Economics of Education Review 30 (2011) 575–581

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

Peer effects on high school aspirations: Evidence from a sample of close and not-so-close friends

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ARTICLE INFO

Article history:
Received 10 July 2009
Received in revised form 23 January 2011
Accepted 25 January 2011

JEL classification:
I2

Keywords:
High school dropout
Peer effects

ABSTRACT

In this paper we investigate how schoolmates influence high school dropout intentions in Catalonia, Spain. Our analysis uses self-reported friends to identify possible peers by assuming that peer influence flows in one direction in cases where one student identifies another as a friend, but the other does not reciprocate. We first estimate the effects of education aspirations of non-reciprocating friends on students' own education aspirations, with and without conditioning on a large set of personality and cognitive characteristics. We then investigate the extent to which the estimated effects are associated with friends' height, weight, BMI, gender and cognitive ability. The estimated impact of non-reciprocating peers' dropout intentions is small and generally not statistically significant: a 10 percentage point increase in the fraction of non-reciprocating peers that intend to drop out increases students' chances of dropping out by about .2 percentage points.

1. Introduction

Many researchers have found evidence of deleterious effects from dropping out of high school. Dropping out may not only affect job opportunities and wages, but also self-reported health, welfare use, unemployment, and even subjective well-being (Oreopoulos, 2007). While most educators, policy makers, and especially parents, aim to avoid dropout outcomes, there is lack of general consensus on how to discourage this. Family factors, whether genetic or environmental or both, obviously play a key role (e.g. Sacerdote, 2007; Solon, 1992; Zimmerman, 2002). But school-related factors, which are more easily modifiable through policy, may also affect dropping out. One of these factors includes exposure to peers (Figlio, 2007; Hanushek, Kain, Markman, & Rivkin, 2003; Hoxby, 2000).

Research on the importance of influences from classroom peers has been confounded by the fact that students are not often randomly exposed to other students. Most poor families are constrained to public schools attended by other disadvantaged children based on what neighborhoods they can afford, while richer families often have some discretion over whether to attend a public or private school. Parsing out these sorting behaviors from peer effects has been addressed by some researchers using idiosyncratic variation from year to year differences in classroom assignment (e.g. Boozer & Cacciola, 2001; Graham, 2008; Hanushek et al., 2003; Henry & Rickman, 2007; Hoxby, 2000; Lavy & Schlosser, 2007). Hoxby, for example, finds that assignment to a class with relatively more females in the class raises expected test scores for both females and males.

These previous approaches attempt to estimate peer effects from being exposed to particular groups, rather than particular individuals. Exposure to more females, for exam-

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doi:10.1016/j.econedurev.2011.01.004

ple, or more students of a particular type increases the chances of social interactions with them. But peer effects may depend more on the extent to which individuals identify others as friends. Bishop et al. (2004), for example, documents the importance of cliques (e.g. jocks and nerds) in affecting every aspect of high school life. Students identify and often want to emulate attitudes and behaviors of friends while ignoring or even condoning behaviors of others. Cliques are not easily identifiable using basic demographic characteristics. Information to recognize cliques, within the classroom, is needed to measure these types of more direct peer effect. This paper attempts to do so using a unique dataset that identifies classroom friends, along with exogenous classroom variation, and detailed information about personality, motivation, and school satisfaction. Our main approach differs from earlier peer effects research in disentangling the influence on pupil's intention to dropout by matching students to the specific friends within classroom out of the whole composition of classroom. Our classrooms usually comprise of all students in an entire secondary school grade. Students generally spend about 35 h a week with the same 25 classmates each week, so the extent of social interaction is likely very high.

