Mechanisms of parental spillovers in the classroom*

Jane Cooley Fruehwirth†

Preliminary and Incomplete

June 16, 2014

Abstract

Despite increasing evidence that spillovers from peer characteristics, such as race, parental resources, and parental education, matter, the source of these spillovers remains a puzzle. I find, consistent with previous research, that peer parental education matters for student achievement, using plausibly random assignment to classrooms within schools. I then consider the source of this spillover, distinguishing between several possible channels that are correlated with parental education: the initial stock of human capital, human capital accumulated throughout the school year, parental involvement in the classroom, and effects on teacher inputs. I find support for direct effects of peer parental classroom involvement and effects on teaching practices in reading, and evidence that peer accumulated human capital can explain spillovers in math.

*I thank Cath Sleeman for excellent research assistance and the Marshall Economic Research Group for their help. I am also grateful for financial support from the British Academy and the Leverhulme Trust's Philip Leverhulme Prize. The paper benefitted from discussions with Anna Vignoles and seminar participants at Cambridge's Faculty of Education, the London School of Economics, the Tinbergen Institute and Ludwig Maximillien University.

†University of Cambridge, Christ's College, INET; Address: Faculty of Economics, Sidgwick Ave, Cambridge, UK CB39DD; jc738@cam.ac.uk.

1 Introduction

A growing literature finds evidence of an effect of peers on children's outcomes (See Sacerdote, 2011; Epple and Romano, 2010; Brock and Durlauf, 2001, for recent reviews). In the school setting, studies show evidence of spillovers from peer gender, race, ability and behavior. Some empirical models also show effects of peer parental characteristics, most commonly restricted to include peer parental education and income (often proxied by free/reduced price lunch status). The finding that parental characteristics of classroom peers affects achievement is puzzling because the parents are generally not present in the classroom and therefore cannot directly affect their child's classmates. In this paper, I consider whether parental education creates spillovers for classroom peers and the source of these spillovers.

Many types of parental inputs, such as parental involvement with the classroom or teacher and home investments, are correlated with parental education, and suggest possible channels of peer parental influences. Understanding the mechanisms through which parental inputs create spillovers can help inform policy. While it may be difficult to affect the education of parents directly, schools may be able to encourage the types of parental inputs that create positive spillovers for the classroom.

There are several explanations for why peer parental education might matter. First, more highly educated parents might be more involved in the school, participating in fundraising or volunteering in the classroom. Second, more highly educated parents may monitor teacher performance and affect teacher effort in a way that benefits (or detracts from) the learning of other students in the classroom. Third, peer parental education may affect student performance indirectly through student effort, behavior or general readiness for school throughout the year, as suggested in Avvisati et al. (2014). For instance, if more-educated parents do a better job of motivating their child to perform well at school, this could contribute to a better learning environment for all students. Likewise, children of better-educated parents may be better prepared for class or less likely to disrupt class, again creating the potential of positive spillovers. Finally, it may be that peer parental education proxies for peer human capital at the start of the school year. Better-educated parents may have children who are more able or better prepared for school, and it is the initial stock of human capital (or ability) of these children which creates positive spillovers in the classroom.

I test these hypotheses using the Early Childhood Longitudinal Survey of Kindergarteners, a nationally representative sample of kindergarteners in the US. I focus on kindergarten outcomes, as the assumption of random assignment to classrooms is most plausible at the start of the child's academic career and because of the larger number of classmate observations. The ECLS-K is well-equipped for distinguishing between these different potential mechanisms of peer influnce because of the rich survey data on both parental and teacher inputs, as well as both cognitive and behavioral measures of students abilities at the begininning and the end of the school year. However, a disadvantage is that not all students were sampled within the school leading to measurement error in peer variables. I apply Sojourner (2013)'s method to deal with missing observations of classroom peers, which involves weighting peer measures by the percentage of peers who are observed in the classroom.

A key challenge for identifying an effect of peers is nonrandom assignment of students to classrooms. In this case, an apparent effect of peer parental education could simply be a result of positive selection, for instance, if the presence of better-educated parents signals better teacher quality in dimensions that are difficult to measure. I eliminate the more salient selection concerns by controlling for school fixed effects, consistent with a large number of papers in the literature, including Hoxby (2000), Lavy et al. (2012), Hanushek et al. (2009). I also test directly for evidence of non-random assignment within schools, and find that results are robust to excluding schools that fail to pass the test for random assignment based on observables. Neidell and Waldfogel (2010) also find support for the assumption of random assignment to classroomms in kindergarten, but that it does not extend to first grade.

This paper contributes to a burgeoning literature on peer effects. Often previous papers have focused on distinguishing between spillovers from peer characteristics and peer ability measured through achievement (Hanushek et al., 2009; Lavy et al., 2012; Vigdor and Nechyba, 2007). This paper expands upon the candidate mechanisms, focusing particularly on the role of peer parental education, and developing new testable implications. In this, it is closer in spirit to work by Lavy and Schlosser (2011) which consider peer effects from gender composition and potential mechanisms, bringing insight from survey data. Also, Bifulco et al. (2011) find that peer mother's education affects high school drop out rates and college attendance, and rule out several possible mechanisms for this, such as academic success. It is also similar in spirit to work by Hoxby and Weingarth (2005), De Giorgi and Pellizzari (2011) and Fruehwirth (2013), which attempt to distinguish between different potential mechanisms underlying peer effects.

In what follows, I first develop in Section 2 different hypotheses related to potential channels of parental spillovers. In Section 3, I discuss the data and describe what measures are correlated with parental education. Section 4 discusses the estimation of the total effect of peer parental education, how I account for measurement error in peer groups and possible non-random assignment, and basic findings. Section 5 then tests the different hypotheses regarding the sources of these spillovers. Section 6 concludes.

2 Mechanisms of Spillovers

In what follows, I describe several possible channels through which peer parental education might create spillovers in the classroom. Consider a classroom with students $i = 1, ..., N$. Students are characterized by their initial stock of human capital at the start of kindergarten (or ability), a_{i0}, and the education of their parents x_i. Let a_i denote a general measure of human capital, motivation and/or effort, which is increasing in initial human capital (at the start of kindergarten) and parental education, i.e.,

$$a_i = g(x_i, a_{i0}).$$

Parental education affects the child input through their home environment and other parental involvement with the child.

Achievement is defined as

$$Y_i = f(a_i, \bar{a}_{-i}, t_i) + \epsilon_i, \tag{1}$$

where t_i denotes teacher effort and ϵ_i captures measurement error in test scores.

Hypothesis 1: Prior Ability. Suppose peer parental education only affects a child's achievement through peer initial human capital. In this case, equation (1) becomes

$$Y_i = f(a_i, \bar{a}_{-i0}, t_i) + \epsilon_i,$$

and $\partial Y_i/\partial \bar{x}_{-i} \geq 0$ is explained by the fact that ability is increasing in parental education and $\partial Y_i/\partial \bar{a}_{-i0} \geq 0$. In this case, conditioning on peer ability at the start of kindergarten would remove any effect of peer parental education. We can even expand this to a setting where ability is multidimensional and includes non-cognitive ability measures, which have been shown to be important (e.g., Cunha et al., 2010).

Hypothesis 2: Proportional Effect. Suppose peer parental education only affects a child's achievement through its effect on $\bar{a}_{-i}$, peer effort or peer preparedness for school throughout the year. In this case, we have that

$$\partial Y_i/\partial x_i = \partial Y_i/\partial a_i \partial a_i/\partial x_i, \text{ and}$$

$$\partial Y_i/\partial x_j = (1/(N-1))\partial Y_i/\partial a_j \partial a_j/\partial x_j,\ j \neq i.$$

We have for each element of x_i, x_{ik} and $x_{ik'}$ for $k \neq k'$ that

$$\frac{\partial Y_i/\partial x_{ik}}{\partial Y_i/\partial x_{jk}} = \frac{\partial Y_i/\partial a_i}{\frac{1}{N-1}\partial Y_i/\partial a_j} \frac{\partial a_i/\partial x_{ik}}{\partial a_j/\partial x_{jk}} = \frac{\partial Y_i/\partial a_i}{\frac{1}{N-1}\partial Y_i/\partial a_j} = \frac{\partial Y_i/\partial x_{ik'}}{\partial Y_i/\partial x_{jk'}}.$$

This hypothesis suggests the intuitive property that if achievement is increasing in a_j of peers and own parental education, that the effect of own and peer parental education should be proportional across different dimensions of parental education. This would also apply to other x's that are assumed to only create spillovers through peer ability. A model where there is also simultaneity (or only simultaneity), so that $a_i = g(x_i, \bar{a}_{-i}, a_{i0})$ would produce the same testable implication if there is only 1 peer or in a linear-in-means framework. This is developed further in Section 5.2 for the linear-in-means model.

Hypothesis 3: Teacher Effort. Suppose peer parental education affects outcomes only through effects on teacher effort. Consider a setting where parents put pressure on teachers, and teacher pressure is increasing in parental education. Let teacher effort, t_i be a function of own and peer parental education, so that $t_i = h(x_i, \bar{x}_{-i})$. Suppose parents encourage teacher effort in a way that benefits all students, such as encouraging the teacher to put more time into academic instruction relative to other activities. In this case, $\partial t_i/\partial \bar{x}_{-i} \geq 0$ and $\partial Y_i/\partial \bar{x}_{-i} = \partial Y_i/\partial t_i \partial t_i/\partial \bar{x}_{-i} \geq 0$. I also test this by considering observable measures of teacher effort.

On the other hand, suppose teacher time is limited and exclusive, in the sense that effort put toward one student detracts from effort put toward another student. Suppose also that time allocation depends at least in part on parental effort (which again is assumed to be increasing in parental education). In this case, $\partial t_i/\partial \bar{x}_{-i} \leq 0$ and (conditional on $a_i, \bar{a}_{-i}$) $\partial Y_i/\partial \bar{x}_{-i} = \partial Y_i/\partial t_i \partial t_i/\partial \bar{x}_{-i} \leq 0$. I also text this by seeing whether teachers's reported availability to provide extra assistance to students who are falling behind in reading varies with the peer composition.

