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COGNITIVE AND NON-COGNITIVE PEER EFFECTS IN EARLY EDUCATION

Matthew Neidell  
Jane Waldfogel

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Cognitive and Non-Cognitive Peer Effects in Early Education  
Matthew Neidell and Jane Waldfogel  
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**ABSTRACT**

We examine peer effects in early education by estimating value added models with school fixed effects that control extensively for individual, family, peer, and teacher characteristics to account for the endogeneity of peer group formation. We find statistically significant and robust spillover effects from preschool on math and reading outcomes, but statistically insignificant effects on various behavioral and social outcomes. Of the behavioral and social effects explored, we find that peer externalizing problems, which most likely capture classroom disturbance, hinder cognitive outcomes. Our estimates imply that ignoring spillover effects significantly understates the social returns to preschool.

Matthew Neidell  
Department of Health Policy and Management  
Columbia University  
600 W 168th Street, 6th Floor  
New York, NY 10032  
and NBER  
mn2191@columbia.edu

Jane Waldfogel  
Columbia University  
School of Social Work  
1255 Amsterdam Avenue  
New York, NY 10027  
jw205@columbia.edu

## 1. Introduction

Peer effects have long been of interest in economics and other social sciences because they are a nonmarket interaction with far reaching implications for school and community policies, such as school choice, ability tracking, desegregation, and anti-poverty programs. Since peer effects may manifest themselves in various contexts, empirical research has focused on a wide range of outcomes – including academic performance, mental health, criminal activity, use of public services, and wages – and ages – including primary school, secondary school, higher education, and beyond.<sup>2</sup> More recent estimates of peer effects that use randomized and natural experiments to address endogeneity of peer group formation generally find empirical support for peer effects, although estimates vary considerably in magnitude depending on the outcome and age group studied.

In this paper, we examine peer effects in early education by looking at the effect of peer enrollment in preschool on children's outcomes in kindergarten and the early elementary grades. We focus on preschool because studies consistently demonstrate large private returns from early education on numerous cognitive outcomes.<sup>3</sup> Children may directly share the skills developed in preschool through social interactions in kindergarten, generating knowledge spillovers. Moreover, improved school readiness through early education may contribute to the pace of classroom learning in kindergarten so the entire class indirectly benefits from peer enrollment in preschool. These direct and indirect peer effects may be particularly important at this early age when environmental factors are so vital to development.

Since the peer effect we examine stems from a particular form of education (preschool), rather than inherent characteristics of peers, we interpret it as a spillover effect from early

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<sup>2</sup> We describe previous research on peer effects in section 2.

<sup>3</sup> See Currie (2001), Karoly et al. (2006), Cunha et al. (2006), Blau and Currie (2006), and Ludwig and Phillips (2007) for an overview.

education. Previous research on spillover effects from education has typically focused on social returns from higher education, but the difficulty in finding plausibly exogenous variation in peer educational attainment has led to mixed empirical evidence.<sup>4</sup> We contend that the variation in preschool enrollment used in this analysis, described below, is credible for identifying spillover effects. As growing amounts of state and federal funds are spent on public preschool programs, such as Head Start and public prekindergarten programs, knowledge of spillover effects is essential for assessing the efficient allocation of public investments in early education.

The perhaps more novel contribution of our study is the focus on non-cognitive outcomes in addition to cognitive outcomes. Although preschool has demonstrated positive cognitive benefits, researchers often find negative social and behavioral consequences of preschool (Belsky et al. (2007)). Children with limited self-control or discipline may unintentionally spread this behavior to peers, resulting in negative externalities. Given the growing evidence suggesting the importance of non-cognitive skills in human capital acquisition and earnings (Heckman and Rubinstein (2001), Heckman et al. (2006)), we also explore spillover effects from preschool enrollment on various social and behavioral outcomes.

Furthermore, children with behavioral problems may disturb teacher progress and hinder the learning of their classmates, so that peer behavior may affect academic performance. For example, an unruly child may require the teacher to focus more class time on discipline rather than on the dissemination of knowledge. Peer behavior is often the main rationale behind smaller class sizes (Lazear (2001)) and several peer effects studies on academic performance recognize the role of peer disturbance (Hoxby (2000), Hanushek et al. (2003), Ding and Lehrer

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<sup>4</sup> See Acemoglu and Angrist (1999) and Moretti (2004) for a review of empirical evidence.

(2007)), but with little direct empirical testing.<sup>5</sup> We directly examine the spillover effects of social and behavioral problems on academic achievement.

To account for the endogeneity of peers, which we define as kindergarten classmates, we adopt a fixed effects, value-added approach, comparable to that employed by Hanushek et al. (2003), Arcidiacono and Nicholson (2005), and Ding and Lehrer (2007), and exploit the richness of information in our data, the Early Childhood Longitudinal Study-Kindergarten Class. School fixed effects account for sorting into school districts by comparing children from classes within the same school. To account for selection into classes within a school, we estimate a value added specification that controls for fall kindergarten scores, which are measured shortly after kindergarten begins and unlikely to be affected by peers. Moreover, a considerable advantage of focusing on kindergarten outcomes is the limited scope for student tracking, a potential source of bias in value added models, because children are new to their environment. To account for common shocks during the kindergarten school year, we control extensively for individual, family, peer, and teacher characteristics.

We provide several pieces of evidence to support our empirical strategy. Peer preschool enrollment has small, statistically insignificant effects on fall-K scores, suggesting children are not assigned to peers based on initial ability. All available measures of teacher characteristics are uncorrelated with class average test scores, suggesting teachers are not systematically assigned to children. Parents do not appear to compensate for their child's peer group by increasing other investments over the same time period, such as the frequency they read to their child. Once we include either school fixed effects or fall-K scores or both, our results are

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<sup>5</sup> Several studies explore the effects of neighborhoods on both cognitive and non-cognitive outcomes (see, e.g., Sanbonmatsu et al. (2006)), but data limitations appear to prevent analysis of the effect of peer non-cognitive outcomes on individual cognitive outcomes. One notable exception is Figlio (2007), who focuses on peer disturbance in 6<sup>th</sup> grade students in one school district in Florida.

extremely robust to the inclusion of the numerous covariates available for our analysis, suggesting omitted variable bias is unlikely to plague our analysis. Lastly, in accordance with recent falsification tests for value added models suggested by Rothstein (2008), peer preschool composition in first grade is not significantly related to test score gains in kindergarten.

We find large, statistically significant effects of peer enrollment in preschool on math and reading outcomes. Peer preschool enrollment increase math and reading scores by 0.08 of a standard deviation in kindergarten, which implies the benefits from current preschool enrollment rates are understated by 16-25% if ignoring spillover effects. Furthermore, the impacts of kindergarten peer preschool enrollment persist in magnitude through first and third grade for math scores, though they decline somewhat for reading scores. These estimates imply that ignoring spillover effects significantly understates the social returns to preschool, supporting one rationale for public investment in preschool.

For non-cognitive outcomes, preschool does not appear to directly spillover to peers behaviors but it may indirectly impede peers cognitive achievement through its impact on individual behavior. We find imprecisely estimated spillover effects from peer preschool enrollment on all of our non-cognitive outcomes, although we can not rule out the possibility of preschool peer effects. In terms of the impact of peer non-cognitive development on individual cognitive outcomes, we find that the mean of class behaviors does not have a statistically significant impact. However, the 75<sup>th</sup> and 90<sup>th</sup> percentile of peer externalizing problems, a non-cognitive measure more likely to capture classroom disturbance, hinder math and reading scores, suggesting only a handful of unruly students may be sufficient for disrupting classroom learning. Despite these potential negative spillovers, the spillover effects from preschool are positive on net.

## 2. Background information

Preschool programs consist of a wide array of provisions, including community-based day care centers and preschools, Head Start programs, and school-based prekindergarten programs. There is an extensive body of research documenting private returns to early enrichment programs, such as the High/Scope Perry Preschool Project and Carolina Abecedarian, and Head Start, a public preschool program for disadvantaged children.<sup>6</sup> There is also a fairly large body of research on the effects of general preschool programs (reviewed in Smolensky and Gootman (2003) and Waldfogel (2006)). Evidence generally shows positive effects on cognitive outcomes, though they vary in magnitude across the types of program, but more mixed evidence on non-cognitive outcomes, including both negative (Belsky et al. (2007), Magnuson et al. (2007)) and positive effects (Puma et al. (2005)). The NICHD Study of Early Child Care and Youth Development (SECCYD) was designed to provide a comprehensive assessment of the effects of non-parental care prior to kindergarten. Recent results using the SECCYD data documented improvements in vocabulary but increases in behavioral problems through the end of sixth grade (Belsky et al. (2007)), attracting major headlines and fueling debates over non-parental care (Carey (2007)).<sup>7</sup>

Prior evidence of early education spillover effects is scant.<sup>8</sup> Garces et al. (2002) provide suggestive evidence that spillover effects from Head Start may exist. They indirectly examine this by comparing the effect of attending Head Start for older versus younger siblings. They posit that spillover effects are likely to flow from older to younger siblings because the older

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<sup>6</sup> See Currie (2001), Karoly et al. (2006), Cunha et al. (2006), Blau and Currie (2006), and Ludwig and Phillips (2007) for an overview.

