Peer effects identified through social networks.
Evidence from Uruguayan schools

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Abstract

This paper provides evidence on peer effects in standardized tests by exploiting a unique data set on social networks in Uruguayan primary schools. The identification method enables one to solve the reflection problem via instrumental variables that emerge naturally from the network structure. Correlated effects are controlled for via classroom fixed effects. I find significant endogenous effects in reading, math and to a less extent in science: a one–standard deviation increase in peers' scores increases own scores by about 40 percent of a standard deviation. Simulation exercises show that, when schools are stratified by socioeconomic status, peer effects may amplify educational inequalities. *JEL: I21, I24, O1.*

Keywords: peer effects; networks; education; inequality

1 Introduction

Because peer effects constitute a form of externality, they are of particular relevance to welfare-enhancing policies (Durlauf, 1998; Hoxby, 2000; Glaeser and Scheinkman, 2001). Significant levels of peer influence can have policy implications not only in terms of efficiency but also of inequality. In fact, educational policies ranging from tracking to desegregation programs have been justified in terms of presumed peer effects.[1]

The dependence of individual behavior on peers' behavior can generate a social multiplier or positive feedback loop and can also lead to multiple equilibria (Manski, 1993; Glaeser, Sacerdote and Scheinkman, 2003; Soetevent, 2006). Since social interactions are likely to influence schooling decisions, study habits, and individual aspirations, it follows that socioeconomic stratification in the establishment of social networks has serious implications for the persistence of educational disparities and of broader social inequalities across generations

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[1] In the United States desegregation plans were prompted by the 1954 Supreme Court decision (Brown v. Board of Education) that declared it illegal to segregate schools by race—and later by the Coleman (1966) report that concluded racial segregation has a negative effect on the educational achievement of minority children. Some more recent studies (Guryan, 2004; Card and Rothstein, 2007) have provided some evidence in favor of this hypothesis. Today, there are many countries implementing desegregation programs; most notable is India's nationwide program, the Right to Education Act, which reserves one fourth of private schools placements for disadvantaged children. In turn, tracking has been promoted under the assumption that a high-achieving peer has more effect on another high-achieving student than on a low-achieving student and under the assumption that more homogeneous levels in classrooms allow teachers to target instruction accordingly with students' needs.

(Benabou, 1996; Durlauf, 1996, 2004; Bowles, Loury, and Sethi, 2007; Graham, 2011). Moreover, the search for valuable social interactions can lead to inefficient stratification (Benabou, 1993, 1996; Zanella, 2007).

That being said, much debate has addressed the actual relevance of peer effects especially given the identification challenges posed by any study of social interactions and there is still no consensus on their magnitude. This paper assesses the impact of peer effects in test scores by applying an identification strategy recently developed in three independent papers: Bramoullé, Djebbari and Fortin (2009); De Giorgi, Pellizzari, and Redaelli (2010); and Lin (2010). This strategy exploits information on individual-specific peer groups in which the existence of partially overlapping peers allows for using the characteristics of peers' peers (and of peers' peers' peers) as instrumental variables to obtain an exogenous source of variation in peer behavior. By solving the so-called reflection problem, the strategy enables one to isolate the endogenous peer effect. This is especially important because only endogenous effects can generate a social multiplier, and most previous studies have estimated a composite social effect that includes both endogenous and contextual effects.[2]

The intuition behind this framework is that peers' peers, who are not also the students' peers, can only have an impact on that student's outcomes indirectly by influencing the outcomes of her peers. Including classroom fixed effects allows me to control for the self-selection of students into schools and for unobserved shocks at the class level. The paper shows that, within a given class, there seems to be no self-selection into groups of peers with similar socioeconomic background.

The main contribution of this paper is to identify endogenous peer effects in test scores using a data set that provides information on individual-specific reference groups for the first time in a developing country. It is also the first data set that provides information on individual-specific reference groups for primary school students. To the best of my knowledge, the only other data set with similar characteristics is the National Longitudinal Study of Adolescent Health (Add Health).[3] I use a data set of schools in Uruguay (not previously

[2] A social multiplier or feedback loop occurs when the direct effect of an improvement in one characteristic of an individual has an indirect effect on other individuals through social interactions (Soetevent, 2006).

[3] Calvó-Armengol, Patacchini, and Zenou (2009) and also Lin (2010) use the information in Add Health's social networks to study peer effects in education. Bramoullé et al. (2009) also use the Add Health data set to study peer effects on the consumption of recreational services; Fortin and Yazbeck (2011) use it to study peer effects in fast-food consumption. De

employed for research purposes) that is representative at the country level for sixth-grade students (last year of primary school). Students self-report whom they would like to invite to their house to play and whom they would like to work with for a school assignment. The data present a heterogeneous scenario of schools and students and, most importantly, provide enough variability to allow drawing inferences. Another significant advantage of this data set—compared with those used in most studies that analyze peer effects in test scores—is that here the tests on reading, math, and science were devised and scored by the national educational authority and so are not biased by teachers' perceptions and/or preferences. In this way, each student took the same three tests.[4] Moreover, the data set used in this paper give a very precise idea of what the real peer group is and yield individual-level information not available elsewhere about network formation in different activities (leisure and study). The paper's second contribution is to analyze, by means of a simulation exercise, the possibility that peer effects act to amplify educational inequality. That is, I argue that, in a context of socioeconomic stratification in which students are assigned to schools according to their neighborhood of residence, peer effects widen the educational gap.

I find strong evidence of endogenous effects for both reading and math, and to a less extent for science. A one–standard deviation increase in peers' scores increases the student's scores by 46 percent of a standard deviation in reading (and 42 percent of a standard deviation in math). This effect is smaller than, but comparable to that of having a mother who completed college. In contrast, contextual effects seem not to be significant. The endogenous effects estimated lie between those obtained by Graham (2008) for kindergarten students and those reported by Lin (2010) for adolescents, suggesting that peers' influence on academic achievement decreases with age.

I then employ a simulation exercise to illustrate the extent to which peer effects amplify educational inequality. I estimate that if peers were assigned randomly, then the standard deviations of reading and math scores would decrease by 4.5 percent and 10 percent, respectively. The findings reported here do not directly support any particular policy intervention but do demonstrate that peer effects in learning should be taken into account when designing

Giorgi, Pellizzari and Redaelli (2010) apply a similar strategy to study the influence of classmates on a student's choice of college major at Bocconi University.

[4]Add Health contains information on students' grade-points averages (GPAs).

any educational policy ranging from the decision of where to build a new school—in a system in which students are assigned to the nearest school from their house—to more complex policies.

The paper is organized as follows. Section 2 reviews the main empirical literature on peer effects in education, Section 3 discusses the identification strategy, and Section 4 describes the data. Section 5 reports the main results; Section 6 provides some alternative specifications. Section 7 analyzes the implications of peer effects in a context of socioeconomic segregation, and Section 8 concludes.

2 Related literature

Although peer effects in education have been studied since the 1960s, there is still no consensus on their relevance (Soetevent, 2006). In the last two decades, the empirical literature on peer effects has been subjected to powerful criticisms regarding identification issues raised by Manski (1993, 2000), Moffitt (2001), and Brock and Durlauf (2001). Several studies have attempted to address these econometric challenges, but the evidence on the relevance of peer effects remains mixed.

A first challenge is to isolate peer effects from correlated effects that arise from sorting and/or unobserved omitted variables.[5] In addition, the study of social interactions involves a simultaneity problem or reflection problem: the presence of exogenous effects implies that characteristics affect not only each individual's outcome but also each peer's outcome, but the researcher observes only the equilibrium outcome in which all the individuals' outcomes are jointly determined (Soetevent, 2006). Hence it is extremely hard to find an exclusion restriction (i.e., an explanatory variable of individual outcomes that does not affect indirectly peers' outcomes) that would enable one to separate endogenous from contextual effects in a linear-in-means model (Manski, 1993).[6] In other words, the structural parameters cannot be

[5] As was initially pointed out by Manski (1993), there are three possible effects that can account for similar behavior within a group. First, children may act similarly because they are influenced by their peers' behavior (proxied by outcomes); according to Manski's typology, these are *endogenous* effects. Second, children may attain similar outcomes also because they are influenced by their peers' characteristics. For instance, children may perceive their peers' parents as role models and the involvement of parents in their children's education may also indirectly benefit the children's peers; these are viewed as *exogenous* (or contextual) effects. Finally, children in a class may exhibit similar outcomes owing to the presence of *correlated* effects—as when, for example they are taught by the same teacher or have the same socioeconomic background or are equally motivated to study. Whereas endogenous and exogenous effects reflect the impact of social interactions, correlated effects do not.