Alternatively, parents could shift teacher focus or organization for instruction in ways that affect the functioning of the classroom. For instance, if parents with a university degree encourage the practice of ability grouping in the classroom, this could benefit their children at the expense of children in lower-ability groups. Furthermore, having more parents with a university degree could mean that teachers allocate more time to small group instruction rather than whole class instruction. These hypotheses lead to several testable implications. First, I test whether observable teacher inputs, such as the use of ability grouping, teacher effort, and percent of time in whole class instruction, vary with peer parental education, i.e., $\partial t_i/\partial \bar{x}_{-i} \gtreqless 0$. Second, I test whether the input affects outcomes $\partial Y_i/\partial t_i \neq 0$. Third, I test whether the effectiveness varies by the composition of the classroom, suggesting a motivation for why the teacher might vary their practice with the parental education of peers ($\partial^2 Y_i/\partial t_i \partial \bar{x}_{-i} \gtrless 0$). Finally, I test whether the effectiveness varies by the type of student ($\partial^2 Y_i/\partial t_i \partial \bar{x}_{-i} \gtrless 0$). For instance, a parent with a university degree might advocate for ability grouping if it benefits their child, regardless of the effect on other children in the classroom. In principle, this could suggest that students benefit more from having additional peers whose parents have the same education as their own parents, a hypothesis I can also test.

Hypothesis 4: Direct effect. Suppose parents have a direct effect in the classroom through parental involvement, p_i. In this case,

$$Y_i = f(a_i, \bar{a}_{-i}, p_i, \bar{p}_{-i}, t_i) + \epsilon_i. \tag{2}$$

If $\partial Y_i/\partial \bar{p}_{-i} \geq 0$ and parental involvement is increasing in parental education, we have that $\partial Y_i/\partial \bar{x}_{-i} \geq 0$ even if we hold $\bar{a}_{-i}$ fixed. Furthermore, if this is the only channel for spillovers, we would expect that after conditioning on $\bar{p}_{-i}$, $\partial Y_i/\partial \bar{x}_{-i} = 0$. I test this using parent and teacher reports of volunteering in school.

3 Data

The Early Childhood Longitudinal Survey of Kindergarteners takes a nationally representative sample of kindergarteners in 1998/99 school year and follows them to grade 8. Students were sampled randomly within schools with the aim of sampling about 20 students per school. They then collected a rich set of information from the child's parents and teachers. I focus on the first year of the survey, when children were in kindergarten and there was a releatively large number of student observations per classroom, about 7.5 on average. The teachers also report class size, which I use to correct for measurement error in peers, as discussed in Section 4.2.[1]

I focus primarily on cognitive outcomes for comparison to the literature, but also consider non-cognitive outcomes. The data include direct assessments of reading and math performance and teacher assessments of noncognitive abilities, including approaches to learning, self control, interpersonal skills, externalizing problems, internalizing problems, measured both in the fall and the spring.[2] I focus on externalizing problems, involving disruptive behaviors, which the literature has highlighted as an important mechanism of peer spillovers(e.g. Figlio, 2007; Lazear, 2001).

The parent survey includes information on the highest level of education received by both the father and mother. I create a dummy for whether the parent had at least some education post high school but less than a university degree and then a dummy for having at least a university degree.[3] The parent respondent, generally the mother, also reports whether there is a father resident in the home both in the fall and spring interviews. I code a father as being resident if he is reported as resident in either of the surveys. To deal with missing observations of father's education, I set father's education to be 0 in homes where father's education is missing and there is no father resident and control for whether the father is resident in regressions.[4]

From the teacher survey, I have detailed information on the teacher's back-

[1]Note that class sizes with numbers less than 10 were encoded as 10 and those greater than 30 were encoded as 30 for confidentiality reasons. However, I also sum teacher reports of the number of boys and the number of girls in the class, subtracting the number of students who left and adding the number of students who came to construct an alternative measure of class size. I use this instead for classes that are measured as size 10 or 30, and the alternative measure gives a number that is smaller than 10 (for the case of size 10 classrooms) or larger than 30 (for the case of size 30 classrooms). Out of concern about outliers, I set to missing observations below the 1st percentile (8) and above the 99th percentile (47).

[2]Descriptions of these measures as taken from the ECLS-K User's Guide can be found in the appendix.

[3]I use the composite measures, some of which are imputed using hotdeck imputation to deal with missing values.

[4]There are only about 160 observations where a father's education is reported and there is no father resident.

ground, including gender and race. I measure teacher experience as the number of years the teacher taught any age group. Teacher tenure captures the number of years at the current school. I create several indicators of the type of certification that teacher has: regular, temporary, alternative, and the highest level available. Similarly to Neidell and Waldfogel (2010), I create an indicator of whether the teacher enjoys teaching, which is measured by whether they agreed with the following statements: "I really enjoy my present teaching job", "I am certain that I make a difference in the lives of the children I teach" and "If I could start over, I would choose teaching again as my career." These are measured in the fall of the year, which helps limit endogeneity concerns.

The data also include less common measures of teacher inputs, such as the amount of paid and unpaid preparatory work they put into their classes, whether they discuss curriculum and lesson plans with other teachers, and teacher training. There is also detailed information on how teachers allocate classroom time among different subjects and how they group students for instruction. These variables are useful for helping to inform whether teacher inputs vary systematically with the composition of the classroom.

I identify students' classrooms both using the teacher IDs and whether they were in a full day, morning or afternoon kindergarten class, to account for the fact that some teachers taught 2 half-day classes with different sets of students. To construct peer variables, I use the typical measure of the average of peers observed in the classroom, excluding own characteristics.

The potential sample of students starts at 21,409, but I lose a considerable number because of missing observations. I drop 1546 who are missing either teacher or school identifiers. I drop an additional 1040 who do not report whether they are in a full day or half-day class, which is necessary (in conjunction with the teacher identifiers) for measuring peer groups. I drop students who changed teachers over the course of the survey (686). I also drop students who are missing spring or fall outcomes in math or reading (3031). I then drop students who are missing observations of actual class size (1344), which I need to correct for measurement error in peer measures, and 730 students who are missing measures of mother's education. I then drop students who are the only person observed in the classroom after these restrictions, leaving a potential sample of 12,715.

Table 1 presents summary statistics by mother's education. The average test scores of students are increasing in mother's education. Likewise, non-cognitive outcomes that are conducive to learning are increasing with mother's education, whereas those that are not are decreasing with mother's education. Father's education is also increasing in mother's education, as well as the percentage of father's that are present in the household. The last column presents p-values, which show statistically significant differences across columns. Of families where father's are resident, about 60% of mothers and fathers have the same level of education, 19% of fathers have more education than the mothers, and 21% of mothers have more education than fathers. This variation is useful in distinguising an effect of father's and mother's education.

Interestingly, the ranking for teacher characteristics by mother's education is less clear. For instance, teachers with the highest level of certification are more likely to teach children whose mothers have only a high school degree or less and similarly for teacher tenure. In contrast, teacher experience and whether the teacher enjoys teaching is higher for children whose mothers have more education.

The last part of the table includes variables that are useful in testing the different hypotheses regarding the source of the peer spillovers. For instance, the number of times the parent volunteers in the school over the course of the year increases markedly on average with education, from 2.64 for mothers with a high school degree or less to 7.01 for mothers with a university degree or more. Whether the teacher provides extra assistance to students in the classroom who are struggling in reading 3 or 4 times a week to daily is decreasing across mother's education. The last 2 rows show that mothers with less education are more likely to have their children in classes where teachers use ability grouping on occasion.

Table 1: Mean Characteristics by Mother's Education

		<=HS	>HS	Univ+	P-Value
Student	Math	48.30	51.92	56.25	0.00
	Reading	48.06	51.50	55.64	0.00
	Fall Math	48.09	51.84	56.65	0.00
	Fall Reading	47.35	51.09	56.16	0.00
	Fall SRS approach learn	2.90	3.01	3.16	0.00
	Fall Externalizing	1.65	1.63	1.55	0.00
	Fall Internalizing	1.56	1.52	1.48	0.00
	Fall Self Control	3.04	3.09	3.18	0.00
	Fall Interpersonal	2.92	3.00	3.09	0.00
Parent	Dad HS+	0.16	0.33	0.18	0.00
	Dad University+	0.06	0.19	0.65	0.00
	Dad Present	0.73	0.80	0.92	0.00
Teacher	Enjoy Teaching	0.82	0.83	0.85	0.00
	Female	0.98	0.99	0.98	0.16
	White	0.88	0.91	0.95	0.00
	Experience	14.28	14.68	14.68	0.06
	Tenure	9.64	9.59	9.29	0.12
	Temp Certification	0.09	0.09	0.08	0.04
	Alt Certification	0.01	0.01	0.02	0.05
	Reg Certification	0.23	0.23	0.24	0.44
	Highest Certification	0.66	0.63	0.63	0.01
Other	Times parent volunteers	2.64	4.75	7.01	0.00
	Teacher extra assistance	0.54	0.52	0.51	0.01
	Never group by ability (read)	0.36	0.39	0.41	0.00
	Never group by ability (math)	0.49	0.53	0.55	0.00
	N	5,155.00	4,323.00	3,237.00	

Reported p-values test whether means are significantly different across the 3 categories of mothers' education.

4 Total Effect

Let Y_{ics} denote the outcome of a child in class c and school s, X_i parental education, and $\bar{X}_{-ics}$, the average parental education of peers, excluding i. Achievement is determined according to

$$Y_{ics} = \beta_0 + X_i\beta_X + \bar{X}_{-ics}\beta_{\bar{X}} + \epsilon_{ics}, \tag{3}$$

where ϵ_{ics} denotes the residual. I ignore observable teacher inputs for the moment to simplify notation, but include them in various specification below. The parameter of interest is the effect of peer parental education, $\beta_{\bar{X}}$, not conditioning on other parental/child characteristics (characterized as the *social effect* by Manski (1993)).

