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Peer effects on student achievement: evidence from Chile

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Abstract

This paper reports estimates of peer effects on student achievement, using a 1997 census of eighth-grade achievement in Chile. The data allow detailed measures of peer characteristics to be constructed for each classroom within a school. The paper addresses the endogeneity of peer variables by including school fixed effects that control for unobserved family and student characteristics. The estimates suggest that the classroom mean of mothers' education is an important determinant of individual achievement, though subject to diminishing marginal returns. Additional specifications using family fixed effects are not suggestive that estimates are biased by within-school sorting.

JEL classification: I2

Keywords: Input output analysis; Peer effects; Chile; Achievement

1. Introduction

A common hypothesis is that student outcomes are higher in the presence of "good" peer groups, conditional on individual characteristics like socioeconomic status. Common measures of peer-group characteristics include mean student ability or parental education in a particular school or classroom. Since the Coleman report (Coleman, Campbell, Hobson, McPartland, Mood, Weinfeld et al., 1966), a vast empirical literature in economics and sociology has tested this hypothesis (for reviews, see Jencks & Mayer, 1990; McEwan, 2000; Nechyba, McEwan, & Older-Aguillar, 1999).[1] Two papers, in particular, are usually cited by economists as evidence of peer-group effects (Henderson, Mieszkowski, & Sauvageau, 1978; Summers & Wolfe, 1977).[2]

Most empirical studies use ordinary least squares regressions to estimate the marginal effect of peer vari-

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[1] Several authors find that higher mean levels of school or classroom socioeconomic status are associated with higher individual achievement (Link & Mulligan, 1991; Robertson & Symons, 1996; Willms, 1986; Zimmer & Toma, 2000). Other studies yield some positive results that are, nonetheless, inconsistent enough to give pause. Caldas and Bankston (1997) find that mean SES increases achievement, but that the mean family incomes—proxied by the percentage eligible for free-and-reduced lunch—are negatively associated with outcomes. Bryk and Driscoll (1988) find a rather strong effect of mean SES on achievement that is counter-balanced by a negative effect of increasing mean achievement. Finally, Winkler (1975) finds that the percentage of low-SES students tends to lower white achievement, but not that of black students. There is only limited evidence on the effects of school-wide SES and achievement on attainment. Both Mayer (1991) and Gaviria and Raphael (1997) find that advantaged peer groups tend to lower the probability of dropping out, while Bryk and Driscoll (1988) do not find the predicted influences of mean SES and achievement on attainment.

[2] Economists' interest in obtaining estimates of peer-group effects is not purely academic. To provide one example, various authors have constructed models of education markets, usually with the goal of assessing the potential impact of private school vouchers (e.g., Epple & Romano, 1998; Manski, 1992; Nechyba, 1996). By design, vouchers induce sorting across schools

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ables on individual outcomes, holding constant a range of individual characteristics. However, a growing literature highlights the methodological difficulties inherent to this approach.[3] This paper focuses on the potential correlation of peer variables with the error term which, if ignored, could bias estimates of peer-group effects. To illustrate this pitfall, Evans, Oates and Schwab (1992) observe that families may choose their residences—and schools—based on observed characteristics of potential peer groups. The same families may possess unobserved characteristics, such as greater motivation, that positively influence student outcomes. In this case, observed peer variables are positively correlated with unobserved individual determinants of outcomes, perhaps leading to upward biases in estimates of peer-group effects.

In this paper, I include school fixed effects to account for endogeneity. Since families in a particular school have made similar choices regarding their children's education (and potential peer group), one might expect that families are similar in other ways, including the unobserved characteristics that influence their children's achievement. Using a census of eighth-grade achievement in Chile, I find that including fixed effects does not appreciably alter the estimates of peer effects. These estimates suggest that the average level of mothers' education in a classroom has the strongest links to student achievement. Moreover, the estimates imply a concave relationship, with diminishing marginal returns to increasing levels of this variable.

It is possible that these estimates are still biased by sorting that occurs *within* schools. For example, students could be assigned to classrooms (and peer groups) based on their unobservable characteristics. If these characteristics have positive or negative effects on achievement, then the previous estimates are biased. In additional regression estimates, I include family fixed effects to control for unobservable student characteristics that are constant across siblings. These regressions yield point estimates on mean mothers' education that are generally consistent in sign and magnitude to those of previous estimates, although they are estimated with little precision.