[6] In this model (which is standard in the literature) the outcome of an individual is linearly related to his own characteristics,

recovered from the reduced form owing to collinearities between individual and contextual variables. Another challenge is that identifying social interactions is impossible unless the group composition is known (Manski, 1993, 2000). In what follows, I review the main strategies for overcoming these challenges that have been pursued in previous studies.

2.1 Correlated effects

Sacerdote (2001) and Zimmerman (2003) study peer effects in education by exploiting data on randomly assigned college roommates, where the random assignment allows them to separate social interactions from correlated effects. Graham (2008) suggests a novel method for identifying social interactions using conditional variance restrictions. By using experimental data from project STAR, he distinguishes the excess variance due to peer effects from that due to group-level heterogeneity and/or sorting.[7] Graham's estimates suggest a substantial impact of peer quality on kindergarten achievement.

Hoxby (2000) identifies social interactions by exploiting the variation in gender and racial composition of a grade within schools during adjacent years. Lavy and Schlosser (2011) also rely on variation in gender composition across adjacent cohorts, and Ammermueller and Pischke (2009) use changes in composition across classrooms within the same grade. This strategy is useful for isolating correlated effects provided the changes yield sufficient variation (Nechyba, 2006). Other studies use school-by-grade effects (Calvó-Armengol et al., 2009; Lin, 2010) or school-by-grade and student effects (Hanushek, 2003).

2.2 The reflection problem

Many studies do not disentangle endogenous and exogenous effects and therefore estimate a composite social interaction effect (or assume there is but one form of interaction). This is the case in Hoxby (2000), Sacerdote (2001), Zimmerman (2003), Graham (2008), and Ammermueller and Pischke (2009). Yet, it is especially important to isolate endogenous effects because only they can generate a social multiplier. Hanushek et al. (2003) estimate endogenous and exogenous effects separately by instrumenting the peers' score with their

the corresponding mean characteristics of his peers, and their mean outcome.

[7] The experimental aspect of project STAR enables Graham (2008) to assume that teacher quality is distributed randomly across classrooms.

lagged achievement (though they acknowledge the downward bias inherent in that approach). The reflection problem can also be circumvented by specifying a model in which behavior varies either nonlinearly with group mean behavior or linearly with some characteristic of group behavior other than the mean (Manski, 2000; Brock and Durlauf, 2001).

Another possibility is to find an instrumental variable that directly affects the behavior of some but not all the group members. In this way, endogenous and exogenous effects can be disentangled under a partial-population experimental setting whereby the outcome variable of some randomly chosen members of the group is modified exogenously (Moffitt, 2001). That strategy is applied by Bobonis and Finan (2009), who study neighborhood spillovers from induced school participation of children eligible for the PROGRESA program. Cooley (2010) disentangles endogenous and exogenous effects by utilizing the introduction of student accountability policies in North Carolina public schools. These policies imposed an additional cost on low performance and thus affected the effort only of those who perceived themselves to be in danger of failing. Cooley identifies peer spillovers by comparing classrooms that contain varying percentages of "accountable" students with classrooms of otherwise similar composition but in which students were not held accountable. A novel strategy for disentangling endogenous from exogenous effects involves the use of partially overlapping reference groups (Calvó-Armengol et al., 2009; Laschever, 2009; De Giorgi et al., 2010; Lin, 2010). I detail this strategy in Section 2.

2.3 Reference groups

Data constraints often require the reference group to be defined arbitrarily (Nechyba, 2006). Most papers that study peer effects in education assume that individuals interact within broad groups and are affected by an average intragroup externality that identically affects all the members of a grade within a school or classroom. Given the information on social networks available from the Add Health data set, some studies have considered individual-specific reference groups. Lin (2010) assumes that the individuals named by a student as friends are his reference group and Calvó-Armengol et al. (2009) concentrate on the position of each individual named in a social network (the Katz–Bonacich index[8]).

3 Identification strategy

Bramoullé et al. (2009) determine the conditions under which endogenous and contextual effects can be identified when individuals interact through social networks known by the researcher and when correlated effects are assumed to be fixed within groups. In this paper, I follow their identification strategy. The model developed here is an extension of the linear-in-means model of Manski (1993) and Moffitt (2001), but now each individual has his own specific reference group. Let the structural model for any student i belonging to classroom c be as follows:

$$y_{ci} = \alpha_c + \beta \frac{\sum_{j \in P_i} y_{cj}}{p_i} + \gamma x_{ci} + \delta \frac{\sum_{j \in P_i} x_{cj}}{p_i} + \epsilon_{ci}, \qquad E[\epsilon_{ci}|x_{ci}, \alpha_c] = 0. \quad (1)$$

Here y_{ci} is the test score of student i and x_{ci} is a $1 \times K$ vector of individual characteristics (for simplicity, hereafter we assume that there is only one characteristic). Each student i may have a specific peer group or set of nominated friends P_i of size p_i. The term β captures the endogenous or behavioral effect, and δ captures the exogenous effect of peers' predetermined characteristics. I address the problem of correlated effects by introducing classroom fixed effects that capture unobserved variables common to students in the same classroom. This approach allows for correlation between the classroom's unobserved common characteristics (e.g., teacher quality) and observed characteristics such as parental education. However, individual characteristics are assumed to be strictly exogenous after conditioning on the classroom fixed effect.

Let I_c be the identity matrix for classroom c and let ι be the corresponding vector of 1s. Let G be an $n \times n$ interaction matrix for the n students in classroom c, with $G_{ij} = 1/p_i$ if j was named by i and $G_{ij} = 0$ otherwise. Note that G is row-normalized. The model can be

[8] This measure counts, for each node in a given network, the total number of direct and indirect network paths of any length stemming from that node. Paths are weighted by a factor that decays geometrically with path length.

written in matrix notation as:

$$y_c = \alpha_c \iota_c + \beta G_c y_c + \gamma x_c + \delta G_c x_c + \epsilon_c,$$

$\mathrm{E}[\epsilon_c | x_c, G_c, \alpha_c] = 0.$ (2)

Then to eliminate classroom fixed effects, I apply a "within" transformation via pre-multiplying equation (2) by $D_c = I_c - \frac{1}{n_c}\iota_c \iota_c{}'$. That is, I average equation (1) over all the students in i's classroom and then subtract it from i's equation. The structural model can now be written as:

$$\mathrm{D}_c y_c = \beta D_c G_c y_c + \gamma D_c x_c + \delta D_c G_c x_c + D_c \epsilon_c, \tag{3}$$

where the reduced form is:

$$\mathrm{D}_c y_c = D_c (I_c - \beta G_c)^{-1} (\gamma I_c + \delta G_c) x_c + D_c (I_c - \beta G_c)^{-1} \epsilon_c. \tag{4}$$

Bramoullé et al. (2009) show that if the matrices I, G, G^2, and G^3 are linearly independent, then social interactions can be identified. This implies that $E[DGy|x]$ is not perfectly collinear with (Dx, DGx), which means that $(DG^2x, DG^3x, ...)$ are valid instruments for the outcomes of ones' peers.[9] In other words, the characteristics of a student's peers' peers (and of his peers' peers' peers, etc.) who are *not* her peers serve as instruments for the outcomes of her own peers, thus resolving the reflection problem. The intuition behind this framework is that the characteristics of peers' peers who are not the student's peers can have only an indirect impact on the student's behavior by influencing her peers' behaviors. Bramoullé et

[9] These variables have been previously transformed as deviations from their corresponding classroom mean.

al. (2009) note that a sufficient condition for identification is that the network's diameter (i.e., the maximal distance between any two peers in the student network) be no less than 3. This, in turn, requires that there be at least one case in which: i named j, j named k, and k named l; but i named neither k nor l and j did not name l. Nevertheless, the authors demonstrate that identification often holds also in transitive networks as well, in which case it derives from the directed nature of the network. In more general terms, social effects can be disentangled as long as there is some variation in reference groups. In this paper, identification is based on both the existence of partially overlapping groups (links of distance 3 or more) and on the network's directed nature (i.e., the direction of influence from one node to another).[10]

A crucial identification assumption is that there are no unobserved characteristics that differ among children in a classroom and that also affect both achievement and the likelihood of becoming friends. For instance, if the most able children become friends among themselves and attain better scores than the rest of the class, then the networks will not be exogenous conditional on α_c and x_c and so estimates of social interactions will be inconsistent. Alternatively, if highly disruptive children tend to interact mostly with other disruptive children and also score poorly (owing to this unobserved characteristic and not due to their peers' influence), then inconsitent estimates would again result. Of course, it is not feasible to test whether there is self-selection in terms of unobservables. The following section presents evidence suggesting that at least there is no selection in terms of observables related to parental background.

