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Peer effects identified through social networks. Evidence from Uruguayan schools

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Abstract - This paper provides evidence on peer effects in educational achievement exploiting for the first time a unique data set on social networks within primary schools in Uruguay. The relevance of peer effects in education is still largely debated due to the identification challenges that the study of social interactions poses. I adopt a recently developed identification method that exploits detailed information on social networks, i.e. individual-specific peer groups. This method enables me to disentangle endogenous effects from contextual effects via instrumental variables that emerge naturally from the network structure. Correlated effects are controlled, to some extent, by classroom fixed effects. I find significant endogenous effects in standardized tests for reading and math. A one standard deviation increase in peers' test score increases the individual's test score by 40% of a standard deviation. This magnitude is comparable to the effect of having a mother that completed college. By means of a simulation I illustrate that when schools are stratified by socioeconomic status peer effects may operate as amplifiers of educational inequalities.

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1 Introduction

As peer effects constitute a form of externality, they are of particular interest to welfare enhancing policies (Durlauf, 1998; Hoxby, 2000; Glaeser and Scheinkman, 2001). If the influence of peers proves to be substantial, this has important implications both in terms of efficiency and inequality. In fact, the alleged existence of peer effects has justified educational policies ranging from tracking to desegregation programs.[1]

Due to the dependence of individual behavior on peers' behavior, peer effects determine a social multiplier or feedback loop and can also lead to multiple equilibria (Manski, 1993; Glaeser, Sacerdote and Scheinkman, 2003 Soetevent, 2006). As social interactions are likely to influence schooling decisions, study habits and individual aspirations, socioeconomic stratification in the formation of social networks can have important implications for the persistence of educational disparities and more broad social inequalities across generations (Benabou, 1996; Durlauf, 1996, 2004; Bowles, Loury and Sethi, 2007; Graham, 2010). Moreover, the search for valuable social interactions can lead to inefficient stratification (Benabou, 1993, 1996; Zanella, 2007.

However, the relevance of peer effects has been largely debated due to the identification challenges that the study of social interactions poses 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 peers' peers (and peers' peers peers) characteristics to be used as instrumental variables to obtain an exogenous source of variation in peers' behavior. In this way, the strategy allows to isolate the endogenous peer effect, that is, the impact of peers' test scores on individual test scores. The intuition behind this framework is that

[1] In the US desegregation plans were prompted by the decision of the Supreme Court in the Brown vs Board of Education that declared illegal to segregate schools by race and later by the Coleman report that concluded that racial segregation deteriorated the educational achievement of minority children (Coleman, 1966). Some recent studies have provided some evidence in favor of this hypothesis (Guryan, 2004; Card and Rothstein, 2007). Nowadays there are many countries implementing forms of desegregation programs, most notably India is currently implementing a nationwide program that reserves 25% of seats in private schools for children of socioeconomically disadvantaged families (Right to Education Act). In turn, tracking has been favored under the assumption that a high achieving peer has more effect on another high achieving student than she has on a low achieving student (single crossing property).

peers' peers, who are not the students' peers, can only have an impact on the students' outcomes indirectly by influencing the outcomes of her peers. By including classroom fixed effects I am able to control for self-selection of students into schools and unobserved shocks at the class level. I also show that within a class there does not seem to be self selection into peers of similar parental education.

I use a data set of primary schools in Uruguay (not used for research purposes so far) that provides information on reference groups. Students self report who they would like to invite to their house to play and who they would like to work with for a school assignment. To the best of my knowledge, the only previous data set with similar characteristics is The National Longitudinal Study of Adolescent Health (Add Health).[2] Both Xu Lin (2010) and Antoni Calvó-Armengol, Eleonora Patacchini, and Yves Zenou (2009) use the information in Add Health's social networks to study peer effects in education.[3] Giacomo De Giorgi, Michele Pellizzari and Silvia Redaelli (2010) apply a similar strategy to study the influence of student's who attended the same classes on student's choice of college major at Bocconi University.

I find strong evidence of endogenous effects for both reading and math whereas peer effects are not significant for science. A one standard deviation increase in peers' scores increases the student's scores by 40 percent of a standard deviation in reading (and 37 percent in math). This is smaller, but comparable to the effect of having a mother that completed college. In turn, contextual effects do not seem to be significant. I then try to assess to what extent peer effects may be amplifying educational inequality in a context in which schools are stratified by socioeconomic status. After reshuffling peers randomly in a simulation exercise, I estimate that the standard deviations of reading and math scores decrease by 4.5 percent and 10 percent, respectively.

The main contribution of this paper is to apply a recently developed identification strategy to a new comprehensive data set which is representative at the country level for students in their last year of primary school. A significant advantage of the data set used in this paper

[2]In that study adolescents were asked to name up to five female friends and five male friends and also describe how much time they had spent together in the last week.

[3]Bramoullé et al. (2009) also use the Add Health data set to study peer effects on the consumption of recreational services while Fortin and Yazbeck (2010) study peer effects in fast food consumption.

relative to most studies that analyze peer effects in test scores is that in this case tests on reading, math and science were externally set and marked by the national educational authority and thereby not influenced by teachers' perceptions and/or preferences. Besides, every student sat for the same tests. [4] Also, the data in this study provides unique information about network formation in different activities (leisure and study) and covers a different age group (11-12 year old) than Add Health. A drawback of this data set relative to the one used in De Georgi Pellizzari and Redaelli (2010), is that in the latter there is random assignment into classes. However, the data set used in this paper provides a much more precise idea of what the real peer group is, it presents a much more heterogeneous scenario of schools and students and it provides enough variability to draw inference. The second contribution of the paper is to analyze more in depth the possible implications of the presence of peer effects as amplifiers of educational inequality by means of a simulation exercise. The findings of this paper do not directly support any policy intervention but highlight that peer effects in learning should be taken into account when evaluating any educational policy ranging from the decision of where to build a new school to more complex policies.

The paper is organized as follows. Section 2 reviews the main empirical literature on peer effects in education and Section 3 discusses the identification strategy. Section 4 describes the data. Section 5 reports the main results. Section 6 provides some alternative specifications. Section 7 analyzes the implications of the existence of peer effects in a context of socioeconomic segregation. Finally, 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). Coleman (1966) analyzed the relative importance of different factors in educational achievement and concluded that what matters most is the educational background of peer students, then teacher quality and then school quality. Coleman's findings inspired several studies in sociology and economics. However, the empirical literature on peer effects has been subjected to powerful criticisms related to identification

[4] In turn, Add Health contains information on students' grade point average.

issues raised by Manski (1993, 2000), Moffitt (2001), and Brock and Durlauf (2001). In the last two decades several studies have attempted to address these econometric challenges but the evidence on the relevance of peer effects is still mixed.

As was initially pointed out by Manski (1993) there are three possible effects that can account for similar behavior within a group. Firstly, children may act similarly because they are influenced by their peers' behavior.[5] According to Manski's typology these are endogenous effects. Secondly, 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 or parents' involvement in their children's education may also indirectly benefit their peers. These effects are denominated exogenous or contextual effects. Finally, children in a class may exhibit similar outcomes because of the presence of correlated effects. That is, they are taught by the same teacher or they all have the same socioeconomic background or share the same motivation towards studying. Endogenous and exogenous effects reflect the impact of social interactions whereas that is not the case with correlated effects. But endogenous effects are conceptually different from exogenous effects. Only endogenous effects can generate a social multiplier, that is, a positive feedback loop in which the direct effect of an improvement in one characteristic of an individual has an indirect effect through social interactions (Soetevent, 2006).