2. Empirical approach

2.1. School-specific fixed effects

Consider the following education production function:

$$A_{ijk} = \beta_0 + F_{ijk}\beta_1 + P_{jk}\beta_2 + \varepsilon_{ijk} \quad (1)$$

where the achievement (A_{ijk}) of student i in classroom j in school k is a function of student and family background (F_{ijk}) and characteristics of the classroom peer group (P_{jk}). Unobserved determinants of achievement are captured in an error term (ε_{ijk}). The principal goal is to obtain unbiased estimates of β_2, the marginal effects of peer-group characteristics on achievement, ceteris paribus. There is a possibility, however, that peer variables are correlated with the error term—that is, $Cov(P_{jk},\varepsilon_{ijk}) \neq 0$—which biases estimates of β_2. The correlation could stem from the omission of family determinants of achievement that are correlated with peer variables, because of family sorting described in the Introduction.

The majority of research ignores potential biases. A few, like Evans et al. (1992) have sought to identify instrumental variables that are correlated with P_{jk}, but uncorrelated with ε_{ijk} (also see Gaviria & Raphael, 1997; Robertson & Symons, 1996). Yet, as Moffitt (2001) observes, these papers may not succeed in identifying exogenous variation in peer variables. By relying on variation in the gender and race composition of adjacent grades, Hoxby (2000) may identify variation that is more credibly exogenous; she finds fairly consistent evidence of peer effects. More recent work has used the random assignments of roommates at elite colleges to identify consistent peer effects, but these estimates may be limited in their generalizability (Sacerdote, 2001; Zimmerman, 1999).

This paper's identification strategy relies upon within-school variation in peer characteristics. In part, families choose schools based on observed peer characteristics. Let us assume that the families and students who choose a particular school (and peer group) share similar preferences, motivations, and other unobserved characteristics that influence the outcomes of their children. In this case, the error term has a school-specific component (u_k) that is constant for each individual in a school, and an idiosyncratic component that varies across individuals within classrooms and schools (v_{ijk}):

$$\varepsilon_{ijk} = u_k + v_{ijk}$$

Biased estimates of β_2 may result from correlation between peer-group variables and the school-specific component ($Cov(P_{jk},u_k) \neq 0$). To purge estimates of bias, one approach is to treat the u_k as fixed, instead of random.

There are two potential limitations to the approach. First, estimates of β_2 can only be obtained if there is within-school variation in the peer-group characteristics. In many empirical applications—mainly because of limited data—peer variables are school averages, and thus constant for every student in a school. Under these circumstances, the fixed effects absorb variation in peer variables and prevent the estimation of separate coef-

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that could affect student outcomes if peer-group effects are important. When modeling education production, however, these papers are forced to assume the existence of such effects.

[3] For discussions, see Duncan, Connell, and Klebanov (1997), Glewwe (1997), Manski (1993), Moffitt (2001), Nechyba et al. (1999), and Tienda (1991).

ficients. This paper, however, uses separate peer measure for each classroom within a school.[4]

Second, endogeneity may not be fully addressed. The identification strategy explicitly assumes that the allocation of students to classrooms is random within schools, conditional on observed student characteristics. It is possible, however, that unobservable determinants of achievement may vary across classrooms within a school. There are two likely sources of such variation. In some schools, students are tracked into classrooms by ability. School personnel may rely upon student traits that are unobserved to researchers, but that influence achievement. This raises the possibility that researchers will confound the influences of unobserved student characteristics and peer characteristics in high-ability classrooms. Similarly, motivated parents may pressure school personnel to assign their children to higher tracks. If these parents also produce higher outcomes among their children, then one confounds individual and peer determinants of achievement.

These biases are perhaps more likely to occur in schooling contexts that favor a strong use of tracking—and thus allow for extensive sorting within schools based on unobservable student characteristics. There is mixed evidence on whether Chile, the subject of this paper, tracks less extensively than countries such as the US.[5] Hence, one must admit the possibility that fixed-effects estimates based on Eq. (1) are biased.

2.2. Family-specific fixed effects

As an additional test, I estimate models with fixed effects for each family.[6] To do so, I use a subsample of siblings and twins, imputed from the larger data set (the data and imputation procedure are described in the next section). Consider a modified specification of the production function:

$$A_{ifjk} = \beta_0 + F_{ifjk}\beta_1 + P_{jk}\beta_2 + \mu_f + \varepsilon_{ifjk} \quad (2)$$

where f indexes each family, and the μ_f are a series of family-specific fixed effects. If peer variables are correlated with unobserved determinants of achievement that are constant within families—such as home environment, ability, preferences, or motivation—then the inclusion of fixed effects will ameliorate bias in estimates of β_2.

