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Essays in the Economics of Education: The Role of Peers and Socio-Emotional Skills

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An aspiring friend is a friend indeed: school peers and college aspirations in Brazil*

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Abstract

Aspirations are a fundamental determinant of one's effort and investments. Due to its consequences for individuals' future outcomes, understanding the process of aspirations formation helps to inform public policies. This work asks whether peers play a role in such a process. I use novel data on Brazilian students' networks, matched with administrative data, and investigate whether students' college aspirations spill over to their friends. The employed methodology acknowledges that social cliques are formed endogenously and addresses this challenge by modeling friendship formation based on similarities in predetermined characteristics and on students' previous chances of interaction. Using the predicted friendship links, I explore network structures and use predicted friends of friends' characteristics as instruments for friends' aspirations. The results show evidence of positive, significant, and quite large peer effects on aspirations - an extra friend aspiring to go to college increases on average 10.8% the likelihood that a student will also aspire to it. In a discussion about the possible mechanisms, I verify the existence of peer effects on certain social norms in the school and on the effort that students make towards studying Math. Peers' performance and socioemotional skills, however, do not have an impact on student's performance and socioemotional skills, respectively. Finally, I investigate if friends' aspirations impact students' future outcomes. While friends' aspirations do not influence students' future proficiency, an extra aspiring friend decreases in 5.9 percentage points the likelihood of dropping out of high school.

Keywords: Peer effects, Social Networks, Education, Aspiration, Human Capital Accumulation

JEL Codes: I24, I25, I29

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1 Introduction

The capacity to aspire to a better standard of living is an important driver of one's effort and investments (Appadurai, 2004; Ray, 2006). The recent literature on development economics considers the lack of such a capacity - or aspirations failures - as a psychological constraint that might trap people into poverty (Dalton, Ghosal, & Mani, 2016; Genicot & Ray, 2017). Understanding the determinants of aspirations is hence a powerful tool for policymaking. Such determinants might help to explain, for instance, important patterns of behavior, such as under-investment in education (Kearney & Levine, 2014).

The contributions to the theoretical literature argue that aspirations emerge in social contexts, through individuals' comparisons with similar others (Appadurai, 2004; Bogliacino & Ortoleva, 2013; Genicot & Ray, 2017; Ray, 2006). This important social element of aspirations construction calls for investigations on how exactly peers influence an individual's level of aspirations. Empirical works have found that peers' socioeconomic status is associated with individuals' aspirations (e.g. Janzen, Magnan, Sharma, and Thompson (2017); Stutzer (2004)). However, an important question still needs further investigation: do peers' aspirations influence one's own aspiration, over and above socio-economic considerations? That is, after controlling for socioeconomic status, do peers still influence individuals' aspirations, through their own level of aspiration?

This work investigates peer effects on students' college aspirations - that is, how peers' desire to pursue a college degree impacts students' desire to also pursue such a degree. To do so, I rely on a unique social networks data collected from middle school students in Brazil to address the main challenges that emerge in the identification of peer effects. Differently from standard linear-in-means models, this work does not assume that all individuals in a students' reference group are equally connected or have the same influence on each other. Instead, it acknowledges that individuals in social networks are idiosyncratically connected and that homophily - i.e., the tendency to form social clicks with similar others - plays an important role in friendship formation.

Linking these network data with administrative data, I model friendship formation based on homophily in predetermined characteristics and on random allocation of students into classes when first enrolling at middle school. Next, based on the model's predicted connections, the identification strategy uses predicted friends of friends' characteristics as instrumental variables for friends' aspirations. It also uses network fixed effects and a broad set of controls to eliminate other possible correlated effects.

College aspirations is a quite relevant measure of aspirations in the educational scenario of developing countries. On the one hand, these countries have a high earnings premium of tertiary education, compared to other OECD and partner countries. On the other hand, they have low percentages of adults attaining such a level of education. Brazil is a good example: someone with a bachelor's degree in Brazil earns over 2.4 times what someone who only attained upper secondary education earns - the highest earnings premium among OECD and partner countries. Still, only 15% of the adult population in the country has attained tertiary education - well below the OECD average of 37% (OECD, 2017). Hence, aspiring to a college degree in a developing country is a good indicator of aspirations towards a good living standard.

I document that college aspirations at 9^{th} grade, the last grade of middle school in Brazil, is positively associated with students' future outcomes at school, such as the likelihood of finishing high school, and class attendance and performance during high school.

I find evidence of positive, significant, and quite large peer effects on aspirations: an extra friend aspiring to a college degree increases a students' likelihood of also aspiring to it from 3.7 p.p. (5.4%) to 14.7 p.p. (21.6%), depending on the number of nominated friends. The results are quite homogeneous for different students' characteristics, such as gender, race, and parental education. On a discussion about the possible mechanisms behind such an impact, I verify the existence of peer effects on some social norms in school and on the amount of time that students dedicate to studying math. However, differently from other works on peer effects, I do not find significant peer effects on reading and math performance.

I also do not find peer effects on students' socio-emotional skills.

I finally ask whether peers' aspirations influence students' future outcomes in school, such as retention, dropout, class attendance, and performance. While there is no impact of peers' aspirations on students' performance or class attendance, I do find that peers' aspirations decreases the likelihood of dropping out of school by, on average, 5.9 percentage points.

This study adds to traditional works in the sociology literature on aspirations (Kao & Tienda, 1998; Sewell & Hauser, 1975; Sewell & Shah, 1968). Sociologists have long verified the existence of a positive correlation between peers' aspirations and one's own aspirations (see, for instance, the work of Campbell and Alexander (1965); Cohen (1983); Duncan, Haller, and Portes (1968)). Identification issues, however, have prevented these works from establishing causal relationships. In the context of peer effects estimations, correlated effects - socioeconomic background, school quality, or homophily in friendship formation - might deliver high correlation between a student's outcomes and her peers' outcomes even in the absence of peers' influence. Moreover, the reflection problem - the simultaneity of outcomes that emerges in groups' interactions - will most likely overestimate any existing peer effects (Manski, 1993).[1] Hence, correlational studies say little about the true impact that peers exert on one's aspirations. The identification strategy employed in this work allows me to establish a causal relationship between aspirations and peers' aspirations.

This work also contributes to the literature on peer effects (see Sacerdote (2011) for a review). Most of the works on primary and secondary schools focus on peer effects in test scores and look at different sources of peer effects - such as ability, gender or racial composition, parental characteristics, or behavior (Austen-Smith & Fryer Jr, 2005; Carrell & Hoekstra, 2010; Fruehwirth & Gagete-Miranda, 2019; Hanushek, Kain, Markman, & Rivkin, 2003; Hoxby, 2000; Lavy & Schlosser, 2011; Marotta, 2017). Other studies focus on peer effects in students' attitudes and behavior, such as substance use, school dropout, and criminal activity (Case & Katz, 1991; Gaviria & Raphael, 2001). When it comes to the influence

[1] For a discussion about the challenges on the estimation of peer effects, see Angrist (2014).

that peers have on students' aspirations, Jonsson and Mood (2008) show that having high-achieving peers might depress the average students' desire to attend college. To the best of my knowledge, however, no work so far has focused on the impact that peers' aspirations have on students' aspirations, and what are the mechanisms behind this. The work of Norris (2017) is the closest related to mine. The author exploits Add-Health data to show how peers' attitudes about school influence one's own attitudes. Even though Norris (2017)'s measure of attitude about school has a component of desire to go to college, it also has other components, such as how much students feel they are part of their current school. So one cannot isolate in his work the influence that peers' desire to go to college has on students' desire to it.

The remaining of this work is divided as follows. The next section discusses the framework in which this investigation is based and the gaps that it aims to fill. Section 3 presents the data that I exploit, as well as the measure of aspirations that I use. I also discuss in this section the validity of such a measure. Sections 4 and 5 presents the identification strategy and the main results, respectively. I discuss the mechanisms behind the results in section 6 and show how peers' aspirations also impact students' future outcomes in section 7. Section 8 presents some final remarks.

2 Aspirations and the importance of peers

Theoretical contributions to the literature on aspirations' formation have established the importance of social context in determining individuals' aspirations. In his very influential work about the topic, Ray (2006) develops the notion of "aspirations window", which is the set of individuals or similar others to whom people compare when establishing their aspirations. Ray (2006) builds on the work of Appadurai (2004), who also makes the point that "aspirations are never simply individual [...]. They are always formed in the interaction and in the thick of social life" (Appadurai (2004), p. 68). Ray (2006) and Appadurai (2004) also show that due to systematic differences in the networks and social interactions of the rich and the poor, the level of aspirations is very dependent on one's socio-economic status. While the rich can easily set paths to fulfill their goals, the poor might end up trapped in aspirations failures - when their aspirations are either too low or so high that they look unattainable (Dalton et al., 2016; Genicot & Ray, 2017; Ray, 2006) - or lack their "capacity to aspire" (Appadurai, 2004).[2]

Empirical works have shown evidence of this socially determined nature of aspirations. Individuals' relative wealth (that is, how rich or poor they are compared with the ones within their aspirations window) impacts their well-being (Bursztyn, Ferman, Fiorin, Kanz, & Rao, 2017; Card, Mas, Moretti, & Saez, 2012; Fafchamps & Shilpi, 2008; Ferrer-i Carbonell, 2005), and/or the determination of their own aspirations (Janzen et al., 2017; Stutzer, 2004). This might lead to aspirations frustrations, especially in more unequal settings. Indeed, Kearney and Levine (2014) show that low-income students are more likely to drop out of school if the gap between the bottom and the middle of the income distribution in the state where they live is larger. In related work, La Ferrara (2019) explores data from PISA and investigates how poverty and inequality are associated with aspirations. While inequality in the analyzed countries is not a significant predictor of students' expectations of finishing university, it is negatively with their expectations of finding a job.

The aforementioned studies relate closely with the theories of conspicuous consumption (Veblen, 2005) and relative income (Duesenberry et al., 1949) that essentially state that individuals consumption do not depend only on goods' prices and income constraint, but also on the level of consumption of similar others, in a "keeping up with the Joneses" fashion. However, the notion of aspirations window developed by Ray (2006) also encompasses other forms of aspirations that do not emerge from envy but the observation or admiration of others' experiences.

More related to this second idea of aspirations' formation, some works show the influence

[2]To see a comprehensive revision about the literature on aspirations formations and aspirations traps and frustrations, see the work of La Ferrara (2019).

of role models in shaping individuals' aspirations. Macours and Vakis (2009), for instance, explore a double randomization in Nicaragua to show the importance of local leaders in determining households' investments. In a field experiment, the authors randomize three different transfer programs both to households and to local female leaders. They find that interacting with leaders who randomly received the largest program package increased households' investments in human capital and income diversification. It also affected their attitudes towards the future. Another two field experiments in Ethiopia (Bernard, Dercon, Orkin, Taffesse, et al., 2014) and Mexico (Lybbert & Wydick, 2016), show how being exposed to the successful experience of people from similar SES increases individuals' aspirations and investments. Bernard et al. (2014) also shows how these impacts spill over to peers, which is yet another instance of role models.

The importance of role models in determining aspirations might be especially consequential for the young, due to the high return of human capital investments that they make at that stage of their life. The work of Beaman, Duflo, Pande, and Topalova (2012) illustrates this idea quite well. They explore a natural experiment in West Bengal, where one-third of village councils are randomly selected to be reserved for a woman chief councilor. Using villages in which such positions were never selected as control, the authors show that having a female leader decreases the gender gap in parents' and in adolescents' aspirations. It also increased girls' educational attainment, erasing the gender gap in educational attainment in treated villages. Other works show the importance of parents (Dhar, Jain, & Jayachandran, 2019; Dossi, Figlio, Giuliano, & Sapienza, 2019; Rangel, 2015) and teachers (Botelho, Madeira, & Rangel, 2015; Carlana, 2019; Papageorge, Gershenson, & Kang, 2016) in passing along stereotypes and attitudes that might influence students' aspirations.

School peers also play a paramount role in shaping students' aspirations. However, the literature that investigates such a role seldom focuses on the direct impact that peers' aspirations have on students' own aspirations. Instead, it focuses mainly on how social image, peer pressure, or information impact students' efforts and investments in school. The work of Azmat and Iriberri (2010), for instance, shows how making relative performance available to students motivates them to increase their effort in school, even if they were evaluated according to their absolute grade. Jonsson and Mood (2008), in turn, show how being in a disadvantaged position regarding peers harms aspirations. Adding to this literature, some works show how students' attitudes change when they can be observed by their peers (Bursztyn, Egorov, & Jensen, 2019; Bursztyn & Jensen, 2015). In particular, (Bursztyn et al., 2019) show that students lower their educational investments when they are concerned about hiding effort in settings where effort is stigmatized, or when they are concerned about hiding low ability, in settings where ability is rewarded. Other works show how increasing educational returns for only a group of students impacts the educational outcomes of peers who do not have their educational returns directly impacted (Abramitzky, Lavy, & Pérez, 2018; Ballis, 2019). Abramitzky et al. (2018) show evidence that information diffusion is a possible mechanism behind such an impact.

Even if many of the aforementioned works do now focus directly on aspirations, they are consistent with the hypothesis that (i) peers influence students' aspirations; (ii) this happens not only due to their performance or socio-economic status but also through their own aspirations; and (iii) information diffusion, competition, or compliance to social norms might be mechanisms behind the direct impact that peers' aspirations have on students' own aspirations. This work investigates precisely these points. In the next sections, I show how to causally identify peer effects on students' aspirations, how this effect is positive and quite large, and how compliance to social norms might be a mechanism behind such an impact.

3 Data and measure of aspirations

The data used in this work come from a survey conducted in 2011 on students from the 9^{th} grade of 85 state-owned middle schools of Sao Paulo (Brazil).[3] The students answered a comprehensive questionnaire about their personal profile, how happy or satisfied they were

[3] In Brazil, the 9^{th} grade is the last grade of middle school.

with their life, what were their study habits, and what were their aspirations. One block of questions mapped students' social networks. They were asked to nominate their four best friends or colleagues in their grade (which, in most schools, comprehends more than one classroom). Importantly, it is possible to link the nominated students to school rosters, and also to find their own answers to the questionnaire. As so, it is possible to map the network for all students of 9^{th} grade in each school.

Another important block of questions was dedicated to understanding students' educational aspirations. One specific question of this block asked until when students would like to keep studying if this choice was *entirely* up to them. I use this question to build my measure of aspirations towards pursuing a college degree, which I call *college aspirations*. This is a binary variable that takes value equal one if students answered that they would like to keep studying until they get a college degree and zero otherwise. Since students were asked to reveal their preferences towards their educational future disregarding any kind of constraint that they might face to achieve such a future, this measure captures students' true aspirations, and not only their expectations for the future.

The survey also approached other traits and beliefs that might be associated with college aspirations. First, it had a block of questions asking about students' personal profile. From it, it is possible to identify students socio-emotional skills such as self-esteem, self-efficacy, self-control, agreeableness, rapport with peers, and locus of control.[4] Second, it asked students which probability they attributed for them to find a job in the future if they have a university degree. This question measures students' perceptions about college returns, which might influence their aspirations towards pursuing a college degree. Finally, students were also asked about possible impediments for them to keep studying in the future. Two impediments, in particular, might also be related to students' college aspirations: (1) their concern about being stigmatized as "nerd" if they put too much effort into school - I call this variable "Fear of nerd stigma"; and (2) the fact that their friends pressure them to find a job and start

[4] For more details about these instruments and how they were extracted, see PROJECT W/ MADEIRA AND RANGEL.

earning their own money - I call this variable "Peer pressure to work". I will use students' perceived college returns and these two impediments - which proxy for students' willingness to comply with "bad" social norms in the school - to discuss the mechanism behind my results.

Table 1 presents some descriptive statistics coming from this survey and from administrative data, such as students' college aspirations, their demographic and socioeconomic characteristics, and their proficiency in Language and Math in a diagnostic exam - known as Sao Paulo School Performance Assessment System (SARESP, in the Portuguese acronym) - applied every year to all state-owned schools in Sao Paulo. The table presents the mean and standard error for all students and also for those who aspire and those who do not aspire to a college degree. It also shows this same information for students' friends. First, looking at the sample composed of all students, we see that more than 30% of them do not aspire to a college degree. Second, comparing students who aspire to a college with students who do not, those who do want to go to college are better achieving and have on average better-educated parents. Finally, looking at the average characteristics of students' friends, we see that the friends of students aspiring to a college are also more likely to aspire to it - which could be an indicator of peer effects - but are also more likely of being high achieving students and of having more educated parents - which might exemplify the phenomenon of homophily, that is, people's tendency to befriend with similar others. Homophily is an important confounder in the estimation of peer effects. Section 4 explains how this work overcomes such an issue.

3.1 College aspirations and future outcomes

An important feature of this survey is the possibility to link it with administrative data and to recover students' school path - which allows one to know whether these students dropped out from school at some point or were retained in some grade -, as well as their future performance in SARESP. This allows me to test the association of college aspirations with students' future outcomes in school.

Associating college aspirations with the aforementioned outcomes is an important exercise to understand whether such a measure of aspirations goes in the expected direction. However, it is important to highlight that such an exercise does not allow any kind of causal interpretation. With this caveat in mind, I perform OLS estimations with each of those measures of school outcomes as dependent variables, and college aspirations as the independent variable, also controlling for students' performance, demographics, socioeconomic status, and school fixed effects. Figure 1 presents the point estimate and the 95% confidence interval of these estimations.[5]

As described by the figure, college aspirations are highly associated with the likelihood of having a normal school path during high school (that is, of being at the 12^{th} grade - the last grade of HS - in 2014), with class attendance in reading and math during 11^{th} grade (2013)[6], and with students' performance in the last year of high school (2014). At the same time, college aspirations are negatively correlated with the likelihood of school dropout during high school.

These exercises show that such a measure of aspirations has a predictive power over several important school outcomes, which is an important indicator that it is indeed capturing students' true aspirations.

4 Identification of peer effects

There are several challenges that one faces when seeking to identify endogenous social effects through a linear-in-means model - that is, associating an individual's outcomes with the average outcome of her reference group on the attempt to infer whether the group behavior influences the behavior of individuals inside that group.

The first challenge is the reflection problem (Manski, 1993), namely, a simultaneity bias

[5] Besides parents' education, I also use father's working status, house ownership, internet, and the number of lavatories in the house as measures of the socioeconomic status. I omitted these variables from the figures for the sake of clarity.

[6] It was not possible to recover the information on class attendance for the last year of high school, in 2014.

that emerges due to the fact that an individual might influence the behavior of her group and, at the same time, might be influenced by the group's behavior. In a friendship network, for instance, all friends potentially impact each other, so it is difficult to disentangle if one's behavior is the cause or the consequence of others' behavior.

The second challenge is correlated effects, where people in the same reference group tend to behave alike not because they influence one another but because they share similar unobserved characteristics, such as institutional environments and/or common shocks. For instance, students within a school are influenced by school quality, or maybe by a very inspiring professor.

Finally, connections or friendship links do not happen at random, which makes reference groups themselves endogenous. Several works have shown the important role of homophily in friendship formation. That is, the likelihood that two people will interact with one another is higher if they share similar characteristics, like race or SES (Currarini, Jackson, & Pin, 2009; McPherson, Smith-Lovin, & Cook, 2001; Moody, 2001). An important implication of homophily and the endogenous formations of networks is that neither the connections nor the influence of individuals inside a reference group is equal for everyone. Even students enrolled at the same school and under the mentoring of the same teachers form different cliques to one another. This brings extra challenges to the estimation of peer effects since individuals might have unobserved characteristics correlated to both their outcomes and their links formation.

Several works on the peer effects literature have tackled these identification problems, with different strategies. Some use natural experiments in order to solve correlated effects (Cipollone & Rosolia, 2007; Sacerdote, 2001; Zimmerman, 2003), other use theoretical models of social interactions (Brock & Durlauf, 2001) or network structures (Boucher, Bramoullé, Djebbari, & Fortin, 2014; Bramoullé, Djebbari, & Fortin, 2009; Calvó-Armengol, Patacchini, & Zenou, 2009; De Giorgi, Pellizzari, & Redaelli, 2010; Liu, Patacchini, & Zenou, 2014) in order to address both correlated effects and the reflection problem.

Fewer works have fully acknowledged the implications of endogenous formation of networks and tackled this problem accordingly. Johnsson and Moon (2017) develop a semi-parametric control function approach to deal with this issue. Goldsmith-Pinkham and Imbens (2013) model link formation assuming that individuals with similar observed and unobserved characteristics are more likely to form links, and perform a sample selection correction where network formation and the outcome are jointly determined. König, Liu, and Zenou (2018) use a three-stage least square (3SLS) strategy where, in the first stage, they model link formation based on past network structures as exclusion restrictions that affect current link formation but do not enter the outcome equation (König et al., 2018).[7] The second and third stages are similar to the ones implemented by Bramoullé et al. (2009) and De Giorgi et al. (2010) where friends' outcomes are instrumented by friends' of friends characteristics. The main difference is that, when building the instruments, the endogenous sociometric matrix is replaced by the predicted one that comes from the link formation model. In this work, I follow this 3SLS approach. Besides modeling friendship formation using students' pre-determined characteristics, I also look at their random chances of interacting due to random allocation into classes when students enroll in middle school. In what follows, I formalize my model of friends' influence, the identification issues, and the 3SLS estimation.

4.1 Model of friends' influence

Let a student's college aspirations be affected by the mean college aspirations of her friends, her characteristics such as grades, gender, race, and family background, and by the mean characteristics of her friends. More formally, suppose there is a set of students i, $i = (1...N)$, that belong to network l, $l = (1, ...L)$ [8]. Each student may have a group of friends F_i of size n_i, or may be isolated, where $F_i = \varnothing$. Assume that each student i is not included in her

[7] In an interesting application of this methodology, (Santavirta & Sarzosa, 2019) uses individuals' predetermined characteristics to model link formation.

[8] In this study, each network is formed by all students in 9^{th} grade of each school.

own group of friends, such that $i \notin F_i$.[9] The model is given by[10]

$$y_{li} = \beta \frac{\sum_{j \in F_i} y_{lj}}{n_i} + \gamma x_{li} + \eta \frac{\sum_{j \in F_i} x_{lj}}{n_i} + \mu_l + \upsilon_{li}$$
$$E(\upsilon_{li} | \boldsymbol{X_l}, \mu_l) = 0 \tag{1}$$

where y_{li} is the aspirations level of individual i in network l , which depends on the aspirations level of the friends directly connected to her - the endogenous social effect in Manski's notation (see Manski (1993)) -, on x_{li}, her own characteristics.[11], on the characteristics of her friends - the exogenous social effects in Manski's notation - and on network unobserved fixed effects, μ_l. The only restriction imposed to parameters in this model is that $|\beta| < 1$.

Let G be the adjacency matrix, where element $g_{i,j} = 1/n_i$ if individual i sends a friendship tie to individual j, and $g_{i,j} = 0$ otherwise. Assume that $g_{i,i} = 0$ so that each individual is not part of her own reference group. The above model can then be translated into:

$$\boldsymbol{y_l} = \beta \boldsymbol{G} \boldsymbol{y_l} + \gamma \boldsymbol{X_l} + \eta \boldsymbol{G} \boldsymbol{X_l} + \mu_l + \boldsymbol{\upsilon_l}$$
$$E(\upsilon_l | \boldsymbol{X_l}, \mu_l) = 0 \tag{2}$$

It is easy to see that the reflection problem emerges because the outcome variable y is present on both sides of the equation. To be more explicitly, if one assumes for a moment that $\boldsymbol{G}$ is orthogonal to $\boldsymbol{\upsilon_l}$, it is possible to causally estimate the reduced form of equation

[9]The exclusion of individuals from their own reference group might lead to yet another source of bias, namely the exclusion bias, that causes an underestimation of peer effects (Caeyers & Fafchamps, 2016; Guryan, Kroft, & Notowidigdo, 2009). The exclusion of an individual i from the pool of i's peers creates a mechanical negative relationship between i's characteristics and that of her peers, especially in small samples. The identification strategy adopted in this work - that follows the works of Bramoullé et al. (2009) and De Giorgi et al. (2010) - also addresses this source of bias. For more details, see the work of Caeyers and Fafchamps (2016).