Our survey contains a sample of Catalan (Spanish) students and their self-identified peers in class. Pupils were asked to identify those classmates they consider to have a close relationship with. Often a student identified as a friend also identifies the other as a friend, but in other cases, only one student identifies another student as a friend, but that friend does not reciprocate. Assuming in these asymmetric cases that peer effects flow in one direction – from non-reciprocating friend to friend – allows us to estimate peer effects in the classroom that occur from interaction with self-reported friends, rather than from interaction with overall groups. Rewards associated with tighter relations are greater than those based on a unilateral relationship which indicates proximity between individuals. This finding is in line with previous empirical literature (Alexander & Campbell, 1964; Aloise-Young, Graham, & Hansen, 1994; Vaquera & Kao, 2008; among others). Not surprisingly, the correlation between non-reciprocating friends in dropout intentions is lower than between reciprocating friends. We also explore whether our measured peer effects are driven by particular kinds of student characteristics, such as height and weight. Recent investigations point out the relevance of these anthropometric characteristics on academic outcomes and later on wage earnings profile (e.g. Bowles, Gintis, & Osborne, 2001; Carneiro & Heckman, 2003; Persico, Postlewaite, & Silverman, 2004). Most studies focusing on consequences of height and weight in school conclude that relatively small or heavy students perform worse in school than other students because they are belittled and isolated. In addition, Case and Paxson (2006) point to a likely causal link between a child's height and longer term social-economic outcome, which holds even after conditioning on family wealth, health, and adult height.

The Spanish region where our data originates is of interest in its own right. Dropout rates there are particularly high compared to other regions and other OECD countries. Fig. 1 shows the evolution of the Catalan dropping out rate

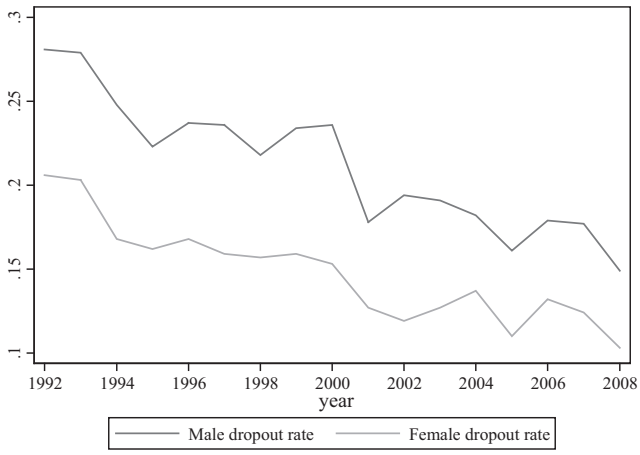

Fig. 1. Schooling dropout rate in Catalonia (1992–2008). Source: Spanish Ministry of Education.

during the period 1992–2008. Although this rate decreased over time, it flattened out after 2002, at around 18% for male students and 12% for female students. Differences by gender are strongly prevalent. The gender differential has also decreased over time (from 8.3% to 4.6% during the period illustrated).

The Spanish governments implemented a series of education reforms during the past twenty years, including raising the minimum school leaving age from 14 to 16 years of age, and reorganizing the secondary school curriculum. Additional decentralization has taken place. By 2003, Spain's 17 regional governments (or Autonomous Communities, ACs) administered more than 90% of the educational budget (MEC, 2006). Likewise, performance problems were not distributed equally among the ACs (see Mora, Escardíbul, & Espasa, 2010). The differences may be attributed to a large extent to the characteristics (level of economic development, nature of the labor market, rural/urban distribution, etc.) of each AC and to the specific educational policies pursued by regional governments.

2. Background

Manski (1993) seminal work is particularly relevant in discussing how non-reciprocating peers can help identify peer effects. Manski isolates four specific reasons why schoolmates might be observed to have similar outcomes. First, schoolmates may behave similarly because they share similar parental or other background characteristics (correlated effects). This arises naturally from schools being related to local housing costs and other demographic patterns. Second, schoolmates are exposed to the same school quality factors that may influence their behavior (ecological factors). Both reasons could explain the correlation between schoolmate outcomes even in the absence of social interaction effects. On the other hand, students may be influenced by whether schoolmates' average parental education levels fall below or above a particular threshold (or tipping point). Social interaction effects, in this case, occur when schoolmates are influenced by the overall distribution of students' background characteristics (contextual effects). Of course, schoolmates may also be

influenced by each other directly (endogenous effects). Endogenous effects are difficult to identify since outcomes are the result of a social feedback or multiplier. For example, Ariel may influence Linda, and Linda in turn may influence Ariel. Manski points out that both interpretation and identification of causal peer effects is impossible without additional assumptions.