There are two important observations to make regarding this empirical approach. First, there must be within-family variance in the variable of interest. In this case, the empirical approach is only feasible if siblings attend different classes within the same school, and hence are exposed to different peer groups. Second, estimates of peer group effects are still subject to potential bias if there is non-random allocation of siblings across classrooms (and peer groups). I return to these points in the following sections.

3. Data

3.1. Full sample

Table 1 provides details on the dependent and independent variables used in the analysis. They were drawn from a 1997 census of eighth-grade achievement conducted by Chile's Ministry of Education. The Spanish and mathematics tests were standardized to a mean of zero and a standard deviation of one. In addition to the achievement tests, the data include parent responses to surveys of family background. From these data I extracted several family and student control variables, including gender, parental schooling, family income, student ethnicity, and the number of books in the family home. I further constructed measures of student peer groups by averaging several variables across each classroom, including MOTHERED, FATHERED, INCOME, and INDIGENOUS.[7]

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[4] To some extent, this decision is ad hoc. Theories of peer interaction do not provide clear guidance on whether the appropriate level of aggregation is the classroom or school. In fact, there is a large literature that searches for the existence of neighborhood effects, using measures of socioeconomic status aggregated to the community level. It is worth emphasizing that the present empirical approach cannot assess the importance of peer effects that function at the level of schools or neighborhoods, because such effects are absorbed by the school dummy variables.

[5] In 1999, Chile participated in the TIMSS international assessment of mathematics and science achievement. These data were not utilized in this paper, in part because they do not have the census coverage necessary to construct detailed measures of peer characteristics. However, they provide some descriptive evidence on the use of tracking in eighth-grade mathematics (Mullis, Martin, Gonzalez, Gregory, Garden, O'Connor et al., 2000, p. 269). In Chile, 15% of students attended schools in which different classes studied different mathematics content, compared with 37% in the US. Twenty-nine percent attended schools with enrichment mathematics, versus 79% in the US. On the other hand, 70% of Chilean students attended schools where classrooms studied the same content, but at different levels of difficulty; this is compared with 49% in the US.

[6] Similar empirical strategies, using samples of twins or other siblings, have been used to estimate the returns to education (e.g., Ashenfelter & Krueger, 1994) and the effect of Head Start (Currie & Thomas, 1995). Aaronson (1998) uses the strategy to test for neighborhood effects.

[7] Note that peer measures might also be constructed by obtaining school averages for student characteristics. In fact, this is the procedure followed in most research, although the decision usually stems from data constraints rather than theories of peer interaction. In conducting this paper's analyses, I also

Table 1
Descriptive statistics and variable definitions

	Mean (standard deviation)			Variable description
	Full sample	Imputed sibling sample	Imputed twins sample	
Dependent variables				
SPANISH	0.00 (1.00)	−0.22 (1.00)	0.13 (0.98)	Number of items correct on the eighth-grade Spanish test, standardized to mean 0 and standard deviation 1
MATH	0.00 (1.00)	−0.15 (0.99)	0.19 (1.00)	Number of items correct on the eighth-grade mathematics test, standardized to mean 0 and standard deviation 1
Peer variables				
MEAN(MOTHERED)	9.75 (2.47)	9.46 (2.51)	10.49 (2.46)	Classroom mean of MOTHERED
MEAN(FATHERED)	10.16 (2.70)	9.85 (2.75)	10.99 (2.69)	Classroom mean of FATHERED
MEAN(INDIGENOUS)	0.05 (0.09)	0.05 (0.09)	0.03 (0.05)	Classroom mean of INDIGENOUS
MEAN(INCOME)	2.99 (3.20)	2.85 (3.32)	3.71 (3.96)	Classroom mean of INCOME
Student/family variables				
FEMALE	0.52	0.55	0.62	1=female, 0=male
INDIGENOUS	0.05	0.05	0.03	1=student's mother identifies herself as indigenous, 0=not
MOTHERED	9.75 (3.78)	9.64 (3.91)	11.02 (3.91)	Years of mother's schooling
FATHERED	10.16 (4.06)	10.01 (4.29)	11.52 (4.10)	Years of father's schooling
INCOME	2.99 (3.98)	2.93 (4.06)	3.84 (4.82)	Monthly family income in pesos÷100,000
BOOK2	0.25	0.26	0.19	1=6–20 books in home, 0=not
BOOK3	0.15	0.13	0.12	1=21–35 books in home, 0=not
BOOK4	0.12	0.12	0.12	1=36–50 books in home, 0=not
BOOK5	0.07	0.06	0.08	1=51–65 books in home, 0=not
BOOK6	0.05	0.05	0.07	1=66–80 books in home, 0=not
BOOK7	0.03	0.03	0.05	1=81–95 books in home, 0=not
BOOK8	0.21	0.23	0.30	1=more than 95 books in home, 0=not
N	163,075	1270	443	