A first challenge is to isolate peer effects from correlated effects that arise from sorting and/or unobserved omitted variables. But the study of social interactions also involves a simultaneity problem or reflection problem: if two individuals affect each other simultaneously it is difficult to isolate the causal effect that one has on the other (Sacerdote, 2001). More broadly, the presence of exogenous effects implies that these characteristics not only affect the individuals' outcome but also the peers' outcome. However, the researcher only observes the equilibrium outcome in which all the individuals' outcomes are jointly determined (Soetevent, 2006). Hence, it is extremely hard to find an exclusion restriction (ie. an explanatory variable of individual outcomes that does not affect indirectly peers' outcomes) and enables one to separate endogenous effects from contextual effects in a linear-in-means

[5] Empirical studies usually proxy behavior with observed outcomes such as test scores.

model (Manski, 1993).[6] In other words, the structural parameters cannot be recovered from the reduced form as a consequence of collinearities between individual and contextual variables. An additional challenge to the study of peer effects is that the researcher should know a priori the group or individuals with whom a student may interact. Indeed, identification of social interactions is not possible when group composition is unknown (Manski, 1993, 2000). In what follows, I review the main strategies that studies have pursued in order to overcome these challenges.

2.1 Correlated effects

Sacerdote (2001) and Zimmerman (2003) study peer effects in education by exploiting data on randomly assigned college roommates. 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 on project STAR, Graham identifies the excess variance due to peer effects from that due to group-level heterogeneity and/or sorting.[7] Graham's estimations suggest a substantial impact of peer quality on kindergarten achievement.

In turn, Hoxby (2000) identifies social interactions by exploiting the variation in gender and racial composition of a grade within a school in adjacent years. Ammermueller and Pischke (2009) use changes in composition across classrooms within the same grade. These strategies are of use for isolating correlated effects as long as such changes provide sufficient variation (Nechyba, 2006). Other studies use school by grade effects (Lin, 2010) or school by grade effects together with student effects (Hanushek, 2003).

2.2 The reflection problem

Many studies do not disentangle endogenous and exogenous effects and thereby estimate a composite social interaction effect or assume one form of interaction only. This is the case in: Sacerdote (2001); Zimmerman (2003); Graham (2008); Hoxby (2000) and Ammermueller

[6] This is the standard model used in the literature in which, the outcome of an individual is linearly related to her own characteristics, the corresponding mean characteristics of her peers and their mean outcome.

[7] The experimental feature of project STAR enables him to assume that distribution of teacher quality is random across classrooms.

et al. (2009). Being able to isolate endogenous effects is of particular importance as only endogenous effects can generate a social multiplier. Hanushek et al. (2003) estimate endogenous and exogenous effects separately by instrumenting the peers' score with their lagged achievement. Boozer and Cacciola (2001) use classmates' past exposure to a class reduction treatment as an instrument for peer achievement. The reflection problem can be overcome also by specifying a model in which behavior varies nonlinearly with group mean behavior or alternatively a model that varies linearly with some characteristic of group behavior other than the mean (Manski, 2000; Brock and Durlauf, 2001).

Another possibility is to use an instrumental variable that directly affects the behavior of some but not all the group members. In this line, endogenous and exogenous effects can be disentangled under a partial-population experiment setting whereby the outcome variable of some randomly chosen members of the group is exogenously modified (Moffitt, 2001). Such strategy is applied by Bobonis and Finan (2009) who study neighborhood spillovers from induced school participation of elegible children to the PROGRESA program. Cooley (2010) disentangles endogenous and exogenous effects through the introduction of student accountability policies in North Carolina public schools. These policies imposed an additional cost on low performance and thereby shifted the effort only of those who perceived themselves to be in danger of failing. Cooley identifies peer spillovers by comparing classrooms with varying percentages of students that are held accountable to classrooms of similar composition where students were not held accountable. A novel strategy for disentangling endogenous from exogenous effects involves using partially overlapping reference groups (Lin, 2010; Calvó-Armengol et al., 2009; De Giorgi et al., 2010; Laschever, 2009). I describe this strategy in depth in Section 3.

2.3 Reference groups

Due to data constraints the reference group is often defined arbitrarily (Nechyba, 2006). In education, most studies assume individuals interact in broad groups and are affected by an average intra-group externality that affects identically all the members of a grade within a school or a classroom. Upon the availability of data on social networks provided by 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 within a grade are her reference group. Calvó-Armengol et al. (2009) concentrate on the position of each individual named in a social network (Katz-Bonacich index).[8]

3 Identification Strategy

Bramoullé et al. (2009) determine the conditions under which endogenous and contextual effects are 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 is an extension of the linear-in-means model developed by 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:

$$\mathrm{y}_{ci} = \alpha_c + \beta \frac{\sum_{j \in P_i} y_{cj}}{p_i} + \gamma x_{ci} + \delta \frac{\sum_{j \in P_i} x_{cj}}{p_i} + \epsilon_{ci}, \qquad E[\epsilon_{ci}|x_{ci}, \alpha_c] = 0 \tag{1}$$

Where y_{ci} is the test score of student i, x_{ci} is a 1xK vector of individual characteristics (for simplicity assume from now onwards 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. β captures the endogenous or behavioral effect while δ reflects the exogenous effect of peers' predetermined characteristics. In order to address the problem of correlated effects, I introduce classroom fixed effects that capture unobserved variables common to students in the same classroom. This assumption allows for correlation between the network's unobserved common characteristics (ie. teacher quality or similar attitude towards studying) and observed characteristics such as parental education. However, individual characteristics are assumed to be strictly exogenous after conditioning on the classroom fixed effect.

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

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

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

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

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

$$\mathrm{D}_c y_c = \beta D_c G_c y_c + \gamma D_c x_c + \delta D_c G_c x_c + D_c \epsilon_c \qquad (3)$$

with the reduced form being:

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

Bramoullé et al. (2009) show that if the matrices I, G, G^2 and G^3 are linearly independent social interactions are identified. This implies $E[DGy|x]$ is not perfectly collinear with (Dx, DGx). If that is so, then $(DG^2x, DG^3x, ...)$ are valid instruments for the outcomes of ones' peers.[9] In other words, the characteristics of the friends' friends of a student (and also friends'friends friends and further) who are not her friends serve as instruments for the outcomes of her own friends, thus solving the reflection problem. The intuition behind this

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

framework is that the characteristics of friends' friends who are not the student's friends can only have an impact on the student's behavior indirectly by influencing the behavior of her friends. Bramoullé et al. (2009) note that a sufficient condition for identification is that the diameter of the network (ie. maximal friendship distance between any two students in the network) is greater than or equal to 3. In a directed network this requires that there is at least one case in which i named j who named k who in turn named l and i did not name k nor l and j did not name l as a friend. However, the authors show that identification often holds in transitive networks as well. In this case identification comes from the directed nature of the network (Bramoullé et al., 2009). In general terms, social effects can be disentangled as long as there is some variation in reference groups. In this paper identification comes from both the existence of partially overlapping groups (links of distance 3 or more) and the directed nature of the network (ie. 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 affect both the likelihood of becoming friends and achievement. 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 estimates of social interactions will be inconsistent. Alternatively, if highly disruptive children tend to interact mostly with disruptive children and also score poorly (due to this unobserved characteristic and not due to their peers' influence), this would also yield inconsistent estimates. Of course, testing whether there is self selection into peers based on unobservables is not feasible. In section 4, I present some evidence that suggests that at least there does not seem to be self selection based on observables.