[10]This model reassembles the one described in Bramoullé et al. (2009) and is a special case of the model described in Manski (1993), where an individual reference group are the friends linked to her.

[11]For the sake of notational clarity, there is only one exogenous characteristic exposed in equation 1 In the next equation, the model is generalized to more characteristics.

2[12]:

$$\boldsymbol{y_l} = (\boldsymbol{I} - \beta \boldsymbol{G})^{-1}(\gamma \boldsymbol{I} + \eta \boldsymbol{G})\boldsymbol{X_l} + (\boldsymbol{I} - \beta \boldsymbol{G})^{-1}\mu_l + (\boldsymbol{I} - \beta \boldsymbol{G})^{-1}\boldsymbol{\upsilon} \tag{3}$$

However, such estimation will only yield unbiased estimates of $(\boldsymbol{I} - \beta \boldsymbol{G})^{-1}\eta$, which will not disentangle the endogenous social effect (β) from the exogenous social effect (η).

Correlated effects would emerge if μ_l was not observed by the modeler, since $\boldsymbol{X_l}$ is only exogenous conditional on μ_l. School quality, for instance, is probably correlated with students' aspirations. Hence, students within the same school are more likely to have similar levels of college aspiration, which could bias estimations upwards. I address this problem by simply controlling the estimations by network fixed effects - in my case, the same as school fixed effects.

Nonetheless, this does not solve the endogeneity of link formation. That is, individuals do not befriend each other at random and homophily plays a great role in friendship formation, which yields $\boldsymbol{G} \not\perp \boldsymbol{\upsilon_l}$. Once again, such correlation would most likely bias estimates upwards, since more similar students have a greater probability of becoming friends and, at the same time, are more likely to have similar aspirations towards college.

As in König et al. (2018), I will tackle the reflection problem and the endogenous formation of friendship using a 3SLS estimation. The first stage models link formation based on homophily in predetermined characteristics. The second and third stages use the predicted friendship connections delivered by the first stage and use friends of friends' characteristics as instrumental variables for friends' aspirations (resembling Bramoullé et al. (2009)). In the remainder of this section, I describe this approach and explain how it overcomes the issues raised above. For the sake of clarity in exposition, I start by describing the last two stages of the implemented strategy, which address the reflection problem, and then I describe the first stage and show how it overcomes the endogenous formation of networks.

[12] Given the restriction on β, $\boldsymbol{I} - \beta \boldsymbol{G}$ is invertible.

4.2 The reflection problem

Through a series expansion of equation 3 and assuming $\beta\gamma + \eta \neq 0$, Bramoullé et al. (2009) show that if $\boldsymbol{I}$, $\boldsymbol{G}$, $\boldsymbol{G}^2$, and $\boldsymbol{G}^3$ are linear independent, it is possible to use ($\boldsymbol{G}^2\boldsymbol{X_l}$, $\boldsymbol{G}^3\boldsymbol{X_l}$, ...) as excluded instruments for $\boldsymbol{Gy}$ and, as so, to identify all the parameters of model 2. [13] The authors prove that if the diameter[14] of the network is greater than or equal to 3, then the linear independence between $\boldsymbol{I}$, $\boldsymbol{G}$, $\boldsymbol{G}^2$, and $\boldsymbol{G}^3$ is guaranteed and the model is identified[15].

Therefore, in order to identify the parameters φ =(β, η, γ), it is possible to follow a 2SLS estimation, where the matrix of explanatory variables $\boldsymbol{\tilde{X}}$ =[$\boldsymbol{Gy_l}$ $\boldsymbol{X_l}$ $\boldsymbol{GX_l}$] is instrumented in the second stage by $\boldsymbol{S}$ = [$\boldsymbol{X_l}$ $\boldsymbol{GX_l}$ $\boldsymbol{G^2X_l}$ $\boldsymbol{G^3X_l}$], such that the final estimates are given by $\hat{\varphi}^{2SLS} = \boldsymbol{(\tilde{X}'P\tilde{X})^{-1}\tilde{X'}Py_l}$, where $\boldsymbol{P = S(S'S)^{-1}S}$.

The intuition behind this strategy is that, unless the network is fully connected, there will always be an individual A in the network whose characteristics will directly affect the outcome of another individual B, but will affect the outcome of a third individual C only indirectly, through the friendship tie between B and C. Therefore, A's characteristics are valid instruments for B's outcomes.

4.3 Endogenous link formation

The aforementioned 2SLS strategy would ensure unbiased estimates of the endogenous and exogenous social effects if friendship links were formed at random - that is, if $\boldsymbol{G} \perp v_l$. However, as stated before, social networks are not formed at random and homophily plays a role in clique formation. König et al. (2018) deal with such an issue including a stage

[13] If correlated effects were not an issue and μ_l could be excluded from the model, this condition would be less restrictive. As a matter of fact, one would need only $\boldsymbol{I}$, $\boldsymbol{G}$, $\boldsymbol{G}^2$ to be linear independent in order for the model to be identified.

[14] As in Bramoullé et al. (2009)[pg 47], "define the distance between two students i and j in the network as the number of friendship links connecting i and j in the shortest chain of students $i_1...i_l$ such that i_1 is a friend of i, i_2 is a friend of i_1, ...and j is a friend of i_l.(...) Define the *diameter* of the network as the maximal friendship distance between any two students in the network (see Wasserman and Faust (1994))."

[15] The counterpart for the diameter size in a model where correlated effects are absent is the presence of *intransitive triads* - that is, when we have a set of three individual i, j, and k such that i is connected to j and j is connected to k but i is not connected to k - in at least some networks

before the 2SLS, where they use predicted networks based on predetermined characteristics to build the IVs that identify the social effects.

The work of Graham (2017) explicitly models network formation based on homophily. The main idea of this model is that the friendship connection $D_{i,j}$ between two agents i and j, depends on the distance between these two agents regarding several agent-level attributes $Z_i = \{z_{1i}, ...z_{Ki}\}$. If we consider $W_{ij} = \sum_{k=1}^{K}(|z_{ki} - z_{kj}|)$ as a measure of the total distance between i and j, then agent i will send a friendship tie to agent j if the total surplus of doing so is positive:

$$D_{i,j} = \mathbf{1}(W'_{ij}\varphi + \theta_i + \theta_j + U_{ij} \geq 0) \tag{4}$$

where $\mathbf{1(.)}$ is an indicator function, $\theta_{i(j)}$ is agent $i(j)$'s fixed effect, and U_{ij} is an idiosyncratic component ($U_{ij} = U_{ji}$ if the network is undirected and $U_{ij} \neq U_{ji}$ if the network is directed). Hence, if we assume that U_{ij} is a standard logistic random variable that is independently and identically distributed across dyads, the conditional likelihood of observing network $\boldsymbol{D} = \boldsymbol{d}$ is

$$Pr(\boldsymbol{D} = \boldsymbol{d}|\boldsymbol{Z}, \boldsymbol{\theta}) = \prod_{i \neq j} Pr(D_{ij} = d|Z_i, Z_j, \theta_i, \theta_j)$$

with

$$Pr(D_{ij=d}|\boldsymbol{Z}, \boldsymbol{\theta}) = \left[\frac{1}{1 + exp((W'_{ij}\varphi + \theta_i + \theta_j)}\right]^{1-d} \left[\frac{exp((W'_{ij}\varphi + \theta_i + \theta_j)}{1 + exp(W'_{ij}\varphi + \theta_i + \theta_j}\right]^{d}$$

for all $i \neq j$.

I model such a probability using the following conditional logistic regression function:

$$Pr(D_{ij=d}|\boldsymbol{Z}, \boldsymbol{\theta}) = \frac{exp((W'_{ij}\varphi + \theta_i + \theta_j)}{1 + exp((W'_{ij}\varphi + \theta_i + \theta_j)} \tag{5}$$

where W_{ij} is the distance in predetermined dyadic characteristics. More specifically, I use individuals similarities on gender and race. I also include binary variables indicating whether individuals i and j were enrolled at the same class when they first enrolled at the state-school in the 6^{th} grade - before 6^{th} grade students were enrolled in municipality schools and their first allocation into classes when arriving at state-schools in the 6^{th} is as good as random. The intuition behind the inclusion of this variable is that conditional on individuals' own characteristics, sharing the same class when they first arrive at their new school should increase their likelihood of being friends while not *directly* impacting their outcomes (in this case, their aspirations levels). Therefore, this variable can be used as an excluded instrument for this first stage of my estimation.

Table 2 presents the results of such estimation. As it is possible to see, sharing the same class in the first year of middle school (class in 2008) are highly correlated with the likelihood of forming friendship ties.

Using the predicted links coming from this model, I replace the original adjacency matrix by the predicted adjacency matrix when building the instruments used to identify model 2. Therefore, in the final estimation of the parameters φ =(β, η, γ), the matrix of explanatory variables $\tilde{\boldsymbol{X}}$ =[$\boldsymbol{Gy_l}$ $\boldsymbol{X_l}$ $\boldsymbol{GX_l}$] is instrumented in the second stage by $\hat{\boldsymbol{S}} = [\boldsymbol{X_l}\ \boldsymbol{GX_l}\ \hat{\boldsymbol{G}}(\boldsymbol{W})^{\boldsymbol{2}}\boldsymbol{X_l}$ $\hat{\boldsymbol{G}}(\boldsymbol{W})^{\boldsymbol{3}}\boldsymbol{X_l}]$, where $\hat{\boldsymbol{G}}(\boldsymbol{W})$ is the predicted adjacency matrix from equation 5, $\hat{\boldsymbol{D}}(\boldsymbol{W})$, row normalized so that each row sums to one. The final estimates are, therefore, given by $\hat{\varphi}^{3SLS} = (\tilde{\boldsymbol{X}}'\hat{\boldsymbol{P}}\tilde{\boldsymbol{X}})^{-1}\tilde{\boldsymbol{X}}'\hat{\boldsymbol{P}}\boldsymbol{y_l}$, where $\hat{\boldsymbol{P}} = \hat{\boldsymbol{S}}(\hat{\boldsymbol{S}}'\hat{\boldsymbol{S}})^{-1}\hat{\boldsymbol{S}}$.

4.4 Potential threats to identification

This section discusses some of the identifying assumptions of the implemented methodology and potential threats that might emerge due to how students' networks were mapped in my data.

As specified in Bramoullé et al. (2009) the identification of peer effects using friends of friends as instrumental variables is only possible if there are intransitive triads in the network - that is, students within a network cannot be all friends among themselves. This would invalidate the exclusion restriction of the instruments since all the friends of my friends would also be my friends. This is why one needs I, G, G^2, and G^3 to be linear independent. As shown in the previous section, Bramoullé et al. (2009) proofs that a sufficient condition to guarantee such linear independence is that the diameter of a network is greater than or equal to 3. The average size of the diameters in my networks is 14.3, with a minimum size of 4 and a maximum size of 22, so the linear independence between I, G, G^2, and G^3 is secured for all schools in my sample.

A second important assumption of Bramoullé et al. (2009) is that networks are fully mapped. That is, we should be able to identify all connections made by all individuals within a network. One needs this assumption to guarantee that intransitive triads in the network are indeed intransitive. In order words, if we observe that A is connected to B, and B is connected to C, but C is not connected to A, we need to be sure that the absence of connection between A and C is not due to missing or censored data. Such an assumption is also important for the model of friendship formation proposed by Graham (2017), since one should be able to identify all connections in a network in order to fully model them.

In that sense, my data might suffer from a ceiling effect, since students were only able to nominate four of their friends. If a student had a fifth or sixth friend in that grade, these connections do not show up in my data. Figure A.1 presents the out-degree distribution, that is, the distribution of the number of friends that each student nominated. Looking at the figure it is possible to see that around 20% of students might be suffering from this ceiling effect since they nominated four friends and it is not possible to know whether there were more friends they would like to nominate. However, it is reassuring to see that this is not the majority of students - around 60% of students nominated either one, two or three friends so they were not censored in any way[16]. Moreover, the work of Griffith (2019) - who uses

[16]Figure A.1 also shows that around 20% of students did not nominate any friend. This proportion is at the same order as the one in Add-Health data (Niño, Cai, & Ignatow, 2016). Exercises - not shown - either controlling for isolated students or excluding them from the estimation show very similar results.

data from Add Health and other smaller survey to investigate the direction of the bias when censoring network data - show that, if anything, censoring the number of friends bias the results *downwards*. Still, in section 5.1 I present some robustness checks to address potential issues with censored networks.

Another potential threat to identification is that students' aspirations towards going to college might be directly affected not only by their friends, but also by other colleagues. A high achieving colleague might either be a good role model, increasing a student's aspiration, or might be seen as a competitor, which could hinder such aspirations. If this colleague is a friend of a friend, the exclusion restriction of the instruments might be threatened. Controlling for classroom fixed effects might alleviate such a problem, since students ranking and competitive dynamics within the classroom will be held constant. Section 5.1 also presents results with such controls.

Besides controlling for classroom FE, I also perform a simulation where I randomly reshuffle students' friends and estimate peer effects using these new connections instead of students' true friends. Since such estimation will capture the impact that colleagues who are not friends have in students' aspirations, it should render smaller coefficients, non-significant in most of the time. Indeed, I find that less than 10% of the coefficients coming from such a simulation are equal to or greater than the results considering the connections with true friends.

5 Results

Table 3 presents results of the main estimations.[17] I use different instruments for friends' aspirations to test for the robustness of the results. In columns (1) and (2), friends' aspira-

[17]For comparative purposes, Table A.1, in the appendix, presents the results from an OLS estimation, the 2SLS estimation proposed by Bramoullé et al. (2009) and De Giorgi et al. (2010), and the 3SLS proposed by König et al. (2018) and used throughout this work. We can see that the OLS estimation is actually smaller than the 2SLS estimation, which my be due to measurement error and/or to the exclusion bias, discussed before. Importantly, however, the results decrease quite considerable when we compare the 2SLS estimation with the 3SLS one. This indicates that homophily might indeed bias the results upwards and shows the importance of properly correcting for it.

tions are instrumented by $\hat{G}(W)^2X$, that is, by predicted friends of friends' characteristics. Columns (3) and (4) use $\hat{G}(W)^3X$ - that is, third-order connections[18] - as instruments. Finally, columns (5) and (6) present both $\hat{G}(W)^2X$ and $\hat{G}(W)^3X$ as instruments. For comparative purposes, columns (2), (4), and (6) include controls for both students' and friends' socio-emotional skills. The results remain quite stable, even after the inclusion of such controls.

First, it is important to highlight that the instruments are quite strong, ranging from a joint significance (F statistic) of 19.5 to 36.5. Second, looking at the estimations, we see that peer effects on aspirations are positive, significant, and quite sizable. Column (1), for instance, has a coefficient of 0.147, which means that if a student passes from having no nominated friends who aspire to a college degree to having all nominated friends who aspire to it, her probability of aspiring to a college degree increases by 14.7 p.p..

Perhaps passing from having no nominated friends aspiring to go to college to having all friends aspiring to it is a too extreme way of interpreting the results. A better way of interpreting them is to think about the marginal impact of having an extra *aspiring friend* - that is, an extra friend aspiring to go to college. Such an impact will depend on the number of nominated friends. As described in section 3, each student could nominate up to four best friends or colleagues. If a student nominates all four friends, the marginal impact of an extra aspiring friend is about 3.4 p.p..[19] If a student nominates three friends, the marginal impact of an extra aspiring friend is 4.9 p.p.. If a student nominates two friends, the marginal impact is 7.35 p.p.. Finally, if a student nominates only one friend, the marginal impact will be 14.5 p.p. - naturally, the same as passing from having no friend aspiring to college to have all friends aspiring to it.

As shown in Table 1, the average number of nominated friends is about 2, so the impact of an extra aspiring friend for the average student is of a 7.35 p.p. increase in her likelihood

[18]Third order connections are the friends of friends of friends.

[19]If a student has four friends, the average of peers' aspirations increases by 0.25 every time an extra friend aspires to go to college. If we multiply this increase by the coefficient of peers' aspirations - which is 0.147 - we get to the marginal effect of 0.034, or 3.4 p.p..

of aspiring to go to college. Since one average 68% of students aspire to a college degree, this translates into an increase of 10.8% on the average aspirations.

5.1 Robustness check

As discussed in section 4.4, the main threat for identification is the fact that some students in the data did not nominate all of their friends. If this is the case, the model of network formation might not be correctly estimated and some excluded instruments used in the estimation of peer effects might be endogenous. In particular, if a friend of a friend is my friend, we should be worried about homophily being driving part of the results shown in Table 3. I've already shown that this is not the case for the majority of my sample since only 20% if students nominated the maximum allowed number of friends. I have also shown that the estimations do not change much when controlling for socio-emotional skills, which are potential drivers of homophily in friendship formation.

Yet, Tables A.2 and A.3 present another two exercises that ensure the robustness of the results. Table A.2 shows some falsification exercises where I investigate the existence of peer effects (the endogenous social effects) in students' socioeconomic status, where the impact of peers should not exist. Columns (1) and (2) investigate whether peers impact parents' education, that is, whether the fact that peers' mother or father have more than high school influences students' mother or father to also have more than high school. Column (3) analyzes whether the fact that peers live in their own house - in opposition to living in a rented or borrowed house - influence students to also live in their own house. These variables are clearly either pre-determined, as in the case of parental education, or very unlikely to be influenced by school peers. Therefore, the presence of peer effects in these variables would indicate that the employed methodology is not completely ruling out the presence of homophily or other correlated effects. However, as shown in the table, this is not the case: none of these exercises deliver significant results of peer effects - the coefficients for mother's education and house ownership are quite high, but one should notice that the instruments are very weak in all these estimations, which could inflate such coefficients. Column (4) of Table A.2 presents again the estimation of peer effects on college aspirations without controlling for the variables analyzed in column (1) to (4), in order to check whether the main results were not being driven only by the inclusion of these controls.

Table A.3 presents two other robustness checks. Column (1) shows the results controlling for classroom FE, such that students' ranking and competitive dynamics within the classroom are taken into consideration. Columns (2) and (3) re-analyze the results in the sub-sample of students who were not censored by the limit in friendship nomination - that is, students who nominated only three friends or less. In this restricted sample, it is possible to map all students' connections with more precision, without incurring the risk of having missing links. The results are remarkably similar to the ones of Table 3.

5.2 Heterogeneous impacts

Table 4 presents estimations considering heterogeneous characteristics of students regarding some of their demographics and socio-economic status. Each of the variables in the columns of the table is interacted with friends' aspirations. Hence, column (1) shows heterogeneous exercises for boys and girls, column (2) display these exercises for non-white and white students, and columns (3) and (4) show the results for students with less/more educated parents (mother in column (3) and father in column (4)).

As we can see, it does not look like a specific group of students is more or less impacted by their friends' aspirations. Such an influence seems to be very homogeneous across different students.

6 Discussion about possible mechanisms

There are at least three mechanisms that could be driving the results. The first is information diffusion. Students might exchange facts and impressions about college returns (both pecuniary and non-pecuniary), as well as about how to get into college - such as application process, fellowships, etc. Such a set of information might help them to form their expectations about the benefits of attending a college and how feasible this is.[20] A second mechanism behind the results is conformity to social norms. Students might either be influenced by their friends to comply with social norms that hinder their aspirations or see college aspirations itself as a social norm to which they decide to comply. As shown in (Bursztyn & Jensen, 2017), there is a burgeoning literature on how the presence of social norms and social pressure change individuals' behavior.

Finally, a third mechanism is that friends who aspire to go to college put more effort in their school activities and influence students to also increase their effort. This in turn can make students revise their own aspirations and expectations. High achieving friends might also be different in their socio-emotional skills.[21] If they influence the socio-emotional skills of their friends, this could lead eventually to a shift in the aspirations of these friends.

Unfortunately, I cannot access all kinds of information that students have about college returns or about how to get into college. However, it is possible to use their perceived college returns to get a sense of whether they are exchanging information regarding college. If students consider such returns when forming their aspirations and, at the same time, inform each other about these returns, then information diffusion might be a mechanism in place. It is also possible to investigate whether "bad" social norms - such as the fear of being stigmatized as a nerd or peer pressure to work - are diffused among friends. If students decide to comply with such norms, they will most likely lower their aspirations, so the spread of such norms could be a mechanism for peer effects on aspirations.

To test that, Table 5 presents exercises that estimate peer effects on perceived college returns, on the fear of nerd stigma, and on peer pressure to work. The methodology imple-

[20] The works of Jensen (2010), Belfield, Boneva, Rauh, and Shaw (2019) and Peter, Spiess, and Zambre (2018), for instance, show how students' perceptions on college returns influence their aspirations, as well as informing students about possibilities to pursue a college degree.

[21] There is an important stream of literature showing how socio-emotional skills are related to performance. For a review, see the work of Almlund, Duckworth, Heckman, and Kautz (2011).

mented in these estimations is the same as the one described in section 4. The difference is that now the dependent variable and the endogenous social effect are not college aspirations and peers' college aspirations, respectively, but each variable in the columns of the table.

The table shows that, while friends do not seem to impact perceived college returns, they do seem to have an influence on students willingness to comply to social norms: for the average student with two friends, an extra friend who sees the fear of a nerd stigma as an impediment to keep studying increases the likelihood of that student feeling the same way in about 11.7 p.p.. The impact of an extra friend who sees peer pressure to work as an impediment to keep studying is about 11.5 p.p.. Therefore, friends seem to impact more the spread of social norms than of information - or at least information about pecuniary college returns.

I also look at proxies for students' effort in school - such as whether they study math more than 30 minutes per day and whether they had more than 90% of class attendance in both reading and math in that school year - and at students performance in reading and math. While these exercises are interesting in their own right - especially the ones looking at peer effects on students' performance - they might also help to inform whether peer effects on aspirations are coming indirectly, through peer effects on school attitudes and outcomes.

Table 6 shows these exercises. We see that friends do seem to have an impact on students' effort: as shown in column (1), if students spend more time studying Math, this also influences their friends to spend time in this activity. However, friends do not impact class attendance or performance. This last result is quite interesting since it adds to the long literature on peer effects on students' performance, which usually finds at least modest peer effects in primary and secondary education (see Sacerdote, 2011, for a review).

Table 7 finally presents estimation of peer effects on socio-emotional skills. These results should be read carefully since the instruments are very weak. This might be what is driving the large and significant results in column (3). Overall, it looks like there are no peer effects on socio-emotional skills.

7 Peers' aspirations and future outcomes

Once the impact of peers on students' aspirations is verified, I investigate whether such influence spillovers to students' outcomes in school.

I have shown in section 3.1 how students' aspirations are associated with school outcomes such as the likelihood of dropping out of school and of having a normal school path. As highlighted before, such associations cannot have a causal interpretation. However, it is possible to use the previous methodology in order to infer the causal impact that friends' aspirations have on students' outcomes in school. There are several reasons for such an impact to emerge. First, as shown in the main exercises, friends' aspirations influence students' own aspirations, which might change their future outcomes. Second, even after considering students' own aspirations, having aspiring friends might help the studying environment and increase students' performance - since these friends are more invested themselves in school activities -, and might decrease the presence of social norms which curb the willingness to keep studying. Moreover, if aspiring friends tend to go further in their studies, this might prevent students from dropping out or being retained, simply because they want to be around their friends.