Source: All variables are taken from the *Sistema de Medición de la Calidad de Educación* (SIMCE), 1997. Standard deviations are only reported for continuous variables.

The main empirical strategy hinges upon the existence of within-school variation in classroom peer characteristics. However, such variation is a double-edged sword: it facilitates the identification of peer effects in specifications with school fixed effects, but its mere existence arouses concerns about the possibility of within-school sorting on unobservables and the accompanying biases. To examine the degree of variation in several variables, Table 2 (Panel A) presents results from a series of one-way ANOVAs.

The within-school standard deviation for SPANISH is 0.84, which is similar to the standard deviation in the full sample. Around 69% of the variance in Spanish achievement occurs within schools. While this might seem large, consider that within-school variance may account for over 90% of the total in the US (e.g., Hanushek, Kain, & Rivkin, 1998). These findings are roughly consistent with Chile's unique institutional arrangements that facilitate student sorting across schools—and perhaps the clustering of "similar" students in schools (since 1980, Chile has allowed unrestricted parental choice of public or private schools, regardless of residence).[8] A similar pattern of results is observed for the independent variable, MOTHERED.

There is substantially less within-school variance for a key peer variable, the classroom mean of MOTHERED. The within-school standard deviation is 0.53 (compared to a full sample result of 2.47) and only 4% of variance occurs within schools. The lack of within-school variance raises a cautionary flag. If the inclusion

estimated specifications with school-average peer measures (obviously excluding school fixed effects), and the estimates were quite similar to OLS estimates with classroom-average measures.

[8] For descriptions and empirical analyses of Chile's voucher system, see McEwan (2001) and McEwan and Carnoy (2000).

Table 2
Within-school variance in selected variables

	SPANISH	MOTHERED	MEAN(MOTHERED)
Panel A: Full sample (*N*=163,075)			
Mean in full sample	0.00	9.75	9.75
Standard deviation in full sample	1.00	3.78	2.47
Standard deviation within schools	0.84	2.95	0.53
Percent of variance within schools	68.7%	59.3%	4.5%
Panel B: Imputed sibling sample (*N*=1270)			
Mean in full sample	−0.22	9.64	9.46
Standard deviation in full sample	1.00	3.91	2.51
Standard deviation within families	0.68	0	0.39
Percent of variance within families	23.3%	0	1.2%
Panel C: Imputed twins sample (*N*=443)			
Mean in full sample	0.13	11.02	10.49
Standard deviation in full sample	0.98	3.91	2.46
Standard deviation within families	0.55	0	0.26
Percent of variance within families	16.1%	0	0.6%

of school fixed effects yields estimates of peer effects that are not statistically different from zero (but similar to the OLS estimates), then it is not plausible to assert that peer effects do not exist. Another explanation is that fixed effects simply removed most variance in peer variables, and that resulting variance was insufficient to obtain precise estimates. This is dealt with further in the next section.

The identification strategy assumes that allocation of students to classrooms is random, conditional on observed characteristics. Even random allocation would clearly produce some exogenous variation in peer characteristics like the classroom mean of MOTHERED. While it is impossible to directly assess whether the variation in the present data is exogenous, one can at least gauge whether the degree of variation is consistent with random allocation. To do so, I carried out a simple simulation.

First, I assigned each student a random number between 0 and 1, drawn from a uniform distribution. Second, I rank-ordered students by this number, within each of their respective schools. Third, I partitioned students into one or more equally-sized “classes,” depending on the original number of classrooms in their respective schools. Fourth, I re-calculated the classroom mean of mother’s education, based on the artificially-constructed “classes.” Fifth, I re-estimated the measures of within-school variance that were reported in Table 2. This exercise produced a within-school standard deviation of 0.41 for the classroom mean of MOTHERED, with roughly three percent of the variance occurring within schools. The simulated estimates are slightly lower than the true estimates, indicating that more within-school sorting is occurring than might be expected under completely random allocation of students to classrooms. It seems plausible that a system engaging in widespread tracking by ability would produce larger gaps across classrooms in the mean of mothers’ education.