4.1 Nonrandom Assignment

A central challenge in identifying the effect of peer parental education is that parents may select into schools or classrooms, thus introducing correlation between $\bar{X}_{-ics}$ and the residual, ϵ_{ics}. For instance, if better-educated parents select better teachers, it may appear that peer parental education matters when in reality it proxies for teachers that are better quality in dimensions that cannot be easily measured. Likewise, if more able students are grouped together, peer parental education may appear to matter simply as a proxy for own innate ability.

Suppose we decompose the residual into the student's unobservable ability (which may be correlated with both own and peer parental education) and unobserved school quality, so that $\epsilon_{ics} = A_{i0} + \alpha_s + \nu_{ics}$. Consistent with most of the literature, I control for prior ability of students through the inclusion of prior achievement, estimating a value-added model. I also control for school fixed effects, an approach often used in the literature to control for selection into schools (e.g. Lavy et al., 2012; Hoxby, 2000; Hanushek et al., 2009). This relies on the assumption that assignment within schools is random.

As this is the child's first year of school, it is more likely that students are randomly assigned to classes within schools, as also shown in Neidell and Waldfogel (2010). However, there still may be concern about selection into classrooms within schools. In Section 4.4, I test whether schools appear to randomly assign students to classrooms based on parental education and show robustness of our findings to restricting the analysis to this subset of schools.

4.2 Measurement Error in Peers

A remaining concern is that I only observe a percentage of peers in the classroom. To the extent that this creates random measurement error, it will bias estimates of the effect of peer parental education toward 0. I follow the approach in Sojourner (2013) to correct for measurement error in observed peers. Let p_c denote the percentage of classroom peers who are observed.[5]

Let $\bar{X}^m_{-ics}$ denote the average peer characteristics for the subset of peers that is missing and $\bar{X}^o_{-ics}$ the average peer characteristics for the observed students. Average peer characteristics is then $\bar{X}_{-ics} = p_c\bar{X}^o_{-ics} + (1-p_c)\bar{X}^m_{-ics}$. Let $d_i = 1$ indicate that the student is observed. Then,

$$\begin{aligned} E(Y_{ics}|X_i, \bar{X}^o_{-ics}, Y_{i0}, p_c, s, d_i = 1) = \beta_0 + X_i\beta_X + p_c\bar{X}^o_{-ics}\beta_{\bar{X}} + Y_{i0}\beta_Y \\ + (1-p_c)E(\bar{X}^m_{-ics}|X_i, \bar{X}^o_{-ics}, Y_{i0}, p_c, s, d_i = 1)\beta_{\bar{X}} \\ + E(\tilde{\epsilon}_{ics}|X_i, \bar{X}^o_{-ics}, Y_{i0}, p_c, s, d_i = 1), \quad (4) \end{aligned}$$

where $\tilde{\epsilon}_{ics} = \alpha_s + \nu_{ics}$. Thus, the unobservability of a subset of peer characteristics adds an extra term to the residual. Suppose that

$$E(\tilde{\epsilon}|X, \bar{X}^o, Y_0, p, s, d = 1) = E(\tilde{\epsilon}|s, d = 1)$$

and

$$E(\bar{X}^m|X, \bar{X}^o, Y_0, p, s, d = 1) = E(X^m|s).$$

As discussed in Sojourner (2013), the first assumption holds if there is random

[5] To simplify exposition, I assume a balanced panel. As I add covariates, I lose observations and need to adjust p_c accordingly.

assignment to classrooms within schools. The second holds if students are missing in similar ways across classrooms, in which case controlling for school fixed effects helps adjust for these missing observations. A sufficient, but not necessary, condition for this is that students are missing at random. However, this condition is also satisfied if for instance low-achieving students are more likely to be missing due to survey non-response, but they are randomly assigned to classrooms within schools and are thus missing at random across classrooms. Given these assumptions, Sojourner (2013) shows that missing data can be controlled by including school fixed effects and allowing these effects to vary by the portion of students who are observed in the classroom p_c. He further shows through Monte Carlos that results are robust to just controlling for school fixed effects and p_c, the approach I take here. The final estimating equation is estimated only on students whose whole set of covariates are observed ($d_i = 1$) as follows:

$$Y_{ics} = \beta_0 + X_i\beta_X + p_c\bar{X}^o_{-ics}\beta_{\bar{X}} + Y_{i0}\beta_Y + p_c\beta_p + \alpha_s + \nu_{ics}.$$

4.3 Results: Total Effect

I begin by estimating an effect of peer mothers' education on children's outcomes in Table 2 using the standard estimator that does not correct for measurment error due to missing peers; the top panel considers math and the bottom panel reading outcomes. Column (1) is a basic OLS regression. I find that peer mothers' education is highly correlated with outcomes, 3.13 and 2.63 in math and reading respectively for peer mothers' having more than a high school degree but less than university degree and 6.06 and 5.33 for peer mothers' having a university degree or more respectively.

In column (2), I include school fixed effects to control both for selection into schools based on unobservable school quality and for unobserved contemporaneous school inputs, which may be correlated with peer mothers' education. The estimated peer effect drops by about a third in both math and reading. I then control for prior achievement in column (3) (without school fixed effects), as an alternative control for selection. Peer effect estimates drop still further from 1.12 to 0.57 in math and 0.99 to 0.72 in reading for peer mothers' with more than a high school degree and 1.62 to 0.52 in math and 1.04 to 0.32 in reading for peer mothers' having a university degree or more.

Column (4) includes both school fixed effects and prior achievement. Estimated effects of peer mothers' education remain fairly stable, suggesting that controlling for prior achievement addresses much of the selection concern.

To help deal with unobservables at the class level, Column (5) adds detailed controls for teacher quality (as listed in Table 1). The marginal effect of peer mothers with more than a high school education remains stable at 0.49 in math and decreases slightly in reading to 0.43. The marginal effect of peer mothers' with a university degree increases slightly in math from 0.72 to 0.85 and remains insignificant in reading at 0.33.

In column (6), I estimate the effect of peer mothers on the subsample of observations that has observations of teacher characteristics but including only school fixed effects and prior achievement. I find that estimated peer effects are almost identical. Teacher characteristis are jointly significant in math (p-value of 0.0003) but not in reading (p-value of 0.68). However, because results are robust to not including teacher characteristics and because I lose 1000 observations when teacher characteristics are included, I do not include teacher controls in the primary specifications.

In Table 3, I re-estimate columns (4) and (5) of Table 2, but applying Sojourner (2013)'s method to correct for measurement error, as described in Section 4.2. Peer variables are interacted with the percent of peers in the classroom that are observed. Columns (1) and (3) present results for reading and math controlling for school fixed effects. Columns (2) and (4) add in controls for teacher quality.

The top panel shows that the estimated effects of peer mothers' education are qualitatively similar to those that do not correct for measurement error, but quantitatively much larger in magnitude. Comparing column (1) to the results in Table 2, column (4), the effect of peer mothers with more than a high school degree is 2.78 in math compared to 0.45, and the effect of peer mothers with a university degree is 3.80 compared to 0.72 by previous estimates. In reading

Table 2: Total Effect of Peer Mother's Education (Not correcting for missing)

	(1)	(2)	(3)	(4)	(5)	(6)
			Math			
Mom high school+	2.91***	2.34***	0.48***	0.48***	0.50***	0.50***
	(0.19)	(0.19)	(0.12)	(0.12)	(0.12)	(0.12)
Mom university+	6.22***	5.07***	0.87***	0.99***	1.05***	1.04***
	(0.22)	(0.23)	(0.13)	(0.14)	(0.15)	(0.15)
Peer mom HS+	3.13***	1.12***	0.57**	0.45**	0.49**	0.49**
	(0.40)	(0.37)	(0.23)	(0.22)	(0.23)	(0.23)
Peer mom univ+	6.06***	1.62***	0.52**	0.72***	0.85***	0.81***
	(0.39)	(0.47)	(0.23)	(0.27)	(0.29)	(0.29)
R Squared	0.13	0.30	0.69	0.74	0.74	0.74
			Reading			
Mom high school +	2.88***	2.42***	0.53***	0.53***	0.48***	0.48***
	(0.20)	(0.19)	(0.13)	(0.12)	(0.13)	(0.13)
Mom university+	6.14***	5.04***	0.78***	0.89***	0.86***	0.86***
	(0.22)	(0.24)	(0.14)	(0.15)	(0.15)	(0.15)
Peer mom HS+	2.63***	0.99**	0.72***	0.57**	0.43*	0.43*
	(0.44)	(0.40)	(0.28)	(0.25)	(0.26)	(0.26)
Peer mom univ+	5.33***	1.04**	0.32	0.44	0.33	0.31
	(0.43)	(0.50)	(0.27)	(0.31)	(0.33)	(0.33)
R Squared	0.11	0.31	0.64	0.72	0.72	0.72
Prior Achievement	N	N	Y	Y	Y	Y
Teacher Chars.	N	N	N	N	Y	N
School FE	N	Y	N	Y	Y	Y
Observations	12715	12715	12715	12715	11713	11713

Notes: i) Standard errors clustered at class level; ii) * $p<0.10$, ** $p<0.05$, *** $p<0.01$. All regressions include controls for class size. Teacher controls include all those listed in Table 1. Column (6) is the same as Column (4), but estimated on the subsample with non-missing teacher controls.