Some researchers have used random assignment of roommates or dormmates to plausibly assume independence between social interaction effects and correlated or ecological effects (e.g. Carrell, Fullerton, & West, 2009; Han & Li, 2009; Sacerdote, 2001; Zimmerman, 2003). This approach identifies a reduced form effect that combines contextual and endogenous social interactions, but cannot determine how large the potential social multiplier actually is. For example, Lukas, with his above average SAT score, may influence Jack (who has a lower score) after being randomly assigned to room together. But Jack may also influence Lukas. A linear estimation model of how student outcomes are affected by their roommates' background characteristics therefore helps determine whether social interaction effects exist at all, but cannot identify the extent to which these effects reinforce or offset each other due to countering effects. Perhaps for this reason, the previous work cited above finds significant peer effects when examining random assignment into groups of about 30 students but only modest or no effects when examining random assignment into groups of 2.

Other recent empirical studies rely on within school year-to-year fluctuations in gender or racial composition. Following Hoxby (2000), a few recent papers (e.g. Lavy & Schlosser, 2007), use variation of the fraction female in a class across cohorts and within school to estimate peer effects. But such an approach identifies mainly contextual peer effects from being exposed to a particular group's characteristics. While interesting, it does not allow for a measure of more direct impacts by individuals that students have close relations with. An increase in the fraction of females increases the likelihood of interacting with females but schoolmates can often control the extent of this interaction. A larger female contingent in a class may also matter simply because it reduces the likelihood of disruption in the class (Lavy & Schlosser, 2007).

Dills (2005) uses the introduction of magnet schools, which attract top-performing students from a school district, as an exogenous variation in class composition. Her results show that the loss of high performing peers negatively affects low performing students, although she cannot measure the impact on the children that were enrolled in the magnet school.[1] The fact that low performers are more affected by their peers than high performers resonates with Sund (2009) who obtains similar results in a study of Swedish high school students.

Our approach focuses on measuring peer effects from friends. Thus, unlike many previous studies, we attempt to measure social interaction effects from a group that an individual identifies being especially close to. We focus on non-reciprocating friends because the social interaction effect in this case likely flows mostly in one direction, from the non-reciprocating friend to the one who identified him or her as friend. By 'non-reciprocating', we mean individuals who are identified by a particular student as a friend, but do not themselves report being friends with this individual. This significantly reduces the possibility of a multiplier effect, making the interpretation of the findings clearer. It also allows us to check whether there exists an effect from a non-reciprocating friend's outcome or characteristic on the friend's outcome. Omitted variable bias would bias these estimates upwards. The influence from non-reciprocating friends may be significantly more than that from reciprocating friends if individuals not yet accepted are more motivated to change.[2] Aloise-Young et al. (1994) find some evidence of this by showing that individuals are more likely to change their smoking status if the non-reciprocating friend they identify smokes than if a reciprocating friend does so.

3. Data

The data for this analysis comes from a sample of secondary school students in Catalonia (a large and relatively wealthy region in Spain). The data sampling took place between February and June of 2008. The survey was targeted at secondary students. Approximately 427 math teachers[3] in Catalonia (in four academic years) were approached to participate and help with student data collection. Of those, 65 carried out the survey in class. We then restricted the sample to those in compulsory years of study and to class size greater than 14 students. Note that some classrooms were much smaller as a consequence of their rural location (results were not sensitive to this constraint). The final sample contains information for 2123 students from 53 high schools (for an average class size of about 22). In most cases, the class represents all students in the grade at a given school.

The questionnaire was supplied online. Since not every high school had computer room facilities or enough time available in the computer rooms, a set of math questions, some of those who agreed to participate received the questionnaire in paper format. The questionnaire contained six blocks of questions: personal data, academic characteristics, questions related to the didactics of mathematics, parental background information, conscientiousness (one out of the five big personality components) and motivation questions and finally, lifestyle conditions. Administrative data was collected from school principals in order to describe school-level environment.

[1] O'Shaughnessy (2007) finds that the presence of schools of different quality, especially in the presence of peer effects, is inefficient and creates equity concerns.

[2] See Thibaut and Kelley (1959) for more discussion on this point.

[3] We are indebted to the Catalan Society in Mathematics and namely to Antoni Gomà for supplying contacts for each of the 427 schools participating in an international math competition. So as to avoid sample selection bias in a primary step we decided to contact to all Catalan high schools. We also obtained administrative data from the Catalan Ministry of Education to construct student sample frequency weights, which did not affect the results significantly.

Table 1
Descriptive statistics based on covariates for the intention to leave high school.