<sup>7</sup> A major concern about the SECCYD analyses is little is done to address the selection of children into non-parental care. Other studies with more successful attempts at confronting selection, however, also find support for negative effects (Magnuson et al. (2007a)), so the effects in the SECCYD may not be due solely to selection.

<sup>8</sup> Evidence of spillover effects from higher education is pervasive (see Moretti (2004) for a review), but differing approaches for dealing with the endogeneity of schooling have yielded different results.

sibling is more likely to influence the younger, and because learned parenting skills may benefit younger children more than older. They find no evidence of spillover effects on educational attainment or earnings when the children are adults, but some evidence on criminal activity.

Evidence on peer effects is vast, so we focus on recent studies most relevant to our study, recognizing we omit several important studies. Since we examine peer effects during kindergarten, we first focus on studies that examined peer effects of primary school-aged children. Hanushek et al. (2003) and Hoxby (2000) found sizeable endogenous peer effects on math scores in grades 3-6 using the Texas schools microdata. Lefgren (2004) found small but statistically significant endogenous peer effects on reading scores of third and sixth graders in Chicago public schools. In terms of exogenous peer effects, Angrist and Lang (2004) found insignificant peer effects from an increase in minority peers in Boston on math, reading, and language scores in the 3<sup>rd</sup>, 5<sup>th</sup>, and 7<sup>th</sup> grades. Ammermueller and Pischke (2006) found positive peer effects of books owned on reading test scores in the 4<sup>th</sup> grade across several countries in Europe. Evidence from studies of neighborhood effects is also relevant in that such neighborhood effects are thought to work, at least in part, through exposure to different groups of peers. Sanbonmatsu et al. (2006) found no statistically significant neighborhood effects on reading and math scores for children ages 6-10 and 11-14 in five large U.S. cities in the Moving to Opportunity (MTO) experiment. Jacob (2004) also found no statistically significant neighborhood effects on academic achievement in children ages 3-13 in Chicago. These studies suggest that peer effects at these young ages exist, but depend on the context and specific type of peer effect examined.

Also relevant to our analysis are studies that focus on non-cognitive outcomes. While most focus on crime, some using the MTO experiment focus on mental health as well. Several

studies using the MTO found neighborhood effects significantly reduced violent crimes by teens (Ludwig et al. (2001)) and improved behavior problems for boys (Katz et al. (2001)), though these effects disappeared or changed directions for boys and improved for girls in later follow-ups (Kling et al. (2004), Kling et al. (2007), Sanbonmatsu et al. (2006)). Bayer et al. (2007) found strong peer effects on criminal activity based on the composition of prison mates. Like cognitive outcomes, these peer effects differ by context, but suggest the possibility of effects fading over time. Our study aims to add to this rich literature by examining peer effects on very young children, where effects may be particularly important, and by simultaneously examining cognitive and non-cognitive outcomes.

### **3. Data**

We use the Early Childhood Longitudinal Study-Kindergarten Class of 1998-99 (ECLS-K), a nationally representative sample of kindergarteners followed through 1<sup>st</sup> and 3<sup>rd</sup> grade.<sup>9</sup> The ECLS-K contains detailed information collected through direct child assessments, parent interviews, and teacher and school questionnaires. We use the K-3 longitudinal file, which contains 17,401 children, and weight all analyses to account for survey non-response so our estimates are representative of the kindergarten class.<sup>10</sup>

Outcome variables - For cognitive outcomes, we use direct cognitive assessments of mathematics and reading skills administered in the spring of kindergarten. The cognitive tests were designed to assess the age-specific achievement of the child. We use standardized scores that assess the performance of children relative to their peers. The scores are standardized to have a mean of 50 and standard deviation of 10, so we can divide our coefficient estimates by 10 to obtain effect sizes of going from no peers in preschool to all peers in preschool.

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<sup>9</sup> Children were also followed through the 5<sup>th</sup> grade, but these data are currently unavailable.

<sup>10</sup> Results are generally unaffected by the use of weights.

Given the difficulty in reliably measuring behavioral and social-emotional outcomes, we use several measures available from both teachers and parents. Teachers rated each individual student on four behavioral and social-emotional skills, including self-control (controlling temper, respecting others' property, accepting peer ideas, and handling peer pressure), interpersonal skills (getting along with people, forming and maintaining friendships, comforting or helping other children, showing sensitivity to the feelings of others, and expressing feelings, ideas and opinions in positive ways), externalizing problems (frequency with which a child argues, fights, gets angry, acts impulsively, and disturbs ongoing activities), and internalizing problems (presence of anxiety, loneliness, low self-esteem, and sadness). Externalizing problems are most consistent with the notion of peer disturbance (Lazear, 2001), so we pay particular attention to this measure.

Teacher's ratings of individual children may be subjectively reported relative to the average behavior of the class. For example, a generally disruptive child may be rated favorably in a class with numerous unruly peers but unfavorably in a class with few unruly peers. Therefore, we also use the parent's rating of their child's self-control as an additional outcome measure. Although the parent's rating may reflect the child's behavior at home rather than at school, the value added specification limits this concern as long as the change in behavior at home from fall to spring correlates well with the change in behavior at school over the same time period.

These measures are adapted from the Social Skills Rating Scale, a widely used survey technique for detecting social and behavioral problems in the classroom. Each construct averages a series of questions rated on a scale of 1 (never) to 4 (very often), so a high score on self-control and interpersonal skills reflects a favorable outcome while a high score on

externalizing or internalizing problems reflects an unfavorable outcome. Their use in predicting future non-cognitive functioning is subject to debate, but these scales have high construct validity as assessed by test-retest reliability, internal consistency, interrater reliability, and correlations with other, more advanced behavioral constructs (Elliott et al. (1988)) and are considered the most comprehensive social skill assessment that can be widely administered in large surveys such as the ECLS-K (Demaray et al. (1995)).

Preschool enrollment – Based on responses to the type of care arrangements in the year before kindergarten and the number of hours spent in each, the ECLS-K created a composite variable indicating the primary type of care (*'plprimpk'*). We use any center based care (Head Start, day care, nursery school, preschool, and prekindergarten) to define preschool enrollment, leaving parental care and relative or non-relative care in the child's or another home as the alternative. Although each type of care may have different effects on children's outcomes, sample sizes are too small to isolate peers effects from each source, so we capture the average peer effect across all types of care.<sup>11</sup>

Control variables - The ECLS-K contains detailed data on the family environment. Although we do not always measure specific inputs into human capital production and the full history of investments, we observe numerous proxies that reflect time and money available for families to invest at specific survey waves. For the mother, we include information on her current employment status, employment status at the period surrounding birth, educational attainment, immigration status, and age she gave birth. For the child, we use gender, race/ethnicity, and an indicator for low birth weight. For household characteristics, we use

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<sup>11</sup> We estimated models that include both Head Start (HS) and non-HS enrollment of class, and, although this stretches our sample and we lose considerable precision, estimates are comparable for the two groups. For math, the coefficient on non-HS is .478 and for HS is .340 (compared to .5 overall) and for reading it is .434 and .623, respectively (compared to .5 overall).

income, number of siblings, presence of father in household, father's employment status and education, central city or suburban residence, the number of grandparents the child has a close relationship with, whether English is spoken at home, and if anyone received food stamps or WIC. For direct measures of inputs, we include the number of books owned, number of records, tapes, or CDs, and an indicator if the parent reads to the child everyday.

Teacher and class variables – To account for teacher and classroom quality, we use data from the fall-K teacher questionnaire, which contains data on the class size and the teacher's background. The background measures include age, education, experience teaching both kindergarten and any grade, years of tenure at current school, gender, race, and type of teaching certificate. Furthermore, teachers were asked whether they enjoy teaching, would choose teaching again, and think by teaching they make a difference in children's lives. We created the variable "love teaching" equal to 1 if they responded 'yes' to all 3 questions.<sup>12</sup> While there are likely unmeasured components of teaching quality, these variables are widely accepted measures of teacher quality (Rivkin et al. (2005)).