Table 8 presents the results of five estimations that measure how peers' aspirations influence students' future outcomes in school. Column (1) shows estimations on students' dropout during high school; column (2) shows estimations on the likelihood of having a normal school path (that is, being at the 12^{th} grade in 2014) for those who did not drop out from school; column (3) shows estimations for a binary variable that indicates whether the student attended to at least 90% of classes in both reading and math in 2013; and columns (4) and (5) present estimations on students performance in reading and math tests that they took in the last year of high school. One can see that even though peers' aspirations does not have an impact on students' future performance, normal school path, or class attendance, it does decrease their likelihood of dropping out of school: for the average student with two friends, an extra aspiring friend decreases the likelihood of dropping out of school by 5.96

p.p..

8 Conclusion

This work overcomes important challenges concerning the estimation of peer effects and investigates the influence that friends' aspirations have on one's own aspirations and future school outcomes. I explore the random allocation of students when first enrolling at middle school to model friendship formation based on homophily on pre-determined characteristics and on students' exogenous chances of interacting. Then, based on the predicted friendship links coming from the model, I use predicted friends of friends' characteristics as instrumental variables for friends' aspirations. This identification strategy overcomes both the problem of endogenous formation of friendships and the reflection problem, largely discussed in the literature of peer effects estimation.

Results show that an extra friend aspiring to go to college not only increases students' own aspirations towards going to college but also decreases students' likelihood of dropping out of school. This brings valuable insights into educational policymaking in developing countries. First, peer effects on aspirations might be a mechanism explaining peer effects on school dropout, result shown by Evans, Oates, and Schwab (1992) and Cipollone and Rosolia (2007), for instance. Aspiring students are less likely to drop out of school. At the same time, as shown here, they also decrease their friends' likelihood of dropping out. Hence, part of the results of peer effects on school dropout could be coming trough peer effects on aspirations.

Second, I find that diffusion of information does not seem to be the mechanism of peer effects on students' aspirations. What does seem to matter as a mechanism here is the existence of social norms and the need students feel to conform to them. Increasing the number of students who aspire to a college degree might lead to a change in certain harmful social norms, such as the stigmatization of students who study hard, which in turn will allow for the realization of students' true educational potential.

Finally, several works show how some educational interventions increase students' aspirations (Carlana, La Ferrara, & Pinotti, 2015; Chiapa, Garrido, & Prina, 2012; Ross, 2017). My results highlight that any impact coming from these interventions spillovers to peers, which should be considered in cost-benefit analysis.

Future works should focus on peer effects in aspirations for contexts different from education attainment. Opportunities in the labor market, for instance, has been shown to increase career aspirations, especially for women (Jensen, 2012). However, peer effects in such a setting might be different from the one found in this work since now one should also consider the presence of competitions for jobs and work hierarchical relations.

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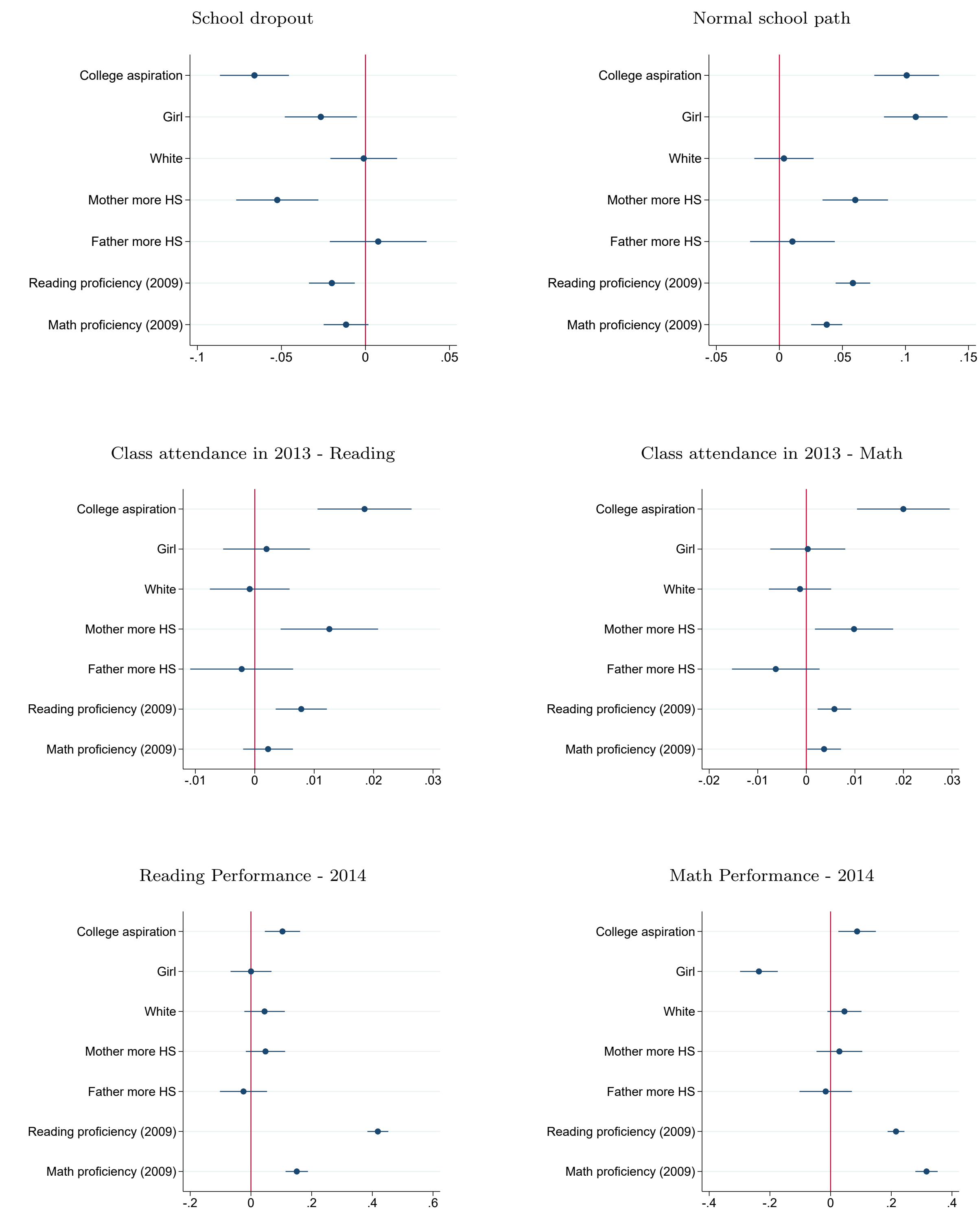
9 Tables & Figures

Figure 1: College aspirations and future outcomes in school



Note: results from OLS estimations with school fixed effects and clustered at school level. All estimations also control for: home ownership, internet at home, and number of lavatories at home.

Table 1: Descriptive Statistics

	All		Coll. aspiration=1		Coll. aspiration=0	
	Mean	Std. Error	Mean	Std. Error	Mean	Std. Error
Own characteristics						
College aspirations	0.68	0.46	1.00	0.00	0.00	0.00
Girl	0.49	0.50	0.56	0.50	0.36	0.48
White	0.33	0.47	0.35	0.48	0.30	0.46
Mother education: more than HS	0.24	0.43	0.26	0.44	0.21	0.41
Father education: more than HS	0.22	0.41	0.24	0.43	0.18	0.38
Reading proficiency (2009)	0.00	1.00	0.14	0.99	-0.30	0.94
Math proficiency (2009)	-0.00	1.00	0.10	1.00	-0.23	0.97
Nominated friends	2.02	1.41	2.16	1.38	1.72	1.42
Friends' characteristics						
College aspirations	0.59	0.42	0.64	0.40	0.47	0.43
Girl	0.43	0.45	0.48	0.45	0.31	0.43
White	0.27	0.34	0.30	0.34	0.22	0.32
Mother education: more than HS	0.21	0.30	0.22	0.31	0.17	0.29
Father education: more than HS	0.19	0.29	0.21	0.30	0.15	0.27
Math proficiency (2009)	0.08	0.66	0.12	0.67	-0.02	0.65
Reading proficiency (2009)	0.10	0.67	0.15	0.68	-0.01	0.63
Nominated friends	1.93	1.35	2.07	1.31	1.63	1.38
Observations	6076		4157		1919	
Number of schools	85					

Note: "College aspiration" is a binary variable that takes value equal 1 if the student indicates that he/she wants to keep studying up to college; Math and Language proficiency are normalized with Mean=0 and SD=1; "Nominated friends is the number friends in the 9^{th} grade nominated by the student".

Table 2: Probability of Forming a Friendship Link

	(1) Raw	(2) OR
$\mathbf{1}[\boldsymbol{x_i} = \boldsymbol{x_j}]$		
Gender	1.504***	4.498***
	(0.049)	(0.222)
Race-white	0.131***	1.140***
	(0.024)	(0.027)
Race-black	0.162***	1.176***
	(0.045)	(0.053)
Class in 2008	1.347***	3.847***
	(0.107)	(0.412)
x_j **characteristics**		
Girl	0.147***	1.158***
	(0.035)	(0.040)
Race-White	0.053**	1.055**
	(0.024)	(0.025)
Race-Black	0.119***	1.126***
	(0.041)	(0.046)
N (potential links)	524,724	524,724

Note: (i) This table shows the estimation of a conditional logistic regression model to predict the likelihood that a student i will send a friendship tie to another student j in the 9^{th} of the same school; Raw is the raw coefficient coming from the model and OR is the odds ratio; the estimation controls for i's fixed effects; (ii) Standard errors clustered at school level; (iii) Class in 2008 is the class where students were allocated when enrolling in the first grade of middle school, when they switch from municipal to state-owned school. The allocation into these first classes is made at random; (iv) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

Table 3: Peer effects on aspirations (N=6,076)

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: College Aspirations					
Friends' college aspirations	0.147***	0.135***	0.151**	0.140**	0.152***	0.133***
	(0.052)	(0.051)	(0.061)	(0.060)	(0.048)	(0.046)
Instruments	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^3X$	$\hat{G}^3X$	$\hat{G}^2X$, $\hat{G}^3X$	$\hat{G}^2X$, $\hat{G}^3X$
IVs' joint significance	36.494	28.566	31.658	24.864	26.408	19.488
Control for Socio-Emot. Skills	No	Yes	No	Yes	No	Yes
Control for own characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Control for friends' characteristics	Yes	Yes	Yes	Yes	Yes	Yes

Note: (i) This table shows estimations of models as the one described in equation 2, where friends' college aspirations is instrumented by the predicted friends-of-friends' characteristics ($\hat{G}^2X$), or by the predicted friends-of-friends-of-friends' characteristics ($\hat{G}^3X$), or by both ($\hat{G}^2X$, $\hat{G}^3X$); (ii) Standard errors clustered at school level; (iii) All regressions include school FE and the following characteristics (for both students and their friends): gender, race, math and reading performance in 2009, parents' education, father working status, home ownership, internet at home, and number of lavatories at home; (iv) Socio-emotional skills: self-esteem, self-efficacy, self-control, agreeableness, rapport with peers, locus of control (for more information, see PROJECT WITH RANGEL AND MADEIRA); (v) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

Table 4: Heterogeneous impacts - (N=6,076)

	(1)	(2)	(3)	(4)
	Dependent variable: college aspirations			
	Boys	Non-white	Mother less HS	Father less HS
Friends' aspirations	0.103	0.105*	0.123**	0.103*
	(0.068)	(0.058)	(0.054)	(0.062)
Friends' aspirations x Variable in column	0.076	0.053	0.035	0.057
	(0.049)	(0.037)	(0.053)	(0.049)
Joint significance of Peers' aspirations				
P-value	0.001	0.011	0.014	0.013
Instruments	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$
IVs' joint significance	24.801	20.997	26.881	21.387
Control for own characteristics	Yes	Yes	Yes	Yes
Control for friends' characteristics	Yes	Yes	Yes	Yes

Note: (i) This table shows estimations of models as the one described in equation 2, where friends' college aspirations is instrumented by the predicted friends-of-friends' characteristics ($\hat{G}^2X$); (ii) Friends' aspirations are interacted with each characteristic in the columns and such interaction is instrumented by $\hat{G}^2X$ interacted with this characteristic as well; (iii) Standard errors clustered at school level; (iv) All regressions include school FE and the following characteristics (for both students and their friends): gender, race, math and reading performance in 2009, parents' education, father working status, home ownership, internet at home, and number of lavatories at home; (v) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

Table 5: Peer effects on college return and on compliance with social norms (N=6,076)

	(1)	(2)	(3)
Dependent variable:	Perceived college returns	Fear of nerd stigma	Peer pressure to work
Endogenous Social Effects	-0.010	0.235***	0.231***
	(0.034)	(0.082)	(0.072)
Instruments	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$
IVs' joint significance	119.275	17.698	19.988
Mean Dep. Var.	0.612	0.256	0.298
Control for own characteristics	Yes	Yes	Yes
Control for friends' characteristics	Yes	Yes	Yes

Note: (i) This table shows estimations of models as the one described in equation 2, where the endogenous social effects (that is, friends' perceived college return in column (1), friends' fear of nerd stigma in column (2), and friends' feeling peer pressure to work in column (3)) are instrumented by the predicted friends-of-friends' characteristics ($\hat{G}^2X$); (ii) Standard errors clustered at school level; (iii) All regressions include school FE and the following characteristics (for both students and their friends): gender, race, math and reading performance in 2009, parents' education, father working status, home ownership, internet at home, and number of lavatories at home; (iv) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

Table 6: Peer effects on school effort and school performance

Dependent variable:	(1) Studies +30min of math/day	(2) More than 90% of attendance (2011)	(3) Reading performance (2011)	(4) Math performance (2011)
Endogenous Social Effects	0.169**	0.096	-0.232	0.053
	(0.083)	(0.096)	(0.232)	(0.359)
Instruments	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$
IVs' joint significance	17.683	6.078	3.659	1.514
Mean Dep. Var.	0.396	0.336	-0.000	-0.000
Control for own characteristics	Yes	Yes	Yes	Yes
Control for friends' characteristics	Yes	Yes	Yes	Yes

Note: (i) This table shows estimations of models as the one described in equation 2, where the endogenous social effects (that is, friends' study +30min of math/day in column (1), friends' attended to more than 90% of classes in 2011 in column (2), and friends' performance in reading and math in 2011 in column (3) and (4), respectively) are instrumented by the predicted friends-of-friends' characteristics ($\hat{G}^2X$); (ii) Standard errors clustered at school level; (iii) All regressions include school FE and the following characteristics (for both students and their friends): gender, race, math and reading performance in 2009, parents' education, father working status, home ownership, internet at home, and number of lavatories at home; (iv) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

Table 7: Peer effects on socio-emotional skills

Dependent variable:	(1) Self-esteem	(2) Self-efficacy	(3) Self-control	(4) Agreea-bleness	(5) Rapport with peers	(6) Locus of control
Endogenous Social Effects	-0.010	-0.004	1.442***	-0.102	0.071	-0.545
	(0.378)	(0.360)	(0.514)	(0.236)	(0.201)	(0.332)
Instruments	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$
IVs' joint significance	1.267	1.179	1.002	1.816	3.206	2.034
Control for own characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Control for friends' characteristics	Yes	Yes	Yes	Yes	Yes	Yes

Note: (i) This table shows estimations of models as the one described in equation 2, where the endogenous social effects (that is, friends' socio-emotional skills - for more information, see PROJECT W/ RANGEL AND MADEIRA) are instrumented by the predicted friends-of-friends' characteristics ($\hat{G}^2X$); (ii) Standard errors clustered at school level; (iii) All regressions include school FE and the following characteristics (for both students and their friends): gender, race, math and reading performance in 2009, parents' education, father working status, home ownership, internet at home, and number of lavatories at home; (iv) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

Table 8: Friends' aspirations and students' future outcomes

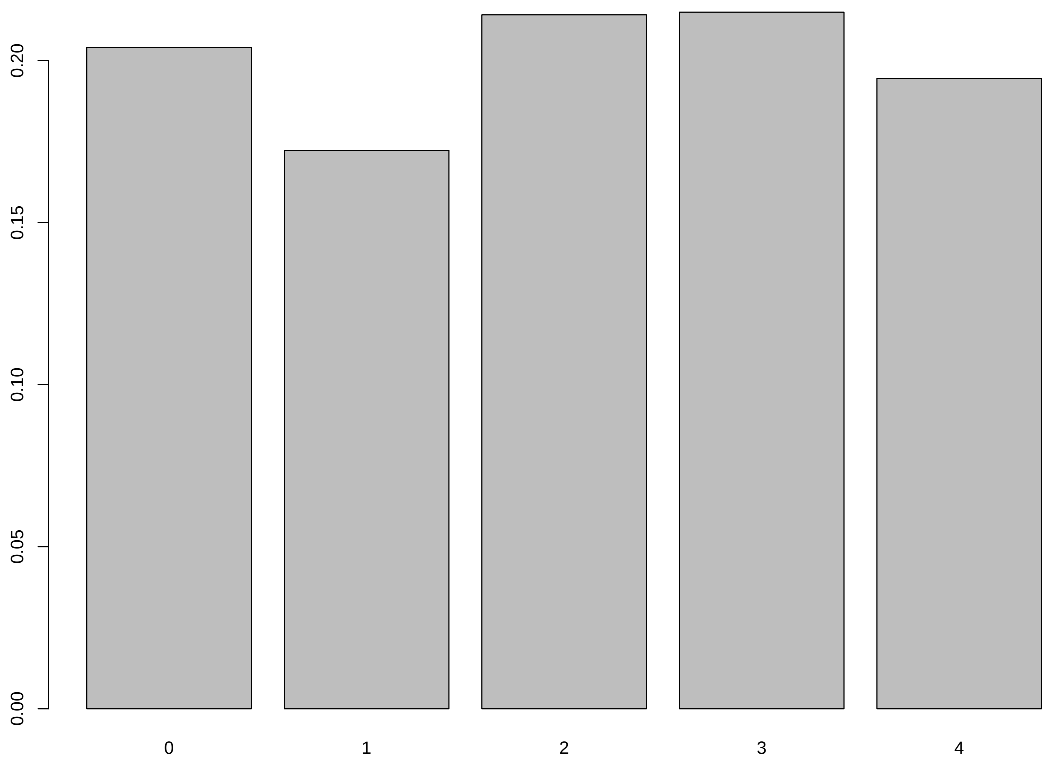
Dependent variable:	(1) Dropout	(2) Normal school path	(3) Attendance 2013	(4) Reading 2014	(5) Math 2014
Friends' college aspiration	-0.119***	0.038	0.009	0.078	0.191
	(0.042)	(0.056)	(0.049)	(0.176)	(0.172)
Instruments	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$
N	6076	5053	4857	3200	3200
IVs' joint significance	36.494	43.256	42.637	39.364	39.364
Mean Dep. Var.	0.201	0.766	0.336	-0.004	-0.011
Control for own characteristics	Yes	Yes	Yes	Yes	Yes
Control for friends' characteristics	Yes	Yes	Yes	Yes	Yes

Note: (i) This table shows estimations of models as the one described in equation 2, where friends' college aspirations is instrumented by the predicted friends-of-friends' characteristics ($\hat{G}^2X$); (ii) The sample size changes depending on the estimation due to students dropout and to whether students took the diagnostic exams in 2014; (iii) All regressions include school FE and the following characteristics (for both students and their friends): gender, race, math and reading performance in 2009, parents' education, father working status, home ownership, internet at home, and number of lavatories at home; (iv) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

A Appendix

A.1 Additional tables and figures

Figure A.1: Out degree distribution



Note: Each student was asked to nominate at most four of their best friends or colleagues at school. This graph shows the distribution of the number of nominated friends by each student.

Table A.1: Peer effects on aspirations - comparing OLS, 2SLS, and 3SLS

	(1)	(2)	(3)
	Dependent Variable: College aspirations		
Friends' aspirations	0.069***	0.201**	0.147***
	(0.022)	(0.085)	(0.052)
Model	OLS	IV: G^2X	IV: $\hat{G}^2X$
IVs' joint significance		24.548	36.494
Control for own characteristics	Yes	Yes	Yes
Control for friends' characteristics	Yes	Yes	Yes

Note: (i) Standard errors clustered at school level; (ii) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$; (iii) All regressions include school FE and the following controls (both for students and their friends): gender, race, math and reading proficiency, father working status, internet at home, and number of lavatories at home.

Table A.2: Falsification exercise

	(1)	(2)	(3)	(4)
	Mother more than HS	Father more than HS	House ownership	College Aspirations
Endogenous Social Effects	0.318*	0.095	0.237	0.146***
	(0.188)	(0.278)	(0.189)	(0.053)
Model	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$
IVs' joint significance	3.249	2.236	2.833	43.491

Note: (i) Standard errors clustered at school level; (ii) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$; (iii) All regressions include school FE and the following controls (both for students and their friends): gender, race, math and reading proficiency, father working status, internet at home, and number of lavatories at home.

Table A.3: Robustness Check

	Dependent Variable: College aspirations		
	(1)	(2)	(3)
Friends' aspirations	0.143**	0.145**	0.142**
	(0.056)	(0.058)	(0.060)
Model	$\hat{G}^2X$	$\hat{G}^2X$	$\hat{G}^2X$
N	6075	4894	4893
Mean Dep. Var.	0.684	0.665	0.665
IVs' joint significance	39.917	31.570	35.898
Control for classroom FE	Yes	No	Yes
Maximum out-degree	4	3	3

Note: (i) Standard errors clustered at school level; (ii) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$; (iii) Math and Reading proficiency are normalized with mean zero and standard-deviation one; (iv) All regressions include school FE and the following controls: gender, race, math and reading proficiency, father working status, home ownership, internet at home, and number of lavatories at home.

Your Peers' Parents: Spillovers from Parental Education*

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October 8, 2019

Abstract

Better-educated parents bestow significant advantages on their children in life; we explore whether this advantage multiplies, spilling over to classmates. Using a nationally-representative sample of US kindergarteners, we find significant effects of the parental education of classmates on math and reading, but not on socio-emotional skills. The effects are economically meaningful: reassigning classrooms so that all students have the same parental education composition would narrow the achievement gap between children of parents who are high-school-educated (or less) and those who are university-educated by 9 to 13 percent. These spillovers are not explained by rich, beginning of the school-year, measures of cognitive and socio-emotional skills, nor by race or socioeconomic status. Interestingly, not all spillovers from parental education are positive. In reading, we find that university-educated parents who are not working full-time create some negative spillovers for the classroom, which appear to come from their children's relatively advanced reading skills.

Key words: peer effects; parental education; teaching practice

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1 Introduction

Numerous studies document the strong ties between parental education and their children's achievement, educational attainment and wages (Bjorklund and Salvanes, 2011). Ninety-three percent of children whose parents have a university degree enroll in postsecondary education, compared to 72 percent of children with less educated parents (Cataldi et al., 2018). Children whose parents have a university degree are also four times less likely to repeat a grade in school, compared to those whose parents have only a high school degree.[1] We explore whether the advantage of having better-educated parents spills over to help or hinder classmates.

There are many reasons we might expect students to benefit from being in classrooms where peers have better-educated parents. Most of these relate to correlations between parental education and cognitive or socio-emotional skills of their children. These correlations could arise for many reasons. For instance, better-educated parents tend to invest more in their children's education; they may be better able to teach their children or engage in better parenting practices (Bianchi and Robinson, 1997; Hill and Stafford, 1974; Guryan et al., 2008). The literature generally supports positive spillovers from better-behaved or higher-achieving classmates (Epple and Romano, 2010; Sacerdote, 2011), though there is a strand suggesting the potential for higher-achieving peers to have negative effects through lower academic self concept (e.g., Marsh et al., 2008). The preparation of classmates could also matter indirectly by affecting the teacher's pedagogical practices. Better-educated parents also tend to be more connected and better able to advocate for their children, which could have positive or negative consequences for classmates, depending on whether this involvement enhances or detracts from their learning (Walsh, 2008).