3.2. Imputed sibling and twins samples

Because the original SIMCE data do not include family identifiers, this information was imputed with the following procedure. First, I identified a comprehensive set of family-specific variables from the parental survey. This included variables in Table 1, such as parental education and income, but it also included a long list of additional variables that are available on the parent questionnaire.[9] Second, I discarded all cases that did not contain a complete set of responses for all variables. Third, I identified students with matching values on all of the preceding family variables, as well as the school code. The inclusion of school codes means that siblings who attend different schools cannot be identified, but it dramatically reduces the likelihood of incorrect matches. This procedure yielded a sample of 1270 students—dominated by pairs, but including a few groups of three—that are denoted siblings.

Given the fact that students attend the same grade, it is likely that a large number of these matches are twins. An additional match on the month and year of birth further limited the sample to 443 students, denoted twins.[10] As a simple check on the quality of the imputed

[9] A full list of variables is available from the author.

[10] The data do not allow one to distinguish between monozygotic and dizygotic twins, and hence there is no means of determining how much genetic material is shared by each set of twins.

match, one can assess how far apart the non-twin siblings are in age. As expected, the large majority of non-twin siblings are separated by 10 months or more. There are a few non-twin "siblings" separated by 1–9 months—88 pairs of students, in all. This is likely explained by the presence of adopted children, cousins, or other young people who are considered children in a shared household environment.

The descriptive statistics for sibling and twins samples are reported in Table 1. On average, students in the sibling sample score lower than the full sample, and live in families with slightly lower socioeconomic status. This may be explained by the fact that siblings in the same grade are closely spaced, and that this is more common among less privileged families. On the other hand, the twins sample is higher achieving and notably higher in socioeconomic status than the full sample.[11]

Panels B and C in Table 2 report on the within-family variance in selected variables. Of particular note are the results for the classroom mean of MOTHERED. They indicate that an exceedingly small portion of the variance occurs within families, due to the fact that many siblings and twins are assigned to the same classroom, or attend classrooms within the same school that are similar in their peer characteristics.

4. Results

4.1. Estimates with the full sample

Table 3 presents two sets of regressions using each dependent variable. In one, regressions are estimated by ordinary least squares (with robust standard errors that are adjusted for clustering within classrooms). In the second, regressions also include school fixed effects. I further estimated regressions that exclude and include squared terms for each peer variable. The purpose of doing so is to test for concavity in the relationship between peer variables and achievement, given that some authors have found evidence of diminishing marginal returns in peer effects (e.g., Henderson et al., 1978; Zimmer & Toma, 2000).

Before interpreting the results on peer effects, I briefly consider the coefficients on individual variables. Female students have greater Spanish achievement, between 0.17–0.20σ. Although males have higher achievement in mathematics, the magnitude of the advantage is smaller (0.03–0.07σ). Indigenous students have lower achievement in both areas, by 0.05–0.08σ. Parental education has positive effects on education, though stronger in the case of mother's education. An increase of one standard deviation in mother's education produces Spanish achievement gains of 0.08σ, and similar math gains. Income has an unexpectedly negative and statistically significant relationship to achievement, although the magnitude is quite small. In general, more books in the home leads to higher achievement. The estimates of all of coefficients on individual variables are fairly robust in sign and magnitude across the various specifications in Table 3.

Turning to the results on peer effects, one can distill several findings from Table 3. First, the magnitudes of the coefficient differences between specifications with and without school fixed effects are not large (although specification tests suggest that fixed effects specifications are preferable).[12] Overall, the inclusion of school fixed effects does little to alter the fundamental conclusion that some peer characteristics—particularly mothers' education—have important effects on Spanish and mathematics achievement. Thus, in the Chilean case, it does not appear that sorting *between* schools produces important biases in estimates of peer-group effects. This is telling, because Chile's large-scale voucher system places fewer constraints on school choices, either public or private, than most schooling systems.