Peer characteristics – Studies typically use population measures of peer characteristics, but such measures are unavailable at the class level. Instead, we compute class average enrollment in preschool and all other peer characteristics directly from the ECLS-K using the teacher identification number to identify students within the same class. Based on the number of students linked to each teacher and teacher reported class size, the ECLS-K sampled 41% of students within a class on average.<sup>13</sup> Although this greatly increases the quantity of peer

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<sup>12</sup> Note that these measures are from fall-K, so it is unlikely that the teacher's responses to these questions have been endogenously affected by the students in their class.

<sup>13</sup> Our computed averages compare favorably with three teacher reported averages available (51.0 vs. 51.5% percent of boys, 15.7 vs. 17.6% percent African-American, and 22.9 vs. 19.2% percent Hispanic), although the latter two may differ from self-reports for reasons other than measurement error – teachers may misclassify the race and ethnicity of students.

characteristics we can control for, it induces measurement error that may bias our results. Fortunately, we can adjust our coefficient estimates for the under sampling of students (Ammermueller and Pischke (2006)), described in more detail below.

For computing effect sizes, the standard deviations of the peer variables are also measured with error. We unfortunately can not adjust the standard deviations of these peer variables because it requires two reports for preschool attendance (Ammermueller and Pischke (2006)).<sup>14</sup> This precludes us from computing effect sizes from a one standard deviation change in peer preschool enrollment, a common metric used to compare results across studies. We can, however, use the standard deviation of 10 for math and reading scores to compute effect sizes from certain changes in preschool enrollment because this is a nationally normed test and our analyses are weighted to provide nationally representative results (normed scores are unavailable for the non-cognitive outcomes).

Sample characteristics –Starting from a possible sample of 17,401 children, we exclude children who were not first-time kindergarteners (i.e., who repeated kindergarten) (665), with preschool information missing (2,897), with only 1 child per class (so peer averages couldn't be computed) (416), and with test scores, teacher identification, and school identification unavailable (480), leaving a possible sample of 12,943 children. Of the above mentioned covariates, there was a large number of missing cases for all teacher and class variables and some parental responses (maternal age at birth, immigration status, grandparents close, early maternal employment, and number of books owned) (3,278), so we impute them using single imputation by chained equations to preserve sample size (Van Buuren et al. (in press)).<sup>15</sup> For the

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<sup>14</sup> When there is classical measurement error, the covariance of the two variables can be used a measure of the true variance (Ammermueller and Pischke (2006)).

<sup>15</sup> We choose not to impute the dependent variables or preschool variable because they do not appear to be missing at random, a necessary assumption for imputation to yield unbiased estimates. Furthermore, because we do not

non-imputed covariates, we use casewise deletion, leaving us with a final sample of 12,501 with math scores, 11,840 with reading scores, and sample sizes for the non-cognitive outcomes ranging from 11,446 to 12,070 depending on the outcome.<sup>16</sup>

#### 4. Empirical strategy

##### A. Econometric model

To estimate peer effects, we focus on the following regression equation:

$$(1) \quad y_{icd}^s = \beta_0 pre_{icd} + \beta_1 \overline{pre_{(-i)cd}} + \beta_2 x_{icd} + \beta_3 \overline{x_{(-i)cd}} + \beta_4 z_{cd} + \beta_5 y_{icd}^f + \alpha_d + \eta_{cd} + \varepsilon_{icd}$$

where  $y$  is the child's cognitive or non-cognitive outcome in kindergarten,  $s$  indicates spring,  $f$  indicates fall,  $i$  is the individual,  $c$  is the classroom, and  $d$  is the school.  $pre$  indicates whether the child was enrolled in preschool and  $\overline{pre_{(-i)cd}}$  is mean enrollment of the class (not including the index child).  $x$  are individual and family level characteristics and  $z$  are classroom and teacher specific characteristics. The error term consists of a group specific component ( $\eta_{cd}$ ) and an individual, idiosyncratic component ( $\varepsilon_{icd}$ ). We adjust all standard errors to account for clustering of students in the classroom. Our main hypothesis to test is that  $\beta_1 = 0$ .

In this specification we omit the class test score ( $\overline{y_{(-i)cd}^f}$ ) from this equation to estimate the reduced form peer effect of preschool: the direct effect on test scores plus the indirect effect on test scores through its impact on class test scores. We interpret this estimate as the spillover effects from early education, which is a policy effect of interest for understanding the efficient allocation of early education.

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impute preschool, we perform single rather than multiple imputation. Standard errors for the preschool variables will be valid, but standard errors for the imputed covariates will be understated (Allison (2002)).

<sup>16</sup> Children excluded from the analysis typically have lower test scores, worse non-cognitive measures, lower preschool enrollment, and come from more disadvantaged backgrounds (not shown). Although we weight our analysis, these patterns suggest our results may not generalize to the entire kindergarten cohort. Without further assumptions about the cause of missing observations, there is little we can do to overcome this concern.

As previously mentioned, we use a sample of students in the class, rather than the entire class, to compute  $\overline{pre_{(-i)cd}}$ . Ammermueller and Pischke (2006) present a straightforward technique for adjusting the coefficient estimates to obtain consistent estimates free from measurement error; we present a version tailored to our model in the appendix. Consistency of peer effects estimate is given by:

$$(2) \quad \hat{\beta}_{1adj} = \hat{\beta}_1 \left( \frac{\overline{N_{cd}} - 1}{n_{cd} - 1} \right)$$

where  $n_{cd}$  is the number of sampled students within a class (8.55) and  $N_{cd}$  is the total number of students in the class (20.51), both observed in the data.<sup>17</sup>

Endogeneity of  $\beta_l$  may arise if parents choose certain schools based on education preferences, so that a school with higher achieving students may reflect unobserved school quality or parental investments (Black (1999)). Moreover, children may sort into different classes based on the quality of the teacher or peers so that the most able students end up with the best quality teachers or peers. Alternatively, schools may assign children to different classrooms based on compatibility with the teacher or to obtain a particular mix of peers they deem optimal.<sup>18</sup>

Equation (1) has several features to aid in identification of  $\beta_l$ . To account for the endogeneity of group formation, we include school fixed effects ( $\alpha_d$ ) to limit our comparison to children in different classrooms within the same school, which accounts for sorting into school districts. We also include fall kindergarten outcomes ( $y_{icd}^f$ ) in equation (1), which are measured before children have had sufficient time to interact with their peers, so we examine how

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<sup>17</sup> Although class sizes vary in the sample, Ammermueller and Pischke (2006) demonstrate that using the overall means in their sample performs well for the level of variation in their sample, which is comparable to the level of variation we observe.

<sup>18</sup> Because we are focusing on exogenous peer effects and not endogenous peer effects, we do not focus on concerns regarding simultaneity bias.

outcomes change over the course of the kindergarten year. Our estimated peer effect is therefore the correlation between the variation within schools in class preschool enrollment and the variation within schools of individual's changes in test scores during kindergarten (conditional on the included covariates).

### B. Validity of econometric model

The summary statistics in Table 1 highlight both the endogeneity of peer preschool enrollment and the strength of our methodology. Columns (1) and (2) show means and standard deviations for the individual level variables and teacher characteristics for all children included in the analysis.<sup>19</sup> Column (3) shows the difference in means of these variables for children in classes below and above the median class preschool enrollment of 59%, respectively, with p-values of the differences shown in column (4). For the most part, children with more peers in preschool perform better on tests, with math and reading scores 1.8 points higher in the above median group. However, these children also come from more advantaged backgrounds, as demonstrated by higher parental income and education, to name a few. Out of the 28 covariates listed in the table, 17 have statistically significant differences for children in classes above and below the median. This suggests the importance of addressing the endogeneity of preschool enrollment.

Columns (5) and (6) highlight the strength of our empirical strategy by presenting these same differences and p-values after adjusting for school fixed effects. Immediately evident is that balance of covariates is achieved, exactly what would occur if children are randomly assigned into classes: of the 41 covariates, only 1 difference is statistically significant. Not only are these differences much less likely to be statistically significant, they are also much smaller in

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<sup>19</sup> We omit the class averages of the individual level variables from this table, but include them in the regression analysis.

magnitude. For example, the difference in family income falls from nearly \$12,000 without school fixed effects to just over \$700 with school fixed effects. When we further adjust for baseline test scores in addition to school fixed effects (not shown), the same balancing occurs. This supports the notion that peer preschool enrollment within schools is uncorrelated with unobservable factors that affect children's outcomes, a necessary condition for obtaining causal estimates of peer effects.