	No intention to dropout	Intention to dropout
School year degree (scale 1–4)	1.94 (0.88)	1.89 (0.89)
Pupil's age	15.93 (1.07)	16.22 (1.18)
Gender (female = 1)	0.53 (0.50)	0.39 (0.49)
Immigrant (being immigrant = 1)	0.08 (0.27)	0.12 (0.33)
Having moved residence	0.24 (0.43)	0.32 (0.47)
Marital status parents (not being married)	0.19 (0.39)	0.26 (0.44)
Pupil's health status degree (scale 1–5)	4.39 (0.66)	4.16 (0.82)
Regularly speaking Castilian	0.31 (0.46)	0.44 (0.50)
Pupil's height in m	1.69 (0.09)	1.71 (0.11)
Pupil's weight in kg	59.77 (10.73)	62.87 (13.73)
Pupil's body mass index	20.87 (3.01)	21.51 (3.82)
Having same peers to last year (yes = 1)	0.79 (0.41)	0.70 (0.46)
Number of extracurricular activities	1.15 (0.78)	0.86 (0.70)
Having siblings who graduated (yes = 1)	0.36 (0.48)	0.38 (0.48)
Having siblings who dropped out (yes = 1)	0.03 (0.17)	0.05 (0.22)
Feeling school centre as a family (scale 1–5)	2.35 (0.84)	2.12 (0.89)
Satisfaction score Math (scale 1–5)	3.44 (1.08)	2.91 (1.25)
Satisfaction score Math's teacher (scale 1–5)	3.80 (1.02)	3.53 (1.18)
Satisfaction score Other teachers (scale 1–5)	3.61 (0.72)	3.24 (0.90)
Satisfaction score with evaluation of teachers (scale 1–5)	3.51 (0.85)	2.94 (1.05)
Satisfaction score atmosphere classroom (scale 1–5)	3.89 (0.96)	3.67 (1.06)
Satisfaction score school centre (scale 1–5)	3.19 (0.94)	3.14 (1.06)
Satisfaction score relationship with father (scale 1–5)	4.29 (0.94)	3.91 (1.19)
Satisfaction score relationship with mother (scale 1–5)	4.48 (0.75)	4.16 (1.04)
Satisfaction score with atmosphere at home (scale 1–5)	4.23 (0.88)	3.89 (1.13)
Grade last year in Math (1–5 categories)	3.24 (1.14)	2.36 (1.10)
Number of books at home (scale 0–4)	2.58 (1.40)	2.21 (1.54)
Educational attainment level father (scale 0–7)	3.51 (1.99)	3.03 (1.95)
Educational attainment level mother (scale 0–7)	3.51 (1.87)	3.24 (1.88)
Doing sport activity (yes = 1)	0.72 (0.45)	0.67 (0.47)
Kind of school centre attended (not public = 1)	0.56 (0.50)	0.45 (0.50)

Note: The measures represent mean values and percentages while standard deviation is reported into brackets.

Table 1 shows descriptive statistics for our sample of Catalan high school students under age 16. The survey asked, "Do you expect to leave high school before graduation (yes or no). If yes, express why you feel this way: (i) I have gone as far as I can; (ii) I see no point in going on; (iii) I do not like school; (iv) I need money; (v) I want to work; (vi) My family needs money; (vii) other". We measure dropout intent as someone responding yes to this question. The proportion intending to drop out (18.2%) is only slightly higher than the actual dropout rate measured by the Spanish government for the corresponding 2008 Catalan cohort (15.6%). Not surprisingly, other research indicates the two variables are highly correlated (e.g. Khoo & Ainley, 2005).

The fraction with dropout intentions is markedly different between boys (23.2%) and girls (14.9%). The data also reveals that a recent change in residence or school (or both) is associated with lower school aspirations.[4] In addition, self-assessed health status is strongly associated with school aspirations. The same applies for obese and overweighted pupils.[5] Having siblings who dropped out of high school is associated with a higher intention to leave high school. Table 1 also shows that pupils attending public schools exhibit a higher tendency to leave high school. Finally, we find that regularly speaking Castilian (Spanish) is associated with a higher probability to report intentions of dropping out among respondents for whom first language is Catalan.