Second, the classroom mean of mothers' education is the most important peer determinant of achievement. A one standard deviation increase leads to a 0.27σ gain in Spanish achievement (relying upon the linear, fixed-effects specification). The mean of fathers' education also has positive effects that are smaller in magnitude. An increasing percentage of indigenous students in a classroom tends to lower achievement, all else equal. However, the magnitude of the effect is rather small. A one standard deviation increase—nine percentage points—only leads to a 0.03σ decline in Spanish achievement (again, relying upon the linear, fixed-effects specification). Finally, mean classroom income has inconsistent and small effects on achievement.

Third, there is some evidence of a concave relationship between peer variables and achievement, most prominently in the case of mothers' education. The squared terms in fixed effects specifications are slightly negative. In the case of Spanish achievement, dimin-

[11] There is no obvious explanation for the divergence. One possibility is that fertility-enhancing drugs—a factor that has increased the incidence of multiple births in the US—were more commonly available to higher-income families during early 1980s.

[12] In separate results that are not reported here, I estimated the specifications with random school effects (using generalized least squares) and fixed school effects. In every case, Hausman tests easily rejected the null hypothesis that there were no differences in coefficient estimates between random and fixed effects models. Thus, in a statistical sense, the fixed effects specifications are to be preferred. As the text notes, however, the magnitude of coefficient differences is not large in a practical sense.

Table 3
Estimates of peer effects using the full sample

	Dependent variable: SPANISH				Dependent variable: MATH			
MEAN(MOTHERED)	0.114**	0.110**	0.078**	0.297**	0.133**	0.116**	0.021	0.300**
	(0.007)	(0.008)	(0.024)	(0.028)	(0.008)	(0.009)	(0.030)	(0.031)
SQUARED	–	–	0.002	−0.010**	–	–	0.006**	−0.010**
			(0.001)	(0.001)			(0.002)	(0.002)
MEAN(FATHERED)	0.015*	0.052**	−0.016	0.106**	−0.010	0.054**	−0.103**	0.043
	(0.006)	(0.008)	(0.020)	(0.027)	(0.008)	(0.009)	(0.025)	(0.029)
SQUARED	–	–	0.002	−0.003*	–	–	0.005**	0.000
			(0.001)	(0.001)			(0.001)	(0.001)
MEAN(INDIGENOUS)	−0.357**	−0.302**	−0.390**	--0.186	−0.201**	--0.300**	−0.515**	−0.232
	(0.059)	(0.106)	(0.112)	(0.167)	(0.068)	(0.112)	(0.134)	(0.176)
SQUARED	–	–	−0.011	−0.258	–	–	0.311	−0.122
			(0.137)	(0.493)			(0.160)	(0.505)
MEAN(INCOME)	0.0004	−0.022**	−0.009	−0.009	0.022**	−0.018*	−0.014	−0.011
	(0.002)	(0.007)	(0.008)	(0.013)	(0.003)	(0.008)	(0.011)	(0.015)
SQUARED	—	—	−0.0001	0.0001	—	—	--0.00001	0.0001
			(0.0003)	(0.001)			(0.0004)	(0.001)
FEMALE	0.201**	0.174**	0.202**	0.174**	−0.030**	−0.070**	−0.029**	−0.070**
	(0.006)	(0.005)	(0.006)	(0.005)	(0.007)	(0.005)	(0.007)	(0.005)
INDIGENOUS	−0.080**	−0.082**	−0.080**	−0.082**	−0.053**	−0.055**	−0.053**	−0.055**
	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
MOTHERED	0.020**	0.021**	0.020**	0.021**	0.015**	0.015**	0.014**	0.015**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
FATHERED	0.011**	0.012**	0.011**	0.012**	0.007**	0.008**	0.007**	0.008**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
INCOME	−0.006**	−0.006**	−0.006**	−0.006**	−0.004**	−0.003**	−0.004**	−0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
BOOK2	0.124**	0.111**	0.129**	0.109**	0.073**	0.077**	0.087**	0.076**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)
BOOK3	0.219**	0.199**	0.225**	0.196**	0.135**	0.133**	0.155**	0.131**
	(0.010)	(0.009)	(0.010)	(0.009)	(0.010)	(0.008)	(0.010)	(0.008)
BOOK4	0.278**	0.248**	0.284**	0.246**	0.182**	0.170**	0.201**	0.168**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)	(0.010)	(0.009)
BOOK5	0.325**	0.278**	0.331**	0.275**	0.228**	0.191**	0.245**	0.189**
	(0.012)	(0.011)	(0.012)	(0.011)	(0.012)	(0.010)	(0.012)	(0.010)