4 Data

The analysis is based on a unique data set: the fifth Evaluación Nacional de Aprendizajes, which took place in October 2009 and comprises a 322-school sample (24 percent of Uruguay's schools) in which approximately 8,600 students were evaluated. The sample is representative of sixth-grade students (the last grade in primary school, students 11–12 years old) and covers children in both private and public schools. The evaluation consists of math, science, and

[10] If student A names B but B does not name A, then B is viewed as A's peer but A is not viewed as B's peer.

reading tests based on Item Response Theory. Tests were created and scored by ANEP, the central authority responsible for education in Uruguay.[11] This is a major advantage compared to data sets in which students are graded by their teachers because teachers' expectations of (or preferences for) their students could distort grading within a class. Every student who was evaluated took the same reading, math, and science test. This evaluation was implemented solely for ANEP's research purposes. That is, students' scores under this evaluation did not have any consequences on their course grades or chances of being admitted to highschool.

The data set includes questionnaire answers from students and their families as well as from teachers and the school principals. Two questions on the students' questionnaire are of particular importance for this study because they provide information on reference groups:

If you were to invite two classmates to play at your house, whom would you invite?

If you were to invite two classmates to work on an assignment for school, whom would you invite?

Figure 1 depicts the network structure resulting from the information provided by answers to these two questions from one actual classroom. Links of distance at least 3 (i.e., that satisfy the identification condition) can be observed.[12] Also, I checked that the matrices I, G, G^2, and G^3 are linearly independent (where G is a matrix that contains all the classroom networks), which is another way of verifying that the identification condition established by Bramoullé et al. (2009) is satisfied.[13]

The reference-group questions mentioned previously dictate that a student name at most 4 peers. This does constitute a limitation, since reference groups exceeding that number are thus not adequately captured. However, it should be taken into account that the problem is not as severe as in studies where nodes are sampled because in this study students name their closest peers first. Considering both questions (party and work), 13 percent named 4 distinct peers who can be identified in the data set (on average they named 2.4 distinct peers).[14] One might expect that students name their closest friends in the "play" question

[11] Administración Nacional de Educación Pública (ANEP).

[12] For example, individual 7 named 8 who named 12 who named 13, 7 did not name either 12 or 13 and 8 did not name 13. In turn, 13 named 9, 14, 2 and 1, none of whom were named by the previous individuals.

[13] This was checked by vectorizing matrices I, G, G^2, and G^3 and verifying that the matrix formed by these four vectors is of rank 4.

[14] It may happen that students named children that either were absent on the date of the evaluation or for whom there is no

but not necessarily in the "work" one but, 65 percent of students repeated at least one peer in the two questions (40 percent repeated the name of one peer and 25 percent repeated the two peers named in the party question in the assignment question, see Table 1).[15]

On average children were named (i.e., were considered part of others' reference-group) 1.7 times in both the play and work question. Students that were named between 1 and 4 times amount to 69 percent in the play question and 66 percent in the work question while 14 percent of students were not named by anyone in either question. This general pattern suggests that children who were named by others as peers are distributed quite uniformly within classrooms—in other words, the whole class did not name the same student. This contributes to identification by increasing the distance in terms of number of links between individuals (since the likelihood of finding links of $distance \geq 3$ would be lower if most of the arrows were pointing toward just a few students). As mentioned before, most children who are named in the work question are also named in the play question; also it is uncommon to be named many times in one question and not at all in the other. Another interesting feature is that the mean of the peer score is higher than that of the individual score. This relation holds even when only the play network is considered, which suggests that being a good student in primary school increases popularity (see Table 2).

Table 3 presents descriptive statistics on the original data set and the final samples for the variables to be used when performing estimates. Even though the family survey provides a wide range of socioeconomic information, there is incomplete information for some students. Naturally, this deficiency complicates the calculation of peer variables. In order to minimize the number of omitted observations, the regressions include only a few variables —all of which have a low percentage of missings and are commonly used in studies on education.[16]

information on family characteristics. Taking into account those students (who cannot be considered in the final estimations), children on average named 2.7 distinct peers, 15 percent named only one peer in the play question and 14.6 named named only one peer in the work question. There are also 249 individuals who are isolated (i.e., who did not name anybody in the two questions).

[15] Note that student i's naming of student j does not imply that the two are actually friends. It might instead be the case that i would like to be friends with j (say, because he admires or likes j even if they are not currently close friends. What matters, however, is that j will likely to exert influence on i for no reason other than i considers j as part of his reference group. The study's identification strategy assumes that children are influenced only by those classmates whom they name.

[16] Table 11 shows estimates including a larger number of regressors and hence employing less observations. The coefficients are similar to the main estimates. Table 12 follows an alternative strategy seeking to include those observations whose own or peers' socioeconomic information is missing. For this purpose, for each set of socioeconomic variables (mother's education, books at home, etc.) a dummy variable indicating that the information is missing is included. Under this strategy endogenous effects are significant for reading and math.

The final sample for each test (math, reading, and science) consists of all individuals for whom we have valid information not only on their score and family characteristics but also on their peers' scores and characteristics as well as on their peers' peers, and their peers' peers' peers characteristics. The number of observations varies in the final data set for each test because the tests were given on separate dates and some (i.e., absent) children did not take them all. The final samples exhibit slightly better socioeconomic characteristics and test scores, though they make up a substantial part (between 75 and 80 percent) of the original samples.

As mentioned in Section 3, the identification strategy would be invalidated if children sort with children who are similar in an unobserved way that is correlated with their academic achievement. In line with Sacerdote (2001), Bayer, Ross, and Topa (2008) and with Drago and Galbiati (2012), I analyze whether there is sorting in terms of observables within a class. I conclude that there is no evidence of sorting in terms of socioeconomic background or academic outcomes. Bayer et al. (2008) remark that this does not prove that there is no sorting on unobservables but it does suggest the assumption is a reasonable one. In these tests, I run two OLS regressions for own socioeconomic characteristics (mother's education index and wealth index) as a function of the corresponding peer characteristic and control for classroom fixed effects. Table 4 shows that neither of the two coefficients turn out to be significant. Table 5 shows complementary descriptive statistics: although 45.5 percent of students whose wealth index is above the class median named only peers whose wealth index is also above that median, also 43.3 percent of students whose wealth index is *below* the class median also named only peers whose mothers' education is above the class median.[17] It can also be seen that students whose mother's education is equal or above the class median have peers similar to those of students whose mother's education is below the class median. The same holds for test scores (see Table 5). In this sense, there does not seem to be self-selection into peers of similar socioeconomic characteristics or school attainment. There is a preference for interacting with individuals of socioeconomic characteristics and scores above the class median but this applies for both those whose own characteristics are above and below the

[17]For instance, 16 percent of students with wealth at or above the class median did not name any peer at or above the class median (ie. only named peers below the class median) while 19 percent of students with wealth below the class median did not name any peer at or above the class median.

class median.

5 Results

In this section I present estimates of peer effects in achievement for reading, math and science standardized tests while following the strategy outlined in Section 3. For computation of the reference group, all distinct peers named in the two questions (play and work) were weighted equally.[18] Table 6 reports OLS estimates with standard errors clustered at the school level both with and without classroom fixed effects.[19] When classroom fixed effects are included, the OLS estimates indicate that endogenous effects are significant only for math (and are very small).

Table 7 presents 2SLS estimates also with standard errors clustered at the school level.[20] Observe that the F-tests of the excluded instruments in the first stage for the math, reading, and science test indicate that weak instruments are not a concern. According to the estimates in Table 7, endogenous effects are large and highly significant for reading, math and science.[21] A one–standard deviation increase in peers' score increases own performance by approximately 40 percent of a standard deviation (46, 42 and 30 percent for reading, math and science, respectively). This is smaller than but still comparable to the effect of having a mother who completed college. These estimates lie between those obtained by Graham (2008) for kindergarten students and those reported by Lin (2010) for adolescents, suggesting that peers' influence on academic achievement decreases with age. Exogenous effects are never significant, suggesting that social interactions operate mainly through peers' actions. A straightforward measure of the social multiplier cannot be computed within this framework: some children are named more often than others, so the aggregate sum of peers' scores is not directly comparable to the sum of individual scores.

[18]Table 15 presents other reference-group specifications.

[19]Clustering at the classroom level does not alter the significance of the estimates. It seemed more reasonable to cluster at the school level because clustering at the classroom level would imply assuming zero error correlation across classrooms within a school. In the final sample there are 395 classrooms or groups in the reading estimates, 392 in the math data set, and 394 for science.

[20]Specifications including a dummy indicating whether the student repeated at least one grade are shown separately due to potential concerns regarding the endogeneity of this variable.

[21]The higher 2SLS than OLS estimates may come as a surprise. Note that De Giorgi et al. (2010) also find a negative bias in the OLS estimates; their explanation applied to this context would suggest the presence of network-specific shocks that work in different directions.