4. Econometric strategy

Our analysis uses self-reported friends to identify possible peers. Pupils were given an alphabetical class list and asked to check in corresponding boxes which individuals they "consider to have a close relationship with". We first estimate the effects of education aspirations of non-reciprocating friends on students' own education aspirations. The average number of nominations was 6.6. Of this, the average number of reciprocate nominations was 4.2 whereas the remaining 2.4 were non-reciprocated nominations. Students reporting an intention to drop out had statistically fewer reciprocating friends than those intending to go on, but the average number of non-reciprocating friends was about the same.

The initial model considers the determinants of high school students' intention to drop out (d_i, being a dichotomous variable) for each student (i). As a consequence of the non-reciprocating friends' characteristics, the reduced form can be expressed as:

$$d_i = \beta_0 + \beta_1 \bar{d}_C + hs_i + \varepsilon_i \quad (1)$$

[4] Note that such moving decisions are often driven by poor performances in the child's previous school or because the previous school did not offer upper-secondary studies.

[5] The BMI measure is calculated as the ratio of individual weight, measured in kilograms, to squared height, measured in metres.

where ($\bar{d}_C$) is the fraction among student i's non-reciprocating friends intending to dropout. Eq. (1) includes high school fixed effects (hs_i).[6] This leads to peer effect estimation within schools.[7]

We also condition on a large vector of non-school related background characteristics: age, gender, immigrant, regular spoken language, self-reported health status, having experienced a change in residence, parent's civil status, age difference between parents and student, parental background characteristics, academic year, and number of siblings that graduated or dropped out of high school.

The empirical strategy does not employ a quasi-experimental design but rather relies on the conditional independence assumption (CIA). We believe, however, that the CIA is more likely to hold in our setting than in a typical peer effects analysis that measures peer characteristics as average characteristics across the neighborhood or classroom. It is more plausible, for example, that students of similar background associate themselves with friends of different education aspirations for reasons independent of the outcome than for students of similar background to attend classes where average characteristics differ significantly for reasons independent of the outcome. Nevertheless, we consider a robustness check in the next section as well as discuss the possibility of remaining bias in the conclusion.

Our dataset contains additional information on previously measured cognitive and non-cognitive ability. We take advantage of this by considering the sensitivity of our results from adding additional covariates for previous academic achievement and personality traits. These are last year's grade in math, indicators for a range of extracurricular activities, and self-reported motivation and attention to detail.[8] While these additional covariates may further improve the plausibility of the CIA, they may also be correlated with previous peer influences due to being in earlier classes with many of the same friends as the ones identified in the survey and thus absorb peer effects from previous years.

Finally, we explore more direct peer relationship mechanisms by estimating how specific characteristics of non-reciprocating friends influence dropout intentions. Previous research finds that high school dropout rates differ by height and weight. Case and Paxson (2006) suggest possible mechanisms behind this association occur through greater cognitive achievement, self esteem, social dominance, from being taller or thinner relative to others in a class or group. Smaller or fatter pupils may face more harassment or rejection from classmates (Bishop et al., 2004). Both situations could lead to lower grades and lack of interest in school in general (Cawley & Spiess, 2008; Persico et al., 2004).

We include average body type, BMI, gender and previous year's mathematics grade (ability proxy) of non-reciprocating peer group to estimate the impact on dropout intent from these variables. An advantage of this approach is that classroom variation for many of these characteristics (e.g. height) is plausibly random. Within classroom variation of these variables remains substantial.[9]

5. Empirical results

Table 2 presents marginal probit estimates of the effect of non-reciprocating peer characteristics on students' own intentions of dropping out.[10] Each row considers only one friends' characteristic at one time. Column 1, row 1, shows the estimate of increasing the fraction of non-reciprocating peers that intend to drop out on students' own intentions of dropping out, conditioning on school and pre-school characteristics. The estimated impact of non-reciprocating peers' dropout intentions is small and not statistically significant: a 10 percentage point increase in the fraction of non-reciprocating peers that intend to drop out increases students' chances of dropping out by .2 percentage points (for reference, the table reports that the difference in the fraction of students with dropout intentions between the 25th and 75th percentile student is 20 percentage points). Column 2 shows these estimates after adding a large set of background covariates. The results do not change. In fact, all the measures of peer characteristics among non-reciprocating peers do not significantly impact dropout intentions. We find similar negative and insignificant estimates when restricting the sample to girls with non-reciprocating girl friends and to boys with non-reciprocating boy friends (−0.116, s.e. = 0.08 and −0.033, s.e. = 0.10, respectively).

