences calculated with district fixed effects and heteroskedastic-consistent standard errors.

F-test of joint significance: F = 6.86, p = 0.000

Table 14: Model Results with Trimmed Sample

	Literacy Post-test			
	Lower Bound		Upper Bound	
Assignment to full day	.254*	.262**	.341**	.357**
	(.104)	(.096)	(.086)	(.082)
Age, centered	—	.359**	—	.205*
		(.088)		(.092)
Age, centered^2	—	-.617**	—	-.572**
		(.170)		(.204)
Female	—	.139*	—	.173**
		(.064)		(.058)
Poverty	—	-.331**	—	-.353**
		(.065)		(.076)
Nonwhite	—	-.076	—	-.189
		(.087)		(.101)
Controls		x		x
School fixed effects	x	x	x	x
Observations	935	935	929	929

Note: Heteroskedastic-consistent standard errors in parentheses, clustered at the classroom level.

* p < 0.05, ** p < 0.01

Appendix

Table A1: Instrumental Variables Model Results

	Literacy Post-test				
	I	II	III	IV	V
Participation in full day	.335**	.355**	.335**	.353**	.269**
	(.109)	(.099)	(.109)	(.099)	(.094)
Literacy pre-test	—	—	—	—	.608**
					(.035)
Age, centered	—	.330**	—	.328**	.029
		(.098)		(.099)	(.091)
Age, centered^2	—	-.776**	—	-.743**	-.541**
		(.203)		(.207)	(.150)
Female	—	.181*	—	.187*	.019
		(.071)		(.072)	(.058)
Poverty	—	-.412**	—	-.404**	-.039
		(.077)		(.072)	(.071)
Nonwhite	—	-.153	—	-.136	-.055
		(.100)		(.102)	(.065)
School fixed effects	x	x	x	x	x
Observations	975	975	972	972	972

Notes: Heteroskedastic-consistent standard errors in parentheses, clustered at the classroom level.

Models I and II include the sample that is non-missing on covariates, excluding the pre-test. Models III–V include the sample that is non-missing on all covariates and the pre-test though only model V includes the pre-test in the estimation.

* $p < 0.05$, ** $p < 0.01$

Table A2: Additional Interacted Model Results

	Literacy Post-test					
	I	II	III	IV	V	VI
Full day	.301**	.327**	.321**	.243*	.321**	.309**
	(.097)	(.129)	(.111)	(.107)	(.111)	(.103)
Full day*Age	-.063	-.034	—	—	—	—
	(.191)	(.198)				
Age, centered	.278	.346	—	—	—	—
	(.171)	(.178)				
Full day*Pre-test	—	—	.207	.211	—	—
			(.192)	(.177)		
Pre-test	—	—	-.404*	-.274	—	—
			(.177)	(.170)		
Full day*Male	—	—	—	—	-.016	.038
					(.146)	(.140)
Male	—	—	—	—	-.180	-.213*
					(.101)	(.100)
Controls		x		x		x
School fixed effects	x	x	x	x	x	x
Observations	972	972	972	972	972	972

Notes: Heteroskedastic-consistent standard errors in parentheses, clustered at the classroom level. Controls include centered age, centered age squared, sex, poverty status, and race/ethnicity.
* $p < 0.05$, ** $p < 0.01$

Table A3: Relationships with Full-day Assignment - Attrition and Missing Data

	Attrition		
	I	II	III
Assignment to full day	-.050*	-.046*	-.051**
	(.020)	(.019)	(.019)
	Missing		
	I	II	III
Assignment to full day	-.004	-.000	-.015*
	(.010)	(.008)	(.007)
District fixed effects		x	
School fixed effects			x
Observations	1129	1129	1107

Note: Heteroskedastic-consistent standard errors in parentheses.

* p < 0.05, ** p < 0.01