Table 3: Total Effect of Peer Parental Education: Mom/Dad Separately

	(1)	(2)	(3)	(4)
	Mom Only			
	Math		Reading	
Mom high school+	0.50***	0.53***	0.54***	0.50***
	(0.12)	(0.12)	(0.12)	(0.13)
Mom university+	1.02***	1.10***	0.88***	0.86***
	(0.14)	(0.15)	(0.15)	(0.15)
Peer mom HS+	2.78***	3.12***	2.68***	2.28**
	(0.84)	(0.87)	(1.01)	(1.02)
Peer mom univ+	3.80***	4.74***	1.28	1.20
	(1.09)	(1.11)	(1.22)	(1.26)
N	12715	11713	12715	11713
R Squared	0.74	0.74	0.72	0.72
	Dad Only			
Dad high school+	0.20	0.21	0.42***	0.43***
	(0.13)	(0.14)	(0.15)	(0.15)
Dad university+	0.78***	0.83***	0.76***	0.76***
	(0.14)	(0.15)	(0.15)	(0.16)
Peer dad HS+	0.61	0.70	3.37***	3.52***
	(1.12)	(1.13)	(1.25)	(1.27)
Peer dad univ+	1.81*	2.26**	1.09	1.15
	(1.00)	(1.05)	(1.21)	(1.27)
Resident father	0.22	0.15	0.47***	0.36**
	(0.14)	(0.15)	(0.16)	(0.16)
Peer dad resident	0.20	-0.44	0.78	-0.47
	(1.23)	(1.28)	(1.45)	(1.43)
N	12384	11399	12384	11399
R Squared	0.74	0.74	0.72	0.72
Teacher Characteristics	N	Y	N	Y

Notes: i) Standard errors clustered at class level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for class size and p_c (percentage of students observed in the class), school fixed effects and prior achievement. Peer variables are interacted with p_c to deal with missing values. Teacher controls include all those listed in Table 1.

(column (3)), the estimated effect of peer mothers with more than a high school degree is 2.68 compared to 0.57 when not corrected for measurement error, and the effect of peer mothers' with a university degree remains insignificant in both settings. Results are similar after controlling for teacher characteristics. The only other papers I am aware of that have dealt with measurement error in peer effects, Ammermueller and Pischke (2009) and Neidell and Waldfogel (2010), also found sizable adjustments in the estimated magnitudes after correcting for the missing observations of a subset of peers.[6]

In the bottom panel of Table 3, I estimate the peer effects from fathers' education. I drop classes where there are no students with resident fathers and control for whether a father is resident and the percent of peers with resident fathers. Recall that father's education is 0 if no education is reported and there is no resident father. I also drop the few observations of schools that only have 1 observation after these restrictions.

I find that peer fathers having more than a university degree matters in math, ranging from 1.81 without teacher controls to 2.26 with teacher controls, and smaller than the effect of peer mothers having a university degree or more (3.80 to 4.74). There is no evidence of an effect from peer fathers having more than a high school degree. However, in reading peer fathers with more than a high school degree is larger than the effect of peer mothers having more than a high school degree, with estimates ranging from 3.37 without teacher controls to 3.52 with teacher controls (compared to 2.68 to 2.28 for mothers). Like in the case of mother's education, spillovers from peer fathers having at least a university degree are not statistically significantly different from 0 in reading. While there is a positive effect of the father being resident on reading scores, there is no evidence of spillovers from the percentage of peers whose fathers' are residents in either reading or math.

One concern with the above specification is that mother's and father's education are highly correlated. Estimating the effect of peer mothers' education without controlling for peer fathers' education could lead us to overstate the

[6]These papers use an ex post method to correct for measurement error, and only work when peers are missing at random.

Table 4: Total Effect of Peer Parental Education: Mom and Dad

	(1)	(2)	(3)	(4)
	Math		Reading	
Dad high school+	0.04	0.05	0.28*	0.30*
	(0.13)	(0.14)	(0.15)	(0.16)
Dad university+	0.46***	0.48***	0.53***	0.53***
	(0.15)	(0.16)	(0.17)	(0.17)
Peer dad HS+	-0.43	-0.51	2.57**	2.85**
	(1.13)	(1.13)	(1.30)	(1.30)
Peer dad univ+	-0.02	0.05	0.61	0.61
	(1.23)	(1.27)	(1.44)	(1.48)
Mom high school+	0.45***	0.48***	0.43***	0.38***
	(0.12)	(0.12)	(0.13)	(0.13)
Mom university+	0.82***	0.89***	0.62***	0.62***
	(0.15)	(0.16)	(0.17)	(0.17)
Peer mom HS+	3.06***	3.52***	2.47**	2.03*
	(0.91)	(0.93)	(1.06)	(1.07)
Peer mom univ+	3.97***	5.02***	1.20	1.38
	(1.38)	(1.38)	(1.48)	(1.50)
Resident father	0.27*	0.20	0.52***	0.40**
	(0.14)	(0.15)	(0.16)	(0.16)
Peer dad resident	0.56	-0.03	1.12	-0.18
	(1.24)	(1.28)	(1.46)	(1.45)
N	12384	11399	12384	11399
R-squared	0.74	0.74	0.72	0.72
Teacher Characteristics	N	Y	N	Y

Notes: i) Standard errors clustered at class level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for class size and p_c (percentage of students observed in the class), prior achievement and school fixed effects. Peer variables are interacted with p_c to deal with missing values. Teacher controls include all those listed in Table 1.

effects of peer mothers' education to the extent there is assortative mating (70 percent of our sample of mothers and fathers have the same education). At the same time, controlling for both mother's and father's education could be problematic again because of the high collinearity, increasing standard errors. In Table 4, we estimate the effects of peer mothers and fathers' education in the same regression as a point of comparison.

Estimated effects are comparable to the estimates in Table 3. For instance, the effect of peer mothers having at least a university degree ranges from 3.97 to 5.02 in math, compared to 3.80 to 4.74 when peer fathers' education is not controlled. There is no effect in reading in either case. The effect of peer mothers with more than a high school degree ranges from 3.06 to 3.52 in math when father's education is controlled, compared to 2.78 to 3.12 when father's education is not controlled and for reading 2.03 to 2.47 compared to 2.28 to 2.68 when father's education is not controlled. In reading, we continue to find spillovers from peer fathers having more than a high school degree, ranging from 2.57 to 2.85, smaller than the estimated effects when peer mother's education is not controlled, 3.37 to 3.52. The biggest difference across the results is that we find no effect of peer fathers having at least a university degree in math once peer mothers' education is controlled.

In what follows, I focus primarily on the role of peer mothers' education because it is significant in determining both reading and math. I also have more observations when I ignore fathers and standard errors are smaller.

To understand effect sizes, recall that the test scores are standardized to have mean 50 and standard deviation 10. Unfortunately, I cannot use the common metric of the size of a 1 standard deviation increase, as I do not know the correct standard deviation given missing peer observations. Consider the preferred estimates from columns (1) and (3) of Table 3. The average class has 0.34 of observed peers with mothers with more than a high school degree but less than a university degree. Doubling this would increase achievement by 0.09 of a standard deviation in math and reading. The average class has 0.25 of observed peers with mothers with at least a university degree. Doubling this would increase achievement by 0.10 of a standard deviation in math and

0.03 in reading (though the latter is not statistically significantly different from 0). Alternatively, these estimates would suggest (with an average class size of 22) that adding 1 more mother with more than a high school degree to the classroom would raise achievement by 0.01 of a standard deviation in math and reading. Note that as one would expect, these estimates are smaller than the direct response to the child's own mother having more than a high school degree, 0.50 in math and 0.54 in reading.

I also consider whether peer parental education affects non-cognitive behaviors, measured through teachers surveys, using the same specifications described above. In this case, I find not evidence of spillovers from peer parental education, so I do not focus on these alternative outcomes.

4.4 Robustness

A key concern with the above identification strategy is whether there is selection into classes within schools. I use Fisher's exact test to look for evidence of selection based on parental education. I consider 2 versions of the test: one that looks at mother's and father's education in isolation (based on the categorization of education into 3 levels) and a second version that takes into account both parents' education. The latter categorizes students into 7 categories, parents having the same level of education (3 categories), mother having more than a high school degree but less than a university degree and father having a high school degree or less and vice versa, mother having a university degree or more and father having less or vice versa.

Table (5) column (1) and (3) present results for math and reading when the sample is restricted to schools with a p-value greater than 0.1 based on mother's education alone for the top panel and father's education alone for the bottom panel. In most cases I fail to reject that schools are assigning students randomly to classrooms based on parental education (around 1000 or less). Estimated effects of peer mother's education are somewhat smaller in both math and reading, but remain significant (1.94 for peer mother's with more than a high school degree in math compared to 2.78 in the full sample; 2.34

in reading compared to 2.68 in the full sample; 3.07 for peer mothers' having a university degree or more in math to 3.80 in the full sample).

Columns (2) and (4) show estimates when the sample is restricted to schools whether students appear to be randomly assigned based on both mothers' and fathers' education based on Fisher's exact test. Again, I lose less than 1000 observations when I restrict the sample to schools with a p-value greater than 0.1. I find that peer mothers having more than a high school degree remains strong in math at 2.19 and drops slightly in reading to 1.97 (though not statistically significantly different from 0). Peer mothers having a university degree or more remains strong in math at 3.15.

Similar findings hold in for father's education in the bottom panel, with the notable exception that peer father's having more than a high school degree is more robust to the sample restrictions than peer mother's education in reading.

5 Testing Hypotheses

After controlling for nonrandom assignment, equation (3) would provide unbiased estimates of the effect of peer parental education. However, I am particularly interested in understanding the source of this effect. I first test whether peer parental education is salient over and above correlated factors that are often considered in the literature, such as income and race. I find that peer parental education remains salient after controlling for these factors.

In what follows, I consider several possible hypotheses developed in Section 2, based on unobservable ability at the start of kindergarten, child behaviors and unobservable effort throughout the year, teacher inputs and direct parental involvement in the classroom.