In this paper, we test whether parental education spillovers exist and investigate the sources of these spillovers. The Early Childhood Longitudinal Survey of Kindergartners (ECLS-K), a nationally representative sample of kindergartners in the US in 1998/99, is

[1] See `https://www.childtrends.org/`.

well-equipped for addressing these questions. It includes rich survey data on both parental and teacher inputs, as well as both cognitive and behavioral measures of students' abilities at the beginning and the end of the school year. A key challenge for identifying an effect of peers is nonrandom assignment of students to classrooms. In this case, an apparent effect of peer parental education could simply be a result of positive selection, i.e., the presence of better-educated parents signals better teacher quality in dimensions that are difficult to measure. We alleviate the more salient selection concerns by controlling for school fixed effects.[2] The focus on kindergarten also helps alleviate concerns about within-school tracking, as parents and principals have less information in the first year of school from which to track students and arguably there is a greater distaste for tracking in early grades. Robustness checks and tests for random assignment within schools largely confirm this.[3]

An additional concern with this strategy is whether there is sufficient within-school variation to identify peer effects.[4] The within-school variation in peer parental education ranges from 0.17 to 0.20 and is more than half of the between-school variation. Furthermore, while the ECLS-K has unusually rich measures for testing our key hypotheses, a disadvantage is that not all students in the school were sampled, leading to measurement error in peer variables. Fortunately, we can address this by applying Sojourner (2013)'s method to correct for the measurement error from missing observations of classroom peers. This involves weighting the observed peer variables by the percentage of classmates that are observed.

We find that peer parental education matters and contributes significantly to existing

[2]A large number of papers rely on school fixed effects to eliminate selection into schools, e.g., Hoxby (2000), Lavy et al. (2012), Hanushek et al. (2009). In some cases, they rely on plausibly random cross-cohort variation in grades with grade-level measures of peers or on random assignment to classrooms, as argued here. Our identification strategy most closely relates to Ammermueller and Pischke (2009) and Gould et al. (2009), which exploit plausibly random variation across classrooms within schools.

[3]More than 80% of schools appear to assign students randomly to classrooms using fairly strict rejection criteria and exploiting rich information on initial achievement, which captures many of the underlying unobservables that might lead to selection. Further, results are robust to restricting to the sub-sample where random assignment appears to hold. Using the same data, Neidell and Waldfogel (2010) also find support for the assumption of random assignment to classrooms in kindergarten, but not for first grade, which fits the intuition that selection occurs more as children age and there is more information available to assign them to classrooms.

[4]See Booij et al. (2017) and Carrell et al. (2013) for recent discussions.

inequalities. The source of these spillovers seems to be primarily from unmeasured skills, not captured by rich measures of initial cognitive and socio-emotional skills, nor by race or socioeconomic status. We develop a test for whether peer parental education primarily proxies for spillovers from some underlying unobserved skill. The test supports this hypothesis for math but not for reading. For reading, we find evidence of negative peer effects from some types of university-educated parents. We further investigate the source of these negative spillovers, using the rich perspective afforded by the teacher and parent surveys.

This paper contributes to a burgeoning literature on peer effects (See Sacerdote, 2011; Epple and Romano, 2010; Brock and Durlauf, 2001, for recent reviews). While a number of studies examine peer spillovers deriving through ability, racial composition, and behavior, less is known about spillovers from parental education. Haraldsvik and Bonesronning (2014), McEwan (2003), and Bifulco et al. (2011) are most related in studying the direct effects of peer parental education on test scores, though at later ages (fifth graders, eighth graders and high-schoolers respectively). Haraldsvik and Bonesronning (2014) and McEwan (2003) focus on Chile and Norway, respectively, and find significant effects of peer parental education on test scores. By contrast, Bifulco et al. (2011) finds no effect of peer parental education on GPA in the US.[5]

We contribute to this sparse literature on the achievement effects of peer parental education by focusing on an earlier age and the US context, where the spillovers may differ from other countries due to different cultural norms, the emphasis teachers put on engagement of parents and pedagogical practices. Kindergarten is an important period for child development (e.g., Cunha et al., 2006) and parental education spillovers are of particular interest in kindergarten because of the important role parents play in supporting the child's education at this age (Cunha and Heckman, 2008; Del Boca et al., 2013; Bono et al., 2016; Vaughan, 2019). Parents may also be more aware and sensitive to how their child's perfor-

[5]Bifulco et al. (2011) and Black et al. (2013) focus on long-run effects of peer parental education for adolescents and come to different conclusions about the importance of parental education spillovers. Our findings also relate in interesting ways to work showing spillovers from education in labor market wages (Moretti, 2004).

mance compares to their classmates, particularly for skills such as knowing how to read, and may adjust their investments or demands on the teacher accordingly, to the benefit or cost of other students in the class.

We also contribute to the literature on spillovers from peer parental education by using rich data on teacher and parent reports to understand underlying sources of these peer effects. Studies have used similar data to understand mechanisms of peer effects, but for other types of peer spillovers where the mechanisms may be different. For instance, Lavy and Schlosser (2011) and Lavy et al. (2012) distinguish between mechanisms of gender and ability spillovers using survey data on student and teacher reports of classroom climate and pedagogy. Duflo et al. (2011) and Jackson (2016) show evidence of an indirect effect of peers arising through effects on teacher inputs. We extend the role of teachers beyond effort and teacher-student relationships to consider what skills the teacher chooses to emphasize. In distinguishing mechanisms, we also benefit from rich survey information from the parent. Our work is also similar in spirit to Hoxby and Weingarth (2005), De Giorgi and Pellizzari (2011), which distinguish between different potential mechanisms underlying peer effects based on modeling assumptions, as we do to provide evidence of spillovers from remaining unobserved skills.

Our research relates in interesting ways to the existing peer effects studies on income-related spillovers (e.g., Ammermueller and Pischke, 2009; Boisjoly et al., 2006; Abdulkadiroglu et al., 2014) and racial composition spillovers (e.g., Hanushek et al., 2009; Hoxby, 2000; Card and Rothstein, 2007; Vigdor and Ludwig, 2008), strong correlates of parental education. We find that controlling for peer income and racial composition does not explain parental education spillovers, suggesting that the mechanisms of these effects could be quite different. An even larger literature illustrates the importance of peer achievement, most frequently measured by the lagged achievement students bring to the classroom (e.g., Sacerdote, 2011, for review). Again, despite having potentially better measures of initial skills from tests taken at the beginning of kindergarten (rather than at the end of the previous year where differential summer learning loss could have occurred (Borman and Boulay, eds, 2004) and unusually rich measures of socio-emotional skills, we find that peer parental education matters in ways not captured by the students' initial performance. These findings re-enforce the importance of studying peer parental education spillovers in their own right.

In what follows, we first describe the data in Section 2. We then describe the empirical model in Section 3, how we deal with measurement error, estimates of the overall effect of peer parental education and robustness to concerns about non-random assignment. Section 4 tests whether initial measures of skill explain the spillovers from peer parental education and describes a test for whether the estimates are consistent with parental education proxying for remaining unobserved skill. Section 5 turns to the puzzle of why university-educated parents in reading fail to generate positive spillovers of equal or larger value to those generated by high-school-educated parents. Section 6 concludes.

2 Data

The Early Childhood Longitudinal Survey of Kindergartners is a nationally-representative sample of kindergartners in the US in the 1998/99 school year. Students were sampled randomly within schools with the aim of sampling about 20 students per school. The data include a rich set of information from the child's parents and teachers. Though the survey is longitudinal, we focus on the first year of the survey, when children were in kindergarten and there was a relatively large number of student observations per classroom, about 7.5 on average. We identify students' classrooms using the teacher ID's combined with whether the child was in a full day, morning or afternoon kindergarten class, to account for the fact that some teachers taught 2 half-day classes with different sets of students. The teachers also report class size, which we use to correct for measurement error in peers, as discussed in Section 3.2.[6] The average class size is 20.44.

[6] Note that class sizes with numbers less than 10 were encoded as 10 and those greater than 30 were encoded as 30 for confidentiality reasons. However, we also sum teacher reports of the number of boys and

We focus primarily on mathematics and reading skills for comparison to the literature, but also consider effects on socio-emotional skills. The data include direct assessments of reading and math performance and teacher assessments of socio-emotional skills, including self-control, interpersonal skills, and externalizing problems, measured both in the fall and the spring. We treat these measures as signaling a common underlying factor and extract that factor using the Regression method. We call this factor *socio-emotional skills*, a measure that is increasing in interpersonal behaviors that are conducive to learning.[7]

The parent survey includes information on the highest level of education attained by both the father and mother. Our measure of parental education takes the value of whichever parent is better-educated and/or the education of whichever parent is present. We create a dummy for whether the parent had at least some education post-high school but less than a university degree and then a dummy for having at least a university degree, which roughly cuts the sample into thirds.[8] Henceforth, we refer to parents who have more than a high-school degree but less than a university degree as *parents with more than a high-school degree* and parents with a university-degree or more as *university-educated parents*. Peer parental education is measured as the proportion of classmates whose parents have more than a high school degree but less than a university degree or at least a university degree, leaving the proportion of classmates with a high school degree or less as the excluded category. The parent survey also includes useful information for testing possible mechanisms of parental spillovers, such as parental home inputs - including activities with the child such as reading

the number of girls in the class, subtracting the number of students who left and adding the number of students who came to construct an alternative measure of class size. We use this instead for classes that are measured as size 10 or 30, and the alternative measure gives a number that is smaller than 10 (for the case of size 10 classrooms) or larger than 30 (for the case of size 30 classrooms). Out of concern about outliers, we set to missing observations below the 1st percentile (8) and above the 99th percentile (47).

[7]Exploratory factor models and principal component model support that treating these measures as loading on a single factor is appropriate; a single component explain almost 0.8 of the variance and there is only one eigenvalue greater than 1. The data also include internalizing behaviors and approaches to learning as alternative measures of socio-emotional skills, but we find little evidence of these other inputs leading to peer spillovers and so exclude them to focus on more externalizing mechanisms that are generally the focus of the literature.

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

books, singing songs, playing games, etc. -, parents' working status - whether they are working full time, part time, or unemployed -, parents' mental health, and information on whether parents are facing financial problems.

From the teacher survey, we have detailed information on the teacher's background, including gender and race. We measure teacher experience as the number of years the teacher taught any age group. Teacher tenure captures the number of years at the current school. The data also include less common measures of teacher inputs from the spring survey, such as the amount of unpaid preparatory work they put into their classes and the amount of time they put into teaching different skills. The teacher reports in the fall survey whether each student knows how to read, uses complex sentences or understand texts read to him/her, which we exploit to understand mechanisms of reading spillovers in Section 5. Appendix Table A.1 includes more detailed descriptions of the variables.

Table 1 presents summary statistics of the final sample by parental education.[9] The average test scores of students are increasing in parental education. Socio-emotional skills are increasing in parental education. Students with higher parental education also have peers with higher parental education, average test scores, and socio-emotional skills. Column 4 presents p-values, which show statistically significant differences across columns. Teachers with more years of tenure at the same school are more likely to teach children whose parents have only a high school degree or less than children whose parents have a university degree or more. In contrast, teacher experience is higher for children whose parents have more education. Almost all the teachers are female and there are no differences across parental education status.

Because we focus on within-school variation to identify peer effects, this naturally raises concerns about whether there is enough variation in our data to identify peer effects. The within-school variation in peer parents having more than a high school degree is 0.20 which is slightly larger than the between-school variation of 0.17. The within-school variation in

[9]Appendix A.1 provides details of the sample selection.

Table 1: Mean Characteristics by Parental Education

		<=HS	>HS	Univ+	P-Value	P-value(FE)
Student	Math	0.72	1.15	1.82	0.00	0.00
	Reading	0.68	1.05	1.69	0.00	0.00
	Fall Math	-0.34	-0.00	0.54	0.00	0.00
	Fall Reading	-0.35	-0.07	0.44	0.00	0.00
	Socio-emotional skills	-0.07	-0.00	0.10	0.00	0.00
	Fall socio-emotional skills	-0.05	0.00	0.08	0.00	0.00
Peer	Parent HS+	0.35	0.38	0.29	0.00	0.00
	Parent Univ+	0.19	0.29	0.53	0.00	0.00
	Fall Math	-0.16	0.03	0.35	0.00	0.63
	Fall Reading	-0.18	-0.04	0.26	0.00	0.32
	Fall socio-emotional skills	-0.02	0.00	0.05	0.00	0.94
Parent	Mom HS+	0.00	0.81	0.19	0.00	0.00
	Mom University+	0.00	0.00	0.74	0.00	0.00
	Dad HS+	0.00	0.53	0.13	0.00	0.00
	Dad University+	0.00	0.00	0.74	0.00	0.00
	Dad Present	0.67	0.81	0.94	0.00	0.00
Teacher	Female	0.99	0.99	0.99	0.87	0.65
	White	0.86	0.91	0.95	0.00	0.65
	Experience	14.42	14.69	14.88	0.08	0.89
	Tenure	9.75	9.77	9.33	0.02	0.68
	N	3,894	4,126	4,180		

Reported p-values test whether means are significantly different across the 3 categories of parental education. The last column presents p-values after including school fixed effects, standard errors are clustered at the school level. Variables defined in Appendix Table A.1.

peer parents having a university degree or more is 0.17 which is still more than half of the between-school variation of 0.27.

3 Total Effect

Let Y_{ics} denote the outcome of a child i in class c and school s, Y_{i0}, i's achievement measured at the start of kindergarten, X_i parental education, measured as dummies for whether the parent has more than high school (but less than university) and whether the parent has a university degree or more. $\bar{X}_{-ics}$ captures the proportion of classmates' parents with more than high school education and the proportion university-educated, excluding i. Achievement is determined according to the commonly-used linear-in-means specification

$$Y_{ics} = \beta_0 + X_i\beta_X + \bar{X}_{-ics}\beta_{\bar{X}} + Y_{i0}\beta_Y + \epsilon_{ics}, \tag{1}$$

where ϵ_{ics} denotes the residual. We condition on initial achievement, estimating the typical value-added specifications that are the focus of the literature. We also condition on teacher background characteristics throughout, but ignore them here to simplify notation. The parameter of interest is the effect of peer parental education, $\beta_{\bar{X}}$ (characterized as the *social effect* by Manski (1993)), i.e., a combination of contextual and endogenous effects. In Section 4 and 5, we explore potential mechanisms underlying this social effect, using both richer data and some simple theory, both suggested paths forward when Manski (1993) initially laid out the reflection problem.

3.1 Nonrandom Assignment

A central challenge in identifying the effect of peer parental education is that parents may select into schools or classrooms, thus introducing correlation between $\bar{X}_{-ics}$ and the residual, ϵ_{ics}. For instance, if better-educated parents select better schools, it may appear that peer parental education matters when in reality it proxies for school quality. Likewise, if more able students are grouped together, peer parental education may appear to matter simply as a proxy for own innate ability.

Consistent with much of the literature, we assume that controlling for school fixed effects addresses selection into schools (e.g. Lavy et al., 2012; Hoxby, 2000; Hanushek et al., 2009). We also assume that assignment to classrooms within schools is random.[10] What makes this plausible in our context is that this is the child's first year of school and there is less scope for tracking than in older grades where both parents and teachers have more information.

[10]This latter assumption diverges from some of the literature which instead measures peer groups at the grade level and relies on random variation across cohorts instead.

Consistent with this hypothesis, Neidell and Waldfogel (2010) present evidence of limited tracking in kindergarten and find more evidence of tracking by the time the child reaches first grade, using these data.

The last column of Table 1 includes p-values for whether parental education is a significant predictor of either peer fall test scores or teacher characteristics, after controlling for school fixed effects. Despite the fact that peer and teacher characteristics are almost all significantly different across parental education without school fixed effects, the characteristics are not significantly different across parental education after including school fixed effects, providing further support for random assignment within schools.[11] We complement this evidence with a test for whether parental education and initial achievement appear to be randomly assigned across classrooms using Fisher's exact test, which is a stronger test in this setting than the balancing tests presented in Table 1. The use of both measures makes it a particularly strong test, as initial achievement would likely capture many of the unobservables that would matter for determining classroom placement and later achievement. We discuss this and further tests for bias from nonrandom assignment in Section 3.

3.2 Measurement Error in Peers

As discussed above, a limitation of these data is that only a sample of classmates are observed, so that there is measurement error in the peer variables. To the extent that this creates random measurement error, it will bias estimates of the effect of peer parental education toward 0. This problem is common in the literature but more severe with the ECLS-K because only about a third of classmates are observed on average. We follow the approach in Sojourner (2013) to correct for measurement error in peer characteristics.

Let p_c denote the percentage of classroom peers who are observed.[12] Let $\bar{X}^m_{-ics}$ denote

[11] Note that peer parental education is not independent across parental education within schools. This is true by construction by using the leave-one-out mean, as discussed in Guryan et al. (2009). We turn to Fisher's exact test to determine apparent random assignment by parental education and complement this with some of the more traditional balancing tests following the correction of Guryan et al. (2009) in Section 3.4.

[12] To simplify exposition, we assume a balanced panel. As we add covariates, we lose observations and

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

$$
\begin{aligned}
E(Y_{ics}|X_i, \bar{X}^o_{-ics}, Y_{i0}, p_c, s, d_i = 1) = \beta_0 + X_i\beta_X + p_c\bar{X}^o_{-ics}\beta_{\bar{X}} + Y_{i0}\beta_Y \\
+ (1 - p_c)E(\bar{X}^m_{-ics}|X_i, \bar{X}^o_{-ics}, Y_{i0}, p_c, s, d_i = 1)\beta_{\bar{X}} \\
+ E(\epsilon_{ics}|X_i, \bar{X}^o_{-ics}, Y_{i0}, p_c, s, d_i = 1), \quad (2)
\end{aligned}
$$

where $\epsilon_{ics} = \alpha_s + \nu_{ics}$. Thus, the unobservability of a subset of peer characteristics adds an extra term to the residual. Suppose that the following assumptions hold:

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

and

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

As discussed in Sojourner (2013), the first assumption holds if there is random assignment to classrooms within schools, so that, for instance, shared unobservable teacher characteristics are not correlated with own and observed peer parental education, initial achievement and the percentage observed. The second holds if students are missing in similar ways across classrooms, in which case controlling for school fixed effects helps adjust for these missing observations. A sufficient, but not necessary, condition for this is that students are missing at random. However, this condition is also satisfied if, for instance, low-achieving students are more likely to be missing due to survey non-response, but they are randomly assigned to classrooms within schools and are missing at random across classrooms. Given these assumptions, Sojourner (2013) shows that missing data can be controlled by including school

need to adjust p_c accordingly.

fixed effects and interacting the school fixed effects with p_c. He further shows through Monte Carlo simulations that results are robust to just controlling for school fixed effects and p_c, which is our preferred approach. Adding in the interactions of the school fixed effects with p_c increases standard errors due to multicollinearity, though we show, as in Sojourner (2013), that results are robust to the more flexible models explored in Sojourner (2013).[13] The final equation is estimated only on students whose whole set of covariates is observed ($d_i = 1$) as follows:

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

3.3 Results: Total Effect

Table 2 presents estimates of the effect of peer parental education using the measurement error correction described in Section 3.2. Columns (1), (4) and (7) present results for reading, math, and socio-emotional skills, controlling for school fixed effects, prior achievement/socio-emotional skills, and teacher characteristics, including gender, a dummy for whether the teacher is white, experience, experience-squared, tenure, and tenure-squared.[14] We cluster all standard errors at the school level to be conservative and account for remaining correlation within schools. Standard errors clustered at the class level are marginally smaller.

The marginal effect of having peers whose parents have more than a high school degree (but less than a university degree) is 0.31 in math, 0.48 in reading, and 0.09 (not statistically significantly different from zero) for socio-emotional skills. The marginal effect of peers with a university degree or more is 0.43 in math, 0.27 (not statistically significantly different from zero) in reading, and 0.05 (not statistically significantly different from zero) for socio-emotional skills. Joint tests of statistical significance (reported at the bottom of the table) support that peer parental education matters for reading and math but not for socio-emotional skills.[15]

[13] See appendix A.3.

[14] Online Appendix Table A.3 provides the full regression results. Results are very similar whether we control for all measures of initial skill or just the measure corresponding to the outcome.

[15] Online Appendix Table A.4 shows results without Sojourner's correction, for the sake of comparison.

Table 2: Total Effect of Peer Parental Education

	Math			Reading			Socio-emotional skills		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Parent more than HS	0.08***	0.07***	0.08***	0.06***	0.05***	0.05**	0.02**	0.02*	0.02**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Parent univ+	0.17***	0.16***	0.17***	0.12***	0.12***	0.12***	0.05***	0.05***	0.06***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Peer parent HS+	0.31**	0.26	0.34**	0.48***	0.48**	0.56***	0.09	0.11	0.16
	(0.14)	(0.17)	(0.17)	(0.16)	(0.19)	(0.20)	(0.10)	(0.12)	(0.13)
Peer parent univ+	0.43**	0.56***	0.53***	0.27	0.36	0.41*	0.06	0.08	0.05
	(0.17)	(0.19)	(0.20)	(0.20)	(0.23)	(0.24)	(0.12)	(0.14)	(0.15)
N	12200	11137	10170	12200	11137	10170	12200	11137	10170
R^2	0.69	0.68	0.68	0.69	0.69	0.69	0.42	0.42	0.42
RA sample	N	Y	Y	N	Y	Y	N	Y	Y
Joint Significance Peer Parental Education									
F-stat	3.68	4.09	3.90	4.80	3.20	3.76	0.40	0.44	0.93
p-value	0.03	0.02	0.02	0.01	0.04	0.02	0.67	0.65	0.40
Bounds (Altonji et al., 2005) - β^*									
Peer parent HS+	0.13	0.10	0.21	0.42	0.48	0.58	0.10	0.13	0.12
Peer parent univ+	0.14	0.28	0.45	0.26	0.40	0.60	-0.03	-0.10	-0.07

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for class size and p_c (percentage of students observed in the class), school fixed effects, prior achievement/socio-emotional skills, and teacher characteristics, such as female, white, experience, experience-squared, tenure and tenure-squared. Peer variables are interacted with p_c to deal with missing values. Columns (2), (5) and (8) correspond to a subsample of schools that appear to be randomly assigning students based on parental education, using a p-value from Fisher's exact test of greater than 0.1, as discussed in Section 3 and including schools that only appear to have 1 classroom for kindergarten in the random assignment sample. Columns (3), (6) and (9) exclude schools where Fisher's test of random assignment of student by initial achievement (high, medium and low) has a p-value greater than 0.1. Socio-emotional skills are measured by extracting a common factor from fall teacher reports of externalizing behaviors, interpersonal skills and self control. β^* is one of the bounds for Peer parent HS+/Peer parent univ+ [see Oster (2019)]. The other bound is the one displayed at the coefficients of the Table.

As discussed above, the key concern with the above identification strategy is whether there is selection into classes within schools. We use Fisher's exact test, which is appropriate to this setting where there are small cell sizes, to look for evidence of selection based first on parental education and then on initial achievement. The test is only valid when there are multiple classes in the school, which is 86% of the sample. Of the schools with multiple classes, only 9% have a p-value of 0.1 or lower, which is a conservative estimate for rejecting random assignment based on parental education. In columns (2), (5) and (8), we define the random assignment sample to be those with a p-values above 0.1.[16] Results are very similar. Finally, in columns (3), (6) and (9), we impose the additional restriction that the schools must also pass Fisher's exact test for whether initial math and reading achievement appear to be randomly assigned across classrooms.[17] This drops an additional 8 percent of the sample based on a very strict rejection criteria of a p-value less than or equal to 0.1. Again results are remarkably similar in this restricted sample compared to the original sample. We use this final restricted "apparent random assignment" sample for the main specifications below to be more conservative.