4 Data

The analysis is based on a unique data set: the fifth Evaluación Nacional de Aprendizajes took place in October 2009 and consists of a sample of 322 schools (24% of Uruguayan schools) in which approximately 8600 students were evaluated. The sample is representative of sixth grade students (children of 11-12 years old, last grade in primary school) and covers

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

children in both private and public schools. The evaluation consists of math, science and reading tests which were externally set and marked by ANEP, the central authority responsible for education in Uruguay.[11] This represents a major advantage compared to data sets in which students are graded by their teachers as teachers may have different preferences or expectations on their students which could influence grading within a class. Every student evaluated took the same reading, math and science test. The data set also includes questionnaires to students, their family, teachers and the principals of the schools.

Two questions in the students' questionnaire are of particular importance for this study as they provide information on reference groups:

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

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

Figure 1 describes the network structure resulting from the information provided by two questions for one actual classroom. Examples of links of distance greater or equal to 3 (that satisfy the identification condition) can be observed.[12] Also, I checked that the matrices I, G, G^2, G^3 are linearly independent (where G is matrix that contains all the classroom networks), satisfying the identification condition established by Bramoullé et al. (2009).[13]

The reference group questions mentioned before determine that a student can name a maximum number of 4 peers. This represents a limitation as the individual's reference group could be larger and then one would not be capturing it completely. Considering both questions (party and assignment) on average children named 2.4 distinct peers who can be identified in the data set.[14] One could have expected that students would name their closest friends in the party question but not necessarily in the assignment one.[15] However, 65% of

[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. 13 in turn, named 9, 14, 2 and 1, who had not been named by the previous individuals.

[13] This was checked by vectorizing matrices I, G, G^2, 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 in the date of the evaluation or that do not have information on family characteristics. Taking into account those students who cannot be considered in the estimations, children on average named 2.7 distinct peers, 15% named only one peer in the party question and 14.6% only named one peer in the assignment question. There are also 249 individuals who are isolated, that is, did not name anybody in the two questions.

[15] Note that the fact a student i named j does not necessarily imply that they are actually friends. It could also be the case that i would like to be friends with j because she admires or likes j even if currently they are not close friends. Nevertheless, what matters is that j is likely to exert influence on i just because i considers j as her reference group. The strategy assumes that children are influenced only by the classmates they name.

students repeated at least one peer in the two questions (40% repeated the name of one peer and 25% repeated the two peers named in the party question in the assignment question, see Table 1).

On average children were named 1.7 times in the party question and also the assignment question (ie. were considered the reference group of others). Table 2 shows the percentage of children named in the two questions and how many times they were named in each. 14% of students were not named by anyone either in the party or the assignment question. In turn, 69% were named between 1 and 4 times in the party question and 66% were named between 1 and 4 times in the assignment question. The general pattern suggests that children who were named by others as peers are distributed quite uniformly within classrooms, that is, the whole class did not name the same student. This contributes to identification as it increases the distance in terms of links between individuals (if all the arrows were pointing towards a few students the likelihood of finding links of distance 3 or more would be lower). As was previously mentioned, most children who are named in the assignment question are also named in the party question and it is not common to be named many times in the party question and to not be named in the assignment question or vice versa. Another interesting feature is that the mean of the average peer score variable is higher than the mean of the individual score. This is so also when only the party network is considered, which could suggest that being a good student increases popularity (see Table 3).

Table 4 presents the descriptive statistics for the selected variables to be used in the estimation for the original data set and the final sample. Even though the family survey provides a wide range of socioeconomic information, not all the students have complete information on all the variables. This is particularly problematic as it complicates the calculation of peer variables. In order to minimize the number of observations that are dropped because of missing information on a certain variable, I include in the regressions only a few variables that have a low percentage of missing and are commonly used in studies on education. The final sample for each test (math, reading and science) consists of all the individuals who have not only valid information on their score and family characteristics but also on their friends' score and characteristics and on their friends' friends, and friends' friends friends characteristics. The number of observations varies in the final data set for each test because tests were implemented in different dates and some children did not sit for all the three tests because they were absent. The final sample exhibits slightly better socioeconomic characteristics and test scores but it is still a substantial part of the original sample (more than 80% of the students that were evaluated).

As mentioned in section 3, the identification strategy would be invalidated if children sort out with children who are similar in an unobserved way which is correlated with their academic achievement. In line with Drago and Galbiati (forthcoming) and Bayer, Ross and Topa (2008) I analyze whether there is sorting on observables and find that this does not seem to be the case for the core indicator of socioeconomic background and predictor of schooling outcomes: mothers' education. As Bayer et al. (2008) argue this does not prove that there is no sorting on unobservables but provides information on whether holding this assumption is reasonable or not. For this purpose, I run OLS regressions for each individual characteristic as a function of the corresponding peer characteristic (Table 5 reports the estimated coefficient for each regression). When classroom fixed effects are included the coefficient on peers' mother education becomes negative and close to zero (approximately -0.1 depending on the education level). Being a repeater is positively related to having friends' who are repeaters. This could be problematic if these variables are correlated to unobservables that also influence scores. However, that repeaters tend to name repeaters could be due to the fact that they have known other repeaters for a longer period relative to the rest of the class and not necessarily due to other unobservables correlated with scores.[16] There is a very high correlation between the students' gender and his/her peers' gender. Table 6 also shows that children of similar socioeconomic background within a class do not seem to sort out. For instance, 44% of students whose mother's education is above the class median named only peers whose mothers' education is also above the class median but 39% of students whose mother's education is below the class median also named only peers whose mothers' education was above the class median. In this sense, assigment to peers within a class seems to be quite random in terms of observable socioeconomic characteristics. It can

[16] Table 11 shows estimates of peer effects excluding classrooms in which the correlation between being a repeater and having peers who are repeaters is high.

also be observed that students who score above or equal to the class median in the reading, math or science have very similar peers compared to students with scores below the class median (see Table 6). This suggests that situations such as high ability students sorting with high ability students or disruptive children that attain low scores interacting only with disruptive children do not seem to prevail.

5 Results

In this section I present estimates of peer spillovers in achievement for reading, math and science standardized tests following the strategy outlined in Section 3. The reference group was computed weighting equally all the distinct peers named in the two questions (party and assignment).[17] Table 7 reports OLS estimates both with and without classroom fixed effects.[18] When classroom fixed effects are included, the OLS estimates suggest endogenous effects are only significant for math and are very small. Table 8 presents 2SLS estimates where standard errors are clustered at the school level.[19] Notice that the F-tests of the excluded instruments in the first stage for the three tests (math, reading and science) indicate that weak instruments are not a concern.