Next, we check our earlier assumption that peer effects among non-reciprocating friends flow in one direction, from the non-reciprocating friend to the individual who nominated that peer as friend. We do so by exploring whether one's own dropout intentions influence non-reciprocating friends (see column 3, Table 2). We find no evidence of this.

[6] Note that schools are relevant per se since schools centres can push students out. See Lee and Burkam (2003) for a comprehensive survey on the mechanisms between dropping out and the organization and structure of school centres: school structure, academic organizations and social organizations.

[7] Adding local neighborhood fixed effects (captured by the residential postal code and the school centre post zone code, which do not always overlap) does not affect the results.

[8] 7 questions were asked about personality and 15 about motivation. For the former, we used the following questions regarding conscientiousness component: I am exacting in my work; I follow a schedule; I get chores done right away; I pay attention to details; I leave my belongings around; I make a mess of things; I shirk my duties. Note that this personality component is the one related to individual ability. For the later, we considered specific questions from Alonso-Tapia and Arce-Sáez (1992). Then, we computed Cronbach's alpha statistic for the scale formed from the pairs of variables (0.76). A factorial analysis allowed us to construct two factors related to personality and four regards motivation questions. The Kaiser–Meyer–Olkin measure of sampling adequacy depicts a meritorious value (0.81). Factors' scores were re-scaled to variables ranged from 0 to 1 so indicating the probability of being motivated and the degree of personal conscientiousness. These factors were introduced into our regression analysis.

[9] A graphical analysis of the within-class variation of these variables is available on request.

[10] At this juncture, we tested the sensitivity of our results to only consider those students that were under the age of 16 (compulsory education). Since quite similar findings were obtained we do not report this for redundancy reasons.

Table 2
Marginal effects of probit estimation on the probability of reporting dropout intentions, including fixed effects, peers influence and control variables.

	(1) FE + pre-school features	(2) FE + all controls	(3) Reverse estimates: FE and all controls
Fraction of non-reciprocating friends intent on dropping out	−0.026 (0.04)	−0.024 (0.03) [0.244] [0.2]	−0.027 (0.02)
Fraction of non-reciprocating friends who are female	−0.012 (0.02)	0.005 (0.02) [0.384] [0.67]	−0.022 (0.03)
Average height of non-reciprocating friends	0.043 (0.05)	0.038 (0.05) [0.068] [0.07]	0.002 (0.00)
Average weight of non-reciprocating friends	−0.001 (0.00)	−0.001 (0.00) [6.327] [0.72]	0.558 (0.48)
Average BMI of non-reciprocating friends	−0.000 (0.00)	0.000 (0.00) [2.264] [9.24]	0.381 (0.25)
Average last year Math's grade of non-reciprocating friends	−0.012 (0.01)	−0.014 (0.11) [0.812] [1]	−0.037 (0.07)

Notes: Sample size is 2123 observations. All regressions include school fixed effects (FE). We obtained very similar results when including school location fixed effects and residence fixed effects. Each row considers one friends' characteristic at one time. Adjusted robust standard errors for clustering at the classroom level are reported in parenthesis. a, b and c denote significance at 1%, 5% and 10%, respectively. Controls include an extensive list of covariates: conscientiousness, age, gender, immigrant, self-reported health status degree, spoken language, having experienced a change in residence or school, parent's civil status, age difference regards each parent, parental background characteristics, academic year, last year math's grade, number of siblings being graduated or having dropped out and the number of extracurricular activities. In column (2) we also give information for the standard deviation and the interquartile range for the covariates into brackets.

Table 3
Marginal effects of alternative estimations including fixed effects, peers influence and control variables.