We further probe possible selection effects into classes based on peer preschool enrollment by replacing the dependent variable in equation (1) with individual level fall-K scores (and omit it from the right hand side). If selection into classes is based on preschool enrollment, then peer preschool enrollment will have a significant effect on fall-K scores. Results from this specification, shown in columns (1) and (2) of Table 2, indicate that fall-K scores are uncorrelated with peer preschool enrollment. The coefficient estimates are small in magnitude and very imprecise. This suggests that, within schools, students do not appear selectively assigned to peer preschool enrollment based on their initial ability. This result in conjunction with the balancing of the adjusted covariates suggest school fixed effects may alone be sufficient for identifying causal peer effects. In fact, as we demonstrate below, the value added specification has little impact on our estimates once we include school fixed effects.

Even if peers are randomly assigned, an additional concern in our analysis is that families may respond to their child's peer group by compensating for low peer quality, leading to spurious estimates of peer effects (Moffitt (2001)). Parent may recognize their child's peer quality either directly by knowing the classmates or indirectly through their child's interim academic performance. Changing peer groups by switching classes is unlikely to arise – less than 3% of respondents in our sample change classes within a school in kindergarten for any

reason. Parents may, however, respond over the course of the year by providing other investments in their child.

To explore whether such compensatory behavior exists, we examine whether peer preschool enrollment affects two parental investments available in the ECLS-K: the number of books the child owns and whether the parent reads to the child everyday. We estimate equation (1) by using the parental investments in place of  $y$ , though we use reports at the end of first grade instead of spring-K for the dependent variable, the earliest time they are asked again in the ECLS-K after the fall-K interview. Shown in columns (3) and (4) of Table 2, children own more books as more peers are enrolled in preschool but are read to less often by their parents, though both estimates are not statistically significant. Although there are other ways parents can compensate for their children's environment, this evidence generally does not support that such behavior exists.

Although the value added model aids in identifying peer effects, there are two specific assumptions that must be met.<sup>20</sup> One, the impact of prior inputs and endowments decay at a constant rate. Although we can not test this assumption directly, we control for several proxies for historical inputs and endowment, and demonstrate that our results are insensitive to the inclusion of these variables.

The second assumption is contemporaneous omitted inputs are not correlated with previous test scores, which may arise if teachers are strategically assigned to students based on their previous performance. We do not think this poses a considerable threat to our model because kindergarten children are mostly in a school for the first time where limited historical information about the student is available. Also, we use fall kindergarten scores as the baseline

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<sup>20</sup> See Hanushek et al. (2003), Todd and Wolpin (2003), and Rothstein (2008) for a full derivation of a comparable model and the necessary identification assumptions.

score, which is obtained shortly *after* students have been assigned to teachers, so school administrators may not be aware of children's initial ability when assigning them to teachers.

We also provide empirical support for this assumption in Panel A of Table 3, which presents results from a regression of the class average scores in fall-K on teacher and class characteristics in kindergarten, such as education, certification, experience, and class size, and school fixed effects. Within each panel, the dependent variable is either the mean or the variance of the individual scores within a class; teachers may have classes with comparable mean levels of ability but a more effective teacher may be assigned to students with a wider range of abilities. Based on an F-test of joint significance, these characteristics are uncorrelated with all class level outcomes, suggesting contemporaneous inputs from teachers are unlikely to bias our results.

Alternatively, if we perform this same analysis for later grades, we find evidence that teachers are systematically assigned to students. In Panel B, we regress class average scores from spring-K on first grade teacher and class characteristics and school fixed effects. In Panel C, we regress spring first grade class average scores on grade three teacher and class characteristics and school fixed effects. Teacher characteristics are correlated with mean math and reading scores even within schools, and are correlated with some of the non-cognitive outcomes. These results suggest teacher sorting is likely to be a concern for examining peer effects from later grades, so our empirical strategy may not be valid for examining the impact of peer preschool enrollment in each grade.

As a general specification check of the value added model, we regress current outcomes on *future* inputs. Future inputs should not be correlated with current test scores, so any evidence to the contrary suggests our model is misspecified (Rothstein, 2008). In columns (5) and (6) of Table 2, we present estimates from a regression of spring kindergarten outcomes on fall

kindergarten outcomes and first grade peer and teachers characteristics (omitting kindergarten peer and teacher characteristics). The coefficient on first grade peer preschool is not statistically distinguishable from zero, further supporting the validity of our empirical strategy.

As further support for this assumption, we exploit the richness of background information available in the ECLS to control for numerous inputs during the kindergarten year. We include in  $x$  various individual level factors and, because we compute peer characteristics from these individual characteristics, we also control extensively for composition of peers in  $\overline{x_{(-i)cd}}$ . Furthermore, we include the teacher and classroom characteristics in  $z_{cd}$ . As we demonstrate below, our estimates of  $\beta_l$  are extremely robust to the inclusion of these variables.

We note that peer preschool enrollment may be correlated with individual preschool enrollment, suggesting it may be difficult to distinguish whether the effects of peer preschool enrollment are due to own enrollment or enrollment of peers. Although individual enrollment may be endogenous, our value added specification will capture the immediate private returns to preschool in the fall-K score. As with other historical inputs, as long as there is constant decay in the private returns to preschool, our estimates for class preschool enrollment will reflect peer effects. We can test this assumption by adding individual preschool to our regression and a) testing whether the coefficient on individual preschool differs from zero and b) assessing whether the coefficient on peer preschool changes. For cognitive outcomes, we find that both individual preschool is not statistically significant and including it does not change the estimated peer preschool effect. For non-cognitive outcomes, we find the coefficient on individual preschool is statistically significant but our estimates of the peer effects are unchanged by including it.<sup>21</sup>

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<sup>21</sup> Results are not shown, but are available from authors upon request.

We also note that the coefficient on individual preschool reflects the additional effects of individual preschool above and beyond any immediate effects, so it provides an additional way of partially assessing the private returns to preschool. Unfortunately, the value added approach precludes us from identifying the full private returns to preschool enrollment since the immediate returns are captured by the lagged test score.

## 5. Results

### A. Cognitive spillover effects from preschool

For both cognitive outcomes, we present results from 4 sets of models: without both school fixed effects and fall-K scores, without school fixed effects but with fall-K scores, with school fixed effects but without fall-K scores, and with both school fixed effects and fall-K score (our preferred specification). Within each model, we estimate 4 specifications: the first includes only individual and peer preschool enrollment as covariates ( $pre_{icd}$  and  $\overline{pre_{(-i)cd}}$ ), the second adds individual level covariates ( $x_{icd}$ ), the third adds group level covariates ( $\overline{x_{(-i)cd}}$ ), and the fourth adds teacher and class characteristics ( $z_{cd}$ ). We make several comparisons across specifications to underscore the strength of our empirical strategy.

Columns (1)-(4) of Table 4 show results for math scores without adjustment for measurement error in the peer variables. Focusing on estimates without school fixed effects and fall-K scores (panel A), we find a statistically significant effect of peer enrollment in preschool of 3.3, which implies that going from no peers in preschool to all peers in preschool raises math scores by 3.3 points. This estimate, however, gets successively smaller in magnitude as we control for more covariates. In column (4), which includes all covariates, the estimate falls to 0.92, though it remains statistically significant at conventional levels. This changing pattern

across specification suggests the likelihood of omitted variable bias, so we can not make strong claims that estimates from column (4) are free from bias.

When we add fall-K scores but continue to omit school FEs (panel B), we find much less variability in estimates across the specifications: they range from 0.55 to 0.70 and are all statistically significant. The stability of estimates suggests the value added approach controls for many confounding factors. Additionally, the coefficient on individual preschool now becomes statistically insignificant, indicating that the value added specification controls well for prior inputs and endowment and most of the effect of preschool on cognitive outcomes is immediate.

When we add school FEs but omit fall-K scores (panel C), we also find that our estimates range minimally as we add more covariates, from 0.55 to 0.75, and remain statistically significant at the 5 or 10% level. This suggests that once we account for sorting into school districts, there is little evidence of sorting into classes within schools based on preschool enrollment, as results from Tables 1 through 3 indicated. Importantly, despite two entirely different sources of variation to identify spillover effects, these results are quite comparable to the value-added results without school FEs, providing further credibility to our estimation strategy.