The estimates in Table 8 indicate that endogenous effects are large and highly significant in reading and math whereas they are not significant for science.[20] A one standard deviation increase in peers' reading score increases own performance by 40% of a standard deviation. This is smaller but comparable to having a mother that completed college. It is also similar in magnitude to the impact of having been held back in school at least one year. Endogenous effects are slightly stronger in reading than in math.[21] These estimates are in between those obtained by Graham (2008) for kindergarten students and those reported by Lin (2010) for

[17] Table 13 presents other reference group specifications.

[18] In the final sample there are 395 classrooms or groups in the reading estimates, 392 in the math data set and 394 for science.

[19] Clustering at the classroom level does not alter the significance of the estimates. It seemed more reasonable to cluster at the school level as clustering at the classroom level would imply assuming zero correlation between classrooms within a school.

[20] The correlation among the tests is around 0.6. The reason why peer effects do not seem to be significant for science should be further explored. Math and reading tests assess core cognitive skills which could be improved by interacting in class with ones'peers. In turn, the science test could contain more areas in which more memory is required. An interesting fact is that there seems to be a higher motivation towards science and it is not perceived as difficult as math or reading. Table 9 shows how often children consider that they almost always understand what they are taught. This percentage is higher in science than in math and reading. Also, the percentage of children who consider that they enjoy a lot what they are taught is higher in science than in math and reading.

[21] In turn, Carrell et al. (2008) find stronger effects in math and science and not significant in foreign language courses and physical education among students in the United States Air Force Academy.

adolescents. This could suggest peers' influence in academic achievement decreases with age. A straightforward measure of the social multiplier cannot be computed in this framework as some children are named more times than others hence the aggregate sum of peers' scores is not directly comparable to the sum of individual scores.

Exogenous effects are never significant, suggesting that social interactions operate mainly through peers' actions. This is also the case in the study by De Giorgi et al. (2010) and in Laschever (2009).[22] Cooley (2010) gets some counterintuitive results as for the impact of contextual effects and argues that after conditioning on peer achievement the expected sign of contextual effects is ambiguous. In turn, Lin (2010) finds that many peers' characteristics are significant in explaining GPA performance.

The fact that the 2SLS estimates are higher than OLS may seem unexpected. One reason why the OLS estimates may be biased downwards is due to classical measurement error in peers' scores. Also, it could be due to the presence of heterogeneous peer effects on students' scores. In that case, (consistent) OLS estimates an average effect across all students while the 2SLS estimand is a weighted average of responses to a unit change in treatment for those whose treatment is affected by the instrument (Angrist and Imbens, 1995).[23] The weighting function could be reflecting how the compliers (peers who due to social interactions [either endogenous or exogenous] increase their own scores) are distributed over the range of scores.[24] The fact that 2SLS estimates are larger than OLS could be due to peers effects being larger for those who have peers who are themselves positively affected by other peers (instrument compliers). It should be noted that De Giorgi et al. (2010) also find a negative bias in the OLS estimates. Their explanation applied to this context suggests the presence of network specific shocks that work in different directions.

[22] Laschever (2009) examines how social ties formed during WWI affect a veterans likelihood of employment in the 1930 census.

[23] Two stage least squares can estimate a local average treatment effect in the presence of heterogeneous treatment effects as long as the monotonicity condition is satisfied. This additional restriction requires that the instrumental variable affects treatment intensity in the same direction for everyone (Angrist and Imbens, 1995). There may be heterogeneous effects due to observable characteristics (ie. treatment effects are homogeneous after conditioning for observable characteristics) or alternatively individuals with the same characteristics may have different effects of the treatment.

[24] Angrist and Imbens (1995) show that 2SLS in a framework of variable treatment intensity produces an average of the derivative with the weight given to each possible value of the treatment variable in proportion to the instrument-induced change in the cumulative distribution function of the treatment variable at that point. In addition, 2SLS with covariates generates an average of covariate-specific average causal responses and 2SLS with multiple instruments generates a weighted average of averages causal responses for each instrument. As the above estimated model includes variable treatment intensity, multiple instruments and covariates, the resulting weights are a combination of all these.

6 Alternative specifications

In this section I provide some alternative specifications for the previously reported results. Table 10 presents the results following the same specification as in Table 8 but including the information provided by approximately 700 observations which are not included in the estimates. These students have complete information on their scores and characteristics but do not have valid information on their friends (either because they did not name any or mostly because the peers they named were absent the day of the tests or do not have information on socioeconomic characteristics) and thereby cannot be included in the regression. However, these observations provide valuable information to compute the peers' peers characteristics and peers' peers peers characteristics of other students.[25] The estimated endogenous coefficients are slightly larger than those in Table 8.

Table 11 replicates estimates in Table 8 but just considering classrooms in which selection on observables among peers is low (measured by the correlation between and individual characteristic and peers' characteristic at the classroom level). The first three columns present the estimates for individuals in which the within classroom correlation between the student's mother education and their peers' mother education is lower than 0.3, that is, classrooms where children do not sort into peers with similar socioeconomic background. Peer effects are still significant in reading and large in magnitude. The next three columns show the estimates for individuals in classrooms in which the correlation between being an repeater and having peers who are repeaters is lower than 0.3. Estimates are significant and large in magnitude for reading, math and science.

In Table 8 I include school level dummies for mothers' education and peers' mothers education and use as instruments an index of peers'peers mothers' education and peers' peers' peers' mothers' education. The instruments are variables with values ranging from 1 to 9 that reflect different levels of education but a variable of years of education cannot be reconstructed precisely.[26] In Table 12 I perform an additional estimation in which instead

[25] I then correct peers' peers characteristics and peers' peers peers characteristics for the cases where these observations were named as direct peers by multiplying by a factor that weights peers without considering them. For instance, if A named B who named C and D and D does not name anybody (or names someone who was absent), I use D's information to compute A's peers' peers characteristics but then I correct by a factor that instead of weighting D's peers and C's peers equally when computing B's peers' peers characteristics, it assigns all the weight to C who is the only one who has valid information on his/her friends.

[26] In the survey mothers were asked to mark yes/no to the following options: 1) did not attend primary, 2) incomplete primary,

of including dummies for different levels of mother education I try to reconstruct years of schooling with some measurement error.[27] In this case, I express in exactly the same way covariates and instruments. The results are quite similar to those in Table 8: endogenous peer effects are large for reading and math and not significant for science while exogenous effects are never significant.

Finally, Table 13 reports the endogenous coefficient estimates obtained when considering alternative reference groups. When using the network information contained in only one question (party or assignment) the test of the null hypothesis loses some power as less observations are then valid (less students have information on their peers and peers' peers) and in general the network information is also weakened (many individuals have less peers). Overall the endogenous coefficient estimates do not differ substantially in the different specifications but it is larger and more significant when considering only the peers named in the assignment question than when considering only the peers named in the party question. This could be due to children choosing better students as their reference group for study purposes. The mean of peer scores is higher in the assignment network than that of the party network. However, as shown in Section 4 most children are named in the two questions. Only 11% were named by at least one person in the party question and were not named by anyone in the assignment question. I also estimated a specification in which a peer who is named in both questions is weighted more than one that is only named in either the party question or the assignment question.[28] In this case, the F-tests of the excluded instruments for reading, math and science always reach acceptable levels and the estimates are slightly smaller in magnitude than those in Table 8.

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

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.