The estimated model is an extension of the standard linear-in-means social interaction model in which student specific reference groups are allowed. This model constrains peer effects to have distributional consequences but no efficiency consequences. As a first attempt to see whether peer effects are heterogeneous among different kinds of students, I estimate peer effects for children with: different levels of mother's education separately. Unfortunately, this reduces the significance of most of the estimates (see Table 8, Table 9 and Table 10). The only endogenous peer effect that is significant for both reading, math and science is the one of children whose mothers have finished primary school but did not complete high school.[22] This could be explained by that category being the largest category in the sample (42 percent of children in the sample share this characteristic). It is interesting that, in reading, the peers' mothers' education (contextual effect) is positive and significant only for children whose own mothers have the lowest education levels. Also, there do not seem to be heterogeneous endogenous peer effects by gender (see Table 8, Table 9 and Table 10). Finally, endogenous effects in reading and math are significant only in public schools (especially ordinary and full-time public schools). However, this could also be due to the fact that ordinary and full-time public schools are the largest categories in the sample.

6 Alternative specifications

In this section I provide some alternative specifications for the previously reported results. Table 11 reports estimates following the same specification as in Table 7 but including additional individual and peer characteristics. This reduces the sample size significantly since for an individual to be included in the estimation her socioeconomic characteristics, her peers' peers' socioeconomic characteristics (and her peers' peers' peers' characteristics) need to be complete. The estimates in Table 11 are similar to those reported in Table 7 although endogenous effects are no longer significant for science.

Table 12 follows an alternative strategy seeking to include those observations whose own or peers' socioeconomic information is missing. For this purpose, for each set of socioeconomic

[22] Also, endogenous effects are large and significant for science for children whose mothers finished secondary or did not finish college but the F-test of the excluded instruments in the first stage is too low in this case.

variables (mother's education, books at home, etc.) a dummy variable indicating that the information is missing is included.[23] Endogeneous effects for reading and math are similar to those reported in the main estimates while endogenous effects for science are no longer significant.

The correlation among the tests (reading, math and science) is around 0.6. The reason why peer effects seem to be less significant for science is a question that should be further explored. One possible explanation is that math and reading tests assess cognitive skills that may improve in response to class interaction with one's peers whereas the science test is likely to contain more questions whose answers require more memory. An interesting fact is that there seems to be somewhat higher levels of motivation toward science, which also is perceived to be less difficult than math or reading (frequencies showing both preferences and perception of degree of difficulty by subject are available upon request).

Table 13 replicates the estimates of Table 7 but while considering only those classrooms in which, among peers, selection on observables (as measured by the correlation between an individual's characteristic and her peers' characteristic at the classroom level) is relatively low. The first three columns of the table present the estimates for individuals for whom the within-classroom correlation between the student's mother education and their peers' mother's education is lower than 0.3. For the reading and math tests, endogenous effects remain significant and large in magnitude while they are no longer significant for science. The next three columns show the estimates for individuals for whom the correlation between being a repeater and having peers who are repeaters is lower than 0.3. In this case, estimates are significant and large in magnitude for all three tests: reading, math, and science.

Table 7 included school-level dummies for mother's education and for peers' mothers' education while using as instruments an index of peers' peers' mothers' education and peers' peers' peers' mothers' education. The instruments are variables with values that range from 1 to 9 and reflect different levels of education. A variable indicating years of education cannot be precisely reconstructed.[24] In Table 14 I perform an additional estimation in which—

[23]Under this strategy on average students named 2.5 distinct peers and only 17 percent named 4 distinct peers.

[24]In the survey mothers were asked to mark yes/no to the following options: (1) did not attend primary, (2) incomplete primary, (3) complete primary, (4) 1 or 2 years of secondary school, (5) 3 years of secondary school, (6) 4 or 5 years of secondary school, (7) complete high school (6 years), (8) incomplete college, (9) complete college.

instead of including dummies for different levels of mother education—I attempt to reconstruct years of schooling with some measurement error.[25] In this way, I express covariates and instruments in exactly the same way. The results for reading and math are quite similar to those in Table 7 for reading and math: endogenous peer effects are large and contextual effects are never significant. Under this specification endogenous effects are not significant for science.

Table 15 reports the endogenous coefficient estimates obtained when considering alternative reference groups. When using the network information contained in only one question (play or work), there are fewer valid observations (fewer students have information on their peers and their peers' peers) and the remaining network is also weakened (many individuals have fewer peers).[26] This may explain the lower F-tests of the excluded instruments for reading when considering the play network and for math when considering the work network. Overall, the endogenous coefficient estimates do not differ substantially across the different specifications, but they are larger when considering only the peers named in the work question than in the play question. This result could be due to children choosing better students as their reference group for study purposes. The mean of peer scores is higher in the work than in the play network. However, most children are named in both questions (only 11 percent were named at least once in the play question and not named in the work question). I also estimate a specification in which a peer who is named in both questions is weighted more than a peer who is named in only one.[27] In this case, the F-tests of the excluded instruments for reading, math, and science always reach acceptable levels, and the estimates are only slightly smaller in magnitude than those reported in Table 7.

7 Potential impact on educational inequality

Social interactions are likely to influence schooling decisions, study habits, and individual aspirations. For this reason, socioeconomic stratification as social networks are forming has a

[25] This variable ranges from 0 to 16. For instance, I assigned 16 years of schooling to mothers who have completed college even though college in Uruguay may take more than 4 years. For answers indicating 1 or 2 years of secondary school, I assumed it was only 1 (i.e., 7 years of schooling).

[26] Recall that a maximum of two peers could be named in each question.

[27] For instance, if a student names A and B in the play question and names A and C in the work question, then the peer score and characteristics are computed while assigning weights of 0.25 to B and C and 0.5 to A.

strong influence on the persistence of educational disparities and on broader social inequalities across generations (Benabou, 1996; Durlauf, 1996, 2004; Bowles et al., 2007; Graham, 2011). In this section, I assess the extent to which inequalities in educational outcomes are amplified by peer effects operating in a context of socioeconomic stratification.

In terms of income distribution, Uruguay is the least unequal country in Latin America; however, inequalities in the Uruguayan educational system are large even when compared to other Latin American countries. In the PISA 2009 math tests, Uruguay achieved the highest mean and the highest scores at the 95th percentile of all the Latin American countries that participated in the tests. But the scores achieved by the 5th percentile of the distribution were lower than those achieved in Chile and Mexico. Furthermore, Uruguay's dropout rates at age 15 are significantly higher than those in Chile.[28] If the same percentage of 15-year-olds attended high school in both countries, then the observed differences between Uruguayan and Chilean test score distributions could be even larger (this is particularly important when one considers that educational inequalities are likely to translate, through wages, into future socioeconomic inequalities). One possible explanation for the larger disparities in test scores in Uruguay is that socioeconomic segregation may be amplifying educational inequality through peer effects. In the Uruguayan public school system, students are assigned to schools according to their neighborhood of residence. This is a critical factor in determining how neighborhood socioeconomic stratification affects education. To illustrate the level of such stratification present in the data set, I computed some simple ANOVA estimates: 42 percent of the variation in the variable that summarizes students' mother's education is due to between-school variance, and 45 percent of the variation in a wealth index (that considers different durable goods a household may own) can also be attributed to differences between schools.

In order to quantify the potential impact of peers on inequality in a context of socioeconomic segregation, I compare the distribution of the actual reading and math scores with the one resulting from reshuffling peers among the sample of children who have the same number of peers.[29] In other words, if an individual originally named 3 peers, then I assign her

[28] In Uruguay, only 82 percent of 15-year-olds attended the educational system; in Chile 97 percent did so (2006 data).

[29] I do not reshuffle among the total data set because the distribution of the number of peers named is not uniformly distributed

randomly 3 new peers that had been named by individuals who in total had named 3 peers (so each of these 3 new peers was named by a different student). In this sense, I maintain the degree of "popularity" (number of times a child is named by others) and the degree of "sociability" (number of peers the child identified) that individuals exhibit in the actual sample. The logic here is that a hypothetical social planner could reassign children to different schools but could not alter how popular and/or sociable they are.[30] I then multiply all the individual characteristics and peer scores and characteristics by the coefficients from the original regressions and add the residuals from the original predicted reading and math scores. Figure 2 compares the actual scores' distributions with the resulting distributions averaged over 100 simulations. As expected, changing actual peers into random peers concentrates the distribution more about its mean and reduces its mass in the high and low achieving tails. The actual reading score has a mean of 512 and a standard deviation of 99, whereas the simulated distribution has (the same mean and) a standard deviation of 94.6; the absolute gap between the 95th and 5th percentiles is reduced from 309.4 to 302.6. The distribution of math scores exhibits a reduced standard deviation (from 100 to 90), and the gap between 95th and 5th percentiles is reduced from 313.1 to 286.7 (see Table 16). A possible explanation for the lack of a greater reduction in inequality is that actual within-school friendship ties are not random: on average students chose better students as peers (this was shown in Table 2 and Table 5). Observe also, that these estimations assume peer effects are homogeneous for all students, the impact of reshuffling students randomly could be much greater if treatment effects were instead heterogeneous among children with different socioeconomic background, in particular, if lower socioeconomic students benefited more from social interactions.