Often peer effect sizes are interpreted in terms of a one standard deviation increase in the variable of interest. The observed classroom peer groups have a standard deviation of 0.26 for the percentage of peers with more than a high school degree but less than a university degree and 0.32 for percentage of peers with a university degree or more. Using these measures would be an overstatement both because standard deviations are likely inflated by the missing observations in our data and also because these estimates assume that it is possible to almost double the percentage with more than a high school degree and/or percentage with a university degree or more, while keeping the other fixed (and thus replacing only students

Results are smaller and less stable than the ones that correct for measurement error in peer characteristics, which is consistent with Sojourner (2013)'s findings.

[16] These results include those with only 1 classroom, but results are similar if we exclude singleton classrooms. Vigdor and Nechyba (2004) use a similar strategy, restricting to an apparent random assignment sample, to identify peer effects and Clotfelter et al. (2006) to examine teacher effects.

[17] To perform a procedure that is synonymous to testing based on the parental education dummies, we divide initial achievement into terciles and check whether these low, medium and high initial achievement students are plausibly randomly distributed across classrooms.

whose parents have a high school degree or less). However, taking these numbers as given and using the preferred estimates in columns (3) and (6), a 1 standard deviation increase in the percentage of peers with more than a high school degree increases achievement by 0.09 of a standard deviation in math and 0.15 if reading. Increasing the percentage with a university degree by one standard deviation increases math scores by 0.17 of a standard deviation and 0.13 in reading.

More realistically, these estimates suggest (with a median class size of 20) that substituting a a child whose parent has a high school degree or less with a child whose parent has more than a high school degree would raise achievement by 0.017 of a standard deviation in math and 0.028 in reading. Having one more peer whose parent has at least a university degree in place of a classmate whose parent has a high school degree or less raises achievement by about 0.027 in math and 0.02 in reading. These effects are smaller but still sizable compared to the direct effect of parental education. Conditioning on prior achievement controls for much of the direct effect of parental education, but none of the peer effect given that this is the child's first exposure to these peers. Furthermore, as we discuss below, the large spillovers from peer parental education make sense given the number of important channels for which they could be proxying.

Our preferred way to think about effect sizes is in terms of the achievement gap between children whose parents have a high-school degree or less and those whose parents have a university degree or more. The children of parents with a high-school degree or less have on average 0.35 proportion peers with more than a high school degree but less than a university-degree and 0.19 with a university degree or more. In contrast, children of university-educated parents have 0.29 and 0.53 respectively. Equalizing so that all children have the same peer parental education composition (0.34 with more than high school but less than university and 0.34 with university degree or more), decreases the gap by about 0.15 of a standard deviation in math and 0.09 in reading, narrowing the overall gap by 13% in math and 9% in reading. These effects are sizable but seem reasonable given the magnitude of change in the percentage university-educated parents.

3.4 Further Robustness

Already we have shown several tests that help mitigate concerns about non-random assignment to classrooms, the main threat to identification. First, the p-values in the last column of Table 1 show that peers fall outcomes and teacher characteristics are not significantly different across own parental education after controlling for school fixed effects. Second, a small set of schools rejects the more stringent Fisher test for whether students are randomly assigned to classrooms based on both parental education and fall test scores; yet, results are robust to removing these schools. Third, a common way to test whether students appear to be randomly assigned to peer groups, based on observable characteristics, is to regress observed individual characteristics on the peer measures, to see whether there are statistically-significant correlations. Table A.5 in the Online Appendix brings this additional test to our preferred sample and shows that in none of the 18 cases is peer parental education jointly significant and only 2 out of 36 individual peer parental education coefficients are significant at the 90% confidence level, which is consistent with random assignment. We apply the Guryan et al. (2009) correction method and controlled all estimations by the school average (excluding student i) of peers whose parents have more than HS and more than university, and for interactions between these averages and p. Fourth, we perform a placebo to see whether peer parental education predicts fall test scores in Table A.6. Peer parental education is neither individually nor jointly significant, providing further support that selection is not driving results.

Finally, as a test for the potential impact of selection on unobservables, we include at the bottom of Table 2, bounds for peer parental education. These bounds were estimated based on the works of Altonji et al. (2005) and extended by Oster (2019). The basic intuition of this test is to create bounds under the assumption that selection on unobservables is comparable in magnitude to selection on observables and taking into account the coefficient stability and change in R-squared from including additional controls.[18] We apply what Oster (2019) points out is the particularly stringent form of this test by assuming the maximum potential explanatory power (if all unobservables were observed) of 1, which is unlikely given measurement error in test scores. As shown in the table, even with this stringent form of the test, all these bounds are far from zero, which indicates that the coefficients on peer parental education in Table 2 are not simply a result of omitted variable bias[19].

An additional bias could occur because of measurement error, in that we have applied the simplified version of the estimator as suggested by Sojourner (2013). Online Appendix Table A.7 expands to include interactions between school dummies and the percentage observed in the school.[20] While we do find the expected trade-off of less precision with more interactions, results are remarkably robust, and if anything are slightly larger in the full expression of the model. These findings support, as in Sojourner (2013), that the bias created by ignoring the interactions of the percentage observed with school fixed effects is negligible.

4 Spillovers from Child Skill Inputs

4.1 Initial skills

Peer initial achievement is perhaps the most intuitive and most frequently considered source of peer effects in the achievement literature. Given the correlation between parental education and initial achievement, we expect that initial achievement could explain a significant portion of the spillovers from peer parental education. The ECLS-K has a couple strengths for testing this hypothesis. First, it includes not only math and reading achievement, but also socio-emotional skills. Second, these skills are measured at the beginning of kinder-

[18] See Online Appendix A.2 for more discussion.

[19] It is worth noticing that these are lower bounds for Math and upper bounds for reading. This difference in direction relative to the main estimates is due to differences in the direction of implied selection from observables. Indeed, the coefficients of peer parental education decrease in Math and increase in reading after the inclusion of controls.

[20] See Online Appendix A.3 for more discussion.

garten. Usually data only include test scores from the previous school year, and this may not be as good a proxy given evidence of differential summer loss in learning (Borman and Boulay, eds, 2004) .

In Table 3, we test whether spillovers from peer parental education remain after controlling for peer achievement at the start of kindergarten. Columns (1) and (4) replicate columns (3) and (6) in Table 2 for math and reading respectively, for comparison. Columns (2) and (5) add in controls for peer average fall test scores in math and reading respectively. Estimated effects of peer parental education drop only marginally, by at most 0.03, and the effects of peer initial achievement are not significantly different from 0. This is surprising. One possible explanation is that there are nonlinear effects of peer initial achievement that are being captured by peer parental education and not by peer average initial achievement. Thus, columns (3) and (6) include controls for low, and high initial test scores, based on being less than the 33^{rd} percentile and greater than the 67^{th} percentile respectively. This cuts the sample into thirds, similarly to the measures of parental education. We also include controls for the percentage of peers who have low, medium or high initial achievement and interact these with the student's own initial achievement dummy, to allow differential effects across student types. These measures of peer initial achievement are jointly significant predictors of reading achievement but not math (p-values of 0.01 and 0.29 respectively). Furthermore, including these additional controls does not change the coefficients on peer parental education significantly. The biggest drop is in the spillovers from peers' parents having a university degree in reading, from 0.41 in the original specification to 0.34. Results remain similar when we control for a quartic in peer average initial achievement or include controls for peer initial achievement in the other subject (not shown).

Alternatively, students may benefit from peers with higher parental education because they have better socio-emotional skills. Table 4 builds on Table 3 by adding in these measures of initial behaviors. Columns (1) and (4) repeat the results for math and reading without controlling for either initial test scores or initial socio-emotional skills. Columns (2) and (5)

Table 3: Peer Initial Achievement (N=10,170)

	Math			Reading		
	(1)	(2)	(3)	(4)	(5)	(6)
Parent more than HS	0.08***	0.08***	0.07***	0.05**	0.05**	0.05***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Parent univ+	0.17***	0.17***	0.16***	0.12***	0.12***	0.14***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Peer parent HS+	0.34**	0.33*	0.32*	0.56***	0.54***	0.53**
	(0.17)	(0.18)	(0.18)	(0.20)	(0.21)	(0.21)
Peer parent univ+	0.53***	0.50**	0.52**	0.41*	0.38	0.34
	(0.20)	(0.21)	(0.21)	(0.24)	(0.24)	(0.25)
Avg fall test score		0.06	-0.10		0.08	-0.02
		(0.09)	(0.17)		(0.10)	(0.15)
Nonlinear initial skill terms			Y			Y
Joint significance of peer parental education						
F-stat	3.90	3.13	3.37	3.76	3.51	3.24
p-value	0.02	0.04	0.03	0.02	0.03	0.04
Joint significance of peer initial achievement spillovers						
F-stat			1.21			2.34
p-value			0.29			0.01

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regression include the same set of controls as in Table 2. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Peer variables are interacted with p_c to deal with missing values. Columns (3) and (6) include dummies for low and high skill (below 33rd percentile and above 67th of the initial achievement distribution), percentage of peers who are high, medium or low initial skill and their interactions.

Table 4: Peer Socio-Emotional Skills (N=10,170)

	Math			Reading		
	(1)	(2)	(3)	(4)	(5)	(6)
Parent more than HS	0.08***	0.08***	0.07***	0.05**	0.04**	0.05***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Parent univ+	0.17***	0.16***	0.15***	0.12***	0.11***	0.13***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Peer parent HS+	0.34**	0.33*	0.32*	0.56***	0.54***	0.52**
	(0.17)	(0.18)	(0.17)	(0.20)	(0.20)	(0.21)
Peer parent univ+	0.53***	0.48**	0.51***	0.41*	0.34	0.31
	(0.20)	(0.20)	(0.20)	(0.24)	(0.23)	(0.24)
Fall socio-emotional skills		0.12***	0.04		0.14***	0.07
		(0.02)	(0.04)		(0.02)	(0.05)
Peer fall socio-emotional skills		-0.17	0.50		-0.26*	0.06
		(0.12)	(0.42)		(0.14)	(0.50)
Avg. fall test score		0.08	-0.08		0.11	-0.00
		(0.09)	(0.16)		(0.10)	(0.15)
Nonlinear initial skill/socio-emotional skills terms			Y			Y
Joint significance of peer parental education						
F-stat	3.90	3.10	3.57	3.76	3.49	3.21
p-value	0.02	0.05	0.03	0.02	0.03	0.04
Joint significance of peer socio-emotional skills						
F-stat			2.69			1.38
p-value			0.00			0.19
Proportionality test						
χ^2	0.36	0.43	0.39	4.65	4.69	4.58
p-value	0.55	0.51	0.53	0.03	0.03	0.03

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regression include the same set of controls as in Table 2. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Socio-emotional skills are measured by extracting a common factor from fall teacher reports of externalizing behaviors, interpersonal skills and self control. Peer variables are interacted with p_c to deal with missing values. Columns (3) and (6) include dummies for low and high socio-emotional skills (below 33rd percentile and above 67th), percentage of peers who are high, medium or low and their interactions along with the nonlinear initial skill terms as in Table 3, columns (3) and (6). We set socio-emotional skills to be 0 for those who are missing observations and include controls for missing socio-emotional skills and the percentage of the classroom that is missing socio-emotional skills.

add in controls for average peer initial test scores and socio-emotional skills. Columns (3) and (6) explore potential nonlinearities by dividing socio-emotional skills into low, medium and high socio-emotional skills, based on the terciles of the socio-emotional skill distribution and interact these terms with the percentage of peers with low, medium or high socio-emotional skills. The effects of peer parental education remain very similar with the additional controls. The biggest change is that the returns to having peers with a university degree drops from 0.41 in column (4) to 0.31 in column (6) for reading. Peer socio-emotional skills are most significant in their effects on math test scores, with a p-value of 0.00 for math and 0.19 for reading.[21]

Taken together, this suggests that measured peer initial skills (both cognitive and socio-emotional) explain very little of the spillovers from peer parental education. This suggests that parental education spillovers warrant particular attention beyond measures of initial skills.

4.2 Unobserved Skills

Despite the richness of the ECLS-K measures of initial skill, there are many dimensions of skills that may not be captured, such as daily preparedness for school or other differential investments that occur during the school year. Furthermore, measurement error could lead us to understate the role of peer initial skill. In this section, we use a simple model to illustrate testable implications of a model of spillovers from peer parental education arising through unobserved skills. To focus on the role of skill spillovers, suppose that there are no direct spillovers from peer parental education, but rather that peer skill matters for achievement

[21]Models that include each of the separate measures of socio-emotional skills independently did not do a better job of explaining the spillovers from peer parental education. We also considered the alternative focus on externalizing behaviors as evidence of bad behavior and the potential for being disruptive which the literature has highlighted as an important mechanism of spillovers (e.g. Figlio, 2007; Lazear, 2001), but results are similar to our chosen composite measure of socio-emotional skill.

over and above the vector of own and peer initial skills captured by $Y_{i0}, \bar{Y}_{-i0}$, i.e.,

$$Y_i = \gamma_s s_i + \gamma_{\bar{s}} \bar{s}_{-i} + Y_{i0}\gamma_Y + \bar{Y}_{-i0}\gamma_{\bar{y}} + \eta_i, \tag{3}$$

where η_i captures measurement error in test scores, $E(\eta_i | s_i, \bar{s}_{-i}, Y_{i0}, \bar{Y}_{-i0}) = 0$. Note that this representation could also be consistent with $\bar{Y}_{-i0}$ being an imperfect proxy for initial skill, so that $\bar{s}_{-i}$ is the remaining measurement error.

To see the indirect effect of parental education, denote the projection of skill on parental education as

$$s_i = \alpha_{hs} X_i^{hs} + \alpha_{uni} X_i^{uni} + u_i,$$

where (X_i^{hs}, X_i^{uni}) denote dummies for more than high school, but less than university-educated parents and university-educated parents respectively and u_i denotes remaining inputs that are uncorrelated with parental education. This production function could be derived from parent utility-maximizing behavior through how they invest in their children, but it could also be interpreted as a reduced-form projection.

Plugging in for skill in equation (3), we have the reduced form we estimated in section 4.1, i.e.,

$$\begin{aligned} Y_i = {} & \gamma_s \alpha_{hs} X_i^{hs} + \gamma_s \alpha_{uni} X_i^{uni} + \alpha_{hs} \gamma_{\bar{s}} \bar{X}_{-i}^{hs} + \alpha_{uni} \gamma_{\bar{s}} \bar{X}_{-i}^{uni} + Y_{i0}\gamma_Y \\ & + \bar{Y}_{-i0}\gamma_{\bar{y}} + \eta_i + \gamma_s(\alpha_{hs} + \alpha_{uni}) u_i + \gamma_s(\alpha_{hs} + \alpha_{uni}) \bar{u}_{-i}, \\ = {} & \delta_{hs} X_i^{hs} + \delta_{uni} X_i^{uni} + \delta_{\bar{hs}} \bar{X}_{-i}^{hs} + \delta_{\bar{uni}} \bar{X}_{-i}^{uni} + Y_{i0}\gamma_Y + \bar{Y}_{-i0}\gamma_{\bar{y}} + \tilde{\eta}_i, \end{aligned}$$

where δ denotes the estimated parameters.

Note that in this model

$$\frac{\delta_{hs}}{\delta_{uni}} \equiv \frac{\alpha_{hs}}{\alpha_{uni}} \equiv \frac{\delta_{\bar{hs}}}{\delta_{\bar{uni}}},$$

a condition we can test. We call this the *proportionality test.* We can use the relationship to conduct a nonlinear test of whether our estimated parameters on parental education and peer parental education are consistent with a model where peer parental education only affects achievement as a proxy for skill, as outlined in equation (3).[22]

The results of this test are included in Table 4. The test fails to reject proportionality in math, but it does reject proportionality in reading. The reason is that math and reading are increasing in own parental education and math is similarly increasing in peer parental education, whereas reading is not. In reading, students benefit from having peers whose parents have more than a high school degree but less than a university degree, but do not benefit from having peers whose parents have a university degree or more.

Note that a nice feature of this test is that it does not impose any assumptions on the relationship between α_{hs} and α_{uni}, but it does require linearity in skill. Thus, one key alternative explanation we will test for reading spillovers is the potential for nonlinearities in Section 5.1.[23] We note also that at an intuitive level, we would expect that spillovers from peer parental education would be increasing across education levels, which is not the case for reading. Section 5 focuses on understanding this puzzle.

4.3 Race and Income Peer Effects

As discussed in Section 1, a significant literature shows peer effects from racial and income composition. Given that parental education is highly correlated with race and income, we test whether peer parental education spillovers are captured by these other commonly-studied peer effects or if they capture a different type of peer effect. Table 5 controls for classroom racial and income composition, measured as proportion of peers who are black, Hispanic, or other non-white, and the proportion eligible for receiving free lunch at school, in addition to peers' initial achievement and socio-emotional skills.[24] Though classroom

[22] A closely related test would be to use peer parental education to instrument for peer contemporaneous achievement, as a proxy for skill, and test whether the "instruments" pass the test of over-identifying restrictions. We find this to be the case for math and not reading, and evidence of strong spillovers from peer contemporaneous achievement.

[23] Recall that we already found that the spillovers from peer parental education were robust to very flexible controls for initial skills, as shown in section 4.1.

[24] Note that we have a more complete measure of the racial and income composition, given that these numbers are reported by teachers for the classroom.

Table 5: Class Racial and Income Composition (N=10,170)

		Math			Reading	
	(1)	(2)	(3)	(4)	(5)	(6)
Parent more than HS	0.08***	0.07***	0.06***	0.05**	0.04*	0.04**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Parent univ+	0.17***	0.14***	0.14***	0.12***	0.10***	0.11***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Peer parent HS+	0.34**	0.34*	0.33*	0.56***	0.53***	0.52**
	(0.17)	(0.18)	(0.17)	(0.20)	(0.21)	(0.21)
Peer parent univ+	0.53***	0.47**	0.51**	0.41*	0.27	0.25
	(0.20)	(0.20)	(0.20)	(0.24)	(0.23)	(0.24)
Peer fall behav.		-0.17	0.48		-0.26*	0.00
		(0.12)	(0.42)		(0.14)	(0.50)
Avg. fall test		0.06	-0.11		0.07	-0.01
		(0.09)	(0.16)		(0.11)	(0.16)
Nonlinear initial skill/socio-emotional skills terms			Y			Y
Joint significance of peer parental education						
F-stat	3.90	3.08	3.61	3.76	3.45	3.25
p-value	0.02	0.05	0.03	0.02	0.03	0.04
Joint significance of peer racial and income composition						
F-stat		4.84	4.30		3.06	3.20
p-value		0.00	0.00		0.00	0.00
Proportionality test						
χ^2	0.36	0.60	0.53	4.65	4.58	4.63
p-value	0.55	0.44	0.47	0.03	0.03	0.03

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regression include the same set of controls as in Table 2 along with students own race, students receiver status of free lunch, the proportion of peers from each race, and the proportion of peers who receive free lunch. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Socio-emotional skill is measured by extracting a common factor from fall teacher reports of externalizing behaviors, interpersonal skills and self control. Peer variables are interacted with p_c to deal with missing values. Columns (3) and (6) include dummies for low and high socio-emotional skills (below 33rd percentile and above 67th), percentage of peers who are high, medium or low and their interactions along with the nonlinear initial skill terms as in Table 3, columns (3) and (6). We set socio-emotional skills, race, and free lunch receiver to be 0 for those who are missing observations and include controls for missing socio-emotional skills/free lunch and the percentage of the classroom that is missing socio-emotional skills/race/free lunch.

racial and income composition are statistically significant predictors of achievement, they do not explain the effects of peer parental education. This provides additional support that peer parental education spillovers in kindergarten are not explained by some of the more commonly-studied peer effect channels.

5 The Reading Puzzle

For reading, the findings so far present a puzzle: why do students receive positive spillovers from peers whose parents have more than a high school degree but do not benefit more from those with a university degree? We test several key hypotheses for the reading puzzle that center around heterogeneous effects of peer parental education: (1) university-educated parents may benefit some students to the detriment of others and (2) some types of university-educated parents may exert negative effects on the teacher or learning in the classroom.

5.1 Heterogeneity by Parental Education

One reason for the lack of spillovers from university-educated parents in reading may be that peers with university-educated parents help university-educated students but hurt students whose parents do not have a university-education. This type of heterogeneity could occur for several reasons. First, it could be that teachers teach more to children of university-educated parents when they have more peers with university-educated parents at the expense of the other students in the classroom. Second, relatedly, it could be that university-educated parents advocate for resources that only help children of university-educated parents to the detriment of other students in the classroom. Finally, an increase in peers' skills could act as an endowment to parents who might respond by decreasing their provision of effort, as found in Pop-Eleches and Urquiola (2013), and Fu and Mehta (2018). This may be especially the case for parents of low-achieving students, whose investment costs are higher.

Table 6 thus considers heterogeneity in peer effects by parental education. Columns (1)

Table 6: Heterogeneous Effects (N=10,170)

	Math		Read	
	(1)	(2)	(3)	(4)
Parent more than HS	0.06	0.06**	0.05	0.04
	(0.04)	(0.03)	(0.04)	(0.03)
Parent univ+	0.10*	0.13***	0.10**	0.11***
	(0.05)	(0.03)	(0.05)	(0.03)
Peer parent HS+	0.32*	0.28	0.52**	0.50**
	(0.18)	(0.19)	(0.21)	(0.22)
Peer parent univ+	0.44**	0.55***	0.22	0.27
	(0.20)	(0.20)	(0.24)	(0.24)
%Peers parents w/ same education	-0.01		0.01	
	(0.06)		(0.06)	
%Peers parents w/ same education x Parent univ+	0.20*		0.11	
	(0.12)		(0.12)	
Parent HS- x Peer parent HS+		0.15		0.05
		(0.14)		(0.13)
Parent HS- x Peer parent univ+		-0.25*		-0.09
		(0.13)		(0.13)
Joint significance of peer parental education				
F-stat	1.92	2.79	2.19	1.88
p-value	0.12	0.03	0.09	0.11

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regression include the same set of controls as in columns (3) and (6) and Table 5. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Peer variables are interacted with p_c to deal with missing values. Variable " %Peers parents w/ same education" is the percentage of peers whose parents have less than high school for students whose parents also have less than high school, the percentage of peers whose parents have high school or more for students whose parents also have high school or more, and the percentage of peers whose parents have an university degree for students whose parents also have an university degree.

and (3) control for the percentage of peers in the classroom whose parents have the same level of education as the students' parents, for math and reading respectively.[25] We also test whether university-educated children benefit particularly more from having peers with the same parental education, i.e., other university-educated classmates. There is little evidence of this in reading either for university-educated parents or for the others.

Columns (2) and (4) of Table 6 consider whether parents who have a high school degree or less are affected more by having classmates with better-educated parents. Again, in reading, there is little evidence that having more peers whose parents have a university degree hurts the students with the least educated parents. Hence, heterogeneous effects by parental education do not explain the lack of spillovers from university-educated parents in reading.

5.2 Heterogeneity by Parental Working Status

Our second hypothesis for the lack of positive spillovers from university-educated parents in reading is that "over-involved" parents exert negative effects in the classroom. A strong candidate for an over-involved parent is one who is unemployed or working part-time and thus has more time to intervene in their child's education.

Table 7 tests whether heterogeneity in parental working status explains the pattern emerging in reading. Columns (1) and (3) simply control for whether one of the student's parents was unemployed or working part-time at the time they answered the ECLS-K spring survey, and also for the percentage of peers whose parents were unemployed/part-time. The results remain unchanged. Columns (2) and (4) include an interaction between parental working status and whether they have a university degree or not and the proportion of peers with university-educated parents not working full time. In this case, the effect of peers with university-educated parents becomes positive in reading and larger in magnitude than

[25] This variable is the percentage of peers whose parents have less than high school for students whose parents also have less than high school, the percentage of peers whose parents have high school or more for students whose parents also have high school or more, and the percentage of peers whose parents have an university degree for students whose parents also have an university degree.