5.1 Hypothesis 1: Unobservable Ability

As discussed in Section 2, one reason that peer parental education may appear to affect test scores is if it proxies for peer "ability" or initial readiness for kindergarten. In Table 6, I test whether results are robust to controlling for

Table 5: Total Effect of Peer Parental Education: Schools with Apparent Random Assignment

	(1)	(2)	(3)	(4)
	Math		Reading	
	Mom Only			
Mom high school+	0.47***	0.42***	0.56***	0.54***
	(0.12)	(0.12)	(0.13)	(0.13)
Mom university+	0.98***	1.00***	0.89***	0.90***
	(0.15)	(0.14)	(0.16)	(0.16)
Peer mom HS+	1.94*	2.19**	2.34*	1.97
	(0.99)	(1.02)	(1.24)	(1.24)
Peer mom univ+	3.07**	3.15***	1.28	1.25
	(1.23)	(1.22)	(1.37)	(1.33)
N	11734	11764	11734	11764
R Squared	0.74	0.74	0.72	0.72
	Dad Only			
Dad high school+	0.26*	0.17	0.52***	0.43***
	(0.14)	(0.14)	(0.15)	(0.16)
Dad university+	0.82***	0.77***	0.79***	0.76***
	(0.15)	(0.15)	(0.16)	(0.16)
Peer dad HS+	1.79	1.11	5.63***	4.70***
	(1.36)	(1.33)	(1.42)	(1.44)
Peer dad univ+	2.41**	1.08	2.81**	1.92
	(1.14)	(1.12)	(1.31)	(1.34)
Resident father	0.15	0.21	0.47***	0.50***
	(0.14)	(0.15)	(0.16)	(0.16)
Peer dad resident	-0.71	-0.01	-0.49	-0.06
	(1.26)	(1.28)	(1.49)	(1.54)
Observations	11586	11452	11586	11452
R Squared	0.74	0.74	0.72	0.72

Notes: i) Standard errors clustered at class level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for class size and p_c (percentage of students observed in the class), prior achievement and school fixed effects. Peer variables are interacted with p_c to deal with missing values. We use Fisher's exact test to see whether schools apparently randomly assign students to classrooms based on parental education and restrict the sample to those with a p-value greater than 0.1. Columns (2) and (4) use a test based on both parents' education, and Columns (1) and (3) base the test on mother's or father's education alone, for the regressions based on mother's and father's education respectively.

prior peer achievement. Columns (1) and (4) replicate columns (1) and (3) in Table 3, the value-added school fixed effects specification for math and reading respectively. Columns (2) and (5) add in controls for peer average fall test scores. Estimated effects of peer parental education are robust but drop by around 0.5, from 2.78 to 2.27 in math and 2.68 to 2.15 in reading for peer mothers having more than a high school degree and 3.80 to 2.83 for peer mothers with a university degree or more in math. Estimated effects of peer fathers education become insignificant in math, but remain robust in reading, going from 3.37 to 3.17 for peer fathers having more than a high school degree.[7]

It could be that peer parental education captures nonlinear effects of peer ability, which are not adequately controlled with average peer ability. To check for this, I predict "low ability" as having fall test scores less than or equal to the 33^{rd} percentile and "high ability" as greater than or equal to the 67^{th} percentile based on math and reading scores respectively. This measure roughly divides the sample into thirds, as does the definition of mother's education. Columns (3) and (6) show estimated peer effects after controlling for the percentage of peers who are high and low ability. The estimated effect of peer mothers having more than a high school degree is now 2.26 in math and 1.98 in reading. The effect of peer mothers having a university degree or more is 2.96 in math and remains insignificantly different from 0 in reading. Peer father's education remains insignificant in math, but peer fathers having more than a high school degree remains statistically significant at 3.37 in reading.

Alternatively, it might be that students benefit from peers with higher parental education because they exhibit better behavior in noncognitive dimension Column (1) of Table 7 shows that students exhibit externalizing behaviors less when they have a mother or father with a university degree or more. The mother or father having more than a high school degree has no effect over having a high school degree or less. Already, this suggests that peer externalizing behaviors do not explain the spillovers from peer parents' having

[7]I try an alternative version where I assume math, reading and general knowledge tests measures ability with error, and control for both own and peer ability predicted from these fall test scores. Results are similar, so I use this more straightforward approach.

Table 6: Decomposing Effect of Peer Parental Education: Ability

	(1)	(2)	(3)	(4)	(5)	(6)
	Mom Only					
	Math			Reading		
Mom high school+	0.50***	0.49***	0.48***	0.54***	0.53***	0.54***
	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
Mom university+	1.02***	1.00***	1.00***	0.88***	0.86***	0.89***
	(0.14)	(0.14)	(0.14)	(0.15)	(0.15)	(0.15)
Peer mom HS+	2.78***	2.27***	2.26***	2.68***	2.15**	1.98**
	(0.84)	(0.86)	(0.85)	(1.01)	(1.01)	(1.00)
Peer mom univ+	3.80***	2.83**	2.96***	1.28	0.33	0.08
	(1.09)	(1.14)	(1.10)	(1.22)	(1.22)	(1.23)
Peer fall test		0.13***			0.14***	
		(0.05)			(0.05)	
Peer Low Ability			-3.39***			-3.40***
			(1.18)			(1.13)
Peer High Ability			0.20			1.31
			(1.01)			(1.05)
	Dad Only					
Dad high school+	0.20	0.19	0.20	0.42***	0.42***	0.43***
	(0.13)	(0.13)	(0.13)	(0.15)	(0.15)	(0.15)
Dad university+	0.78***	0.77***	0.79***	0.76***	0.75***	0.76***
	(0.14)	(0.14)	(0.14)	(0.15)	(0.15)	(0.15)
Peer dad HS+	0.61	0.24	0.45	3.37***	3.17**	3.37***
	(1.12)	(1.08)	(1.09)	(1.25)	(1.25)	(1.23)
Peer dad univ+	1.81*	1.03	1.36	1.09	0.51	0.59
	(1.00)	(0.98)	(0.99)	(1.21)	(1.21)	(1.21)
Peer fall test		0.19***			0.15***	
		(0.05)			(0.05)	
Peer low ability			-4.47***			-4.18***
			(1.23)			(1.15)
Peer high ability			0.12			0.91
			(1.02)			(1.05)

Notes: i) Standard errors clustered at class level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for class size and p_c (percentage of students observed in the class), school fixed effects and prior achievement. Peer variables are interacted with p_c to deal with missing values. Low ability is calculated for each subject based on whether the student scored below the 33rd percentile on the fall test. High abiliy is whether the student scored at or above the 67th percentile. Dummies for high and low ability are included in columns (3) and (6), but not reported.

more than a high school degree. Interestingly (not reported), whether the father is present is also a strong predictor of externalising behaviors, -0.13. I also try treating all non-cognitive behaviors as proxies for behavior, and extractor a common component through factor analysis to deal with measurement error. Results (not reported) are similar.

Columns (2) and (4) then consider whether average peer externalizing behaviors have an effect on math or reading outcomes. In both cases, there is no statistically significant effect and previously observed spillovers from peer parental education remain strong. Columns (3) and(5) then control for the degree of class misbehavior taken from fall teacher reports, dummy variables indicating whether the class misbehaves frequently, infrequently or behaves well most or almost all of the time. In this case, we find that the teacher reports are statistically significantly different from 0 in math, but not in reading. The estimated effects of peer mother's education drops some in math, to 2.07 for more than a high school degree and 3.13 for a university degree or more, while the effect of peer fathers' remains insignificant. In part, the lack of an effect of peer externalizing behaviors in columns (2) and (4) may be because effects rely more on outliers, i.e., having just one classmate who misbehaves, rather than mean behavior. Regressions that control for whether there is at least one bad peer (who scored higher than the 75% on externalizing problems) find an effect in both reading and math, but again the estimated effects of peer parental education remain.

Taken together, we interpret this as evidence that peer initial ability (both cognitive and non-cognitive) matters and explain some of the spillovers from peer parental education. However, spillovers from peer parental education remain after controlling flexibly for peer initial ability, suggesting there are other channels at work.

5.2 Hypothesis 2: Contemporaneous Unobservable

Write the linear approximation for unobservable contemporaneous inputs as

$$A_i = \delta_0 + X_i\delta_X + A_{i0}\delta_A + u_{ics}. \tag{5}$$

Table 7: Student Behavior

	(1)	(2)	(3)	(4)	(5)
	Fall Extern.	Math		Reading	
		Mom only			
Mom high school+	0.00	0.50***	0.47***	0.60***	0.61***
	(0.01)	(0.12)	(0.12)	(0.13)	(0.13)
Mom university+	-0.06***	0.98***	0.97***	0.92***	0.94***
	(0.02)	(0.14)	(0.14)	(0.15)	(0.15)
Peer mom HS+		2.25**	2.07**	2.37**	2.20**
		(0.90)	(0.91)	(1.05)	(1.07)
Peer mom univ+		3.25***	3.13***	1.30	0.84
		(1.12)	(1.12)	(1.25)	(1.26)
Fall Extern.		-0.55***	-0.55***	-0.90***	-0.89***
		(0.08)	(0.08)	(0.09)	(0.09)
Peer fall extern.		-0.57		-0.40	
		(0.64)		(0.67)	
Class behavior: F-test			2.42		0.80
P-value			0.05		0.53
		Dad only			
Dad high school+	-0.02	0.18	0.20	0.41***	0.41***
	(0.02)	(0.13)	(0.13)	(0.15)	(0.15)
Dad university+	-0.12***	0.64***	0.65***	0.72***	0.72***
	(0.02)	(0.14)	(0.15)	(0.15)	(0.15)
Peer dad HS+		0.11	0.28	3.33**	3.49***
		(1.18)	(1.18)	(1.30)	(1.32)
Peer dad univ+		1.29	1.26	0.95	0.79
		(1.03)	(1.04)	(1.20)	(1.23)
Fall Extern.		-0.52***	-0.51***	-0.84***	-0.83***
		(0.08)	(0.08)	(0.09)	(0.09)
Peer fall extern.		-0.69		-0.36	
		(0.66)		(0.68)	
Class behavior: F-test			2.74		0.81
P-value			0.03		0.52

Notes: i) Standard errors clustered at class level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for class size and p_c (percentage of students observed in the class), school fixed effects. Prior achievement is controlled in columns (2)-(6). Peer variables are interacted with p_c to deal with missing values. Class behavior is based on fall teacher reports, a categorical variable of how well behaved the class is.