This is an out-of-sample computational experiment that seeks to proxy (in an extreme way) the possible distributional impact of policies that intervene in the determination of socioeconomic interaction environments for individuals. Durlauf (1996) refers to this type of policies as *associational redistribution*: "an interactions-based perspective alters the redistributive focus away from policies designed to equalize per-student expenditure to those

along socioeconomic characteristics. In particular, children belonging to higher socioeconomic strata tend to name slightly more peers. Since children from higher socioeconomic neighborhoods tend to have better scores, it follows that if peers are reshuffled among all individuals in the data set then the mean of the variable for peers' score will increase slightly (given the lower number of peers named by children in poorer neighborhoods) complicating distributional comparisons.

[30] The estimation does rely on the (fairly extreme) assumption that these randomly matched peers would become friends.

that attempt to equalize the total school environment" (Durlauf, 1996, p. 267). I regard this exercise as useful but am aware of its limitations. First, as Piketty (2000) notes, these policies can provoke controversy because most people consider the choice of one's peers to be an area not within the purview of public policy. Second, evidence regarding the impact of desegregation plans is mixed. Rivkin and Welch (2006, p. 1043) review several studies that assess the impact of school desegregation and conclude that the "effects of integration on black students remains largely unsettled. If there is a marginal consensus, it is that effects are probably small, but beneficial." Third, if peer effects operate mainly via friendship networks, then it will be difficult to determine the impact of moving a child from a school whose average student is from low socioeconomic background to a school whose average student is from a higher average background (or vice versa), since it is not certain that the relocated child would establish any links with children of different characteristics. Evidence from the Add Health data set suggests that mere exposure to more heterogeneous schools does not promote interracial integration per se (Moody, 2001). Also, Carrell et al. (2012) find that grouping low ability students with high ability ones has a negative impact on low ability students. Carrell et al. (2012) interpret this result as grouping low ability with high ability students may have provided more opportunities (relative to random assignment) for increased homophily with low ability students becoming friends among low ability students.[31] Finally, this exercise abstracts from teacher behavior changing in response to student reassignment. Duflo et al. (2011) conclude that tracking could favor both high- and low-achieving students because it facilitates teachers' adaptation of their instruction level especially when teachers are incentivized to instruct to the top of the distribution. However, the wages of public school teachers in Uruguay are not linked to their students' achievement.

8 Conclusions

In this paper I apply a recently developed identification strategy to the first data set for a developing country and also for primary school students that contains both data on educa-

[31] An alternative hypothesis the authors mention refers to the potential relevance of the presence of middle ability students in order to generate positive peer effects for the lower ability students.

tional outcomes and individual-specific reference groups. The strategy I apply enables me to solve the reflection problem and thus disentangle endogenous from contextual effects, two social interaction effects with distinct policy implications. The intuition behind this framework is that peers' peers who are not the focal student's peers can only affect that student's behavior indirectly by influencing the behavior of her peers. In other words, it is assumed that peers' peers' characteristics can be excluded from the structural equation explaining a student's scores and thus can serve as instrumental variables that help explain the peers' scores. Correlated effects are handled with by including classroom fixed effects.

The findings reported in this paper indicate significant peer effects in academic achievement at the primary school level. Estimates suggest that there are strong endogenous peer effects (especially in reading and math) and therefore a large social multiplier. A one–standard deviation increase in a student's peers' score increases the focal student's scores by approximately 40 percent of a standard deviation.[32] This magnitude is smaller yet comparable to that of one's mother having completed college. Descriptive statistics show that, unlike at highschool, at primary school being a good student increases the likelihood of being listed as a peer (ie. popularity). This suggests that peer effects in educational outcomes may be more influential at primary school level than in subsequent educational levels.

In contrast, contextual effects do not seem to be significant suggesting that social interactions operate mainly through peers' actions. This finding confirms the results reported by Laschever (2009) and De Giorgi et al. (2010).[33] In turn, Lin (2010) finds that many peers' characteristics are significant determinants of GPA performance.

The high significance of peer effects signals their potential importance as amplifiers of educational inequalities in socioeconomically stratified environments. That is, if it matters whom one interacts with at school, then differences in social environment will contribute to polarized outcomes. According to the exercise performed in Section 7, if peers were assigned randomly then the standard deviation in scores would decrease by roughly 5–10 percent. However, in order to further conclude on this point a non linear in means model should be

[32] The estimated effects seem to be stronger for reading and math than for science. In contrast, Carrell et al. (2009) find strong effects in math and science but not significant effects in foreign language courses among students at the US Air Force Academy.

[33] Laschever (2009) examines how social ties formed during World War I affected a veterans likelihood of having a job in 1930.

tested (which is not straightforward in a network model).

Social interactions can be viewed as affecting individuals' preferences, constraints and expectations (Manski, 2000). However, research on specific mechanisms remains scarce. Some of the most notable contributions in this respect are Akerlof and Kranton (2002), Kremer and Miguel (2007), Lazear (2001), Austen-Smith and Fryer (2005) and Lavy and Schlosser (2011). There is also relevant evidence from other disciplines, including anthropology and social psychology.[34] In further research it would be particularly interesting to explore the mechanisms through which peer effects operate.

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[34] Doise and Mugny (1984) documented that children can solve problems more effectively when working in pairs or small groups than when working alone: the resulting conflict of views enables one child's perceptions and strategy to stimulate the other child to develop new strategies. A widely studied case of peer pressure in the context of educational attainment involves black students discouraging their black peers from excelling academically, which is viewed as 'acting white' behavior (Fordham and Ogbu, 1986). Individuals exposed to such social interactions are discouraged from investing in education because they fear being rejected by their social peer group.

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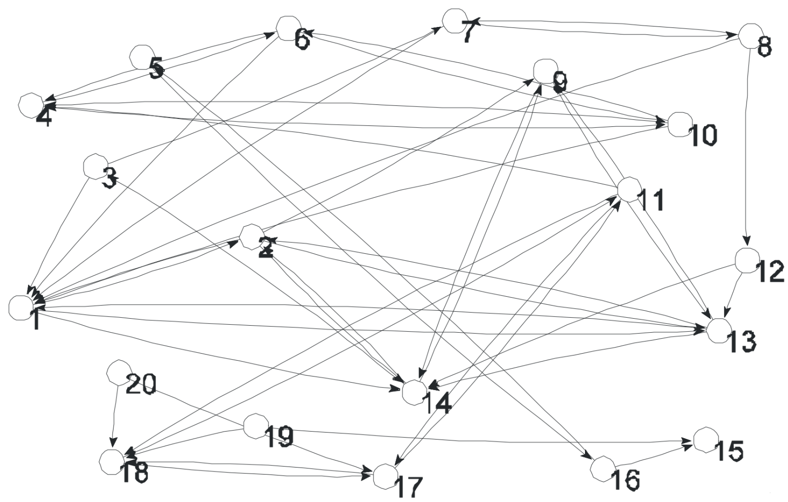

Figure 1: A classroom viewed as a network

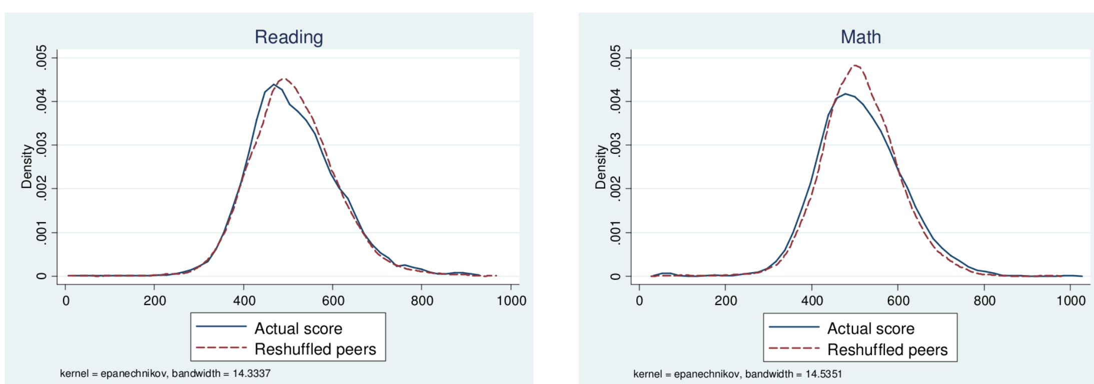

Figure 2: Distributional impact—comparison with random peers

Table 1: Distribution of students according to number of peers named

	Peers in party question	Peers in work question	Total distinct peers	
	Reading final sample			
	Obs	Obs	Obs	%
0	333	265		
1	1920	1887	1147	16.5
2	4700	4801	2930	42.1
3			1973	28.4
4			903	13.0
Total	6953	6953	6953	100
Percentage that named one peer twice				39.5
Percentage that named two peers twice				25.4
	Math final sample			
	Peers in party question	Peers in work question	Total distinct peers	
	Obs	Obs	Obs	%
0	352	274		
1	1997	1962	1204	18.3
2	4244	4357	2799	42.5
3			1806	27.4
4			784	11.9
Total	6593	6593	6593	100
Percentage that named one peer twice				39.9
Percentage that named two peers twice				24.1

Note: Students name peers only once but the two samples (reading and math) have different number of observations because the tests took place at different dates. Reported values for final samples (ie. after dropping observations with incomplete information on own or peer scores and characteristics).