(*continued on next page*)

Table 3 (*continued*)

	Dependent variable: SPANISH				Dependent variable: MATH			
BOOK6	0.339**	0.294**	0.344**	0.292**	0.228**	0.197**	0.243**	0.195**
	(0.013)	(0.012)	(0.013)	(0.012)	(0.013)	(0.011)	(0.013)	(0.011)
BOOK7	0.370**	0.320**	0.375**	0.317**	0.265**	0.224**	0.279**	0.223**
	(0.014)	(0.013)	(0.014)	(0.013)	(0.015)	(0.013)	(0.015)	(0.013)
BOOK8	0.410**	0.354**	0.413**	0.352**	0.330**	0.279**	0.340**	0.278**
	(0.010)	(0.009)	(0.010)	(0.009)	(0.011)	(0.009)	(0.011)	(0.009)
R^2	0.24	0.35	0.24	0.35	0.22	0.40	0.22	0.40
School fixed effects?	No	Yes	No	Yes	No	Yes	No	Yes

Note: ** (*) statistically significant at 1% (5%). Huber/White standard errors, adjusted for clustering within classrooms, are in parentheses. The *N* of all regressions is 163,075. All regressions include a constant. Regressions without fixed effects also include regional dummy variables.

ishing returns imply that the marginal effect of mothers' education declines to zero at a classroom mean of around 15 years. The results are similar for mathematics.

4.2. Estimates with imputed sibling and twins samples

It is possible that the previous results are biased by within-school sorting. Towards assessing this, Table 4 presents regressions that include family fixed effects, using the sibling and twins samples. Given the reduced sample and small within-family variance in peer measures, it would be surprising if the coefficients were estimated with the same degree of precision as before. In fact, there are few statistically significant results among the coefficient estimates. Despite the imprecision, the fixed effects coefficients on the mean of MOTHERED are consistently positive and larger in magnitude than the same coefficients from full sample regressions. This evidence is not strong enough to completely rule out within-school sorting as an explanation for the pattern of results in Table 3. Yet, it is also not consistent with a story in which the consistently positive coefficients on MEAN(MOTHERED) are subject to strong upward bias.

Beyond caveats about the imputed data, there are potential biases in estimates that rely on family fixed effects (for similar discussions, see Currie & Thomas, 1995 and Aaronson, 1998). First, the estimates presume that within-family allocation to peer groups is random. This would be violated, for example, if families or schools identified one sibling as more gifted and maneuvered him or her to a class with a "better" peer group (alternatively, families might take a compensatory approach, and assign less gifted siblings to better classrooms).

Second, the estimates could be biased by spillover effects between siblings. If one sibling benefits from exposure to a "better" peer group, then it is possible that some of those benefits are transmitted to the other sibling, perhaps through home interactions. This could bias effects downward. Overall, however, there is no means of predicting the direction of bias. Within the constraints of the present data, there is little more that can be done to assess these biases.

5. Conclusions

Empirical research often finds that students' outcomes are positively correlated with attributes of their school or classroom peers, conditional on individual characteristics. Yet, these estimates may be biased due to the correlation of peer variables with the error term, perhaps stemming from the sorting behavior of families. This paper presented estimates of peer effects from regressions with school fixed effects—thus controlling for student heterogeneity that is constant across a given school. Overall, they suggest that sorting across schools does not provide a good explanation for the observed pattern of results. The estimates suggested that the mean schooling of mothers exhibited the strongest links to achievement, though with diminishing marginal returns.

If sorting mainly occurs across schools, rather than *within* schools, then the estimates are credibly unbiased. To assess the potential biases from within-school sorting, the paper used imputed samples of siblings and twins from the Chilean achievement census. The inclusion of family fixed effects in regressions controls for unobserved individual characteristics that are constant within families, such as home environments, parental motivations, and, in some cases, ability. While extremely imprecise, the point estimates on the mean of mothers' education are not consistent with the notion that previous estimates are strongly biased upward by within-school sorting. Nevertheless, further evidence is certainly required on this issue.[13]

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[13] The present study has not distinguished among the various channels through which "better" peer groups might affect student achievement (for a discussion of some possibilities, see Jencks & Mayer, 1990 and Nechyba et al., 1999). These could include direct interactions and peer tutoring, improved disciplinary climates that affect the quality and time of instruction, or even differential treatment of some students by teachers. To some extent, this failure to unpack the "black box" is endemic to the literature on peer and neighborhood effects (this includes recent randomized experiments that assess the impact of moving to wealthier neighborhoods; see Katz, Kling, & Liebman, 2001; Ludwig, Duncan, & Hirschfield, 2001). It is also possible that teachers are not assigned randomly across peer groups, and that peer quality is serving as a proxy for teacher quality (for example, good teachers may be assigned the "best" students as a reward, or they may be assigned the "worst" students in a compensatory action). This may be true in the present study (and many others) that do not make detailed controls for teacher characteristics at the classroom level.