Then, peer average unobservable human capital is $\bar{A}_{-ics} = \delta_0 + \bar{X}_{-ics}\delta_X + \bar{A}_{-i0}\delta_A + \bar{u}_{-ics}$. The linear-in-means approximation for the structural achievement production function in equation (1) (again ignoring teacher inputs) is

$$Y_{ics} = \tilde{\gamma}_0 + A_{ics}\tilde{\gamma}_A + \bar{A}_{-ics}\tilde{\gamma}_{\bar{A}} + \zeta_{ics}. \tag{6}$$

Plugging in for A_{ics} and $\bar{A}_{-ics}$ in equation (6),

$$Y_{ics} = \gamma_0 + X_i\gamma_X + \bar{X}_{-ics}\gamma_{\bar{X}} + \gamma_A A_{i0} + \gamma_{\bar{A}}\bar{A}_{0,-ics} + \gamma_u u_{ics} + \gamma_{\bar{u}}\bar{u}_{-ics} + \zeta_{ics},$$

Note that $\gamma_X = \delta_X\tilde{\gamma}_A$ and $\gamma_{\bar{X}} = \delta_X\tilde{\gamma}_{\bar{A}}$. Because X_i is multidimensional (whether the mother has more than a high school degree but less than a university degree (X_{i1}) or whether the mother has at least a university degree (X_{i2})), this produces the following testable implication:

$$H2: \ \frac{\gamma_{X1}}{\gamma_{\bar{X}1}} = \frac{\gamma_{X2}}{\gamma_{\bar{X}2}} = \frac{\tilde{\gamma}_A}{\tilde{\gamma}_{\bar{A}}},$$

where γ_{Xj}, $\gamma_{\bar{X}j}$ refer to the $j = 1, 2$ components of the X vector. This is a simple non-linear hypothesis that we can take to the data. Failing to reject this hypothesis means that the spillover from parental education is approximately proportional to the peer effect across different measures. This provides support for the hypothesis that peer parental education matters through the peers' unobservable human capital.[8]

Interestingly, this does not allow us to distinguish between a model where there is simultaneity in behaviors, such as

$$\begin{aligned} A_i &= \delta_0 + X_i\delta_X + A_{i0}\delta_A + \bar{A}_{-ics}\delta_{\bar{A}} + u_{ics}, \\ Y_{ics} &= \tilde{\gamma}_0 + A_{ics}\tilde{\gamma}_A + \zeta_{ics}, \end{aligned}$$

and a model where students just benefit from higher peer human capital, as in

[8] I continue to deal with unobservable initial ability by controlling for fall achievement. I compare results with and without controls for peer prior ability. I also control for measurement error using the method discussed in Section 4.2.

equation (6). It can be shown by substituting in for A_i and$\bar{A}_{-ics}$ as a function of X_i and $\bar{X}_{-ics}$ that the same proportionality condition holds as above. The model also maps into the following

$$Y_{ics} = \gamma_0' + X_i \delta_X \tilde{\gamma}_A + A_{i0} \delta_A \tilde{\gamma}_A + \bar{Y}_{-ics} \delta_{\bar{A}} + w_{ics}, \tag{7}$$

where $\gamma_0' = \tilde{\gamma}_0 + \delta_0 \tilde{\gamma}_A - \tilde{\gamma}_0 \delta_{\bar{A}}$ and $w_{ics} = u_{ics} \tilde{\gamma}_A - \bar{\zeta}_{-ics} \delta_{\bar{A}} + \zeta_{ics}$, which is closer to the standard linear-in-means model estimated in the literature. Note that in this model peer parental education does not directly affect achievement, but only affects achievement indirectly through peer achievement, which serves as a proxy for the peer unobservables. Thus, we can estimate this model instrumenting for peer achievement with peer parental education and see whether the regressions satisfy the test of over identifying restrictions. With some additional algebra, it can be shown that the system represented by equations (5) and (6) can be written in the form of equation (7). However, if the unobservable is both a function of the peer unobservable (so that there is simultaneity) and there are direct spillovers from the peer unobservable in achievement production, then $\bar{X}_{-i}$ now enters directly into equation (7) as a proxy for the peer unobservable, so that peer parental education is no longer a valid exclusion. However, the proportionality test still holds, which is why I focus on the latter test.[9]

Table 8 presents χ^2-tests of proportionality for parameter estimates presented in Table 3, considering separately the effects of peer mothers' and peer fathers' education. In all cases, I fail to reject proportionality in math. However, the opposite is true for reading. This follows because of the positive effect of peer parents having more than a high school degree, but the lack of an effect of peer parents having at least a university degree, despite a positive direct effect of a child's own parent (mother or father) having a university degree on my achievement. Thus, math supports that the spillovers derive through contemporaneous unobservables which are correlated with parental education, whereas reading results do not support this.[10]

[9] See appendix B for details.

[10] I reach the same conclusion if instead I estimate equation (7) and test whether peer

Table 8: Tests for proportionality: Mom/Dad Separately

	(1)	(2)	(3)	(4)
	Math		Reading	
	Mom Only			
χ^2	1.11	0.86	3.74	2.64
$p-value$	0.29	0.35	0.05	0.10
	Dad Only			
χ^2	0.02	0.01	4.99	5.09
$p-value$	0.89	0.91	0.03	0.02
Teacher Characteristics	N	Y	N	Y
	Mom Only			
χ^2	1.11	0.99	3.99	3.97
$p-value$	0.29	0.32	0.05	0.05
	Dad Only			
χ^2	0.00	0.01	4.99	5.67
$p-value$	0.99	0.91	0.03	0.02
Fall avg. peer ability	Y	N	Y	N
Fall high/low peer ablity	N	Y	N	Y

The tests in panel 1 are based on parameter estimates presented in Table 3. The tests in panel 2 are based on columns (2), (3), (5), and (6) of Table 6.

5.3 Hypotheses 3: Teacher effort allocation

The finding of positive spillovers from peer parental education could also be explained if more highly educated parents encourage teacher effort in a way that creates positive spillovers for other students. I consider whether the number of paid and unpaid hours spent preparing for class, teacher training and number of times they meet to discuss curriculum or lesson plans vary with peer mother's education, controlling for prior achievement in reading and math, school fixed effects, and teacher characteristics. None of these measures are predicted by own or peer mother's education. An exception is unpaid time preparing for class, which is increasing in whether own and peer mothers have a university degree or more. However, this does not predict math or reading value-added. One concern is measurement error in teacher effort, so I try an alternative version that treats different combinations of these measures as indicators of a common underlying factor, teacher effort, which I extract using factor analysis. Again, there is no evidence that this measure of teacher effort is correlated with value-added. It also does not appear that the amount of time teachers allocate to math and reading instruction (relative to other subjects) varies by the parental education of peers.

I also have teacher reports of how frequently they provide extra assistance for students who are struggling with reading. I find that frequent extra assistance for failing students is reported more in classes where there is a high percentage of peers whose mothers have a university degree, whether or not I condition on the ability composition of the classroom. Furthermore, this is not correlated with the percentage of peers who are low achieving, suggesting little evidence of crowding out, in the sense that mothers with more education put more demands on the teacher which crowds out the time the teacher can invest in other students. However, this may suggest that in classes where students have more highly educated mothers, teachers have more time to invest in struggling students, possibly as a result of students with more highly educated mothers needing less time investment.

parental education satisfies the test of overidentifying restrictions. I reject the test of overidentifying restrictions in reading, but fail to reject it in math.

I also consider whether there is evidence that students benefit more from having peers of the same parental education. This would be consistent with the hypothesis that parents encourage teachers to shift resources in a way that benefits their own child, possibly at the expense of other children. I find no evidence that spillovers from peer parental education vary by either the parents own education or the initial ability of the child (results not reported). In part this could be driven by the identification strategy, in that there may not be sufficient variation remaining after controlling for school fixed effects to test this sort of nonlinear hypothesis.

Alternatively, it could be that this type of effect would be picked up in the ability dimension, i.e., parents might advocate more for resources that would help children of similar abilities to their own child. In Table 9, I include interactions of the dummies for student ability (high or low) with the percentage of peers who are high and low. I find that for both math and reading these interactions are jointly significant (p-value of 0.01 in math and 0.00 in reading). However, students do not benefit more from peers of their own type in the ability dimension. High types receive smaller spillovers if they are grouped with more high types, and low ability students receive strong positive spillovers from having more high ability students in the classroom in reading. Even with these interactions, the spillovers from peer mother's education remain strong and of similar magnitudes to those reported in Table 3.

Another interesting dimension which teachers might vary with the composition of the classroom is how they group students for instruction. In particular, I consider whether the teacher reports never grouping students by ability (53% in math and 39% in reading). In this case, I do find that teacher practices vary by peer parental education in ways that matter for achievement, as shown in Table 10. Columns (1) and (4) show that teachers are more likely to never use ability grouping in math and reading when there is a higher percentage of peers whose mothers have more than a high school degree. Columns (2) and (5) show that the choice to never use ability grouping is positively correlated with reading, but not math performance. Finally, columns (3) and (6) examine whether the effectiveness of this practice varies with the composition of the

Table 9: Non-linear effects of peer ability

	(1) Math	(2) Reading
Mom high school+	0.48***	0.52***
	(0.12)	(0.12)
Mom university+	1.00***	0.89***
	(0.14)	(0.15)
Peer mom HS+	2.28***	2.12**
	(0.85)	(1.00)
Peer mom univ+	2.88***	0.13
	(1.09)	(1.22)
Peer low ability	-2.95**	-2.80**
	(1.31)	(1.27)
Peer high ability	0.48	1.08
	(1.05)	(1.09)
Peer low×low	-1.64	-1.39
	(1.20)	(1.32)
Peer low×high	0.55	0.86
	(1.18)	(1.24)
Peer high×low	0.91	3.14***
	(1.00)	(1.08)
Peer high×high	-1.38**	-1.87**
	(0.66)	(0.77)
Observations	12715	12715
R Squared	0.74	0.72

Notes:
i) Standard errors clustered at class level
ii) * p<0.10, ** p<0.05, *** p<0.01
All regressions include controls for the student level equivalent of the peer variable, prior achievement, class size, p_c (percentage of students observed in the class), and school fixed effects. Peer variables are interacted with p_c to deal with missing values.

classroom or the child's mother's education. In math, we now find some evidence that the practice of never grouping students by ability benefits students whose mothers have a high school degree or less, whereas in reading there is no evidence of variation in effects by mother's education. We also cannot detect statistically significant variation in effectiveness by the composition of peer mother's education in the classroom, though there is some weak evidence that there are more spillovers from never grouping in classrooms that have higher percentages of students whose mothers have a high school degree or less.