In the value-added specification with school FEs (panel D), our estimates range even less as we add more controls, ranging from 0.53 to 0.51. The spillover estimate that includes only individual preschool, fall-K scores, and school FEs is 0.52, and including the full set of over 70 covariates slightly reduces the estimate to 0.51. Breaking this down further, our estimates change from 0.52 in column (3) to 0.51 in column (4) when we add the measures of teacher quality and classroom characteristics, further supporting the notion that teacher sorting is unlikely to drive our results. The individual preschool effect now is essentially zero, suggesting

our peer preschool variable is in fact capturing the impact of peers. The R-squared ranges from 0.74 to 0.75, suggesting the value added approach is controlling for a considerable amount of variability in math scores. Many of the control variables we add are widely found to be highly predictive of children's achievement, so it is impressive that our spillover estimates are unaffected by their inclusion. In fact, comparing the estimates from column 4 of panel C to panel D, we see our R-squared increase from 0.41 to 0.75, but our estimates for peer preschool only changes from 0.55 to 0.51. Despite the vast increase in explanatory power of the regression, the stability of our estimate further demonstrates the robustness of our methodology.

Turning to reading scores in columns (5)-(8) we find not only the same pattern of robustness across models and within specifications but estimates that are nearly identical in magnitude as the math scores. Estimates range from 0.50 to 0.61 in the specification with school fixed effects and fall-K scores and are statistically significant at the 5% level. This remarkable stability across important predictors and different sources of variation is supportive that our empirical strategy is uncovering causal spillover effects and not merely correlations.<sup>22,23</sup>

To assess the magnitude of these spillover effects, we would ideally like to compute effect sizes from a standard deviation change in peer preschool enrollment to facilitate comparisons to other peer effects studies. We can use equation (2) to adjust our coefficients for measurement error in peer preschool enrollment and use the normalized standard deviation of 10

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<sup>22</sup> We also estimate our model using the gain in test scores in kindergarten as the dependent variable rather than including fall-K scores as an independent variable. Using our full specification with school fixed effects and all covariates, the estimate for math scores is 0.493 and for reading scores is 0.495, and both are statistically significant at the 5% level. These results are also largely insensitive to excluding the covariates, ranging from 0.464 to 0.495 for math scores and 0.495 to 0.538 for reading scores.

<sup>23</sup> As an additional specification check, we also estimate models that include the class average fall-K scores (excluding the reference child) to control for unobserved group effects not accounted for in the group level covariates. Estimates are largely unaffected by including the class average scores: the estimate for math scores changes slightly from 0.506 to 0.471 and on reading scores is virtually unchanged at 0.507.

for test scores, but we unfortunately can not make an adjustment to the standard deviation of peer preschool enrollment with the data at hand.

As an alternative approach for assessing magnitude, we compute the spillover effects from various changes in preschool enrollment rates and compare it to estimated private returns from preschool programs; this yields estimates of the missed benefits from ignoring spillover effects. For example, we compute the spillover effects of moving from no children in preschool to the current rate of preschool enrollment, which gives estimates of the missed benefits from current preschool enrollment rates. For estimates of the private returns to preschool, we use estimates from Gormley and Gayer (2005) for private returns to prekindergarten and from Puma et al. (2005) for private returns to Head Start. Although our preschool variable captures a wide array of provisions and may not be directly comparable to these private returns,<sup>24</sup> we choose these two because both have strong research designs and provide readily comparable effect sizes for cognitive outcomes. Gormley and Gayer (2005), who exploited the introduction of a universal prekindergarten program in Tulsa, Oklahoma by comparing those born just after the cutoff date for admission to those born just before, found cognitive outcomes of prekindergarten attendees improved by 0.39 of a standard deviation in kindergarten (denoted '*GG*' in the chart below). The Head Start Impact Study (Puma et al. (2005)), which randomly assigned children into Head Start, found statistically significant effect sizes ranging from 0.16 to 0.24 across several cognitive assessments measured at age 4. Since the estimates from Puma et al. (2005) reflect intent to treat, to maintain consistency with our estimates we instead use the treatment on the treated estimates from Ludwig and Phillips (2007) that range from 0.24 to 0.36 (denoted '*LP low*' and '*LP high*', respectively, in the chart below).

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<sup>24</sup> See footnote 11.

We compute the following spillover rates using estimates from our preferred specification: 1) going from 0% to 100% enrollment, 2) going from 0% to the current enrollment rate of 45.8%, and 3) going from the current enrollment rate to 100%.<sup>25</sup> The third comparison is potentially important for understanding the impacts of implementing universal preschool, although we recognize this makes the strong assumption that current quality of preschool remains unchanged under universal preschool. The following chart shows computations for the 3 alternatives.

$\Delta_{preschool}$ <i>enrollment</i>	(1)	(2)	(3)		
	$B_{1adj}/\sigma_y$	$(B_{1adj}/\sigma_y)*\Delta_{preschool}$	<i>column (2)/private returns</i> <i>GG</i>	<i>LP low</i>	<i>LP high</i>
0-100%	0.132	0.132	0.338	0.548	0.366
0-45.8%	0.132	0.076	0.155	0.252	0.167
45.8-100%	0.132	0.055	0.183	0.297	0.198

Notes:  $B_{1adj}$  is the measurement error adjustment given in equation (2) using coefficient estimates from column (4) from Panel D of Table 2.  $\sigma_y$  is the standard deviation of test score (=10).

Estimates from the second row imply that the benefits from current preschool enrollment rates are understated by 16-25% if ignoring spillover effects and estimates from the third row imply that the benefits of moving from current preschool enrollment rates to universal preschool are understated by 18-30%. Results from this chart suggest spillover effects are an important component of private returns; the social returns to preschool are considerably understated if spillover effects are overlooked.

### B. Non-cognitive spillover effects from preschool

Turning to the behavioral and social-emotional outcomes, shown in Table 5, we only present results from our preferred specification (value added with school fixed effects and full set of controls) because we examine 5 different dependent variables, though we find similar

<sup>25</sup> We cannot simply add the last two to get the first one because peer effects are multiplier effects. We recognize this assumes linear peer effects, which may be inappropriate if those currently enrolled are most likely to benefit from preschool.

robustness patterns across specifications as with the cognitive outcomes (not shown). In contrast to cognitive outcomes, we do not find statistically significant spillover effects on behavioral or social-emotional outcomes. In fact, for none of the 5 outcomes in any of the specifications do we find a statistically significant effect. We also perform a joint test across all 5 outcomes by performing a seemingly unrelated regression, and the p-value from this test is 0.39. Although the estimates are not statistically significant, the effect sizes of moving from no peers in preschool to all peers in preschool (using the sample standard deviations) are comparable in magnitude to the effect sizes for the cognitive outcomes, though we can not be certain these effect sizes are correct because the standard deviations of the non-cognitive outcomes are measured with error. Therefore, we can not rule out the possibility of behavioral spillover effects, and we further probe this below.

We also find of interest that the individual preschool effect persists despite including fall-K scores. That is, children who went to preschool have social or behavioral problems that increase over the course of the kindergarten year. Although the value-added approach does not yield estimates of the full behavioral effect – fall-K scores absorb initial behavioral effects – the fact that we find a continued effect is strongly suggestive that adverse individual level behavioral effects from preschool exist.

### C. The impact of peer non-cognitive development on individual cognitive outcomes

We next examine whether peer non-cognitive development has an effect on cognitive outcomes by including in equation (1) the class average from fall-K of each of the non-cognitive measures. If peer behavior disrupts classroom learning, we expect to find positive coefficients on self-control and interpersonal skills and negative coefficients on externalizing and internalizing problems. Shown in columns (1) and (4) in Table 6, we find that the class averages

of the behavioral and social-emotional outcomes have no statistically significant effect on cognitive outcomes, according to both individual t-tests and a joint F-test that circumvents concerns regarding multicollinearity of the non-cognitive measures. Furthermore, including these measures has little effect on our estimates of the preschool spillover effects.

One concern with using the mean of class behaviors is it may only take one disruptive students to inhibit classroom instruction (Lazear, 2001), and this may not be captured in the mean. We instead include the 75<sup>th</sup> and 90<sup>th</sup> percentile of class behaviors, which will more reliably detect the presence of any disruptive children. Shown in the subsequent columns in Table 6 (columns (2)-(3), and (5)-(6)), we find a statistically significant negative effect for externalizing problems on math scores, which is the behavioral measure most consistent with disruptive behaviors. For reading scores, none of the non-cognitive outcomes are statistically significant individually, but a joint F-test suggests they have a statistically significant impact on reading scores. We find little difference between estimates from the 75<sup>th</sup> and 90<sup>th</sup> percentile for both math and reading scores, which suggests only a handful of unruly peers are sufficient for disrupting the academic progress of their classmates.