[27] This variable goes from 0 to 16. For instance, I assigned 16 years of schooling to mothers who have completed college but college in Uruguay may take more than 4 years. For the case of answers indicating 1 or 2 years of secondary school I assumed it was just 1 (that is, 7 years of schooling).

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

peer effects for children with different levels of mother's education separately. However, when doing so estimates tend to lose significance (see Table 14). The only endogenous effect that is significant for both reading and math is the one for children whose mothers have finished primary school but did not complete highschool. This could be due to the fact that this is the largest category in the sample (42% of children in the sample share this characteristic). It is interesting that the peers' mother education (contextual effect) is positive and significant in reading only for children whose own mothers have the lowest education levels. Endogenous peer effects are significant for both females and males. In reading the endogeneous effect seems to be larger for females whereas it is the opposite case for math.

7 Potential impact on educational inequality

Social interactions are likely to influence schooling decisions, study habits and individual aspirations. For this reason, socioeconomic stratification in the formation of social networks can have important implications for the persistence of educational disparities and more broad social inequalities across generations (Benabou, 1996; Durlauf, 1996, 2004; Bowles, Loury and Sethi, 2007; Graham, 2010). In this section, I try to assess to what extent inequalities in educational outcomes are amplified by peer effects operating in a context of socioeconomic stratification.

Although Uruguay is the least unequal country in terms of income distribution in Latin America, 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 percentile ninety five compared to all the Latin American countries that participated in the tests. But the scores achieved by the percentile five of the distribution were lower than those achieved by Chile and Mexico (both with a lower mean). Furthermore, Uruguay's drop out rates at age fifteen are significantly higher than those in Chile, if the same percentage of fifteen year old students attended high school in both countries, these greater inequalities in test scores observed in Uruguay potentially could be even larger.[29] These severe educational inequalities are likely to translate into greater

[29] In 2006, only 82% of fifteen year olds attended the educational system in Uruguay compared to 97% in Chile.

socioeconomic inequalities in the future through wages. One possible determinant of these high degree of inequality is that socioeconomic segregation may be contributing to amplify inequality through peer effects. In the Uruguayan public school system students are assigned to schools according to their neighborhood of residence. This is particularly important in terms of how neighborhood socioeconomic stratification impacts on education. In order to illustrate the level of socioeconomic stratification present in the data set I computed some simple ANOVA estimates: 42% of the variance in the variable that summarizes students' mother education is due to between school variance and 45% of the variation in a wealth index that considers different durable goods a household may own also is attributed to differences between schools.

In order to try to quantify the potential impact of socioeconomic segregated peers in inequality, 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.[30] That is, if an individual originally had named 3 peers I assign him randomly 3 new peers that had been named by individuals who in total had named 3 peers (each of these 3 new peers was named by different students). In this sense, I maintain the degree of popularity (number of times a child is named by others) and the degree of sociability (children maintain the number of friends they originally had) individuals in the actual sample exhibit. This makes sense as all a hypothetic social planner would be able to do is reassign children to different schools but not alter how popular and/or sociable they are.[31] I then multiply all the individual characteristics and peer characteristics by the coefficients of 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 would make the distribution more concentrated around its mean and would reduce its mass in the top achieving tail and the low achieving tail. The actual reading score has a mean of 512

[30] I do not reshuffle among the total data set because the distribution of the number of peers named is not uniformly distributed along socioeconomic characteristics. In particular, children belonging to higher socioeconomic strata tend to name slightly more peers. As children from higher socioeconomic neighborhoods tend to have better scores this determines that when peers are reshuffled among all individuals in the data set the mean of the peerscore variable slightly increases (because of the lower number of peers named by children in poorer neighborhoods) and thereby complicates distributional comparissons.

[31] Still, the estimation relies on the extreme assumption that these randomly matched peers would become friends.

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 percentile 95 and percentile 5 drops from 309.4 to 302.6. In turn, the distribution of math scores reduces its standard deviation from 100 to 90 and the gap between percentile 95 and percentile 5 drops from 313.1 to 286.7 (see Table 15). One possible reason why the impact in terms of inequality reduction is not larger is that actual friendship ties within schools do not seem to be driven by schooling achievement as was shown in Table 6. Also, notice that these estimations assume peer effects were homogeneous for all students, the impact of reshuffling students randomly could be much greater if in turn treatment effects are 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 intends to proxy in an extreme way which could be the distributional impact of policies intervening in the determination of socioeconomic interaction environments for individuals. Durlauf (1998) defines these type of policies associational redistribution: "...an interactions-based perspective alters the redistributive focus away from policies designed to equalize per-student expenditure to those that attempt to equalize the total school environment." (Durlauf, 1998, p. 267).[32] I regard it as a useful exercise but i am aware of its limitations. First, as Piketty (2000) notes, these policies can be particularly controversial as individuals generally consider the choice of peers as something public policy should not interfere. 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 this makes it difficult to assert the impact of moving a child from a school with a low average socioeconomic background to one with a higher average background or vice versa, as it is not certain whether he/she would establish a link with children of different characteris-

[32] These policies are generally more justified in situations in which equality can be improved without affecting efficiency or when both can be improved. Incorporating the efficiency consequences of different distributions of associations would imply a non linear in means framework which is scarce in the literature of peer effect in education. One recent contribution in this line is that of Graham, Imbens and Ridder (2009).

tics. For instance, evidence from the Add Health dataset suggests simple exposure to more heterogeneous schools does not promote interracial integration per se.[33] Finally, this exercise abstracts from changes in teacher behavior due to student reassignment. Duflo, Dupas and Kremer (forthcoming) conclude that tracking could favor both high and low achieving students as it allows teachers to better adapt their instruction level if they face incentives to teach to the top of the distribution. However, it should be noted that in Uruguay public school teachers' wages are not linked to their students' achievement.

8 Conclusions

In this paper I apply a recently developed identification strategy to a unique data set of primary schools in Uruguay. This strategy enables me to solve the reflection problem and hence disentangle endogenous effects from contextual effects, two social interaction effects with very distinct policy implications. The intuition behind this framework is that peers' peers who are not the student's peers can only have an impact on the student's behavior indirectly by influencing the behavior of her peers. Correlated effects are dealt with by including classroom fixed effects. Standard errors are clustered at the school level.

The findings of this paper point to significant peer effects in academic achievement at primary school level. The estimates suggest there are strong endogenous peer effects: a one standard deviation increase in ones' peers score increases own scores by 40 percent of a standard deviation in reading and 37 percent of a standard deviation in math. This magnitude is smaller but comparable to having a mother that completed college. In turn, contextual effects do not seem to be significant, suggesting that it is the others' achievement what matters for own outcomes and not their characteristics.

The high significance of peer effects signals their potential importance as amplifiers of educational inequalities in socioeconomically stratified environments. That is, if whom one interacts at school with matters and if schools are highly stratified in terms of socioeconomic background, differences in the social environment will contribute to polarization in outcomes. The exercise performed in Section 7 suggests that if peers were assigned randomly, the

[33] See Moody (2001).

standard deviation in scores would decrease roughly between 5% and 10%.