		(1) FE + pre-school features	(2) FE and controls
Fraction of dropout intentions	Classroom influence	0.033 (0.05)	0.040 (0.04)
	Cliques influence (including reciprocating)	0.048 (0.02)a	0.063 (0.02)a
Fraction of females	Classroom influence	−0.199 (0.08)b	−0.173 (0.04)a
	Cliques influence (including reciprocating)	−0.053 (0.03)c	−0.040 (0.02)b
Average height and weight	Height classroom influence by gender	−0.490 (0.43)	−0.263 (0.37)
	Weight classroom influence by gender	0.005 (0.00)	0.005 (0.00)
	Height cliques influence (including reciprocating)	0.050 (0.07)	0.031 (0.07)
	Weight cliques influence (including reciprocating)	−0.001 (0.00)	−0.001 (0.00)
	Classroom influence	−0.063 (0.02)a	−0.009 (0.01)a
Average last year Math's grade	Cliques influence (including reciprocating)	−0.064 (0.01)b	−0.075 (0.01)a

Notes: Sample size is 2123 observations. All regressions include school fixed effects (FE). We obtained very similar results when including school location fixed effects and residence fixed effects. Each row considers one friends' characteristic at one time. Adjusted robust standard errors for clustering at the classroom level are reported in parenthesis. a, b and c denote significance at 1%, 5% and 10%, respectively. Controls include an extensive list of covariates: conscientiousness, age, gender, immigrant, self-reported health status degree, spoken language, having experienced a change in residence or school, parent's civil status, age difference regards each parent, parental background characteristics, academic year, last year math's grade, number of siblings being graduated or having dropped out and the number of extracurricular activities.

The lack of peer influence from non-reciprocating friends raises the question of whether our results would differ from examining effects between closer, reciprocating friends instead. We show results using the sample of reciprocating sample of friends in Table 3. Interpretation of these results is complicated by the fact that we can no longer rely on our assumption that the direction of influence runs one way. Mutual friends may be influenced by underlying background characteristics that are similar between them (correlated effects), or may be influenced by each other (endogenous effects). We condition on a wide array of covariates to remove correlated effects, but acknowledge that this approach may not remove them completely. Nevertheless, the analysis is helpful in providing an upper-bound for peer influences between reciprocating friends.

Table 3 shows that students with a higher proportion of friends with dropout intentions are more likely to state that they intend to drop out themselves.[11] With reciprocating peers, a 10 percentage point increase in the fraction of peers with dropout intentions is associated with a 0.48–0.63 percentage point increase in one's own likelihood of reporting dropout intentions. A similar result arises when considering average cognitive ability among non-reciprocating peers: greater average cognitive ability among peers is associated with students' own likelihood of saying they intend to drop out. In addition, the greater fraction of females within cliques the lower the intention to drop out (1 percentage point from a 20 percentage point increase).

6. Conclusion

This paper uses new data from Catalonia, Spain of high school students and their self-identified friends to estimate influences on dropout intent. Pupils were asked to identify classmates they consider close friends. In some cases, one student identifies another student as a friend, but that friend does not reciprocate. Assuming in these asymmetric cases that peer effects flow in one direction – from non-reciprocating friend to friend – allows us to estimate peer effects in the classroom that occur from interaction with self-reported friends, rather than from interaction with overall groups. We also estimate effects among reciprocating friends using a wide set of background covariates, and view these estimates as upper bounds.

[11] This finding corroborates previous findings as for the positive influence of females within classrooms (Ammermueller & Pischke, 2006; Hoxby, 2000; Lavy & Schlosser, 2007).

Our evidence suggests schoolmate effects from non-reciprocating friends on dropout intent are relatively small. A 10 percentage point increase in the fraction of non-reciprocating friends intending to dropout increases students' chances of dropping out by .2 percentage points. This small and insignificant effect also holds after conditioning on a large list of confounding and contextual effects.

We find more significant effects when considering students that both self-report each other as friends. Among these closer, reciprocating friends, we estimate that a 10 percentage point increase in the proportion among reciprocating friends intending to dropout increases the likelihood of also having dropout intentions by 1–2 percentage points. While these larger estimates include a wide array of family, neighborhood, and school controls, they likely represent upper bounds because we may not have accounted for all factors that determine these friendships and subsequent dropout intentions.

Our results add to the literature on peer effects by considering impacts from specific peers rather than changes to a classroom's average characteristics. They suggest that non-reciprocating peers have little influence on students that identify them as friends. If peer effects exist, they likely occur from closer friends and interactions.

Acknowledgements

Toni Mora gratefully acknowledges the financial support under grant AGAUR 2008-BE1-00121 and 09SGR102 (Generalitat of Catalonia). Likewise, Toni Mora would like to thank OISE and Economics Department his stay in the University of Toronto, during which the main work on this paper was completed. Likewise, authors are indebted to Josep-Lluís Carrión-i-Silvestre for programming help. The usual disclaimer applies.

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