Table 7: Parents working status (N=10,170)

	Math		Reading	
	(1)	(2)	(3)	(4)
Parent HS+	0.06***	0.06***	0.04**	0.05**
	(0.02)	(0.02)	(0.02)	(0.02)
Parent univ+	0.14***	0.18***	0.11***	0.11***
	(0.02)	(0.03)	(0.02)	(0.03)
Peer parent HS+	0.34*	0.36**	0.52**	0.56***
	(0.18)	(0.18)	(0.21)	(0.21)
Peer parent univ+	0.51***	0.80**	0.25	0.76**
	(0.20)	(0.31)	(0.24)	(0.34)
Some parent unemp/part-time	-0.02	0.00	0.01	0.00
	(0.02)	(0.02)	(0.02)	(0.02)
Peer some parent unemp./PT	0.01	0.15	0.02	0.29
	(0.15)	(0.20)	(0.18)	(0.24)
Some parent unemp./part-time x univ		-0.06		0.01
		(0.04)		(0.04)
% Peer some parent unemp./part-time x Univ		-0.41		-0.74*
		(0.35)		(0.38)
Joint significance of peer parental education				
F-stat	3.71	3.94	3.25	4.53
p-value	0.03	0.02	0.04	0.01
Joint significance of parental working status				
F-stat		0.66		1.86
p-value		0.52		0.16
Proportionality test				
χ^2	0.54	0.21	4.63	1.01
p-value	0.46	0.65	0.03	0.31

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regression include the same set of controls as in columns (3) and (6) and Table 5. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Variable "Some parent unemp/part-time" is a binary variable that takes value equal one if one of the parents is working less than 35 hours/week, or not working at all, and zero otherwise. Missing parents' working status are replaced with values of 0 and controls for missing values are included. Peer variables are interacted with p_c to deal with missing values.

the spillovers from parents with more than a high-school degree. This is driven by a large negative coefficient on peers with university-educated parents not working full time. Furthermore, the proportionality test – carried out considering only parents with a university degree working full time – no longer rejects the hypothesis that the impact of peer parental education on reading comes through increases in peers' skills.[26]

We next investigate what drives this negative coefficient. It is important to note that the effect of percentage of peers who have some parent unemployed or part-time with a university degree is weakly positive, once you take into account the strong positive effect of peers with university-educated parents and the weakly positive main effect of peers with some parent unemployed/part-time (though this is not statistically significantly different from 0). But, the mystery remains what the countervailing negative effect is that prevents them from producing the strong positive spillovers of other university-educated parents.

We consider several candidate explanations for this countervailing negative effect. First, it could be that parents are not working full time for a reason related to problematic behavior of the child, thus it proxies for behavioral difficulties. Second, if unemployed parents are less happy because of financial stress or are not working for reasons related to health or stress, this could affect their children and their peers (Vallotton et al., 2016). Third, it could be that these university-educated parents may exert undue pressure on the teacher making him/her less effective. Finally, it could be that these parents affect what the teacher teaches, potentially even just indirectly through the skill the child brings to the class, in ways that are sometimes detrimental for the class.

5.2.1 Parental Inputs, Unfavorable Parent or Child Characteristics

We begin by considering how parental education and working status relates to parental investment, parental well-being and child behaviors. Column (1) of Table 8 shows that if

[26] Interestingly, results (not shown) are the opposite for high-school educated parents who do not work full time. If anything, they create positive spillovers in the classroom.

Table 8: Testing for harmful effects of parents' education and working status on home inputs and child socio-emotional skills

	Parental inputs	Par. mental illness	Financial problems	Healthy relationship w/ child	Child Socio-emotional
Parent HS+	0.30***	-0.21***	-0.01	0.06**	0.04***
	(0.04)	(0.04)	(0.02)	(0.03)	(0.01)
Parent univ+	0.40***	-0.24***	-0.08***	0.07	0.05***
	(0.07)	(0.06)	(0.02)	(0.04)	(0.02)
Some parent unemp/part-time	0.08*	0.15***	-0.03	0.02	0.02
	(0.04)	(0.04)	(0.02)	(0.03)	(0.01)
Some parent unemp./part-time x univ	0.12*	-0.16***	0.00	-0.05	0.06***
	(0.07)	(0.06)	(0.02)	(0.05)	(0.02)
N	9900	7384	7458	9624	9479
Joint significance of parental working status					
F-stat	8.02	6.70	2.14	0.57	12.08
p-value	0.00	0.00	0.12	0.57	0.00

Notes: i) Standard errors clustered at school level; ii) * $p<0.10$, ** $p<0.05$, *** $p<0.01$. All regressions include controls for school fixed effect. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Socio-emotional skills are measured by extracting a common factor from fall teacher reports of externalizing behaviors, interpersonal skills and self control. Variable "Parental inputs" is a factor underlying questions in the fall questionnaire about activities parents perform with their children. Variable "Parental mental illness" is a factor extracted from the correlation between parents spring report on their psychological well-being. Variable " Financial problems" is a binary variable that takes value equal one if parents resported on the spring report if they have arguments about money often or sometimes, and zero if they have arguments about money hardly ever or never. Variable "Healthy relationship w/ child" is a factor extracted from the correlation between parents spring report on their relationship with their child. Variable "Some parent unemp/part-time" is a binary variable that takes value equal one if one of the parents is working less than 35 hours/week, or not working at all, and zero otherwise. Missing parents' working status are replaced with values of 0 and controls for missings are included.

anything non-full time, university-educated parents invest more in their children.[27] Column (2) shows that the likelihood of the parent presenting mental illness decreases with parents' education and that the mental health of university-educated parents who are not working full time is comparable to those who are. Columns (3) and (4) further show no relationship parental working status and the likelihood of having financial problems or a healthy relationship with their child. Column (5) of Table 8 shows that socio-emotional skills are actually higher for children of more educated parents and this is even more so for parents who are not working full time.[28] Combined, these findings suggest that if anything, university-educated parents who are not working full time invest more in their children and have children with stronger socio-emotional skills suggesting no apparent disadvantage of the child that would explain the negative spillovers.

5.2.2 Teacher Satisfaction

Table 9 tests for evidence of negative pressure on teachers through teacher satisfaction and unpaid preparation hours. These measures are taken from the teacher survey and described in further detail in Table A.1. We estimate whether the percentage university-educated parents in the class, the percentage not employed full-time or the percentage university educated and not employed full-time predict whether the teacher enjoys his/her job (column 1), whether the teacher feels like he/she makes a difference (column 2), whether the teacher would choose to be a teacher again (column 3), whether the teacher perceives parents as supportive (column 4) and the teacher-reported number of unpaid preparation hours (column 5). There is no evidence that having more students with university-educated parents who are not working full-time affects teacher satisfaction or coerces the teacher to work additional time preparing

[27]"Parental inputs" is a factor underlying questions in the ECLS-K fall parents' questionnaire about activities parents perform with their children, such as reading books, telling stories, singing songs, among others.

[28]These results use socio-emotional skills at the fall of Kindergarten, but results are similar if we instead use spring socio-emotional skills as our dependent variable.

Table 9: Testing for harmful effects of parents' education and working status on teachers

	Teachers' enjoyment	Teacher feels he/she makes difference	Teacher would choose to be a teacher again	Teacher perceives parents as supportive	Unpaid preparation hours
% parent univ+ in class	0.00	0.14	0.38	-0.16	0.51
	(0.54)	(0.54)	(0.53)	(0.40)	(0.46)
% parent unemp./PT in class	0.06	0.44	0.10	-0.01	0.50
	(0.36)	(0.36)	(0.36)	(0.27)	(0.31)
% parent unemp./PT x univ+ in class	-0.02	-0.08	-0.51	0.28	-0.17
	(0.64)	(0.64)	(0.63)	(0.48)	(0.55)
N	1854	1855	1851	1861	1851
Joint significance of parental working status					
F-stat	0.02	1.05	0.36	0.26	1.63
p-value	0.98	0.35	0.70	0.77	0.20

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for class size and p_c (percentage of students observed in the class), school fixed effects, and teacher characteristics, such as female, white, experience, experience-squared, tenure and tenure-squared. Variables of parental education and working status are averaged by classes and interacted with p_c to deal with missing values. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Variable "Teachers' enjoyment" refers to how strongly teachers agree with the statement "I really enjoy my present teaching job". Variable "Teacher feels he/she makes difference" refers to how strongly teachers agree with the statement "I am certain I am making a difference in the lives of the children I teach". Variable "Teacher would choose to be a teacher again" refers to how strongly teachers agree with the statement "If I could start over, I would choose teaching again as my career". Variable "Teacher perceives parents as supportive" refers to how strongly teachers agree with the statement "Parents are supportive of school staff". Variable "Some parent unemp/part-time" is a binary variable that takes value equal one if one of the parents is working less than 35 hours/week, or not working at all, and zero otherwise. Missing parents' working status are replaced with values of 0 and controls for missings are included.

for class.[29]

5.2.3 More Detailed Measures of Reading Skills

Our last hypothesis is that children of university-educated parents may be better-prepared for school in ways that detract from their classmates' learning. We hypothesize that the strong benchmark of knowing how to read at the beginning of kindergarten could be a particular mechanism of negative spillovers that may not be captured by our measure of initial reading performance. Table 10 tests whether parental education and working status predict whether the child is capable of using complex sentences, whether the child understands texts or histories read to him/her and whether the child reads simple books, as measured through teacher reports at the fall of kindergarten. In all cases, we observe that the interaction between university-educated parents and not working full-time is positive and statistically significantly different from 0.

Table 11 considers whether these additional reading skills have negative effects on classmates beyond the controls for initial socioemotional and reading skills, and other controls in Table 5. We add each measure of reading proficiency in a given area separately, the percentage of peers with that skill in levels and squared (to capture nonlinear effects depending on heterogeneity in the classroom). First, note that each of these reading skills is positively related to reading achievement. Yet, we find that the percentage of peers exhibiting this proficiency is a negative and statistically significant predictor of reading performance. The quadratic terms, while never statistically significant, are positive. By magnitudes (at least for understanding texts and knowing how to read), they suggest that the negative spillovers from these skills go away and become positive as the proportion of peers in the classroom who have this skill increases to about 30% for knowing how to read and 55% for understanding

[29] A concern with these results is that they rely on variation within school, which may be limited. We also fail to find evidence that having more parents who are university-educated and not working full time affects teacher satisfaction or unpaid preparation hours when we do not include school fixed effects. We also test if having more university-educated parents who are not working full-time particularly helps children of university-educated parents. Again, we find no evidence of this.

Table 10: Effects of parental education and working status on child's fall reading proficiency

	Child uses complex sentences (1)	Child understands texts read to her (2)	Child reads (3)
Parent HS+	0.07***	0.08***	0.03***
	(0.01)	(0.01)	(0.01)
Parent univ+	0.15***	0.18***	0.07***
	(0.02)	(0.02)	(0.01)
Some parent unemp/part-time	-0.04***	-0.03**	-0.01
	(0.01)	(0.01)	(0.01)
Some parent unemp./part-time x univ	0.06***	0.05**	0.04**
	(0.02)	(0.02)	(0.02)
N	9992	9906	8437
Joint significance of parental working status			
F-stat	5.75	3.79	2.79
p-value	0.00	0.02	0.06

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for school fixed effect. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Descriptions of proxies for reading skills are in Table A.1. Variable "Some parent unemp/part-time" is a binary variable that takes value equal one if one of the parents is working less than 35 hours/week, or not working at all, and zero otherwise. Missing parents' working status are replaced with values of 0 and controls for missings are included.

Table 11: Effect of peers who already know how to read (N=10,170)

	Math			Read		
	(1)	(2)	(3)	(4)	(5)	(6)
Parent HS+	0.06***	0.06***	0.06***	0.04**	0.04**	0.04**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Parent univ+	0.13***	0.13***	0.14***	0.10***	0.10***	0.11***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Peer parent HS+	0.33*	0.33*	0.31*	0.53**	0.55***	0.51**
	(0.17)	(0.17)	(0.17)	(0.21)	(0.21)	(0.21)
Peer parent univ+	0.51***	0.49**	0.48**	0.31	0.31	0.25
	(0.20)	(0.20)	(0.19)	(0.24)	(0.24)	(0.24)
Child uses complex sentences	0.14***			0.13***		
	(0.02)			(0.02)		
% Peers use complex sentences	-0.24			-0.63***		
	(0.23)			(0.24)		
(% Peers use complex sentences)^2	0.03			0.18		
	(0.47)			(0.45)		
Child understands texts read to her		0.13***			0.13***	
		(0.02)			(0.02)	
% Peers understand texts read to them		-0.11			-0.59***	
		(0.23)			(0.23)	
(% Peers understand texts read to them)^2		0.07			0.54	
		(0.48)			(0.42)	
Child reads			0.11***			0.27***
			(0.03)			(0.05)
% Peers read			-0.28			-1.11***
			(0.37)			(0.43)
(% Peers read)^2			0.49			1.79
			(1.05)			(1.32)
Joint significance of peer parental education						
F-stat	3.68	3.37	3.41	3.27	3.51	3.03
p-value	0.03	0.03	0.03	0.04	0.03	0.05

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regression include the same set of controls as in columns (3) and (6) and Table 5. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Descriptions of proxies for reading skills are in Table A.1. Variable "Some parent unemp/part-time" is a binary variable that takes value equal one if one of the parents is working less than 35 hours/week, or not working at all, and zero otherwise. Missing parents' working status and proxies for reading proficiency are replaced with values of 0 and controls for missings are included. Peer variables are interacted with p_c to deal with missing values.

texts. Finally, it is useful to see that similar patterns do not hold in math, suggesting that it is something particular about reading preparation. These negative spillovers are interesting in their own right and also provide a potential channel through which university-educated parents who are not working full-time could negatively effect the achievement of classroom peers.

The literature points to a couple reasons that higher initial reading skills of classmates may actually be harmful for a student's learning. For instance, the literature finds that having higher-achieving peers can have harmful effects on a student's self-concept (Marsh et al., 2008). Alternatively, teachers may adjust their teaching to accommodate more advanced students in ways that harms others in the classroom (Jackson, 2016; Duflo et al., 2011).[30] While we do not have a measure of academic self-concept, we can test whether teachers change what they teach to meet the heterogeneity and advanced skills they experience in the classroom. Each column of Table 12 has as a dependent variable a different reading-related activity that is set to 1 if the teacher reports doing the activity on a daily basis and 0 otherwise. The results suggest a higher probability of spending time on more complex activities like learning new vocabulary, composing stories, and reading aloud fluently, when there are more students who demonstrate a given reading proficiency.[31]

Taken together, these results offer an explanation for the countervailing negative effects of non-full time university-educated parents on classmates. These parents seem to be investing in their children at home more than parents working full time. This however has a negative impact on the children's classmates, since it creates a heterogeneity in the classroom which induces teachers to shift learning activities towards more advanced students.

[30] Note that for this to be the case, it would need to occur in ways not captured by the initial skill measures in Table 3, which we allowed to enter nonlinearly, and in a way that is particular to reading.

[31] Ideally, we would like to test for nonlinearities in teacher adaptation as classes become more homogeneous in the given skill. There is not enough variation in the data to test this.

Table 12: Reading Proficiency and Teaching Activities

	New vocabulary (1)	Read books (2)	Compose stories (3)	Predictions based on text (4)	Communicate complete ideas orally (5)	Write complete sentences (6)	Read aloud fluently (7)
Skill: Child uses complex sentences							
% students w/ skill in class	0.47**	0.23	0.14	0.12	0.34**	0.33*	-0.01
	(0.20)	(0.19)	(0.15)	(0.18)	(0.15)	(0.18)	(0.17)
Skill: Child understands texts read to her							
% students w/ skill in class	0.56***	0.52**	0.40**	0.48**	0.13	0.37*	0.24
	(0.22)	(0.21)	(0.16)	(0.20)	(0.17)	(0.20)	(0.19)
Skill: Child reads							
% students w/ skill in class	1.01***	0.71*	0.08	0.44	0.37	0.12	0.62*
	(0.38)	(0.37)	(0.28)	(0.35)	(0.29)	(0.35)	(0.34)
N	1848	1843	1819	1836	1834	1827	1812

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for class size and p_c (percentage of students observed in the class), school fixed effects, and teacher characteristics, such as female, white, experience, experience-squared, tenure and tenure-squared. Variables with the percentage of students with skill in class are interacted with p_c to deal with missing values. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Descriptions of proxies for reading skills and teaching activities are in Table A.1.

6 Conclusion

This paper shows that peer parental education matters for student achievement. The effects are sizable. Equalizing the classrooms so that children of parents with a high school degree or less and those with a university-degree have the same parental education composition would narrow the achievement gap between these students by 13 percent in math and 9 percent in reading. These results have important policy implications regarding sources of inequality, suggesting that the sorting of students to elementary schools based on parental education is an important way that the advantage of better-educated parents is multiplied in ways that contribute to persistent and even widening inequality when children are still in kindergarten.

We then test the source of spillovers. We find that rich measures of initial cognitive and socio-emotional skills fail to explain the spillovers from peer parental education, even after allowing for non-linear effects. Furthermore, racial and income composition, other commonly-measured peer effects and correlates of parental education, similarly do little to explain the spillovers from peer parental education. This leads us to conclude that peer parental education matters in ways that are not captured by other commonly-studied peer effects (Sacerdote, 2011), and that failure to account for them may significantly understate the importance of peers.

While the robustness of the peer parental education spillovers itself is an important finding for the literature, it also presents us with an interesting puzzle as to what explains peer parental education spillovers. A strong candidate mechanism is contemporaneous skills, such as daily preparedness for school, or other remaining unmeasured skills that might spill over to classmates. We develop a test for whether the structure of spillovers from parental education is consistent with the skill channel. This exploits the intuition that is implicit in the peer effects literature, that if achievement is increasing in own parental education, it should also be increasing in peer parental education. This hypothesis holds in math but not reading. For reading we see instead that students do not benefit from classmates whose parents have a university degree, whereas they benefit significantly from parents who have more than a high-school degree but less than a university degree. This holds despite the fact that reading achievement is increasing in the child's own parental education.

We investigate this puzzle for reading. We find that university-educated parents who work full time generate large positive spillovers to achievement, in a way that is consistent with our unobserved skill hypothesis test. However, university-educated parents who are unemployed or working part-time fail to generate positive spillovers, leading to the overall small coefficient we observe on peer effects from university-educated parents in the initial specification. This suggests some countervailing negative spillovers from university-educated parents who are not working full time. We do not find evidence that these parents negatively affect teachers' well-being. We also do not find evidence that these parents are more stressed, face greater financial strain or have children with more behavioral problems. Rather, if anything these parents invest more in their children's reading skills. In particular, we find that children of university-educated parents who are not working full-time are more likely to begin kindergarten knowing how to read and possessing other reading skills. While this leads to higher reading tests scores for these children, it creates negative spillovers in the classroom, when other students do not possess these skills. We find evidence that this may be driven by a shift in teaching practices in these classrooms towards more advanced activities.

Overall these results suggest that spillovers from having better-educated parents in the classroom are significant and generally positive in kindergarten. Interestingly, it also highlights the challenges teachers face in teaching populations who are diverse in initial reading proficiency. This raises the interesting question whether teachers could adopt alternative practices that mitigate the negative spillovers we find in reading or create an environment where other students benefit from their relatively over-prepared classmates.

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A Online Appendix

A.1 Data details

The potential sample of students starts at 21,409, but we lose a considerable number because of missing observations. We drop 1546 who are missing either teacher or school identifiers. We drop an additional 1040 who do not report whether they are in a full day or half-day class, which is necessary (in conjunction with the teacher identifiers) for measuring peer groups. We drop students who changed teachers over the course of the survey (686). We also drop students who are missing spring or fall outcomes in math or reading (3031). We then drop students who are missing observations of actual class size (1427) and 440 students who are missing measures of parental education. We then drop students who are the only person observed in the classroom after these restrictions (307). We further drop observations with missing teacher characteristics (532) and small schools with the number of students sampled less than 5 or the bottom 1 percentile (200). This leaves a sample of 12,200 observations.

A.2 Bounding Estimates

Table A.3 brings all the coefficients estimated in Table 2. It also brings an additional test to check the robustness of our results. This is a test for omitted variable bias proposed by Oster (2019) that considers the changes in coefficient and in the R-square when adding controls to a regression. We apply the assumption that selection on unobservables is proportional to selection on observables ($\delta = 1$ in her notation). We also assume, as is consistent with Altonji et al. (2005), that the maximum R^2 is 1 if all controls and treatment effect were included. Oster points out that this is likely to be an over-adjustment in most cases given the presence of measurement error, but we apply this here to be conservative. Formally, one of the bounds, $\widetilde{\beta}$ (with correspondent $\widetilde{R}$ as the R^2 of that regression), is the one estimated in a regression that includes all observed controls - in our case, those displayed in Table A.3.

The other bound is given by

$$\beta^* = \widetilde{\beta} - [\overset{o}{\beta} - \widetilde{\beta}]\frac{1 - \widetilde{R}}{\widetilde{R} - \overset{o}{R}}$$

where $\overset{o}{\beta}$ (with correspondent $\overset{o}{R}$) is the coefficient estimated in a regression with a smaller set of controls.[32] If zero is not included in the set $[\widetilde{\beta}, \beta^*]$, one can reject the hypothesis that the estimated results are driven by omitted variables bias. These results show that our math and reading estimates are remarkably robust.

A.3 Robustness - Interactions with school groups

As discussed in Section 3.2, our preferred approach to control for missing data is to include school fixed effects and p_c in our estimating equations but not the interactions. Sojourner (2013) shows this approach improves precision since it deals with the high collinearity created by the interactions of p_c with school fixed effects. However, there is a tradeoff here between precision and bias. According to the Sojourner (2013), an intermediate solution is to partition schools into K groups based on similarity in the key variable of interest and to interact each p_c with the group dummy (rather than the school dummy). Sojourner (2013) shows through Monte Carlo simulations increasing groups leads to less biased estimates at the cost of considerably less precision.

In order to check the robustness of our main estimations, we divide schools into K-ciles based on the parents' years of education, which assumes that missing and observed students have the same ranking of parental education across schools. That is, for two different schools s and s', we assume $E(T^o|s) < E(T^o|s') \Rightarrow E(T^m|s) < E(T^m|s')$, where T refers to parents' years of education.[33]

Table A.7 shows robustness of our estimates when schools are divided into 100-groups

[32] In our case, this set of controls corresponds to parental education, percentage observed, and percentage of peer with university education or more (for the estimation of the coefficient on peer percentage with more than a high school degree) or percentage of peers with more than a high school degree (for the estimation of percentage peers with a university degree or more).

[33] Sojourner (2013) shows that estimators are robust even when this assumption fails.

(columns (1) and (4)), 300 groups (columns (2) and (5)), and the full estimator where we interact p_c with school dummies (columns (3) and (6)). Estimates are remarkably robust across the estimators, consistent with the evidence in Sojourner (2013), though they do lose precision as we include more interactions.

Table A.1: Variable Descriptions

Variable
Definition
Student Characteristics
Math and Reading
IRT test scores taken at Fall and Spring of kindergarten. Standardized to have mean 0 and standard deviation 1 based on fall test scores.
Free lunch
Binary variable that takes values equal one if child receives free/reduced price lunch during fall.
Socio-emotional skills
Factor extracted from the correlation between teacher reports of externalizing behaviors, interpersonal skills and self control using the regression method for the fall and Bartlett method for the spring measure.
Child uses complex sentences
Binary variable that takes values equal one if teacher reports on fall survey that "the child uses complex sentence structures" in a proficient or intermediate way, and zero whether the child not yet uses it, is beginning to use it, or is in progress on its use.
Child understands texts read to her
Binary variable that takes values equal one if teacher reports on fall survey that "the child understands and interprets a story or other text read to him/her" in a proficient or intermediate way, and zero whether the child not yet uses it, is beginning to use it, or is in progress on its use.
Child reads

Binary variable that takes values equal one if teacher reports on fall survey that "the child reads simple books independently" in a proficient or intermediate way, and zero whether the child not yet uses it, is beginning to use it, or is in progress on its use.