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

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[34] Doise and Mugny (1984) have documented that children working in pairs or in small groups come to solve problems more effectively than when they work alone. This can lead to a conflict of views, in which one child's perceptions and strategy directly stimulate the other's to develop new strategies. A widely studied case of peer pressure in the context of educational attainment is how black peers discourage other blacks from excelling academically by considering it an 'acting white' behavior (Fordham and Ogbu, 1986). Individuals exposed to these social interactions have disincentives to invest in education due to the fact that they may be rejected by their social peer group. Peer effects may even operate on the way teachers react to students. Ferguson (2003) suggests there is evidence that teachers' perceptions, expectations, and behaviors interact with students' beliefs, behaviors, and work habits in ways that help to perpetuate the gap in academic attainment observed between blacks and whites.

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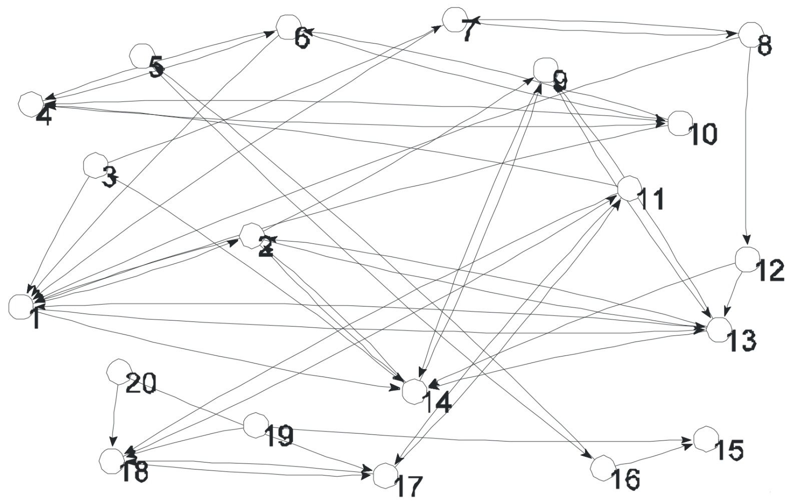

Figure 1: A classroom viewed as a network

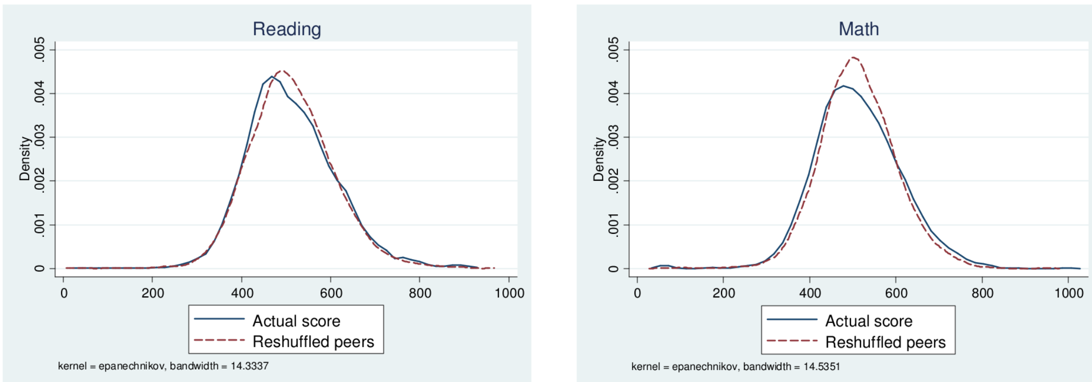

Figure 2: Distributional impact: comparison with random peers

Table 1: Distribution of students (reading final sample)

Distribution of students and number of peers named				
		Assignment question		
Party	0	1	2	Total
0	0	186	147	333
1	181	1144	595	1920
2	84	557	4059	4700
Total	265	1887	4801	6953
Percentage that named one peer twice				
		Assignment question		
Party	0	1	2	Total
1	-	68.2%	51.4%	56.6%
2	-	47.8%	34.3%	35.3%
Total	-	55.4%	35.4%	39.5%
Percentage that named two peers twice				
		Assignment question		
Party	0	1	2	Total
2	-	-	43.4%	37.5%
Total	-	-	36.7%	25.4%

Table 2: Distribution of students according to how many times they are named in the two questions

	Assignment question								
Party	0	1	2	3	4	5	6	7	8
0	14.4%	5.0%	2.1%	0.7%	0.3%	0.1%	0.0%	0.0%	0.0%
1	7.4%	12.8%	5.9%	2.5%	0.9%	0.2%	0.2%	0.1%	0.0%
2	2.8%	7.1%	7.8%	4.0%	1.2%	0.6%	0.1%	0.1%	0.1%
3	0.8%	2.6%	4.1%	2.8%	1.6%	0.7%	0.4%	0.1%	0.1%
4	0.3%	0.7%	1.2%	1.6%	1.1%	0.6%	0.3%	0.2%	0.1%
5	0.1%	0.2%	0.4%	0.5%	0.5%	0.3%	0.2%	0.1%	0.1%
6	0.0%	0.0%	0.1%	0.1%	0.1%	0.1%	0.1%	0.1%	0.0%
7	0.0%	0.0%	0.0%	0.0%	0.1%	0.0%	0.1%	0.1%	0.0%
8	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%

Distribution in final sample after dropping observations with incomplete information.

99.7% of observations reported on this table, the remainder was named more than 8 times in one question.

Table 3: Mean individual and peer scores by network

Network	Mean individual score	Mean peer score
Reading		
Party and assignment	511.6	525.9
Party	514.2	522.7
Assignment	513.8	534.5
Math		
Party and assignment	512.5	528.0
Party	515.3	524.3
Assignment	514.9	537.8
Science		
Party and assignment	512.0	523.8
Party	514.1	520.9
Assignment	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

Table 4: Descriptive statistics

	Full sample			Final sample		
	Obs	Mean	SD	Obs	Mean	SD
Female	8805	0.49	0.50	6953	0.51	0.50
Repeated (1 or more ys)	8781	0.31	0.46	6953	0.26	0.44
Mother: $\leq$ primary	7722	0.30	0.46	6953	0.28	0.45
Moth: incompl HS	7722	0.42	0.49	6953	0.42	0.49
Moth: HS-incompl college	7722	0.15	0.36	6953	0.16	0.37
Moth: compl college	7722	0.13	0.33	6953	0.14	0.34
Reading score	8605	501.6	101.9	6953	511.6	99.0
Math score	8371	501.6	102.4	6953	511.5	100.1
Science score	8402	501.1	101.1	6598	512.0	95.0
Number of peers named	8623	2.42	1.04	6953	2.38	0.91
Other variables in the data set no included to minimize loss of observations						
Father: $\leq$ primary	7259	0.32	0.47	6489	0.30	0.46
Fath: incompl HS	7259	0.45	0.5	6489	0.45	0.50
Fath: HS-incompl college	7259	0.14	0.35	6489	0.15	0.36
Fath: compl college	7259	0.09	0.29	6489	0.10	0.30
Numb. persons in house	7862	4.92	1.85	6948	4.86	1.80
Books: less 10	6979	0.28	0.45	6208	0.26	0.44
Books: btw 10 & 50	6979	0.35	0.48	6208	0.35	0.48
Books: more than 50	6979	0.37	0.48	6208	0.38	0.49
Slum	7862	0.12	0.32	6742	0.11	0.31

Final sample statistics for reading estimates except for math & science scores.

Table 5: Individual characteristics regressed on peers' characteristics.