One logical explanation for these results is that peer mothers' education simply proxies for initial ability, and teaching practices (and effectiveness) vary with the ability composition of the classroom. I run a different version of these regressions in panel 2 replacing mother's education with whether the child is high or medium ability based on the second and third terciles of fall test scores. Interestingly, the choice never to use ability grouping is not correlated with the ability composition of the classroom. However, consistent with the above results, there is some evidence that the choice never to use ability groupings benefits low types in particular. Overall, mother's education is playing a role over and above initial ability in determining whether teachers use ability grouping or not.

Ideally, it would be interesting to know if these grouping pracitices are driven by the heterogeneity in the ability composition of the classroom. The data are not well-equipped to test this, as I only observe a sample of the students in the classroom. I consider the difference between the twentieth and eightieth percentiles of fall outcomes as a measure of disparity. Interestingly, the percentage of peers whose mothers have more than a high school degree and the percentage of mothers with a university degree or more predict lower dispersion. However, there is no evidence that the effect of ability grouping varies when there is more (or less) dispersion in initial ability. Again, this could be a result of data limitations.

Table 10: Teacher Never Groups by Ability

	Math			Reading		
	(1)	(2)	(3)	(4)	(5)	(6)
	No ability grouping			No ability grouping		
Peer mom HS+	0.37**	3.23***	4.08***	0.27**	2.19**	3.06**
	(0.15)	(0.88)	(1.09)	(0.13)	(1.03)	(1.19)
Peer mom univ+	0.15	4.79***	5.07***	0.01	1.53	1.72
	(0.20)	(1.15)	(1.64)	(0.17)	(1.29)	(1.40)
Never group		0.03	0.43*		0.46**	0.62**
		(0.15)	(0.24)		(0.18)	(0.28)
Mom HS+×no group			-0.45*			0.27
			(0.24)			(0.26)
Mom Univ+×no group			-0.33			-0.05
			(0.27)			(0.30)
Peer HS+×no gorup			-1.68			-2.24
			(1.18)			(1.40)
Peer Univ+×no group			-0.65			-0.76
			(1.64)			(1.37)
Peer med. ability	-0.08	3.68***	3.67**	-0.10	4.29***	3.91***
	(0.19)	(1.33)	(1.68)	(0.14)	(1.18)	(1.39)
Peer high ability	-0.02	4.20***	5.01***	-0.05	5.51***	6.42***
	(0.18)	(1.26)	(1.53)	(0.15)	(1.28)	(1.49)
Never group		0.09	0.46		0.50***	1.01***
		(0.15)	(0.30)		(0.18)	(0.31)
Med. ability×no group			-0.45*			-0.60**
			(0.27)			(0.29)
High ability×no group			-0.27			-0.68**
			(0.27)			(0.32)
Peer med.×no group			-0.08			1.08
			(1.60)			(1.56)
Peer high×no group			-1.40			-2.14
			(1.38)			(1.39)
Observations	11404	11404	11404	11596	11596	11596

Notes: i) Standard errors clustered at class level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for prior achievement, class size, p_c (percentage of students observed in the class), teacher characteristics and school fixed effects. Columns (1) and (4) control for peer fall outcomes in math and reading respectively; the dependent variable is teacher report that they never use ability grouping. Peer variables are interacted with p_c to deal with missing values. Medium and high ability in the bottom panel are measured by fall math and reading outcomes respectively; medium ability is roughly the middle tercile, and high ability is roughly the top tercile.

5.4 Hypothesis 4: Parental Involvement

The most direct way that parental efforts could spill over to classroom peers is through their involvement in the school. Given that the comparison is focused within schools, the most relevant aspect of parental involvement may come through volunteering time in the classroom. In Table 11, I consider the effect of number of times the parent volunteers in the school or to serve on a school committee in a given academic year on outcomes. Column (1) shows that mothers with more education volunteer more times over the course of the academic year. Peer parental education matters, which may be indicative of simultaneity in volunteering behavior. Column (2) considers whether own parental volunteering is a function of peer parental volunteering (i.e., whether there is simultaneity) using peer mother's education as an exclusion to predict peer parental volunteering hours. The instruments are strong predictors of peer parental volunteering with a Wald F-statistics of 19.5. Furthermore, the test of over-identifying restrictions fails to reject that they are valid instruments, with a p-value of 0.91. This regression shows that parental volunteering is in fact increasing in the hours that peer parents volunteer. One concern is whether peer parental volunteering proxies for teacher effort. While I cannot rule this out, these regressions control for teacher characteristics, and these characteristics do not predict volunteering behavior (joint p-value of 0.72). I also try including the extended measures of teacher effort (results not reported), and again reject that these are jointly significant for explaining volunteering behavior.

Columns (3) and (5) present results for reading and math respectively when I control for average time peer parents volunteer. Note that without an exclusion I cannot separately identify an effect of peer parental volunteering from own parental volunteering on test scores, so I interpret the effect of peer parental volunteering (not controlling for own parental volunteering) as having a direct effect on test scores plus an indirect effect of raising own parental volunteering. Note that unlike in the case of predicting volunteering hours, peer parental education fails the test of overidentifying restrictions in the reading regression. The effect of peer parental volunteering is insignificant in math but significantly different from 0 in reading, with a value of 0.15.

On average peer parents volunteer 4 times a year. Increasing peer parental volunteering by 1 hour would raise reading outcomes by 0.015 of a standard deviation.

There are also teacher reports of the percentage of parents who volunteer regularly to help out in the classroom or school. Columns (4) and (6) use these measures as alternative controls for classroom volunteering. I find in both math and reading that having a class with 76-100% of parents volunteering regularly is positively related to test scores. However, the dummies for class volunteering are only jointly significant in reading, with a p-value of 0.00, while the test for joint significance in math gives a p-value of 0.41. This alternative measurement is useful as it may not have issues with missing observations. However, it is difficult to compare effect sizes across columns because we do not know how the designation of "regularly" volunteering compares to the number of times that a parent volunteers in a year.

Overall this provides some support that peer parental volunteering in the classroom matters more in reading than in math. This could be reasonable if parent volunteers often help by reading books with children or other reading-oriented tasks. Unfortunately, the survey does not include information on what the parent volunteers do. This may help explain why peer mothers having a university degree or more does not create positive spillovers in reading. I find that mothers are more likely to work full time if they have more education, and this is decreasing in the education of the father and whether the father is present, suggesting that father's education may play a similar role of freeing more time for mothers to volunteer. I find that the number of times the parent volunteers is decreasing in whether the mother is working full time and only increasing in mother's education if the mother is not working full time, as might be expected. However, I am not able to show that the peer effects from mother's education in reading vary by whether the mother is working full time or not, which may be a result of insufficient variation in the data.

Table 11: Effect of Peer Parental Volunteering in Class

	(1)	(2)	(3)	(4)	(5)	(6)
	Times Parent Volunteers		Math		Reading	
Mom high school+	0.98***	0.95***	0.51***	0.50***	0.46***	0.49***
	(0.16)	(0.17)	(0.12)	(0.13)	(0.13)	(0.13)
Mom university+	1.57***	1.50***	1.10***	1.10***	0.81***	0.84***
	(0.24)	(0.24)	(0.15)	(0.15)	(0.16)	(0.16)
Peer mom HS+	1.95*		3.23***	3.22***	2.30**	2.54**
	(1.11)		(0.95)	(0.95)	(1.06)	(1.05)
Peer mom univ+	3.63**		4.88***	4.79***	0.23	0.82
	(1.65)		(1.22)	(1.18)	(1.33)	(1.30)
Peer volun. hrs		0.93**	0.06		0.15**	
		(0.38)	(0.06)		(0.07)	
% Volunteer Regularly (teacher report)						
1-25%				0.26		0.27
				(0.29)		(0.29)
26-50%				0.30		-0.03
				(0.35)		(0.34)
51-75%				0.59		-0.28
				(0.40)		(0.40)
76-100%				0.73*		1.18***
				(0.40)		(0.45)
N	11045	11045	11045	10789	11045	10789
R Squared	0.23	-0.03	0.74	0.74	0.72	0.73

Notes: i) Standard errors clustered at class level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for prior achievement, class size, p_c (percentage of students observed in the class), school fixed effects. Peer variables are interacted with p_c to deal with missing values. Column (1) includes controls for prior achievement in reading and math, and school fixed effects. All regressions control for teacher characteristics as described in Table 1.

6 Conclusion

I use rich data from the ECLS-K to first show that peer parental education matters for student achievement. I find that mother's education creates spillovers in both math and reading, but father's education only creates spillovers in reading.

I then test several potential theories regarding the source of spillovers. While initial cognitive abilities explain some of the spillovers from peer parental education, spillovers from peer parental education remain after controlling for test scores at the beginning of the year and initial non-cognitive abilities.

I find support for the theory that spillovers from peer parental education derive primarily through the child's human capital accumulated throughout the schools. This could arise through simultaneity in human capital accumulation (or effort) or through direct spillovers from peer human capital in achievement production. I show that these hypotheses yield a proportionality test, namely that the direct effect of my mother's education relative to my peers' mothers' education is equivalent across different measures of education (because it is equivalent to the ratio of the marginal effect of a child's own human capital to his peers' human capital).

I then consider whether peer parental education matters through an effect on teacher effort. I test several hypotheses related to this, and do not find strong support for this being a dominant mechanism. Most notably, I see that observable measures of teacher effort, such as unpaid time spent planning, do not seem to vary with peer parental education. However, there is evidence that teacher uses of grouping strategies within the classroom vary by peer parental education (and not the ability composition), particularly in reading. Thus, there may be some sense in which the peers' parents are affecting the way teachers organize students for instruction and that this has consequences to reading performance.

Finally, I test hypotheses related to direct effects parents might have through volunteering to help in the classroom or school. I find evidence that having more parents volunteer creates positive spillovers in reading but not math, which may help to explain why the proportionality assumption is rejected in reading.