Table 4
Estimates of peer effects using the imputed sibling and twins samples

	Dependent variable: SPANISH				Dependent variable: MATH			
Panel A: Imputed sibling sample (*N*=1270)								
MEAN(MOTHERED)	0.102*	0.153	0.235	0.656	0.123*	0.148	−0.067	0.650*
	(0.048)	(0.097)	(0.159)	(0.343)	(0.050)	(0.085)	(0.183)	(0.274)
SQUARED	–	–	−0.008	−0.027	–	–	0.010	−0.027
			(0.008)	(0.017)			(0.010)	(0.015)
MEAN(FATHERED)	−0.004	−0.019	−0.064	−0.732	−0.044	−0.104	−0.051	−0.685*
	(0.046)	(0.130)	(0.131)	(0.390)	(0.048)	(0.110)	(0.158)	(0.345)
SQUARED			0.003	0.034			0.001	0.030
			(0.006)	(0.019)			(0.008)	(0.018)
MEAN(INDIGENOUS)	−0.907*	1.415	−0.273	3.277	−0.484	0.084	−0.386	1.782
	(0.356)	(1.669)	(0.668)	(3.248)	(0.357)	(1.469)	(0.709)	(2.437)
SQUARED	–	–	−1.062	−6.436	–	–	−0.265	−7.030
			(0.944)	(11.494)			(0.877)	(10.319)
MEAN(INCOME)	0.013	−0.062	0.055	0.342	0.058**	0.017	0.017	0.090
	(0.020)	(0.134)	(0.059)	(0.250)	(0.022)	(0.089)	(0.066)	(0.250)
SQUARED	–	–	−0.001	−0.022*	–	–	0.000	−0.004
			(0.002)	(0.011)			(0.002)	(0.010)
R^2	0.25	0.77	0.26	0.77	0.24	0.81	0.24	0.81
Family fixed effects?	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Imputed twins sample (*N*=443)								
MEAN(MOTHERED)	−0.050	0.388*	0.127	0.764	0.070	0.218	−0.059	0.673
	(0.095)	(0.175)	(0.416)	(0.826)	(0.094)	(0.170)	(0.389)	(0.804)
SQUARED	–	–	−0.008	−0.026	–	–	0.008	−0.032
			(0.018)	(0.031)			(0.018)	(0.030)
MEAN(FATHERED)	0.098	−0.250	−0.009	−1.661**	0.031	0.017	0.054	−1.089*
	(0.091)	(0.180)	(0.350)	(0.450)	(0.089)	(0.169)	(0.315)	(0.435)
SQUARED	–	–	0.004	0.066**	–	–	−0.001	0.053*
			(0.015)	(0.020)			(0.013)	(0.021)
MEAN(INDIGENOUS)	−0.634	2.648	3.654	6.562	−1.359	−2.769	1.202	7.380
	(1.318)	(2.804)	(2.727)	(5.464)	(1.496)	(4.537)	(2.842)	(7.110)
SQUARED	–	–	−24.750*	−17.533	–	–	–14.752	–51.267
			(11.368)	(23.886)			(12.691)	(29.273)
MEAN(INCOME)	−0.013	−0.357*	0.014	−0.313	0.025	−0.012	−0.010	0.036
	(0.033)	(0.175)	(0.082)	(0.357)	(0.033)	(0.122)	(0.091)	(0.456)
SQUARED	–	–	−0.001	−0.002	–	–	0.001	0.000
			(0.003)	(0.016)			(0.003)	(0.018)
R^2	0.30	0.85	0.31	0.85	0.29	0.84	0.29	0.84
Family fixed effects?	No	Yes	No	Yes	No	Yes	No	Yes

Note: ** (*) statistically significant at 1% (5%). Huber/White standard errors, adjusted for clustering within classrooms, are in parentheses. All regressions include a constant, and the same control variables in Table 3 (unless they are swept out by family fixed effects).

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