We also note that the coefficient on peer preschool becomes slightly larger in these specifications as well. This is not surprising because, as previously mentioned, Table 4 produces reduced form estimates of the impact of peer preschool enrollment. That is, the results from Table 4 represent the direct effect of peer preschool enrollment plus the indirect effect through its impact on class averages. Therefore, since we have partialled out the negative indirect effect from class averages in Table 6, the direct effect from peer preschool enrollment increases. This also indicates that although the results from Table 6 suggest some negative spillover effects from

preschool, the reduced form estimates from Table 4 indicate the net spillover effects from preschool are positive.

#### D. Persistence of spillover effects

Even if students show initial gains from peers, it is possible these effects disappear over time as children age, given evidence of fade-out effects from Head Start (e.g., Currie and Thomas (1995)). In Table 7, we explore the persistence of effects by looking at the effects of peer preschool enrollment on spring 1<sup>st</sup> and 3<sup>rd</sup> grade outcomes. For math scores, we find comparable point estimates (0.68 for first grade and 0.60 for third grade) to the spillover estimates in the spring of kindergarten. Estimates are less precise, which could be due in part to the smaller sample that results from attrition, though they remain statistically significant for the first grade results. For reading scores, we find some evidence the impacts fade over time, with estimates falling to 0.36 in first grade and 0.37 in third grade, though this difference is not statistically significant. This slightly fading impact is consistent with the negative coefficient on individual preschool (from Table 4), which suggest private returns to preschool on reading decreases over time. Both sets of estimates are also generally insensitive to the inclusion of control variables.<sup>26</sup>

#### E. Heterogeneity of spillover effects

Results thus far have examined the average impact of peer preschool enrollment, but the impact may vary across children. To assess heterogeneous effects, we interact peer preschool with several characteristics of the child: own preschool enrollment, food stamp recipient, race, immigration status, gender, and presence of father. Shown in Table 8, we do not find statistically significant interaction effects for any of the variables we examine, though the lack of an

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<sup>26</sup> For behavioral and social-emotional outcomes (not shown), the estimates remain statistically insignificant and become considerably smaller in magnitude for externalizing and internalizing problems.

interaction effect with own preschool enrollment, shown in column (1), is interesting in its own right. A negative coefficient on the interaction term would suggest that once a child attends preschool they no longer benefit from their peers enrollment because they already gained the skills from their own attendance. The fact that we do not find a statistically significant interaction term dismisses the notion that 100 percent enrollment would no longer result in spillover effects.

## **6. Conclusion**

This paper explores whether spillover effects from early education programs exist by examining the effect of peer enrollment in preschool on children's outcomes in kindergarten. To address concerns regarding omitted variables, we estimate value added models with school fixed effects that control extensively for individual, family, peer, and teacher characteristics. Although kindergarten outcomes are of interest in their own right, focusing on this age group strengthens our empirical strategy because there is less sorting into classes based on prior outcomes. Numerous sensitivity analyses support the interpretation of our estimates as causal estimates of peer effects.

We find robust, significant spillover effects from preschool on math and reading scores that appear to persist through the third grade. We find little evidence to support direct social and behavioral spillovers, but we find that peer non-cognitive development, particularly externalizing problems, impacts student achievement, suggesting an indirect route through which preschool has negative impacts. The social returns to preschool, however, are positive on net.

These results have three implications. One, significant spillover effects from preschool suggest a potentially suboptimal allocation of preschool enrollment. Two, our results that peer externalizing behaviors impact individual cognitive outcomes supports the key assumption

through which smaller class sizes improve academic achievement (Lazear (2001)). Three, and most generally, our evidence of strong peer effects as early as kindergarten support contentions that policies such as school choice and ability tracking will have significant consequences for the academic achievement of children.

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## Appendix

In this section we sketch the measurement error adjustment in equation (2) based on the derivation in Ammermueller and Pischke (2006) with school fixed effects.<sup>27</sup> Suppose we are interested in estimating the following equation (omitting other covariates for clarity):

$$y_{icd} = \beta_1 w_{cd} + \varepsilon_{icd}$$

where  $w_{cd} = \overline{pre_{(-i)cd}}$  is the average preschool enrollment less the index child for the entire class of students ( $N_{cd}$ ). We only observe a sample of students in the class ( $n_{cd}$ ), and denote the measured average preschool enrollment by  $w_{cd}^*$ . The estimate of  $\beta_1$  converges to:

$$p \lim \hat{\beta}_1 = \frac{\text{cov}(y, w^*)}{\text{var}(w^*)} = \frac{\text{cov}(\beta_1 w + \varepsilon, w^*)}{\text{var}(w^*)} = \frac{\beta_1 \text{cov}(w, w^*)}{\text{var}(w^*)}$$

We must define the covariance and variance terms in the above equation, where  $P_{-i}$  is the set of all students in the class and  $S_{-i}$  is the set of sampled students in the class:

$$\begin{aligned} \text{var}(w^*) &= \text{var}\left(\sum_{S_{-i}} \frac{pre_i}{n_{cd} - 1}\right) = \left(\frac{1}{n_{cd} - 1}\right)^2 \text{var}\left(\sum_{S_{-i}} pre_i\right) = \left(\frac{1}{n_{cd} - 1}\right)^2 (n_{cd} - 1) \text{var}(pre_i) = \left(\frac{1}{n_{cd} - 1}\right) \text{var}(pre_i) \\ \text{cov}(w, w^*) &= \text{cov}\left(\sum_{P_{-i}} \frac{pre_i}{N_{cd} - 1}, \sum_{S_{-i}} \frac{pre_i}{n_{cd} - 1}\right) = \left(\frac{1}{N_{cd} - 1}\right) \left(\frac{1}{n_{cd} - 1}\right) \sum_{P_{-i}} \sum_{S_{-i}} \text{cov}(pre_i, pre_i) \\ &= \left(\frac{1}{N_{cd} - 1}\right) \left(\frac{1}{n_{cd} - 1}\right) (n_{cd} - 1) \text{var}(pre_i) = \left(\frac{1}{N_{cd} - 1}\right) \text{var}(pre_i) \end{aligned}$$

Plugging these terms in gives:

$$p \lim \hat{\beta}_1 = \frac{\beta_1 \left(\frac{1}{N_{cd} - 1}\right) \text{var}(pre_i)}{\left(\frac{1}{n_{cd} - 1}\right) \text{var}(pre_i)} = \beta_1 \left(\frac{n_{cd} - 1}{N_{cd} - 1}\right).$$

We can use the measured averages for  $N_{cd}$  and  $n_{cd}$  from our sample to adjust our estimate of  $\beta_1$  to obtain consistent estimates of the peer effect:

$$\hat{\beta}_{1adj} = \hat{\beta}_1 \left(\frac{\overline{N_{cd}} - 1}{n_{cd} - 1}\right).$$

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<sup>27</sup> The main difference is we assume no measurement error in the individual level covariates and therefore derive results for a bivariate regression.

Table 1. Summary Statistics

	1	2	3	4	5	6
	mean	std dev	difference	p-value	difference (FE)	p-value (FE)
<u>Dependent variables</u>						
math spring K	51.080	9.667	1.854	0.000	0.328	0.019
reading spring K	50.960	9.583	1.840	0.000	0.326	0.022
self-control spring K (teacher rating)	3.196	0.621	-0.055	0.004	-0.026	0.035
externalizing problems spring K	1.655	0.641	0.053	0.002	0.028	0.014
interpersonal relations spring K	3.136	0.632	-0.019	0.317	-0.019	0.119
internalizing problems spring K	1.556	0.508	0.001	0.963	0.010	0.326
self-control spring K (parent rating)	2.881	0.498	0.035	0.002	-0.010	0.217
<u>Class &amp; teacher characteristics</u>						
class average preschool	0.580	0.258	0.416	0.000	0.194	0.000
class size	20.651	4.381	-0.160	0.436	0.042	0.473
teacher age	41.727	9.978	-0.036	0.940	0.217	0.449
years tenure at school	9.344	7.850	-0.070	0.855	0.164	0.474
years teach K	9.154	7.714	0.059	0.879	0.039	0.870
total years teaching	12.948	9.218	-0.123	0.788	0.011	0.969
teacher male	0.015	0.123	-0.009	0.136	0.001	0.725
teacher white	0.862	0.345	0.002	0.916	-0.007	0.433
temporary teaching certificate	0.084	0.277	0.007	0.578	0.007	0.409
alternate teaching certificate	0.014	0.120	0.006	0.251	-0.001	0.853
regular teaching certification	0.223	0.416	0.030	0.127	0.006	0.600
highest teaching certification	0.656	0.475	-0.048	0.034	-0.012	0.360
some graduate school	0.335	0.472	-0.071	0.001	-0.003	0.797
masters or more	0.374	0.484	-0.005	0.845	0.000	0.993
<u>Background characteristics</u>						
individual preschool	0.580	0.494	0.415	0.000	0.193	0.000
maternal age at birth	23.886	5.406	1.207	0.000	0.065	0.339
grandparents close	2.215	1.242	0.070	0.014	0.027	0.136
# of siblings	1.436	1.062	-0.078	0.001	0.000	0.991