Table 2: Mean individual and peer scores by network

Network	Mean individual score	Mean peer score
Reading		
Play and work	511.6	525.9
Play	514.2	522.7
Work	513.8	534.5
Math		
Play and work	512.5	528.0
Play	515.3	524.3
Work	514.9	537.8
Science		
Play and work	512.0	523.8
Play	514.1	520.9
Work	513.9	531.1
School type (reading scores)		
Private schools	577.1	591.2
Ordinary public schools	516.9	530.0
Full-time (public)	488.4	505.3
Critical social context (public)	463.6	478.2
Rural (public)	476.9	477.9

Note: Reported values for final samples (ie. after dropping observations with incomplete information on own or peer scores and characteristics). Scores are standardized to a mean of 500 and a standard deviation of 100.

Table 3: Descriptive statistics

	Full sample			Reading final sample			Math final sample			Science final sample		
	Obs	Mean	SD	Obs	Mean	SD	Obs	Mean	SD	Obs	Mean	SD
Female	8805	0.49	0.50	6953	0.51	0.50	6593	0.51	0.50	6598	0.51	0.50
Repeated	8781	0.31	0.46	6953	0.26	0.44	6593	0.25	0.44	6598	0.25	0.43
Moth. 1	7722	0.30	0.46	6953	0.28	0.45	6593	0.28	0.45	6598	0.28	0.45
Moth. 2	7722	0.42	0.49	6953	0.42	0.49	6593	0.42	0.49	6598	0.42	0.49
Moth. 3	7722	0.15	0.36	6953	0.16	0.37	6593	0.16	0.37	6598	0.16	0.37
Moth. 4:	7722	0.13	0.33	6953	0.14	0.34	6593	0.14	0.35	6598	0.14	0.35
Reading score	8605	501.6	101.9	6953	511.6	99.0	6593					
Math score	8371	501.6	102.4				6593	512.53	100.08			
Science score	8402	501.1	101.1				6593			6598	512.00	94.98
Numb. peers	8623	2.42	1.04	6953	2.38	0.91	6593	2.33	0.91	6598	2.33	0.91

Notes: The variable Repeated is a dummy for having repeated at least one grade. Moth. 1 is a dummy for having a mother who finished primary school or less, Moth. 2 is a dummy for having a mother with incomplete secondary school, Moth. 3 is a dummy for having a mother that completed secondary school or has incomplete college, Moth. 4 is a dummy for having a mother that completed college. Other variables not included in the final sample in order to minimize loss of observations are: number of persons in the household, information on preschool attendance, number of books at home, a dummy that indicates whether the student lives in a slum and a wealth index that considers different durable goods a household may own.

Table 4: Own socioeconomic characteristics regressed on peer characteristics. Evidence of no selection on observables

	Mother educ. index	Wealth index
Same variable for peers	-0.01	0.03
	(0.02)	(0.03)
Obs	6,953	4,928
Number of clusters	318	309
Classroom fixed effects	Yes	Yes

Notes: The mothers education index ranges from 1 to 9 and summarizes different levels of education (years of education cannot be reconstructed precisely). Standard errors (in parentheses) are clustered at the school level. Both peer scores and own score have been normalized. The wealth index weights different durable goods a household may own through factor analysis. The durables considered are: boiler, washing machine, phone, car, microwave and computer.*** p<0.01, ** p<0.05, * p<0.1

Table 5: Distribution of students' and their peers' characteristics relative to the class median

% of peers	Distribution of students whose wealth is:	
$\geq$ to class median	$\geq$ to class median	$<$ class median
0%	15.89%	19.1%
25%	0.54%	0.56%
33%	5.9%	4.23%
50%	19.12%	21.5%
67%	9.44%	9.17%
75%	3.54%	2.19%
100%	45.56%	43.25%
Total	100%	100%
% of peers	Distribution of students whose mother's education is:	
$\geq$ to class median	$\geq$ to class median	$<$ class median
0%	10.55%	13.82%
25%	0.84%	1.11%
33%	5.65%	6.18%
50%	21.52%	21.78%
67%	12.31%	13.19%
75%	5.17%	5.11%
100%	43.97%	38.81%
Total	100%	100%
% of peers	Distribution of students whose reading scores are:	
$\geq$ to class median	$\geq$ to class median	$<$ class median
0%	16.43%	17.45%
25%	2.03%	1.52%
33%	8.36%	7.49%
50%	26.88%	25.3%
67%	13.45%	13.59%
75%	4.40%	4.40%
100%	28.44%	30.24%
Total	100%	100%
% of peers	Distribution of students whose math scores are:	
$\geq$ to class median	$\geq$ to class median	$<$ class median
0%	15.42%	18.41%
25%	1.44%	1.44%
33%	7.90%	7.21%
50%	24.67%	25.19%
67%	12.57%	12.14%
75%	4.55%	4.55%
100%	33.45%	31.05%
Total	100%	100%
% of peers	Distribution of students whose science scores are:	
$\geq$ to class median	$\geq$ to class median	$<$ class median
0%	17.22%	18.85%
25%	1.73%	1.92%
33%	7.76%	8.07%
50%	25.86%	25.23%
67%	12.71%	11.15%
75%	3.87%	5.03%
100%	30.86%	29.75%
Total	100%	100%

Table 6: OLS estimates

	Reading	Math	Science	Reading	Math	Science
Endogenous effect	0.17***	0.31***	0.27***	0.01	0.08***	0.03
	(0.02)	(0.03)	(0.05)	(0.02)	(0.02)	(0.02)
Own characteristics						
Female	0.13**	0.02	-0.02	0.13**	0.04	-0.01
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)
Mother: incompl HS	0.20***	0.16***	0.20***	0.18***	0.14***	0.19***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Mother: comp HS-	0.57***	0.42***	0.49***	0.48***	0.37***	0.43***
incomp colllege	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Mother: compl college	0.80***	0.66***	0.64***	0.69***	0.61***	0.60***
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)
Contextual effects						
Female	0.01	0.02	0.03	0.05	-0.01	0.01
	(0.06)	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)
Mother: incompl HS	0.20***	0.07*	0.10*	0.15***	0.07*	0.11**
	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)
Mother: comp HS-	0.40***	0.33***	0.33***	0.30***	0.29***	0.29***
incomp college	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.06)
Mother: compl college	0.49***	0.34***	0.26***	0.35***	0.32***	0.31***
	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
Obs.	6953	6593	6598	6953	6593	6598
R-squared	0.22	0.27	0.21	0.06	0.05	0.04
Classroom fixed effects	No	No	No	Yes	Yes	Yes

Standard errors (in parentheses) are clustered at the school level. Both peer scores and own scores have been normalized. *** p<0.01, ** p<0.05, * p<0.1