	Same variable for peers	Same variable for peers
Mother: $\leq$ primary	0.31***	-0.07***
	(0.02)	(0.02)
Moth: incompl HS	0.16***	-0.11***
	(0.02)	(0.02)
Moth: HS-incompl college	0.19***	-0.12***
	(0.02)	(0.02)
Moth: compl college	0.45***	-0.09***
	(0.01)	(0.01)
Mother educ. index	0.60***	-0.01
	(0.01)	(0.02)
Female	0.97***	0.99***
	(0.01)	(0.01)
Repeated (1 or more ys)	0.44***	0.20***
	(0.02)	(0.02)
Obs	6953	6953
Classroom fixed effects	no	yes

Linear probability model for female and repeated.
Standard errors in parentheses
The mother education index ranges from 1 to 9 and summarizes different levels of education.
Years of education cannot be reconstructed precisely.

Table 6: Distribution of students' and their peers' characteristics relative to the class median

% of peers with mothers' education above or equal to class median	Student with mothers' education above or equal to class median	Student with mother's education below 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%
Obs	4428	2525
Average % of peers above median	68.90%	64.66%
% of peers with reading scores	Student with reading scores	Student with reading scores
above or equal to class median	above or equal to class median	below 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%
Obs	3590	3363
Average % of peers above median	57.44%	58.13%
% of peers with math scores	Student with math scores	Student with math scores
above or equal to class median	above or equal to class median	below 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%
Obs	3405	3188
Average % of peers above median	60.57%	57.92%
% of peers with science scores	Student with science scores	Student with science scores
above or equal to class median	above or equal to class median	below 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%
Obs	3415	3183
Average % of peers above median	58.18%	56.74%

Table 7: OLS

	Reading	Math	Science	Reading	Math	Science
Endogenous effect	0.15***	0.29***	0.25***	-0.02	0.04**	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Own characteristics						
Female	0.12**	-0.00	-0.03	0.11**	0.01	-0.02
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Repeat	-0.45***	-0.51***	-0.36***	-0.48***	-0.54***	-0.37***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Mother: incompl HS	0.14***	0.10***	0.15***	0.11***	0.07**	0.13***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Mother: compl HS-incompl college	0.45***	0.31***	0.40***	0.37***	0.25***	0.35***
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Mother: compl college	0.67***	0.54***	0.54***	0.58***	0.49***	0.52***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Contextual effects						
Female	-0.00	0.01	0.01	0.04	-0.03	-0.01
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Repeat	-0.05	0.10***	-0.01	-0.17***	-0.11***	-0.12***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Mother: incompl HS	0.14***	0.03	0.06	0.09**	0.01	0.06
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Mother: compl HS-incompl college	0.30***	0.25***	0.26***	0.21***	0.20***	0.22***
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)
Mother: compl college	0.40***	0.28***	0.20***	0.28***	0.26***	0.25***
	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)
Observations	6,953	6,593	6,598	6,953	6,593	6,598
R-squared	0.26	0.31	0.23	0.11	0.11	0.07
Classroom fixed effects	no	no	no	yes	yes	yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Own score and peer score normalized.

Table 8: 2SLS

	Reading	Math	Science
Endogenous effect	0.40***	0.37***	0.22
	(0.11)	(0.13)	(0.16)
Own characteristics			
Female	0.11*	0.02	-0.01
	(0.06)	(0.05)	(0.05)
Repeat	-0.45***	-0.51***	-0.36***
	(0.03)	(0.03)	(0.03)
Mother: incompl HS	0.08***	0.05**	0.12***
	(0.03)	(0.02)	(0.03)
Mother: compl HS-incompl college	0.33***	0.22***	0.32***
	(0.04)	(0.04)	(0.04)
Mother: compl college	0.51***	0.43***	0.48***
	(0.05)	(0.04)	(0.05)
Contextual effects			
Female	-0.04	-0.02	-0.01
	(0.07)	(0.05)	(0.06)
Repeat	0.08	0.12	-0.02
	(0.08)	(0.10)	(0.08)
Mother: incompl HS	0.04	-0.04	0.02
	(0.04)	(0.05)	(0.06)
Mother: compl HS-incompl college	0.02	0.10	0.12
	(0.09)	(0.08)	(0.10)
Mother: compl college	-0.07	0.06	0.10
	(0.14)	(0.11)	(0.15)
Excluded instruments (first stage)			
Peers' peers motheduc	0.07***	0.06***	0.08***
	(0.02)	(0.02)	(0.02)
Peers' peers peers motheduc	0.08***	0.07***	0.03
	(0.02)	(0.02)	(0.03)
Observations	6,953	6,593	6,598
F test excluded inst	13.89	11.91	10.38
P-val overidentification test	0.81	0.37	0.94
Number of clusters	318	316	318
Classroom fixed effects	yes	yes	yes

Standard errors clustered at the school level in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Own score and peer score normalized.

Table 9: Degree of difficulty and preferences for reading, math and science

Can you easily understand what is taught in class?

	Reading	Math	Science
Almost always	40.0%	35.7%	44.0%
Sometimes	50.7%	54.1%	47.6%
Almost never	9.4%	10.2%	8.4%

Do you like what is taught in class?

	Reading	Math	Science
Almost always	59.2%	65.0%	67.6%
Sometimes	33.5%	30.1%	25.8%
Almost never	7.3%	4.9%	6.6%

Table 10: 2SLS using additional information

	Reading	Math	Science
Endogenous effect	0.43***	0.40***	0.25
	(0.12)	(0.13)	(0.17)
Own characteristics			
Female	0.10*	0.01	-0.01
	(0.06)	(0.05)	(0.05)
Repeat	-0.44***	-0.50***	-0.35***
	(0.03)	(0.03)	(0.03)
Mother: incompl HS	0.08***	0.06**	0.12***
	(0.03)	(0.02)	(0.03)
Mother: compl HS-incompl college	0.33***	0.22***	0.31***
	(0.04)	(0.04)	(0.05)
Mother: compl college	0.50***	0.43***	0.48***
	(0.05)	(0.04)	(0.05)
Contextual effects			
Female	-0.03	-0.00	0.00
	(0.07)	(0.05)	(0.06)
Repeat	0.10	0.15	0.01
	(0.08)	(0.10)	(0.09)
Mother: incompl HS	0.04	-0.05	0.02
	(0.04)	(0.05)	(0.06)
Mother: compl HS-incompl college	0.01	0.09	0.11
	(0.10)	(0.08)	(0.11)
Mother: compl college	-0.09	0.05	0.08
	(0.14)	(0.11)	(0.15)
Observations	6,953	6,593	6,598
F test excluded inst	13.46	11.62	10.62
P-val overidentification test	0.75	0.37	0.91
Number of clusters	319	320	322
Classroom fixed effects	yes	yes	yes

Standard errors clustered at the school level in brackets.
*** p<0.01, ** p<0.05, * p<0.1
Own score and peer score normalized.