Table 1. Summary Statistics (continued)

family income	52167	44329	11648	0.000	731	0.171
# books owned	76.706	59.694	6.536	0.000	1.111	0.141
# records, tapes, CDs owned	15.417	17.705	1.695	0.000	0.445	0.076
child age	6.207	0.342	0.002	0.832	0.003	0.563
parent read to child everyday	0.458	0.498	0.035	0.002	0.008	0.285
mom employed FT	0.461	0.499	-0.003	0.799	-0.004	0.583
mom employed PT	0.222	0.416	0.006	0.503	0.001	0.853
dad employed FT	0.718	0.450	0.000	0.992	0.007	0.276
dad employed PT	0.026	0.159	0.000	0.956	0.001	0.615
central city	0.351	0.477	0.011	0.635	0.000	0.157
suburban	0.423	0.494	0.112	0.000	0.000	0.901
male	0.508	0.500	-0.005	0.584	-0.007	0.296
black	0.144	0.351	0.079	0.000	0.004	0.355
hispanic	0.167	0.373	-0.076	0.000	-0.014	0.003
mother HS dropout	0.124	0.329	-0.054	0.000	-0.006	0.223
mother HS graduate	0.634	0.482	-0.058	0.000	0.000	0.979
father HS dropput	0.102	0.303	-0.054	0.000	0.000	0.930
father HS graduate	0.471	0.499	-0.078	0.000	-0.007	0.310
immigrant	0.172	0.377	-0.047	0.000	-0.009	0.094
father present	0.784	0.412	-0.015	0.177	0.004	0.481
early maternal employment	0.755	0.430	0.010	0.272	0.000	0.941
low birthweight (<5.5 lbs.)	0.074	0.262	-0.004	0.496	-0.005	0.165
english at home	0.902	0.297	0.055	0.000	0.005	0.292
mother or child WIC	0.451	0.498	-0.086	0.000	-0.003	0.680
receive food stamps	0.172	0.377	-0.011	0.290	-0.001	0.865

# of individuals=12501, # of schools=901, # of classes=2436

All values are weighted by sampling probability. 'difference' is the difference in means of the variables for children in classes above vs. below the median class preschool enrollment of 59%. 'p-value' is from t-test of variables below/above median that cluster on class. 'FE' adjusts variables for school fixed effect.

Table 2. Tests of Peer Group Exogeneity

	1	2	3	4	5	6
	Fall-K Outcomes		Compensatory Behavior		Future inputs	
	Math	Reading	# books owned	read to child everyday	Math	Reading
individual preschool	0.922	0.96	3.151	0.000	-0.095	-0.187
	[0.166]***	[0.177]***	[2.602]	[0.010]	[0.118]	[0.128]
class preschool	0.056	0.033	3.928	-0.010	-0.025	0.244
	[0.318]	[0.357]	[4.002]	[0.020]	[0.222]	[0.255]
# of individuals	12501	11840	11481	11526	10463	9923
# of schools	901	891	900	900	886	874
# of classes	2436	2275	2450	2450	2349	2194
<u>Covariates</u>						
school fixed effects	Y	Y	Y	Y	Y	Y
fall-K score	N	N	Y	Y	Y	Y
individual characteristics	Y	Y	Y	Y	Y	Y
group characteristics	Y	Y	Y	Y	Y	Y
teacher & class chars	Y	Y	Y	Y	Y	Y

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Robust standard errors that cluster on class in brackets. All regressions are weighted by sampling probability. 'Individual characteristics' listed in Table 1, 'group characteristics' are class means of individual characteristics (not including reference child), and 'teacher & class characteristics' are listed in Table 1. Columns 5 & 6 include class preschool, group background characteristics, and teacher and class characteristics from 1st grade. Columns (1) and (2) present estimates of equation (1) using individual level fall-k test scores as the dependent variable. Columns (3) and (4) present estimates of equation (1) using measures of parental investments in place of test scores. Columns (5) and (6) present estimates of equation (1) measuring all covariates in the 1st grade.

Table 3. Test of Teacher Assignment: Effects of Teacher Characteristics on Class Average Fall-K Scores

	1	2	3	4	5	6	7
	Math	Reading	Self-control (teacher)	Interpers- onal skills	External- izing problems	Internal- izing problems	Self- control (parent)
<u>A. Regress fall-K class average outcome on K teacher and class characteristics</u>							
<b>Dependent variable: mean of class scores</b>							
F-test teacher & class characteristics = 0	1.00	1.17	0.78	1.29	0.38	0.73	0.54
Prob > F	0.45	0.29	0.69	0.20	0.98	0.75	0.91
<b>Dependent variable: variance of class scores</b>							
F-test teacher & class characteristics = 0	0.33	1.36	0.64	0.96	0.44	0.71	1.12
Prob > F	0.99	0.17	0.83	0.49	0.96	0.77	0.34
# of classes	2413	2252	2331	2331	2386	2356	2441
<u>B. Regress spring-K class average outcome on 1st grade teacher and class characteristics</u>							
<b>Dependent variable: mean of class scores</b>							
F-test teacher & class characteristics = 0	1.95	1.97	1.22	1.89	1.18	1.61	1.02
Prob > F	0.03	0.02	0.26	0.03	0.29	0.08	0.43
<b>Dependent variable: variance of class scores</b>							
F-test teacher & class characteristics = 0	0.47	0.83	0.86	0.76	0.89	1.42	0.52
Prob > F	0.93	0.62	0.59	0.69	0.55	0.15	0.91
# of classes	2634	2516	2720	2716	2731	2712	2685
<u>C. Regress spring-1st grade class average outcome on 3rd grade teacher and class characteristics</u>							
<b>Dependent variable: mean of class scores</b>							
F-test teacher & class characteristics = 0	1.91	2.40	0.82	0.62	1.27	0.39	1.16
Prob > F	0.05	0.01	0.59	0.79	0.25	0.94	0.32
<b>Dependent variable: variance of class scores</b>							
F-test teacher & class characteristics = 0	0.41	1.27	1.56	1.47	1.29	0.36	0.76
Prob > F	0.93	0.25	0.12	0.15	0.24	0.96	0.66
# of classes	2130	2086	2159	2152	2155	2149	2175

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. All regressions are weighted by sampling probability, include teacher and class characteristics as described in Table 1, and include school fixed effects. F-test includes 9 degrees of freedom.

Table 4. Spillover Effects of Preschool on Spring Kindergarten Cognitive Outcomes