Parent Characteristics

Parental Education

The maximum of father's or mother's education if both reported; Parent HS+: parents who some education/training beyond a high school degree, but no university degree. Parent Univ+: parents who have at least a 4-year university degree.

Parental inputs

Factor extracted from the correlation between parents fall report on home environment, activities and cognitive stimulation.

Questions:

In a typical week, how often do you or any other family member do the following things with child?

(1) Read books to child?

(2) Tell stories to child?

(3) Sing songs with child?

(4) Help child to do arts and crafts?

(5) Involve child in household chores, like cooking, cleaning, setting the table, or caring for pets?

(6) Play games or do puzzles with child?

(7) Talk about nature or do science projects with child?

(8) Build something or play with construction toys with child?

(9) Play a sport or exercise together?

Possible answers:

(a) Not at all

(b) Once or twice

(c) 3 to 6 times

(d) Everyday

Some parent unemp./part-time

Binary variable that takes value equal one if either the father or the mother were not working full time during the fall survey, and zero otherwise.

Parental mental illness

Factor extracted from the correlation between parents spring report on their psychological well-being.

Questions:

(1) How often during the past week have you felt that you were bothered by things that don't usually bother you?

(2) How often during the past week have you felt that you did not feel like eating, that your appetite was poor?

(3) How often during the past week have you felt that you could not shake off the blues even with help from your family or friends?

(4) How often during the past week have you felt that you had trouble keeping your mind on what you were doing?

(5) How often during the past week have you felt depressed?

(6) How often during the past week have you felt that everything you did was an effort?

(7) How often during the past week have you felt fearful?

(8) How often during the past week have you felt that your sleep was restless?

(9) How often during the past week have you felt that you talked less than usual?

(10) How often during the past week have you felt lonely?

(11) How often during the past week have you felt sad?

(12) How often during the past week have you felt that you could not get going?

Possible answers:

(a) Never

(b) Some of the time

(c) A moderate amount of the time

(d) Most of the time

Financial problems

Binary variable that takes value equal one if parents reported on the spring report if they have arguments about money often or sometimes, and zero if they have arguments about money hardly ever or never.

Healthy relationship w/ child

Factor extracted from the correlation between parents spring report on their relationship with their child.

Questions:

(1) Most of the times I feel that child likes me and wants to be near me.

(2) Even when I'm in a bad mood, I show child a lot of love.

(3) I express affection by hugging, kissing, and holding child.

(4) child and I often have warm, close times together.

Possible answers:

(a) Completely true

(b) Mostly true

(c) Somewhat true

(d) Not at all true

Teacher Inputs

Experience

Number of years teaching any grade

Tenure

Number of years teaching at current schools

Hours preparing classes unpaid

Taken from teacher categorical responses to how much unpaid hours that spend preparing for class in a given week. Midpoints of ranges taken and capped at 17.5 for 15 or more hours per week.

Teachers' enjoyment

Binary variable that takes value equal one if teachers strongly agree with the statement "I really enjoy my present teaching job", asked in the spring survey, and zero otherwise.

Teacher feels he/she makes difference

Binary variable that takes value equal one if teachers strongly agree with the statement "I am certain I am making a difference in the lives of the children I teach", asked in the spring survey, and zero otherwise.

Teacher would choose to be a teacher again

Binary variable that takes value equal one if teachers strongly agree with the statement "If I could start over, I would choose teaching again as my career", asked in the spring survey, and zero otherwise.

Teacher perceives parents as supportive

Binary variable that takes value equal one if teachers strongly agree with the statement "Parents are supportive of school staff", asked in the spring survey, and zero otherwise.

Teaching Activities

New vocabulary

Binary variable that takes value equal one if teacher reports on the spring survey that children in his/her classroom perform activities on "discuss new or difficult vocabulary" daily, and zero otherwise.

Read books

Binary variable that takes value equal one if teacher reports on the spring survey that children in his/her classroom perform activities on "read books they have chosen for themselves" daily, and zero otherwise.

Compose stories

Binary variable that takes value equal one if teacher reports on the spring survey that children in his/her classroom perform activities on "compose and write stories or reports" more than twice a month, and zero otherwise.
Predictions based on text
Binary variable that takes value equal one if teacher reports on the spring survey that children in his/her classroom perform activities on "making predictions based on text" more than twice a month, and zero otherwise.
Communicate complete ideas orally
Binary variable that takes value equal one if teacher reports on the spring survey that children in his/her classroom perform activities on "communicating complete ideas orally" more than twice a month, and zero otherwise.
Write complete sentences
Binary variable that takes value equal one if teacher reports on the spring survey that children in his/her classroom perform activities on "composing and writing complete sentences" more than twice a month, and zero otherwise.
Read aloud
Binary variable that takes value equal one if teacher reports on the spring survey that children in his/her classroom perform activities on "read aloud fluently" more than twice a month, and zero otherwise.

Table A.2: Summary of other variables

		Mean	SD	N
Student	Free lunch	0.25	0.43	12,200
	Black	0.15	0.35	12,189
	Hispanic	0.11	0.31	12,189
	Other race	0.11	0.31	12,189
	Uses complex sentences	0.36	0.48	12,200
	Understands texts read to her	0.33	0.47	12,200
	Reads	0.09	0.29	12,200
Peer	Free lunch	0.25	0.33	12,200
	Black	0.16	0.26	11,633
	Hispanic	0.09	0.16	11,633
	Other race	0.08	0.17	11,633
	Uses complex sentences	0.36	0.34	12,200
	Understands texts read to her	0.33	0.33	12,200
	Reads	0.09	0.20	12,200
Parent	Some parent unemp./part-time	0.53	0.50	12,200
	Mental illness	-0.65	1.40	11,434
	Financial problems	0.32	0.47	12,200
	Healthy relationship w/ child	-0.04	1.00	11,548
Teacher	Enjoyment	0.62	0.49	12,172
	Feels makes difference	0.61	0.49	12,178
	Would be a teacher again	0.60	0.49	12,127
	Teacher perceives parents as supportive	0.25	0.43	12,200
	Hours preparing classes unpaid	0.91	0.48	12,132
Teaching activities	New vocabulary	0.58	0.49	12,150
	Read books	0.53	0.50	12,130
	Compose stories	0.13	0.34	11,976
	Predictions based on text	0.65	0.48	12,085
	Communicate complete ideas orally	0.82	0.38	12,075
	Write complete sentences	0.36	0.48	12,045
	Read aloud fluently	0.28	0.45	11,888

Table A.3: Total Effect of Peer Parental Education (All Controls)

	Math			Reading			Socio-emotional skills		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constant	1.06***	1.09***	1.14***	1.11***	1.10***	1.16***	-0.01	-0.09	-0.10
	(0.13)	(0.15)	(0.15)	(0.21)	(0.24)	(0.27)	(0.10)	(0.10)	(0.11)
Parent more than HS	0.08***	0.07***	0.08***	0.06***	0.05***	0.05**	0.02**	0.02*	0.02**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Parent univ+	0.17***	0.16***	0.17***	0.12***	0.12***	0.12***	0.05***	0.05***	0.06***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Peer parent HS+	0.31**	0.26	0.34**	0.48***	0.48**	0.56***	0.09	0.11	0.16
	(0.14)	(0.17)	(0.17)	(0.16)	(0.19)	(0.20)	(0.10)	(0.12)	(0.13)
Peer parent univ+	0.43**	0.56***	0.53***	0.27	0.36	0.41*	0.06	0.08	0.05
	(0.17)	(0.19)	(0.20)	(0.20)	(0.23)	(0.24)	(0.12)	(0.14)	(0.15)
Fall outcome	1.04***	1.04***	1.04***	1.13***	1.13***	1.13***	0.72***	0.73***	0.73***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Teacher characteristics									
Female	0.08	0.03	0.04	-0.01	0.02	0.02	-0.10	-0.05	-0.01
	(0.07)	(0.09)	(0.09)	(0.16)	(0.19)	(0.23)	(0.07)	(0.06)	(0.06)
White	-0.05	-0.04	-0.01	-0.02	-0.01	0.00	0.03	0.05	0.04
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.03)	(0.03)	(0.04)
Experience	-0.01*	-0.01*	-0.01	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Experience^2	0.00**	0.00**	0.00	0.00	0.00	-0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Tenure	0.00	0.00	0.00	0.01	0.01	0.01	0.00	0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Tenure^2	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	12200	11137	10170	12200	11137	10170	12200	11137	10170
R^2	0.69	0.68	0.68	0.69	0.69	0.69	0.42	0.42	0.42
RA sample	N	Y	Y	N	Y	Y	N	Y	Y
Joint Significance Peer Parental Education									
F-stat	3.68	4.09	3.90	4.80	3.20	3.76	0.40	0.44	0.93
p-value	0.03	0.02	0.02	0.01	0.04	0.02	0.67	0.65	0.40
Bounds (Altonji et al., 2005) - β^*									
Peer parent HS+	0.13	0.10	0.21	0.42	0.48	0.58	0.10	0.13	0.12
Peer parent univ+	0.14	0.28	0.45	0.26	0.40	0.60	-0.03	-0.10	-0.07

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for class size and p_c (percentage of students observed in the class), and school fixed effects. Peer variables are interacted with p_c to deal with missing values. Columns (2), (5) and (8) correspond to a subsample of schools that appear to be randomly assigning students based on parental education, using a p-value from Fisher's exact test of greater than 0.1, as discussed in Section 3 and including schools that only appear to have 1 classroom for kindergarten in the random assignment sample. Columns (3), (6) and (9) exclude schools where Fisher's test of random assignment of student by initial achievement (high, medium and low) has a p-value greater than 0.1. Socio-emotional skills are measured by extracting a common factor from fall teacher reports of externalizing behaviors, interpersonal skills and self control. "Fall outcome" is the students' prior achievement regarding the dependent variable. β^* is one of the bounds for Peer parent HS+/Peer parent univ+ [see Oster (2019)]. The other bound is the one displayed at the coefficients of the Table.

Table A.4: Total Effect of Peer Parental Education (no correction)

	Math			Reading			Socio-emotional skills		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Parent more than HS	0.08***	0.07***	0.08***	0.05***	0.04**	0.04**	0.02*	0.02*	0.02**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Parent univ+	0.17***	0.16***	0.16***	0.12***	0.12***	0.12***	0.05***	0.05***	0.05***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Peer parent HS+	0.06*	0.06	0.07**	0.08**	0.08**	0.08**	0.00	-0.00	0.01
	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)
Peer parent univ+	0.09**	0.11**	0.11**	0.06	0.09*	0.12**	-0.02	-0.02	-0.02
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.03)	(0.03)	(0.03)
N	12200	11137	10170	12200	11137	10170	12200	11137	10170
R^2	0.69	0.69	0.69	0.69	0.69	0.69	0.42	0.42	0.42
RA sample	N	Y	Y	N	Y	Y	N	Y	Y
Joint Significance Peer Parental Education									
F-stat	2.83	2.99	3.54	2.68	2.55	3.07	0.43	0.31	0.89
p-value	0.06	0.05	0.03	0.07	0.08	0.05	0.65	0.73	0.41

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include school fixed effects, prior achievement/socio-emotional skills, and teacher characteristics, such as female, white, experience, experience-squared, tenure and tenure-squared. Columns (2), (5) and (8) correspond to a subsample of schools that appear to be randomly assigning students based on parental education, using a p-value from Fisher's exact test of greater than 0.1, as discussed in Section 3 and including schools that only appear to have 1 classroom for kindergarten in the random assignment sample. Columns (3), (6) and (9) exclude schools where Fisher's test of random assignment of student by initial achievement (high, medium and low) has a p-value greater than 0.1. Socio-emotional skills are measured by extracting a common factor from fall teacher reports of externalizing behaviors, interpersonal skills and self control.

Table A.5: Balancing tests

	Parent more than HS	Parent univ+	Fall Read	Fall Math	Fall Self Self Control	Fall Interpersonal Skills	Fall Externalising Problems	Fall Internalising Problems	Fall Approaches to learning
Peer parent HS+	-0.37**	-0.00	-0.14	0.16	-0.02	-0.07	-0.10	0.20	-0.11
	(0.18)	(0.15)	(0.23)	(0.22)	(0.24)	(0.25)	(0.23)	(0.21)	(0.23)
Peer parent univ+	-0.13	-0.12	-0.36	0.03	-0.13	-0.13	0.05	0.30	-0.21
	(0.19)	(0.20)	(0.28)	(0.27)	(0.28)	(0.30)	(0.27)	(0.24)	(0.29)
N	10170	10170	10170	10170	9821	9730	10008	9893	10113
Joint significance of peer parental education									
F-stat	2.24	0.22	0.81	0.27	0.12	0.09	0.24	0.80	0.25
p-value	0.11	0.80	0.45	0.77	0.88	0.91	0.79	0.45	0.78
	White	Black	Hispanic	Free lunch receiver	Some parent unemp./part-time	Parental inputs	Par. mental illness	Financial Problems	Healthy relationship with child
Peer parent HS+	-0.05	0.10	-0.10	0.04	0.07	-0.09	-0.80*	-0.04	0.24
	(0.11)	(0.07)	(0.08)	(0.10)	(0.15)	(0.46)	(0.45)	(0.14)	(0.29)
Peer parent univ+	0.04	0.01	-0.03	-0.02	0.22	-0.45	-0.70	0.06	0.05
	(0.11)	(0.07)	(0.08)	(0.10)	(0.17)	(0.52)	(0.43)	(0.14)	(0.33)
N	10159	10159	10159	10170	10170	9900	9531	10170	9624
R^2	0.03	0.03	0.00	0.12	0.00	0.00	0.02	0.00	0.00
F-stat	0.34	1.41	0.97	0.36	1.05	0.46	1.78	0.33	0.41
p-value	0.71	0.24	0.38	0.70	0.35	0.63	0.17	0.72	0.66

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regressions include controls for class size and p_c (percentage of students observed in the class), and school fixed effects. Peer variables are interacted with p_c to deal with missing values. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3.We apply the Guryan et al. (2009) correction method and controlled all estimations by the school average (excluding student i) of peers whose parents have more than HS and more than university, and for interactions between these averages and p.

Table A.6: Placebo tests: Effect of Peers Parents on Fall Test Scores (N=10,170)

	Fall Math	Fall Read
Parent more than HS	0.22***	0.21***
	(0.02)	(0.02)
Parent univ+	0.56***	0.52***
	(0.03)	(0.03)
Peer parent HS+	0.24	-0.01
	(0.21)	(0.21)
Peer parent univ+	0.15	-0.30
	(0.24)	(0.25)
Joint significance of peer parental education		
F-stat	0.78	0.48
p-value	0.58	0.82

Notes: i) Standard errors clustered at school level; ii) * p<0.10, ** p<0.05, *** p<0.01. All regression include the same set of controls as in Table 2, but initial achievements. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. Peer variables are interacted with p_c to deal with missing values.

Table A.7: Expanded Sojourner Correction for Measurement Error (N=10,170)

	Math			Read		
	(1)	(2)	(3)	(4)	(5)	(6)
Parent more than HS	0.08***	0.09***	0.09***	0.05***	0.05***	0.05**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Parent univ+	0.17***	0.17***	0.17***	0.13***	0.13***	0.11***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Peer parent HS+	0.40**	0.44**	0.48*	0.62***	0.66***	0.56*
	(0.18)	(0.21)	(0.28)	(0.20)	(0.23)	(0.29)
Peer parent univ+	0.54**	0.55**	0.56	0.56**	0.59**	0.24
	(0.24)	(0.26)	(0.35)	(0.27)	(0.28)	(0.32)
Number of school groups	100	300	743 (all)	100	300	743 (all)
Joint significance of peer parental education						
F-stat	3.12	3.04	1.77	4.95	4.51	1.89
p-value	0.04	0.05	0.17	0.01	0.01	0.15

Notes: i) Standard errors clustered at school level; ii) * $p<0.10$, ** $p<0.05$, *** $p<0.01$. All regressions include controls for class size and p_c (percentage of students observed in the class), school fixed effects, prior achievement, and teacher characteristics, such as female, white, experience, experience-squared, tenure and tenure-squared. The sample corresponds to a subsample of schools that appear to be randomly assigning students based on parental education and based on fall test scores as discussed in Section 3. p_c is interacted with peer variables and school groups to deal with missing values, as discussed in A.3.

Behavioral and emotional traits of *paulista* youth

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Abstract

Several studies have shown that socio-emotional traits are highly associated with educational and labor market outcomes. However, investigations about such an association are still scarce in developing countries. The present study brings the results of an extensive survey conducted in low-performing public schools in Sao Paulo, Brazil, which measured students' personality traits. We find that our measures of socio-emotional skills are positively associated with school attendance, students' performance in both blind exams and under teachers' evaluation, and school progress and enrollment. Socio-emotional skills also explain a large portion of racial and gender gaps in students' outcomes. Educational policies that decrease gaps in socio-emotional skills might, therefore, reduce racial and gender disparities in school performance and dropout in Brazil.

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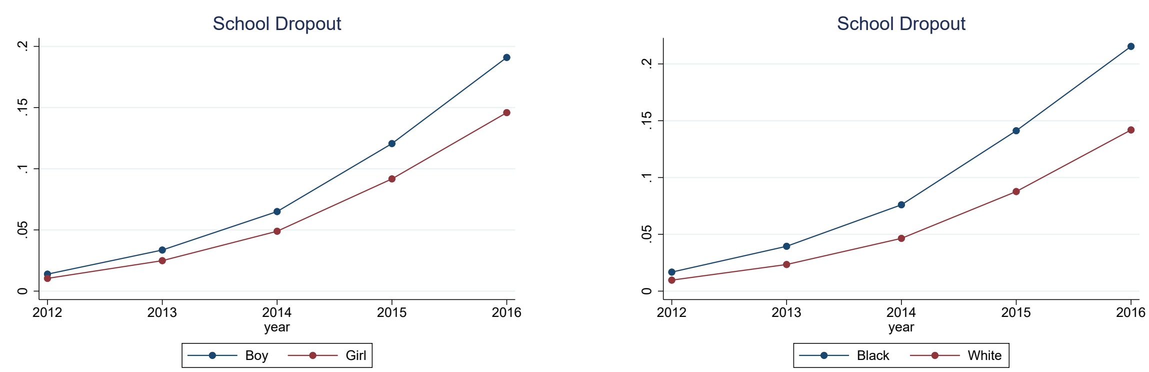
1 Introduction

Even though developing countries have remarkably improved their educational attainment in the past decades, low secondary school completion rates still pose a significant challenge to increase their human capital accumulation. Countries such as Mexico, Colombia, Turkey, and Brazil have more than 15% of school-aged youth not enrolled in upper-secondary education, well above the OECD average of 7% (OECD, 2017). Furthermore, the rates of out-of-school youth are very unequally distributed regarding race and gender (Busso, Bassi, & Muñoz, 2013; Fortin, Oreopoulos, & Phipps, 2015). Figure 1, for instance, compares the evolution of dropout rates between boys and girls (panel (a)), and blacks and whites (panel (b)), for students who were in the 7^{th} grade in 2011 in Sao Paulo, Brazil. In the last year of secondary school (2016), 14.6% of girls and 14.4% of white students had dropped out of school compared to 19.0% of boys and 21.5% of black students.



(a) Gender gap

(b) Racial gap

Figure 1: Gender and racial gaps in school dropout - 7^{th} graders in 2011 in Sao Paulo, Brazil

Note: the figure shows the evolution in dropout rates of the cohort of students in the 7^{th} grade in 2011 in the state of Sao Paulo, Brazil.

There are numerous factors associated with school dropout, such as high direct and opportunity costs of education (Angrist & Lavy, 2009; Banerjee, Jacob, Kremer, Lanjouw, & Lanjouw, 2000), school quality (Hanushek, Lavy, & Hitomi, 2008), and students' low performance in school (Dalton, Glennie, & Ingels, 2009; Ekstrom et al., 1986; Foley, Gallipoli,

& Green, 2014).

In particular, more recent literature in developed countries has highlighted the role of socio-emotional or personality skills as a fundamental determinant of school attendance and attainment. Studies primarily based on U.S. data have shown that such traits predict school and work success almost as much as cognitive skills (Coneus, Gernandt, and Saam (2010); Duckworth, Peterson, Matthews, and Kelly (2007); Duckworth and Seligman (2005); Heckman and Rubinstein (2001); O'Connell and Sheikh (2009), and see Almlund, Duckworth, Heckman, and Kautz (2011) for a review). However, measures of personality skills and studies relating them with school and/or labour market outcomes are still scarce in developing countries. Furthermore, recent papers have shown that the direct application of instruments measuring socio-emotional abilities in developed countries with no adaptation to different contexts could lead to failures in measuring such abilities in developing countries (Laajaj & Macours, 2017; Laajaj et al., 2019). This brings extra challenges to the investigation of such skills in less-developed settings.

In the present study, we leverage a database collected in more than 33,000 students from about 200 schools in Sao Paulo, Brazil, to assess the importance of socio-emotional skills for school outcomes in the country. In particular, the goals of this study are threefold. First, it validates new measures of socio-emotional skills that were based on well-established instruments in the literature but adapted to meet cultural and language components of Brazilian students - especially those coming from low SES backgrounds. Second, it investigates the predictive power of such measures for both students' current and future outcomes, such as performance, grade retention, and school dropout. Finally, it examines how much personality traits explain gender and racial gaps in such outcomes, focusing especially in school dropout.

This paper contributes to emerging literature investigating the importance of socio-emotional skills in developing countries (Cabezas, Cuesta, & Gallego, 2011; Cunningham, Torrado, & Sarzosa, 2016; Glewwe, Huang, Park, et al., 2011; Krishnan & Krutikova, 2013;

Nordman, Sarr, & Sharma, 2015; Wang et al., 2016). Our investigation is related to Lleras (2008)'s study, which finds that personality traits explain a meaningful portion of the socioe-cononomic, sex, and racial and ethnic gaps in educational attainment for American students. It is not evident, however, that the impact of such traits on individuals' outcomes will be the same for developing and developed countries. On the one hand, the returns of personality skills might be higher in less developed settings due to their scarcity - just like the case for cognitive abilities. On the other hand, if the impact of personality traits depends on a minimum threshold of cognitive skills - literacy, for instance - we might not observe a high return to personality skills in developing countries, where many individuals don't meet this threshold.

Our results show that socio-emotional skills are very predictive of students' current and future school outcomes, even after controlling for cognitive skills, socioeconomic status, and school fixed effects. Socio-emotional skills also explain a substantial portion of gender and racial gaps in schools attainment and attendance.

2 Data

Our data come from a survey taken by approximately 33,700 students in the 7th and 9th grades of 203 low-performing schools in Sao Paulo, Brazil. The main goal of this survey was to access students' profile, such as their studying habits, their expectations about the future, and their socio-emotional traits. We combine students' answers with administrative data to recover information on their performance, retention, dropout, and socioeconomic indicators. Table 1 shows some summary statistics of our sample.

2.1 Measures of socio-emotional skills

The instruments used to measure students' socio-emotional skills were adapted from the international literature. They were based in the woks of Rotter (1966), to measure locus of control; Tangney, Baumeister, and Boone (2004), to measure self-control; Bandura (1997) to measure self-efficacy; and Rosenberg (1986), to measure self-esteem. We also based some of our questions on the Strengths & Difficulties Questionnaires [1] to build measures of agreeableness and rapport with peers.