Table 7: 2SLS estimates

	Reading	Math	Science	Reading	Math	Science
Endogenous effect	0.46***	0.42***	0.30**	0.40***	0.37***	0.22
	(0.09)	(0.10)	(0.14)	(0.11)	(0.13)	(0.16)
Own characteristics						
Female	0.14**	0.04	0.01	0.11*	0.02	-0.01
	(0.06)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)
Repeat				-0.45***	-0.51***	-0.36***
				(0.03)	(0.03)	(0.03)
Mother: incompl HS	0.13***	0.11***	0.16***	0.08***	0.05**	0.12***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Mother: comp HS-	0.42***	0.31***	0.38***	0.33***	0.22***	0.32***
incomp college	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
Mother: compl college	0.59***	0.53***	0.55***	0.51***	0.43***	0.48***
	(0.06)	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)
Contextual effects						
Female	-0.04	-0.01	0.00	-0.04	-0.02	-0.01
	(0.08)	(0.05)	(0.06)	(0.07)	(0.05)	(0.06)
Repeat				0.08	0.12	-0.02
				(0.08)	(0.10)	(0.08)
Mother: incompl HS	0.06	-0.03	0.03	0.04	-0.04	0.02
	(0.05)	(0.05)	(0.06)	(0.04)	(0.05)	(0.06)
Mother: comp HS-	0.03	0.12	0.14	0.02	0.10	0.12
incomp college	(0.10)	(0.08)	(0.11)	(0.09)	(0.08)	(0.10)
Mother: compl college	-0.09	0.07	0.07	-0.07	0.06	0.10
	(0.13)	(0.11)	(0.14)	(0.14)	(0.11)	(0.15)
Excluded instruments (first stage)						
Peers' peers motheduc	0.09***	0.08***	0.09***	0.07***	0.06***	0.08***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Peers' peers peers motheduc	0.11***	0.10***	0.05**	0.08***	0.07***	0.03
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
F-test excluded inst	22.04	17.29	14.75	13.89	11.91	10.38
p-val overidentification test	0.58	0.21	0.86	0.81	0.37	0.94
Obs.	6953	6593	6598	6953	6593	6598
Number of clusters	318	316	318	318	316	318
Classroom fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors (in parentheses) are clustered at the school level. Both peer scores and own score have been normalized. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Heterogeneous effects in reading

	Mother's education				Gender	
	≤ Primary	Incompl highschool	HS-incompl college	Complete college	Females	Males
Endogenous effect	-0.01	0.53***	0.94	-0.08	0.68***	0.44***
	(0.16)	(0.15)	(0.72)	(0.54)	(0.14)	(0.13)
Contextual effects						
Female	-0.02	-0.10	0.10	0.07	0.01	-0.09
	(0.11)	(0.10)	(0.26)	(0.28)	(0.12)	(0.11)
Moth. incompl HS	0.27***	-0.06	-0.23	0.34	-0.02	0.15**
	(0.07)	(0.07)	(0.23)	(0.35)	(0.08)	(0.07)
Mother: comp HS-	0.43***	-0.13	-0.53	0.56	-0.10	0.12
incomp college	(0.16)	(0.14)	(0.39)	(0.41)	(0.16)	(0.13)
Moth. compl college	0.38	-0.15	-0.83	0.40	-0.44*	0.06
	(0.28)	(0.19)	(0.73)	(0.48)	(0.24)	(0.16)
Own characteristics	Yes	Yes	Yes	Yes	Yes	Yes
F-test	12.68	32.33	3.82	2.51	12.45	13.87
Obs.	1924	2919	1038	868	3549	3397
	School type					
	Private	Public ordinary	Public full time	Public critic context		
Endogenous effect	0.25	0.40**	0.42**	0.27		
	(0.65)	(0.16)	(0.16)	(0.28)		
Contextual effects						
Female	-0.01	-0.06	-0.01	-0.02		
	(0.17)	(0.12)	(0.13)	(0.15)		
Moth. incompl HS	-0.24	0.03	0.09	0.15**		
	(0.18)	(0.09)	(0.09)	(0.08)		
Mother: comp HS-	-0.02	0.07	-0.19	0.08		
incomp college	(0.18)	(0.17)	(0.18)	(0.27)		
Moth. compl college	-0.14	-0.02	0.15	-0.44		
	(0.46)	(0.22)	(0.29)	(0.30)		
Own characteristics	Yes	Yes	Yes	Yes		
F-test	1.06	21.66	10.55	9.38		
Obs.	1297	2721	1861	1074		

Standard errors (in parentheses) are clustered at the school level. Both peer scores and own score have been normalized. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Heterogeneous effects in math

	Mother's education				Gender	
	≤ Primary	Incompl highschool	HS-incompl college	Complete college	Females	Males
Endogenous effect	0.27	0.55**	-0.14	0.28	0.46**	0.46***
	(0.23)	(0.23)	(0.49)	(0.86)	(0.19)	(0.13)
Contextual effects						
Female	-0.04	0.02	0.20	-0.19	-0.03	-0.05
	(0.10)	(0.10)	(0.17)	(0.27)	(0.09)	(0.08)
Moth. incompl HS	0.10	-0.16*	0.21	0.07	-0.05	-0.06
	(0.10)	(0.09)	(0.22)	(0.32)	(0.08)	(0.08)
Mother: comp HS-	0.15	-0.02	0.45	0.20	0.22	0.08
incomp college	(0.18)	(0.13)	(0.32)	(0.28)	(0.13)	(0.13)
Moth. compl college	0.13	-0.07	0.67	0.01	0.11	0.05
	(0.28)	(0.21)	(0.40)	(0.27)	(0.22)	(0.16)
Own characteristics	Yes	Yes	Yes	Yes	Yes	Yes
F-test	23.50	17.09	3.82	1.33	20.75	10.39
Obs.	1791	2761	997	844	3363	3222
	School type					
	Private	Public ordinary	Public full time	Public critic context		
Endogenous effect	2.27	0.59***	0.45*	0.62***		
	(2.61)	(0.21)	(0.23)	(0.22)		
Contextual effects						
Female	0.30	0.07	-0.04	-0.10		
	(0.57)	(0.09)	(0.11)	(0.15)		
Moth. incompl HS	-0.09	-0.09	-0.01	-0.09		
	(0.64)	(0.11)	(0.10)	(0.10)		
Mother: comp HS-	-0.12	0.01	0.13	-0.06		
incomp college	(0.67)	(0.17)	(0.15)	(0.24)		
Moth. compl college	-0.59	-0.13	0.39	-0.25		
	(0.96)	(0.23)	(0.33)	(0.25)		
Own characteristics	Yes	Yes	Yes	Yes		
F-test	0.34	15.23	11.53	5.49		
Obs.	1271	2570	1744	1008		

Standard errors (in parentheses) are clustered at the school level. Both peer scores and own score have been normalized. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Heterogeneous effects in science

	Mother's education				Gender	
	≤ Primary	Incompl highschool	HS-incompl college	Complete college	Females	Males
Endogenous effect	-0.09	0.36*	1.30**	0.39	0.52*	0.33
	(0.29)	(0.22)	(0.66)	(0.90)	(0.28)	(0.24)
Contextual effects						
Female	-0.01	0.04	0.20	-0.25	-0.06	0.10
	(0.13)	(0.09)	(0.27)	(0.39)	(0.12)	(0.11)
Moth. incompl HS	0.15	0.04	-0.35	-0.02	-0.03	0.04
	(0.11)	(0.09)	(0.39)	(0.53)	(0.10)	(0.11)
Mother: comp HS-	0.30	0.04	-0.51	0.23	0.02	0.21
incomp college	(0.24)	(0.15)	(0.50)	(0.55)	(0.23)	(0.17)
Moth. compl college	0.45	0.07	-0.70	-0.03	-0.17	0.14
	(0.36)	(0.25)	(0.66)	(0.59)	(0.34)	(0.24)
Own characteristics	Yes	Yes	Yes	Yes	Yes	Yes
F-test	12.57	26.09	5.18	1.15	11.58	15.04
Obs.	1792	2762	995	844	3369	3220
	School type					
	Private	Public ordinary	Public full time	Public critic context		
Endogenous effect	0.34	0.39	0.41	-0.01		
	(0.28)	(0.28)	(0.32)	(0.42)		
Contextual effects						
Female	-0.11	0.04	0.01	0.03		
	(0.16)	(0.10)	(0.12)	(0.16)		
Moth. incompl HS	0.13	-0.08	0.00	0.12		
	(0.17)	(0.11)	(0.17)	(0.13)		
Mother: comp HS-	0.21	0.06	0.00	0.08		
incomp college	(0.18)	(0.22)	(0.24)	(0.26)		
Moth. compl college	0.09	-0.02	0.05	-0.09		
	(0.21)	(0.28)	(0.48)	(0.34)		
Own characteristics	Yes	Yes	Yes	Yes		
F-test	9.99	11.34	6.56	2.46		
Obs.	1276	2565	1750	1007		

Standard errors (in parentheses) are clustered at the school level. Both peer scores and own score have been normalized. *** p<0.01, ** p<0.05, * p<0.1