	1	2	3	4	5	6	7	8
	Math				Reading			
<u>A. No school FE, no fall-K score</u>								
individual preschool	1.958 [0.197]***	0.854 [0.167]***	0.847 [0.166]***	0.816 [0.165]***	1.662 [0.202]***	0.736 [0.182]***	0.705 [0.180]***	0.654 [0.178]***
class preschool	3.300 [0.456]***	1.211 [0.327]***	0.992 [0.325]***	0.920 [0.324]***	2.791 [0.455]***	1.289 [0.378]***	1.069 [0.378]***	0.946 [0.372]**
R-squared	0.02	0.30	0.31	0.31	0.02	0.22	0.23	0.24
<u>B. No school FE, fall-K score</u>								
individual preschool	0.013 [0.111]	0.051 [0.112]	0.074 [0.112]	0.063 [0.112]	-0.162 [0.124]	-0.114 [0.126]	-0.102 [0.126]	-0.123 [0.125]
class preschool	0.547 [0.236]**	0.612 [0.234]***	0.695 [0.241]***	0.663 [0.241]***	0.494 [0.265]*	0.631 [0.263]**	0.747 [0.274]***	0.679 [0.274]**
R-squared	0.68	0.69	0.69	0.70	0.62	0.63	0.64	0.64
<u>C. School FE, no fall-K score</u>								
individual preschool	1.207 [0.179]***	0.724 [0.165]***	0.719 [0.165]***	0.698 [0.164]***	1.081 [0.180]***	0.578 [0.170]***	0.557 [0.169]***	0.525 [0.169]***
class preschool	0.751 [0.359]**	0.652 [0.312]**	0.588 [0.312]*	0.549 [0.311]*	0.841 [0.389]**	0.752 [0.343]**	0.597 [0.345]*	0.528 [0.344]
R-squared	0.30	0.41	0.41	0.41	0.29	0.39	0.39	0.40
<u>D. School FE, fall-K score</u>								
individual preschool	0.028 [0.106]	0.003 [0.106]	0.003 [0.106]	-0.002 [0.106]	-0.112 [0.112]	-0.149 [0.113]	-0.149 [0.113]	-0.163 [0.112]
class preschool	0.524 [0.215]**	0.527 [0.213]**	0.517 [0.212]**	0.506 [0.213]**	0.608 [0.247]**	0.599 [0.244]**	0.544 [0.249]**	0.504 [0.248]**
R-squared	0.74	0.75	0.75	0.75	0.71	0.72	0.72	0.72
Observations	individuals=12501, schools=901, classes=2436				individuals=11840, schools=891, classes=2275			
SUR joint-test	$\chi^2(2)=7.99$	$P > \chi^2=0.02$						
<u>Covariates</u>								
individual characteristics	N	Y	Y	Y	N	Y	Y	Y
group characteristics	N	N	Y	Y	N	N	Y	Y
teacher & class chars.	N	N	N	Y	N	N	N	Y

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Robust standard errors that cluster on class in brackets. All regressions are weighted by sampling probability. See notes to Table 2 for description of covariates. All columns contain regression results of equation (1) using spring-K math and reading scores as the dependent variable. Panels B and D include fall-K math and reading scores as independent variables. 'SUR joint test' is a test of joint significance of class preschool on the math and reading equations from columns (4) and (8).

Table 5. Spillover Effects of Preschool on Spring Kindergarten Noncognitive Outcomes

	1 Self-control (teacher)	2 Interpersonal skills	3 Externalizing problems	4 Internalizing problems	5 Self-control (parent)
individual preschool	-0.042 [0.010]***	-0.043 [0.011]***	0.032 [0.010]***	0.019 [0.009]**	-0.007 [0.009]
class preschool	-0.029 [0.024]	-0.038 [0.025]	0.014 [0.021]	0.028 [0.020]	-0.023 [0.018]
R-squared	0.52	0.5	0.6	0.42	0.43
# of individuals	11636	11446	11919	11759	12070
# of schools	881	883	889	887	901
# of classes	2324	2321	2380	2342	2462
SUR joint-test	$\chi^2(5)=5.17$	$P>\chi^2=0.39$			
<u>Covariates</u>					
school fixed effects	Y	Y	Y	Y	Y
fall-K score	Y	Y	Y	Y	Y
individual characteristics	Y	Y	Y	Y	Y
group characteristics	Y	Y	Y	Y	Y
teacher & class chars	Y	Y	Y	Y	Y

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Robust standard errors that cluster on class in brackets. All regressions are weighted by sampling probability. See notes to Table 2 for description of covariates. All columns contain regression results of equation (1) using spring-K non-cognitive measures as the dependent variable. 'SUR joint test' is a test of joint significance of class preschool on all non-cognitive equations in columns (1) through (5).

Table 6. Spillover Effects of Noncognitive Outcomes on Spring Kindergarten Cognitive Outcomes

	1	2	3	4	5	6
		Math			Reading	
class preschool	0.600 [0.239]**	0.679 [0.238]***	0.666 [0.238]***	0.482 [0.279]*	0.613 [0.276]**	0.599 [0.277]**
class self-control	-0.089 [0.312]	0.198 [0.245]	0.095 [0.231]	0.265 [0.338]	0.185 [0.256]	0.246 [0.233]
class interpersonal skills	-0.099 [0.259]	-0.057 [0.216]	0.054 [0.212]	-0.016 [0.290]	0.292 [0.248]	0.330 [0.235]
class externalizing problems	-0.315 [0.232]	-0.526 [0.168]***	-0.555 [0.140]***	0.418 [0.275]	-0.177 [0.208]	-0.091 [0.163]
class internalizing problems	-0.204 [0.230]	0.076 [0.164]	0.060 [0.144]	0.115 [0.277]	-0.115 [0.196]	-0.062 [0.155]
Mean or percentile of noncognitive outcomes	mean	75th	90th	mean	75th	90th
F test noncognitive = 0	0.93	5.28	9.24	0.78	3.45	4.21
Prob > F	0.45	0.00	0.00	0.54	0.01	0.00
# of individuals	11257	11257	11257	10768	10768	10768
# of schools	885	885	885	871	871	871
# of classes	2256	2256	2256	2134	2134	2134
<u>Covariates</u>						
school fixed effects	Y	Y	Y	Y	Y	Y
fall K score	Y	Y	Y	Y	Y	Y
individual characteristics	Y	Y	Y	Y	Y	Y
group characteristics	Y	Y	Y	Y	Y	Y
teacher & class chars	Y	Y	Y	Y	Y	Y

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Robust standard errors that cluster on class in brackets. See notes to Table 2 for description of covariates. All columns contain regression results of equation (1) using spring-K math and reading scores as the dependent variable. 'Self-control' is from the teacher report.

Table 7. Spillover Effects of Preschool on Cognitive Outcomes in 1st and 3rd Grade

	1	2	3	4	5	6	7	8
	Math				Reading			
<u>1st grade</u>								
class preschool	0.660	0.614	0.657	0.679	0.490	0.473	0.368	0.361
	[0.386]*	[0.369]*	[0.372]*	[0.370]*	[0.456]	[0.433]	[0.441]	[0.434]
# of individuals	10084	10084	10084	10084	9530	9530	9530	9530
# of schools	894	894	894	894	884	884	884	884
# of classes	2376	2376	2376	2376	2213	2213	2213	2213
<u>3rd grade</u>								
class preschool	0.514	0.509	0.576	0.602	0.403	0.358	0.407	0.374
	[0.428]	[0.403]	[0.388]	[0.391]	[0.502]	[0.476]	[0.464]	[0.461]
# of individuals	10062	10062	10062	10062	9466	9466	9466	9466
# of schools	894	894	894	894	883	883	883	883
# of classes	2375	2375	2375	2375	2211	2211	2211	2211
<u>Covariates</u>								
school fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
fall K score	Y	Y	Y	Y	Y	Y	Y	Y
individual characteristics	N	Y	Y	Y	N	Y	Y	Y
group characteristics	N	N	Y	Y	N	N	Y	Y
teacher & class chars	N	N	N	Y	N	N	N	Y

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Robust standard errors that cluster on class in brackets. All regressions are weighted by sampling probability. See notes to Table 2 for description of covariates. All columns contain regression results of equation (1) using spring 1st or 3rd grade math and reading scores as the dependent variable.

Table 8. Heterogeneity of Spillover Effects of Preschool on Spring Kindergarten Outcomes

interaction variable:	1 own preschool attendance	2 receive food stamps	3 black	4 immigrant	5 father present	6 male
<u>Math</u>						
class preschool	0.637 [0.326]*	0.363 [0.249]	0.620 [0.236]***	0.521 [0.244]**	0.850 [0.408]**	0.488 [0.275]*
class preschool interaction	-0.224 [0.408]	0.649 [0.516]	-0.694 [0.604]	-0.067 [0.513]	-0.459 [0.453]	0.036 [0.374]
<u>Reading</u>						
class preschool	0.168 [0.382]	0.492 [0.285]*	0.525 [0.281]*	0.454 [0.270]*	0.127 [0.467]	0.455 [0.326]
class preschool interaction	0.557 [0.441]	0.057 [0.604]	-0.116 [0.653]	0.367 [0.601]	0.508 [0.529]	0.097 [0.417]
school fixed effects	Y	Y	Y	Y	Y	Y
fall K score	Y	Y	Y	Y	Y	Y
individual characteristics	Y	Y	Y	Y	Y	Y
group characteristics	Y	Y	Y	Y	Y	Y
teacher & class characteristics	Y	Y	Y	Y	Y	Y

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Robust standard errors that cluster on class in brackets. All regressions are weighted by sampling probability. See notes to Table 2 for description of covariates.