Table 11: Estimations excluding classrooms that exhibit some selection on observables among peers

	Classrooms with low correlations among individual's and peers' parental education			Classrooms with low correlation among individuals and peers being repeaters		
	Reading	Math	Science	Reading	Math	Science
Endogenous effect	0.34**	0.28	0.18	0.42***	0.38***	0.36**
	(0.14)	(0.18)	(0.19)	(0.12)	(0.15)	(0.16)
Own characteristics						
Female	0.11*	0.02	-0.01	0.10	0.01	0.08
	(0.06)	(0.05)	(0.06)	(0.07)	(0.06)	(0.07)
Repeat	-0.46***	-0.50***	-0.36***	-0.44***	-0.50***	-0.35***
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Mother: incompl HS	0.09***	0.07***	0.12***	0.09***	0.05*	0.13***
	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Mother: compl HS-incompl college	0.35***	0.22***	0.32***	0.31***	0.20***	0.27***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Mother: compl college	0.52***	0.44***	0.49***	0.47***	0.45***	0.47***
	(0.06)	(0.05)	(0.05)	(0.07)	(0.06)	(0.06)
Contextual effects						
Female	-0.02	-0.02	-0.02	-0.02	0.01	-0.06
	(0.08)	(0.05)	(0.06)	(0.08)	(0.06)	(0.07)
Repeat	0.04	0.05	-0.06	0.07	0.14	0.01
	(0.09)	(0.13)	(0.09)	(0.08)	(0.11)	(0.09)
Mother: incompl HS	0.04	-0.04	0.03	0.06	-0.07	-0.03
	(0.05)	(0.05)	(0.07)	(0.05)	(0.06)	(0.07)
Mother: compl HS-incompl college	0.04	0.10	0.11	-0.03	-0.02	-0.04
	(0.10)	(0.08)	(0.11)	(0.10)	(0.09)	(0.11)
Mother: compl college	0.02	0.07	0.12	-0.02	0.04	-0.04
	(0.15)	(0.13)	(0.16)	(0.16)	(0.14)	(0.17)
F test excluded inst	7.97	5.92	7.22	9.34	9.51	7.34
P-val overidentification test	0.67	0.56	0.85	0.83	0.20	0.82
Observations	6,095	5,680	5,690	4,426	4,127	4,098
Classroom fixed effects	yes	yes	yes	yes	yes	yes

Standard errors clustered at the school level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Own score and peer score normalized.

Table 12: Years of schooling instead of school dummies

	Reading	Math	Science	Reading	Math	Science
Endogenous effect	0.34**	0.37*	0.09	0.35**	0.33*	0.10
	(0.17)	(0.21)	(0.20)	(0.15)	(0.18)	(0.21)
Own characteristics						
Female	0.12**	0.01	-0.02	0.12**	0.01	-0.01
	(0.06)	(0.05)	(0.05)	(0.06)	(0.04)	(0.05)
Repeat	-0.44***	-0.49***	-0.35***	-0.43***	-0.49***	-0.35***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Moth. years of schooling	0.05***	0.04***	0.05***	0.05***	0.04***	0.05***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Contextual effects						
Female	-0.03	-0.01	-0.01	-0.03	-0.01	-0.01
	(0.08)	(0.05)	(0.06)	(0.07)	(0.05)	(0.06)
Repeat	0.04	0.13	-0.06	0.04	0.10	-0.06
	(0.09)	(0.14)	(0.09)	(.09)	(0.13)	(0.09)
Moth. years of schooling	-0.00	0.01	0.02	-0.00	0.01	0.02
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
Excluded instruments (first stage)						
Peers' peers moth. yearsch	0.05***	0.04***	0.04***	0.04***	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Peers' peers peers moth. yearsch				0.03*	0.02	-0.01
				(0.02)	(0.02)	(0.02)
F test excluded inst	17.50	15.50	15.25	9.50	7.97	7.72
P-val overidentification test				0.83	0.53	0.68
Observations	6,953	6,593	6,598	6,953	6,593	6,598
Classroom fixed effects	yes	yes	yes	yes	yes	yes

Standard errors clustered at the school level in brackets
*** p<0.01, ** p<0.05, * p<0.1
Own score and peer score normalized.

Table 13: Other reference group specifications

Endogenous effects	Reading	Math	Science
Party network	0.37	0.30**	0.31*
	(0.27)	(0.14)	(0.17)
F test	3.21	8.30	8.12
Obs	6,458	6,057	6,054
Essay network	0.56***	0.42**	0.13
	(0.11)	(0.21)	(0.15)
F test	13.69	6.32	14.55
Obs	6,529	6,160	6,141
Weighting peers named twice more	0.37***	0.34**	0.20
	(0.11)	(0.13)	(0.15)
F test	13.96	11.79	12.02
Obs	6,953	6,953	6,598

Standard errors clustered at the school level in brackets.
*** p<0.01, ** p<0.05, * p<0.1
Own score and peer score normalized.

Table 14: Heterogeneous effects

	Mother's education				Gender	
	≤ Primary	Incompl HS	HS-incompl college	Compl college	Females	Males
Reading						
Endogenous effect	-0.20	0.33**	1.49	-0.14	0.59***	0.44***
	(0.23)	(0.14)	(0.89)	(0.61)	(0.16)	(0.17)
Exogenous effects						
Female	0.01	-0.04	0.04	0.07	-0.02	-0.08
	(0.11)	(0.09)	(0.29)	(0.28)	(0.11)	(0.11)
Repeat	-0.29*	0.07	0.77	-0.34	0.12	0.10
	(0.16)	(0.10)	(0.79)	(0.54)	(0.12)	(0.12)
Moth. incompl HS	0.21***	-0.02	-0.20	0.32	-0.03	0.11
	(0.07)	(0.06)	(0.31)	(0.33)	(0.07)	(0.07)
Moth. compl HS-incomp college	0.39**	-0.02	-0.61	0.54	-0.10	0.08
	(0.17)	(0.11)	(0.36)	(0.39)	(0.15)	(0.13)
Moth. compl college	0.44	0.04	-1.20	0.41	-0.37	0.03
	(0.3)	(0.16)	(0.74)	(0.48)	(0.24)	(0.17)
F test excluded instruments	6.4	14.13	2.04	1.20	8.94	7.95
Obs	1924	2919	1038	868	3549	3397
Math						
Endogenous effect	0.18	0.42***	-0.44	0.42	0.35**	0.49***
	(0.21)	(0.23)	(0.54)	(0.97)	(0.17)	(0.17)
Exogenous effects						
Female	-0.05	0.03	0.03	-0.22	-0.08	-0.05
	(0.09)	(0.09)	(0.17)	(0.24)	(0.08)	(0.08)
Repeat	-0.01	0.21	-0.83	0.04	0.12	0.16
	(0.15)	(0.17)	(0.58)	(0.90)	(0.13)	(0.14)
Moth. incompl HS	0.07	-0.11	0.10	0.14	-0.05	-0.09
	(0.08)	(0.07)	(0.22)	(0.32)	(0.06)	(0.08)
Moth. compl HS-incomp college	0.08	0.04	0.33	0.20	0.18*	0.05
	(0.15)	(0.10)	(0.27)	(0.29)	(0.10)	(0.12)
Moth. compl college	0.18	0.05	0.66	0.03	0.16	0.02
	(0.22)	(0.17)	(0.36)	(0.29)	(0.17)	(0.16)
F test excluded instruments	11.31	6.31	2.29	0.63	9.09	8.01
Obs	1791	2761	997	844	3363	3222

Standard errors clustered at the school level in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Own score and peer score normalized.

Table 15: Changes in the distribution of reading and math scores

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