Table 11: 2SLS estimates including other regressors

	Reading	Math	Science
Endogenous effect	0.38**	0.58**	0.19
	(0.18)	(0.28)	(0.18)
Contextual effects			
Female	-0.03	-0.01	-0.07
	(0.07)	(0.06)	(0.06)
Mother: incompl HS	0.04	-0.10	0.02
	(0.05)	(0.07)	(0.06)
Mother: comp HS-	0.07	0.02	0.15
incomp college	(0.10)	(0.11)	(0.09)
Mother: compl college	-0.02	-0.09	0.07
	(0.17)	(0.17)	(0.14)
Numb. persons in house	0.02	0.01	-0.01
	(0.01)	(0.01)	(0.01)
Books: btw 10 & 50	-0.05	-0.09	0.00
	(0.07)	(0.09)	(0.06)
Books: more than 50	-0.08	-0.16	0.07
	(0.09)	(0.13)	(0.09)
Preschool age 2, 3 or 4	0.05	0.01	0.03
	(0.06)	(0.05)	(0.06)
Preschool age 5 or never	-0.08	-0.10	-0.09
	(0.08)	(0.09)	(0.08)
Slum	-0.04	0.01	0.07
	(0.07)	(0.08)	(0.06)
Own characteristics	Yes	Yes	Yes
F-test excluded inst	17.01	10.56	22.90
Obs.	5674	5369	5375
Classroom fixed effects	Yes	Yes	Yes

Notes: Numb. persons in house is the number of persons in the household. Dummies for the number of books at home are defined as follows: between 10 and 50 books and more than 50 books (less than 10 omitted). Preschool dummies are defined as follows: started attending preschool during age 2, 3 or 4, started attending preschool at age 5 or never attended (attended preschool since age 1 or less omitted). Finally, slum indicates whether the student's house is located in a slum. Standard errors (in parentheses) are clustered at the school level. Both peer scores and own score have been normalized. *** p<0.01, ** p<0.05, * p<0.1

Table 12: 2SLS estimates including students with missing information

	Reading	Math	Science
Endogenous effect	0.33***	0.51***	0.35
	(0.10)	(0.12)	(0.26)
Contextual effects			
Female	0.00	-0.03	-0.15
	(0.06)	(0.05)	(0.23)
Mother: incompl HS	0.03	-0.08*	-0.13
	(0.04)	(0.04)	(0.08)
Mother: comp HS-	0.02	0.02	-0.08
incomp college	(0.07)	(0.07)	(0.13)
Mother: compl college	-0.04	-0.07	-0.07
	(0.09)	(0.08)	(0.17)
Mother: info missing	-0.02	0.04	-0.25
	(0.12)	(0.14)	(0.16)
Books: btw 10 & 50	-0.01	-0.03	-0.06
	(0.05)	(0.05)	(0.06)
Books: more than 50	-0.03	-0.08	-0.06
	(0.07)	(0.07)	(0.07)
Books: info missing	-0.06	-0.04	-0.03
	(0.06)	(0.06)	(0.06)
Numb. persons in house	0.00	-0.01	0.16
	(0.01)	(0.01)	(0.17)
Numb. persons info missing	0.07	-0.02	-0.06
	(0.16)	(0.19)	(0.07)
Preschool age 2, 3 or 4	0.07	-0.02	-0.05
	(0.05)	(0.05)	(0.13)
Preschool age 5 or never	-0.06	-0.13*	-0.02
	(0.07)	(0.07)	(0.05)
Preschool info missing	0.15	-0.04	-0.07
	(0.12)	(0.14)	(0.10)
Slum	-0.04	-0.01	-0.06
	(0.06)	(0.05)	(0.12)
Slum missing	-0.09	-0.10	-0.00
	(0.08)	(0.08)	(0.02)
Wealth index	0.01	-0.01	0.00
	(0.02)	(0.02)	(0.05)
Wealth index missing	0.05	0.04	0.03
	(0.05)	(0.06)	(0.07)
Own characteristics	Yes	Yes	Yes
F-test excluded inst	15.16	16.92	8.59
Obs.	8126	7387	7351

Notes: Numb. persons in house is the number of persons in the household. Dummies for the number of books at home are defined as follows: between 10 and 50 books and more than 50 books (less than 10 omitted). Preschool dummies are defined as follows: started attending preschool during age 2, 3 or 4, started attending preschool at age 5 or never attended (attended preschool since age 1 or less omitted). Finally, slum indicates whether the student's house is located in a slum. The wealth index considers different durable goods a household may own. For each set of socioeconomic variables a dummy variable indicating that the information is missing is included. Standard errors (in parentheses) are clustered at the school level. Both peer scores and own score have been normalized. *** p<0.01, ** p<0.05, * p<0.1

Table 13: Estimations *excluding* classrooms that exhibit some selection on observables among peers

	Classrooms with low correlation among individual's and peers' mother education			Classrooms with low correlation among individual's and peers being repeaters		
	Reading	Math	Science	Reading	Math	Science
Endogenous effect	0.49***	0.49**	0.28	0.48***	0.35***	0.45**
	(0.17)	(0.21)	(0.18)	(0.15)	(0.13)	(0.21)
Contextual effects						
Female	-0.04	0.00	0.00	-0.01	0.02	-0.05
	(0.09)	(0.06)	(0.06)	(0.09)	(0.06)	(0.08)
Mother: incompl HS	0.04	-0.06	0.04	0.07	-0.04	-0.04
	(0.06)	(0.07)	(0.08)	(0.06)	(0.06)	(0.10)
Mother: compl HS-	0.00	0.07	0.12	-0.03	0.03	-0.05
incomp college	(0.13)	(0.12)	(0.13)	(0.13)	(0.10)	(0.15)
Mother: compl.	-0.09	-0.02	0.08	-0.06	0.08	-0.10
college	(0.20)	(0.18)	(0.19)	(0.21)	(0.14)	(0.24)
Own characteristics	Yes	Yes	Yes	Yes	Yes	Yes
F-test excluded inst	18.89	15.14	19.65	19.65	10.17	14.90
Obs.	6095	5680	5690	4426	4127	4098
Classroom fixed effects	yes	yes	yes	yes	yes	yes

Standard errors (in parentheses) are clustered at the school level. Both peer scores and own score have been normalized. *** p<0.01, ** p<0.05, * p<0.1

Table 14: Years of schooling instead of school dummies

	Reading	Math	Science
Endogenous effect	0.39***	0.41**	0.16
	(0.15)	(0.18)	(0.19)
Own characteristics			
Female	0.14**	0.04	0.00
	(0.06)	(0.05)	(0.05)
Moth. years of schooling	0.06***	0.05***	0.06***
	(0.01)	(0.01)	(0.01)
Contextual effects			
Female	-0.03	-0.00	0.01
	(0.08)	(0.05)	(0.06)
Moth. years of schooling	-0.00	0.01	0.02
	(0.02)	(0.02)	(0.02)
Excluded instruments			
Peers' peers moth. yrsch	0.05***	0.04***	0.05***
	(0.01)	(0.01)	(0.01)
Own characteristics	Yes	Yes	Yes
F-test excluded inst	24.41	21.34	19.70
Obs.	6953	6593	6598
Classroom fixed effects	Yes	Yes	Yes

Standard errors (in parentheses) are clustered at the school level. Both peer scores and own score have been normalized. *** p<0.01, ** p<0.05, * p<0.1

Table 15: Other reference group specifications

Endogenous effects	Reading	Math	Science
Play network	0.46**	0.37***	0.41**
	(0.19)	(0.12)	(0.18)
F-test excluded inst	6.77	12.66	21.74
Obs.	6458	6057	6054
Work network	0.57***	0.50***	0.19
	(0.17)	(0.17)	(0.15)
F-test excluded inst	24.63	7.91	34.32
Obs.	6529	6160	6141
Weighting peers named twice more	0.43***	0.39***	0.28**
	(0.09)	(0.10)	(0.13)
F-test excluded inst	21.79	17.33	16.48
Obs.	6953	6953	6598
Own characteristics	Yes	Yes	Yes
Contextual effects	Yes	Yes	Yes

Standard errors (in parentheses) are clustered at the school level. Both peer scores and own score have been normalized.*** p<0.01, ** p<0.05, * p<0.1

Table 16: Changes in the distribution of reading and math scores

	Reading		Math	
Percentile	Actual score	After reshuffling	Actual score	After reshuffling
5th	369.4	368.6	367.5	376.2
10th	395.0	397.5	396.0	406.3
15th	414.2	417.3	418.5	427.2
20th	428.7	434.0	432.1	442.4
25th	446.3	448.8	447.2	454.9
30th	453.9	461.5	458.4	466.7
35th	468.4	473.1	472.5	478.3
40th	479.5	484.2	480.4	488.5
45th	488.5	494.9	493.9	498.8
50th	501.5	506.0	505.5	509.1
55th	515.2	517.1	518.7	519.2
60th	528.8	528.9	531.6	530.1
65th	541.1	541.8	544.9	541.8
70th	556.8	555.2	558.0	555.3
75th	572.4	569.1	573.6	568.7
80th	588.9	586.2	592.0	582.4
85th	613.0	606.2	614.4	601.8
90th	642.3	631.4	639.0	625.4
95th	678.8	671.3	680.7	662.9
95th - 5th	309.4	302.6	313.1	286.7