References

Ammermueller, Andreas and Jŏrn-Steffen Pischke, "Peer effects in European primary schools: Evidence from PIRLS," *Journal of Labor Economics*, 2009, *27*, 315–348.

Avvisati, Francesco, Marc Gurgand, Nina Guyon, and Eric Maurin, "Getting Parents Involved: A Field Experiment in Deprived Schools," *The Review of Economic Studies*, 2014, *81* (1), 57–83.

Bifulco, Robert, Jason M. Fletcher, and Stephen L. Ross, "The Effect of Classmate Characteristics on Post-secondary Outcomes: Evidence from the Add Health," *American Economic Journal: Economic Policy*, February 2011, *3* (1), 25–53.

Brock, William A. and Steven N. Durlauf, "Interactions-Based Models," in James Heckman and Edward Leamer, eds., *Handbook of Econometrics*, Vol. 5, Amsterdam: Elsevier, 2001, pp. 3297–3380.

Cunha, Flavio, James J. Heckman, and Susanne M. Schennach, "Estimating the Technology of Cognitive and Noncognitive Skill Formation," *Econometrica*, 05 2010, *78* (3), 883–931.

Epple, Dennis and Richard Romano, "Peer Effects in Education: A Survey of the Theory and Evidence," in Jess Benhabib, Alberto Bisin, and Matthew O. Jackson, eds., *Handbook of Social Economics*, Vol. 1B, Amsterdam, The Netherlands: North-Holland, 2010, chapter 20, pp. 1053–1164.

Figlio, David N., "Boys Named Sue: Disruptive Children and Their Peers," *Education Finance and Policy*, 2007, *2* (4), 376–394.

Fruehwirth, Jane Cooley, "Identifying peer achievement spillovers: Implications for desegregation and the achievement gap," *Quantitative Economics*, 2013, *4* (1), 85–124.

Giorgi, Giacomo De and Michele Pellizzari, "Understanding Social Interactions: Evidence from the Classroom," IZA Discussion Papers, Institute for the Study of Labor (IZA) April 2011.

Hanushek, Eric A., John F. Kain, and Steven G. Rivkin, "New Evidence about Brown v. Board of Education: The Complex Effects of School Racial Composition on Achievement," *Journal of Labor Economics*, 2009, *27* (3), 349–383.

Hoxby, Caroline, "Peer Effects in the Classroom: Learning from Gender and Race Variation," Working Paper 7867, National Bureau of Economic Research August 2000.

Hoxby, Caroline M. and Gretchen Weingarth, "Taking Race Out of the Equation: School Reassignment and the Structure of Peer Effects," 2005. Working Paper.

Lavy, Victor and Analia Schlosser, "Mechanisms and Impacts of Gender Peer Effects at School," *American Economic Journal: Applied Economics*, April 2011, *3* (2), 1–33.

_ , M. Daniele Paserman, and Analia Schlosser, "Inside the Black Box of Ability Peer Effects: Evidence from Variation in the Proportion of Low Achievers in the Classroom," *Economic Journal*, 03 2012, *122* (559), 208–237.

Lazear, Edward P., "Educational production," *Quarterly Journal of Economics*, 2001, *116* (3), 777–803.

Manski, Charles, "Identification of endogenous social effects: The reflection problem," *The Review of Economic Studies*, 1993, *60* (3), 531–542.

Neidell, Matthew and Jane Waldfogel, "Cognitive and Noncognitive Peer Effects in Early Education," *Review of Economics and Statistics*, 2013/10/31 2010, *92* (3), 562–576.

Sacerdote, Bruce, *Peer Effects in Education: How Might They Work, How Big Are They and How Much Do We Know Thus Far?*, Vol. 3 of *Handbook of the Economics of Education*, Elsevier, June

Sojourner, Aaron, "Identification of Peer Effects with Missing Peer Data: Evidence from Project STAR," *Economic Journal*, 06 2013, *123* (569), 574–605.

Vigdor, Jacob and Thomas Nechyba, "Peer effects in North Carolina Public Schools," in P.E. Peterson and L. Woessmann, eds., *Schools and the Equal Opportunity Problem*, MIT Press, 2007.

A Non-Cognitive Skills (Source: ECLS-K User Guide)

- The Approaches to Learning Scale (Teacher SRS) measures behaviors that affect the ease with which children can benefit from the learning environment. It includes six items that rate the child's attentiveness, task persistence, eagerness to learn, learning independence, flexibility, and organization.

- The Self-Control (Teacher SRS) Scale has four items that indicate the child's ability to control behavior by respecting the property rights of others, controlling temper, accepting peer ideas for group activities, and responding appropriately to pressure from peers.

- The five Interpersonal Skills (Teacher SRS) items rate the child's skill in forming and maintaining friendships, getting along with people who are different, comforting or helping other children, expressing feelings, ideas and opinions in positive ways, and showing sensitivity to the feelings of others.

The two problem behavior scales reflect behaviors that may interfere with the learning process and with the child's ability to interact positively in the classroom.

- Externalizing Problem Behaviors (Teacher SRS) include acting out behaviors. Five items on this scale rate the frequency with which a child argues, fights, gets angry, acts impulsively, and disturbs ongoing activities.

- The Internalizing Problem Behavior (Teacher SRS) Scale asks about the apparent presence of anxiety, loneliness, low self-esteem, and sadness. This scale comprises four items.

B Proportionality with 2 sources of spillovers

For illustrative purposes consider the simpler setting where there are only 2 students. Results generalize to the N student setting with additional algebra. Suppose we assume a model where there are both direct spillovers from the peer unobservable in achievement and in the production of the unobservable, so that

$$A_i = \delta_0 + X_i\delta_X + A_j\delta_{\bar{A}} + u_i \tag{8}$$

$$Y_i = \beta_0 + A_i\beta_A + A_j\beta_{\bar{A}} + \zeta_i. \tag{9}$$

Note that I also ignore the role of intial ability in equation (8).

First I show that proportionality still holds in this more general setting with 2 types of spillovers. To see this, first plug in for A_j in equation (8) and solve for $A_i(X_i, X_j)$, so that

$$A_i = \frac{\delta_0(1+\delta_{\bar{A}})}{1-\delta_{\bar{A}}^2} + X_i\frac{\delta_X}{1-\delta_{\bar{A}}^2} + X_j\frac{\delta_X\delta_{\bar{A}}}{1-\delta_{\bar{A}}^2} + \frac{u_i + u_j\delta_{\bar{A}}}{1-\delta_{\bar{A}}^2}.$$

Then plugging in for $A_i(X_i, X_j)$ and $A_j(X_j, X_i)$ in equation (9),

$$Y_i = \gamma_0 + X_i(\delta_X \frac{\beta_A + \delta_{\bar{A}}\beta_{\bar{A}}}{1-\delta_{\bar{A}}^2}) + X_j(\delta_X \frac{\delta_{\bar{A}}\beta_A + \beta_{\bar{A}}}{1-\delta_{\bar{A}}^2}) + v_i,$$

where $v_i = \frac{u_i\delta_{\bar{A}} + u_j}{1-\delta_{\bar{A}}^2}\beta_{\bar{A}} + \zeta_i$ and $\gamma_0 = \beta_0 + \frac{\delta_0(1+\delta_{\bar{A}})}{1-\delta_{\bar{A}}^2}(\beta_A + \beta_{\bar{A}})$.

To show proportionality, note that taking the ratio of the marginal effect of own (γ_X) and peer parental education ($\gamma_{\bar{X}}$) yields

$$\frac{\gamma_X}{\gamma_{\bar{X}}} = \frac{\beta_A + \delta_{\bar{A}}\beta_{\bar{A}}}{\delta_{\bar{A}}\beta_A + \beta_{\bar{A}}}.$$

This is only a function of parameters related to A, and so it one-dimensional, which means that it must be equal across multi-dimensional X, so that proportionality holds.

Next, I show that if we write this in the form where achievement is a function of peer achievement, X_j is no long a valid exclusion, so that the test of over identifying restrictions cannot be applied. Plugging in for $A_j(Y_j, A_i)$ in equation (8) yields

$$A_i = \frac{\delta_0}{K} - \frac{\beta_0\delta_{\bar{A}}}{(1-\beta_A)K} + X_i\frac{\delta_X}{K} + Y_j\frac{\delta_{\bar{A}}}{(1-\beta_A)K} + \frac{u_i}{K} - \frac{\zeta_j}{(1-\beta_A)K},$$

where $K = 1 + \frac{\beta_{\bar{A}}\delta_{\bar{A}}}{1-\beta_A}$. We can also write Y_i as a function of A_i by plugging in for A_j using equation (8), so that

$$Y_i = \beta_0 + \delta_0\beta_{\bar{A}} + A_i(\beta_A + \delta_{\bar{A}}\beta_{\bar{A}}) + X_j\delta_X\beta_{\bar{A}} + u_i\beta_{\bar{A}} + \zeta_i.$$

Plugging in for $A_i(X_i, Y_j)$, we get

$$Y_i = \gamma_0 + X_i\gamma_X + X_j\gamma_{\bar{X}} + Y_j\gamma_Y + w_i,$$

where $\gamma_0 = \beta_0 + \delta_0\beta_{\bar{A}} + (\frac{\delta_0}{K} - \frac{\beta_0\delta_{\bar{A}}}{(1-\beta_A)K})(\beta_A + \delta_{\bar{A}}\beta_{\bar{A}})$, $\gamma_X = \frac{\delta_X}{K} + Y_j\frac{\delta_{\bar{A}}}{(1-\beta_A)K}(\beta_A + \delta_{\bar{A}}\beta_{\bar{A}})$, $\gamma_{\bar{X}} = \delta_X\beta_{\bar{A}}$, and $w_i = \frac{u_i}{K} - \frac{\zeta_j}{(1-\beta_A)K}(\beta_A + \delta_{\bar{A}}\beta_{\bar{A}}) + u_i\beta_{\bar{A}} + \zeta_i$. Importantly, X_j enters as a proxy for peer ability, as does Y_j. This means that X_j is no longer a valid exclusion for identifying γ_Y. Thus, the test of over-identifying restrictions cannot inform whether there are spillovers from unobserved human capital in this more general model.