We translated and adapted selected questions of the instruments above to build our measures of socio-emotional skill. Because this was a sample of low-performing students, we framed the items most simply and straightforwardly, such that even students with reading problems would understand them. In each question, students were presented a statement and should say how much they agreed with it, or how similar to their own beliefs and attitudes each statement was. To avoid that students would always choose intermediary answers by default, we eliminated this option, such that students would always have to either agree or disagree with some statement, but could choose the level of (dis)agreement. The locus of control scale was the only one different from this structure: students were presented two different sentences - each of them expressed an idea that was closer to either a more *external* locus of control or a more *internal* one - and were asked to choose the one that they identified more with.

Before extracting the factors underlying students' answers to these questions, we corrected such answers for acquiescence, namely individuals' tendency to agree to everything that is asked (Watson, 1992). We did that by creating an acquiescence index, which was the mean of all opposite pairs of questions in the questionnaire[2], and extracting such an index from all items in the questionnaire.

After the correction for acquiescence, we performed a principal component analysis for 7th and 9th graders altogether, including all items except for those measuring locus of control. We retained the factors with the five greatest eigenvalues. Because the locus of control scale had a different structure, we measured it by building a standardized summative score of

[1] See http://www.sdqinfo.org/.

[2] For instance, an opposite pair of questions is *(a) I feel like I have several good qualities*, and *(b) Sometimes I feel like I am useless.*

all items in the scale. Hence, we extracted six factors in total, each of them representing one of the scales described above. Table 2 shows the factor loadings of each item in the instrument[3]. It shows that our instrument has good construct validity: in general, items intended to measure one latent factor do group together in their loading to that factor.

2.2 Socio-emotional traits and demographic characteristics

Before moving to the associations between socio-emotional traits and school outcomes, it is interesting to check whether such traits vary depending on age, race, and gender. We analyze this in figures 2 to 4.

Figure 2 shows differences in the mean of socio-emotional skills depending on the grade students are enrolled. We see that age and education, which move together across grades, seem to be related to the traits we measure. Interestingly, not all dimensions move in the same direction when contrasting kids of different ages/grades. There are noticeable improvements in self-esteem, self-efficacy, and locus of control. In comparison, self-control, agreeableness, and rapport with pears decrease.

Figure 3 shows differences in the mean of socio-emotional skills depending on students' race. One can see that black students are clearly disadvantaged in all dimensions of socio-emotional traits we measure. Differences between whites and brows are mostly nonexistent.

Finally, Figure 4 shows differences in the mean of socio-emotional skills depending on students' gender. Gender differences do emerge quite clearly: girls outperform boys in agreeableness, rapport with peers, locus of control, and self-efficacy. Yet, boys seem better in terms of self-esteem and self-control.

However, it is important to notice that since there are more girls and non-blacks in higher grades, part of the difference across races and gender emerges from grade/age composition or respondents.

[3]We omit loadings lower than .3 for the sake of clarity, .

3 Contemporaneous and longitudinal associations between socio-emotional traits and school outcomes

In this section, we exploit administrative data to associate students' socio-emotional traits with their outcomes in school in 2011 - the same year when the survey was taken - and in the following years.

Table 3 shows how socio-emotional skills relate with contemporaneous school outcomes. Columns (1) and (2) display estimations for reading and math performance in a standardized test taken by all state-owned schools in Sao Paulo - SARESP. Columns (3) and (4) present results for classroom teachers' evaluations in reading and math, respectively. Finally, column (5) shows the estimation for a binary variable equal to one if a student had more than 95% of school attendance in 2011. All regressions control for parental education, parents working status, students' previous performance in SARESP, and schools' fixed effects.

We first notice that locus of control seems to be the construct most associated with students' contemporaneous outcomes. Except for school attendance, locus of control has the highest association with each of the outcomes, compared to other socio-emotional traits. Interestingly, comparing students' performance in SARESP (columns (1) and (2)) with the grades that teachers assign to them (columns (3) and (4)), we see that self-efficacy seem to matter significantly more for the first than for the second. At the same time, self-control is not associated with performance in SARESP, but it is quite relevant in explaining teachers' grades. It is also the construct most associated with school attendance. Self-esteem, agreeableness, and rapport with peers are also, in general, positively associated with school contemporaneous outcomes. Finally, we notice that white students perform better in all outcomes while girls perform better in all but Math in SARESP. 7th graders perform worse in SARESP but better in all other outcomes.

Figures 5 and 6 show preliminary evidences that socio-emotional skills are also associated with students' future outcomes. The figures show matrices of grade attrition from 7th/9th grade onward. Each column of the matrices shows the grade where students are enrolled each year (the rows of the matrices). The last column has information about students who had dropped out of school each year. In each non-empty cell of these matrices, we plot the mean of socio-emotional skills (measured at baseline, before transitions occur) for the sample of students contained in that cell. The figures make it very clear that the students who follow the normal school trajectory in the following years are those who had higher socio-emotional skills in 2011 and that students who repeated grades or left school are those with lower levels of such skills.

Table 4 displays estimations of the association between socio-emotional skills and students' future outcomes, controlling for SES, their performance in SARESP in 2011, and school fixed effects. Columns (1) - 7th grade - and (4) - 9th grade - show the results for a binary variable that takes value equal to one if the students had what we call a "normal school path" - that is, the students finish school at the right time, without dropping out or being retained. Columns (2) and (3) show the performance in the 9th grade SARESP for reading and math, respectively, for students who were in the 7th grade in 2011. Finally, columns (5) and (6) show the performance in the 12th grade SARESP for reading and math, respectively, for students who were in the 9th grade in 2011.

Once more, locus of control is the most associated construct with students' performance. Self-control, very associated with class attendance, is also very predictive of the likelihood of students having a normal school path, especially for students in the 9th grade. For students in the 7th grade, self-esteem also seems to matter quite a lot for both their path in school and school performance. Compared to the other constructs, rapport with peers does not seem to have too much predictive power, except for the likelihood of having a normal school path for students in the 7th grade. This might be an indicator of the strong influence of peers in school attendance, as documented in the literature (Sacerdote, 2001).

Interestingly, being white is predictive of students' likelihood of having a normal school path for students in the 7th grade, but not for students in the 9th grade. This might indicate that black students more likely to fall behind in school had already done so by 9th grade. In turn, girls are always more likely to have a normal school path than boys, regardless of the grade.

Overall, this section showed evidence that our measures of socio-emotional skills are very predictive of both contemporaneous and future students' outcomes. While the patterns found here are interesting in their own right, they are also very important in showing the predictive validity of our instruments. That is, not only they are significantly associated with expected students' behavior, but such a relation is also quite persistent across time - the constructs are predict students' outcomes even several years after they were measured.

4 Socio-emotional skills and school gaps

The motivating graphs in Figure 1 show that the likelihood of dropping out of school is not evenly distributed across students. On the contrary, there are considerable gender and racial gaps in school dropout, with boys and black students being more likely not to finish high school than girls and white students. In this section, we investigate how much of this gap can be explained by differences in students' socio-emotional skills. While dropout gaps are our main object of interest, we also look at gaps in contemporaneous and future performance since they are important factors related to school dropout. To perform such an investigation, we use the decomposition proposed by Gelbach (2016).

4.1 Gaps' decomposition

We start with a model of school outcomes determination:

$$Y_{i,j} = \alpha_j + \gamma_{gl} Girl_i + \gamma_{bl} Black_i + X_i^{SES}\beta_{SES} + X_i^{Cog}\beta_{Cog} + X_i^{SocEm}\beta_{SocEm} + \epsilon_i \quad (1)$$

where Y_{ij} are different measures of school outcomes for student i in school j, α_j are school fixed effects, and ϵ_i is an individual error term. Our primary parameters of interest are γ_{gl}, and γ_{bl}, the coefficients on the gender, and race indicator variable. X_i^{SES} is a vector containing demographic and socioeconomic variables, such as parental education, and parents' working status. X_i^{Cog} contains information on students' previous performance[4] on SARESP for both reading and math, as a measure of students' previous cognitive skills. Finally, X_i^{SocEm} brings our measures of socio-emotional skills.

Our main goal is to estimate the effects of the inclusion of socio-emotional skills on the estimate of γ_{gl}, and γ_{bl}, controlling for the inclusion of other determinants of school outcomes, such as cognitive skills, socio-economic status, and school quality. A common approach in this kind of estimation is to estimate first equation 1 controlling only for the girl and black indicator variables and then sequentially include other controls to see how much they explain the racial and gender gaps. However, as shown by Gelbach (2016), the observed effect of each set of variables included in a model changes depending on the order of inclusion.

In order to address this concern, Gelbach (2016) proposes a decomposition strategy, which is grounded in the formula for sample omitted variable bias. Consider, for instance, a base specification that regresses Y_{ij} only on the girl and black indicators, excluding α_j X_i^{SES}, X_i^{Cog}, and X_i^{Ncog} from the regressors. To simplify notation, call X_1 the vector containing the girl and black indicators and X_2 the vector containing α_j, X_i^{SES}, X_i^{Cog}, and X_i^{Ncog}. Call β_1^{base} the coefficient of the base specification

$$Y_{ij} = X_1\beta_1 + \epsilon_i \tag{2}$$

and β_1^{full} and β_2 the coefficients of the full specification:

$$Y_{ij} = X_1\beta_1 + X_2\beta_2 + \epsilon_i \tag{3}$$

[4]For estimations on contemporaneous performance - that is, performance in 2011 - and the likelihood of having a normal school path, "previous performance" is the results from SARESPs in the 5th grade - for students that were in the 7th grade in 2011-, and 7th grade - for students that were in 9th grade in 2011. For estimations on the future proficiency in SARESP, "previous performance" is the results from SARESPs in 2011.

Based on the discussion about omitted variable bias, Gelbach (2016) shows that since $plim\ \widehat{\beta}_1^{base} = \beta_1 + \delta$, and assuming that $\widehat{\beta}_1^{full}$ is consistent for β_1, δ is exactly the difference we look for when comparing the black/girl gap in the base and full specifications. That is, $\widehat{\delta} \equiv \widehat{\beta}_1^{base} - \widehat{\beta}_1^{full}$. Given that $\widehat{\delta} = (X_1'X_1)^{-1}X_1'X_2\widehat{\beta}_2$, if X_2 contains K variables, the contribution of the k^{th} variable to the black/girl gap is given by β_k multiplied by Γ_k, where Γ_k are the estimates of the coefficients on X_1 from an auxiliary model with X_{2k} as the dependent variable[5].

Gelbach (2016) also shows that since $\widehat{\delta} = \sum_k \widehat{\delta}_k$, the contribution of different covariates for the gender and racial gap can be summed into different groups. Therefore, it is possible to recover group-specific contributions to such a gap. In our estimations, we look at the contributions of demographics and socioeconomic status (which include school fixed effects), students' previous proficiency in reading and math, and socio-emotional skills. With this approach, we are able to understand the contribution of socio-emotional skills for differences in gender and racial gaps in school outcomes and compare such contribution with one of other important groups of variables.

4.2 Explained Gaps

Tables 5 and 6 show the gaps decomposition for the 7th and 9th grades, respectively. For each outcome of interest, the first estimation (columns (1), (3), (5), (7), and (9)) shows the race and gender gaps as in equation 2, that is, with no other controls besides indicators of race and gender. The second estimation (columns (2), (4), (6), (8), and (10)) shows these gaps as in equation 3, that is, including all other controls, such as students' socio-emotional skills, past performance, and SES. These last columns also show how much of the gender and racial gaps are explained by each group of controls - namely socio-emotional skills, previous reading performance, previous math performance, and SES.

For the sake of visualization, figures 7 to 12 show graphically the results presented at

[5] See Gelbach (2016) for more details

tables 5 and 6. In each sub-figures, the graph on the left compares the gender/racial gaps in estimations with and without controls, and the graph on the right shows how much of this difference (in percentage) is explained by previous reading scores, previous math scores, socio-emotional skills, and SES.

The results for contemporaneous school outcomes (Figures 7 and 8) show that socio-emotional skills explain a much larger share of the gaps in the 7th grade when compared to the 9th grade. Socio-emotional skills explain at least 25% of the gap in the 7th grade (Figure 7 Panel (c)), while they explain at most 18% in the 9th grade (Figure 8, Panel (b)). Such a difference might be due to boys or blacks students with low levels of socio-emotional skills being left behind in school before they get to the 9th grade.

If we focus on the 7th grade, we see an interesting pattern emerging from this exercise. Socio-emotional skills explain more of differences in proficiency between boys and girls than according to race, which seems to reflect mostly previous learning delays. Panel (a) in Figure 7, for instance, shows that socio-emotional skills represent 30% of the explained racial gap in reading, while past reading and math performance represent, together, more than 50% of the explained gap. Panel (b), in turn, shows that socio-emotional skills represent more than 55% of the explained gender gap in reading, while past reading and math performance represent less than 40%. Similar patterns are observed for math (Panels (c) and (d)).

When it comes to school progress (Figure 9 - 7th grade - and Figure 10 - 9th grade), once more we see that socio-emotional skills explain a more substantial portion of the racial and gender gaps in 7th grade than in 9th grade. The grade difference here may be related to the fact that retention between 7th and 9th is not based on performance, while after 9th, it is. Unlike contemporaneous performance, however, socio-emotional skills represent around 20% of the explained gaps for both girls and black students, while past performance represents about 60% of the explained gaps.

Finally, when we look at students' future outcomes in school (Figure 11 - 7th grade - and Figure 12 - 9th grade), we notice that socio-emotional skills explain very little of the racial and gender gaps, even for 7th graders. Again, this might be due to the selection of students with higher socio-emotional skills in the sample that progresses normally in school and for which we have data on future outcomes.

5 Conclusion

There are at least three takeaways from the results presented in this study. First, personality traits are very predictive of students' current and future outcomes in developing countries. Hence, educational policies that increase such traits in students might be very effective in improving educational outcomes in these countries. The fact that socio-emotional skills are more malleable than cognitive skills (Almlund et al., 2011) indicates that focusing on these skills as remedial policies for low educational attainment might deliver satisfactory results, as shown by Wang et al. (2016).

Second, even though personality traits explain a substantial portion of educational disparities between boys and girls, and blacks and whites, discrepancies in cognitive skills between black and white students are still more important in explaining the racial gaps in achievement. Therefore, it is crucial to bear in mind that socio-emotional skills are no panacea, and policies that aim to increase the level of such skills should be combined with policies that re-mediate the lack of cognitive abilities, especially for disadvantaged students.

Third, the fact that personality traits are not so relevant in explaining racial and gender gaps in the 9th as they are in the 7th grade indicates that students with low-levels of socio-emotional abilities are being left behind - we actually show this phenomenon in section 3. This finding indicates that early adoption of policies that aim to raise personality abilities is likely to be more effective in increasing the outcomes of low-ability students.

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6 Tables and Figures

Table 1: Summary Statistics (N= 33,722)

	Mean	Std.Dev.
Students in 7th grade	0.57	0.50
Age	13.31	1.31
Ethnicity: Brown	0.51	0.50
Ethnicity: Black	0.11	0.32
Non-reported mother education	0.29	0.45
Mother's education: Primary School or less	0.45	0.50
Mother's education: HS	0.21	0.41
Mother's education: College or more	0.05	0.22
Mother works	0.59	0.49
Mother got very sick since beginning of year	0.25	0.43
Non-reported father education	0.32	0.47
Father's education: Primary School or less	0.45	0.50
Father's education: HS	0.18	0.38
Father's education: College or more	0.05	0.22
Father works	0.73	0.44
Father got very sick since beginning of year	0.18	0.39

Table 2: Factor loadings

Variable	Factor1	Factor2	Factor3	Factor4	Factor5	Dominant interpretation
self_control_1			0.4478			
self_control_2			0.5357			
self_control_3			0.4501			
self_control_4			0.4983			
self_control_5				0.5044		
self_control_6			0.4924			Self- Control
self_control_7					0.3356	
self_control_8			0.4372			
self_control_9				0.5124		
self_control_10			0.6133			
self_control_11			0.6343			
self_efficacy_1		0.538				
self_efficacy_2		0.5276				
self_efficacy_3		0.5526				
self_efficacy_4		0.5682				
self_efficacy_5		0.5546				Self- Efficacy
self_efficacy_6		0.5986				
self_efficacy_7		0.5132				
self_efficacy_8		0.575				
self_efficacy_9		0.5502				
self_esteem_1	-0.4254					
self_esteem_2	0.652					
self_esteem_3	-0.4954	0.3001				
self_esteem_4	-0.3655					
self_esteem_5	0.5321					Self- esteem
self_esteem_6	0.7427					
self_esteem_7	0.7623					
self_esteem_8	0.3695					
self_esteem_9	0.6961					
self_esteem_10	-0.5028					
agreeableness_1		0.3326		0.4412		
agreeableness_2				0.4512		Agreeableness
agreeableness_3				0.4676		
rapport_peers_1					0.6969	
rapport_peers_2					0.5957	Rapport w/ Peers
rapport_peers_3					0.6311	

Table 3: Socio-Emotional Skills and Contemporaneous School Outcomes

	(1) Reading (SARESP)	(2) Math (SARESP)	(3) Reading (teacher)	(4) Math (teacher)	(5) More than 95% of attendance
Self-esteem	2.088***	1.656***	0.130***	0.117***	0.011***
	(0.202)	(0.192)	(0.008)	(0.009)	(0.002)
Self-efficacy	4.666***	2.845***	0.109***	0.105***	0.006***
	(0.225)	(0.210)	(0.008)	(0.009)	(0.002)
Self-control	-0.270	0.282	0.135***	0.143***	0.023***
	(0.194)	(0.191)	(0.008)	(0.008)	(0.002)
Agreeableness	0.822***	0.226	0.123***	0.115***	0.017***
	(0.181)	(0.174)	(0.009)	(0.009)	(0.002)
Rapport w/ peers	2.667***	1.611***	0.064***	0.039***	0.009***
	(0.217)	(0.177)	(0.008)	(0.010)	(0.002)
Locus of control	5.559***	4.408***	0.139***	0.173***	0.016***
	(0.240)	(0.217)	(0.008)	(0.010)	(0.002)
White	1.984***	1.999***	0.057***	0.050***	0.003
	(0.361)	(0.372)	(0.015)	(0.018)	(0.005)
Girl	8.495***	-4.351***	0.659***	0.416***	0.041***
	(0.426)	(0.325)	(0.024)	(0.024)	(0.005)
Students in 7th grade	-4.224***	-15.956***	0.484***	0.458***	0.147***
	(0.886)	(0.777)	(0.040)	(0.058)	(0.009)
N	31,325	31,325	33,297	33,296	33,720
Mean Dep. Var.	208.11	218.77	5.99	5.85	0.21
Control for SES	Yes	Yes	Yes	Yes	Yes
Control for school FE	Yes	Yes	Yes	Yes	Yes
Control for previous SARESP	Yes	Yes	Yes	Yes	Yes

Note: Standard-errors clustered at school level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Table 4: Socio-Emotional Skills and Future School Outcomes

	7^{th} grade			9^{th} grade		
	(1) Normal school path	(2) Future score Reading	(3) Future score Math	(4) Normal school path	(5) Future score Reading	(6) Future score Math
Self-esteem	0.028***	1.021***	1.098***	0.013***	0.029	0.531
	(0.003)	(0.297)	(0.356)	(0.004)	(0.407)	(0.442)
Self-efficacy	0.017***	1.940***	1.549***	-0.002	2.978***	1.422***
	(0.004)	(0.334)	(0.353)	(0.004)	(0.386)	(0.412)
Self-control	0.030***	-0.481	0.885***	0.035***	0.543	0.335
	(0.003)	(0.323)	(0.339)	(0.004)	(0.447)	(0.456)
Agreeableness	0.022***	1.048***	0.646**	0.017***	0.093	0.003
	(0.003)	(0.293)	(0.317)	(0.004)	(0.411)	(0.391)
Rapport w/ peers	0.012***	0.746**	0.307	0.006	0.770*	0.033
	(0.003)	(0.308)	(0.315)	(0.004)	(0.432)	(0.448)
Locus of control	0.006	3.206***	1.679***	0.013***	3.933***	2.826***
	(0.004)	(0.330)	(0.394)	(0.004)	(0.446)	(0.397)
White	0.015**	1.541**	-0.281	0.002	2.489***	1.750**
	(0.006)	(0.615)	(0.638)	(0.008)	(0.783)	(0.835)
Girl	0.075***	8.229***	-5.022***	0.126***	3.039***	-6.608***
	(0.008)	(0.691)	(0.743)	(0.009)	(0.851)	(0.948)
N	18,024	10,347	10,347	13,301	7,330	7,330
Mean Dep. Var.	0.60	225.76	239.21	0.61	256.86	260.35
Control for SES	Yes	Yes	Yes	Yes	Yes	Yes
Control for school FE	Yes	Yes	Yes	Yes	Yes	Yes
Control for previous SARESP	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard-errors clustered at school level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Table 5: Explained gender and racial gaps in school outcomes - 7th grade

	Reading proficiency		Math proficiency		Normal school progress		Future proficiency - Reading		Future Proficiency - Math	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Black	-11.160***	-3.720***	-11.683***	-4.299***	-0.109***	-0.040***	-11.618***	-1.558	-11.743***	-1.567
	(1.009)	(0.848)	(0.930)	(0.793)	(0.013)	(0.012)	(1.477)	(1.070)	(1.475)	(1.144)
Girl	12.767***	7.800***	-1.858***	-4.714***	0.108***	0.072***	12.563***	8.182***	-4.551***	-5.223***
	(0.580)	(0.504)	(0.544)	(0.480)	(0.007)	(0.007)	(0.821)	(0.643)	(0.817)	(0.670)
Explained GAPs										
Black										
Socio-Emotional Skills		-2.341***		-1.738***		-0.013***		-0.942***		-0.741***
		(0.285)		(0.218)		(0.002)		(0.189)		(0.142)
Reading Performance		-2.267***		-1.357***		-0.021***		-5.709***		-3.161***
		(0.311)		(0.183)		(0.002)		(0.722)		(0.414)
Math Performance		-1.792***		-3.032***		-0.021***		-3.165***		-5.625***
		(0.192)		(0.298)		(0.002)		(0.355)		(0.602)
SES		-1.040***		-1.257***		-0.014***		-0.244		-0.650*
		(0.226)		(0.230)		(0.003)		(0.326)		(0.348)
Total		-7.440***		-7.384***		-0.069***		-10.061***		-10.176***
		(0.613)		(0.560)		(0.005)		(1.078)		(1.013)
Girl										
Socio-Emotional Skills		2.788***		1.857***		0.007***		0.868***		0.263
		(0.207)		(0.175)		(0.002)		(0.192)		(0.195)
Reading Performance		1.776***		0.764***		0.024***		4.730***		2.619***
		(0.179)		(0.103)		(0.002)		(0.391)		(0.234)
Math Performance		-0.215**		-0.214		-0.003***		-1.321***		-2.347***
		(0.094)		(0.166)		(0.001)		(0.188)		(0.327)
SES		0.619***		0.449***		0.009***		0.104		0.137
		(0.133)		(0.134)		(0.002)		(0.176)		(0.188)
Total		4.967***		2.856***		0.036***		4.381***		0.672
		(0.370)		(0.343)		(0.004)		(0.608)		(0.586)
Non-cog. skills	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Reading	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Math	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
SES	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	18,025	18,025	18,025	18,025	18,025	18,025	10,347	10,347	10,347	10,347

Table 6: Explained gender and racial gaps in school outcomes - 9th grade

	Reading proficiency		Math proficiency		Normal school progress		Future proficiency - Reading		Future Proficiency - Math	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Black	-12.806***	-2.633***	-11.442***	-2.912***	-0.061***	-0.004	-17.031***	-5.416***	-15.584***	-4.838***
	(1.155)	(0.886)	(1.036)	(0.869)	(0.014)	(0.013)	(1.772)	(1.362)	(1.634)	(1.396)
Girl	15.840***	7.847***	-1.785***	-5.163***	0.155***	0.126***	8.048***	3.010***	-6.347***	-6.757***
	(0.729)	(0.577)	(0.653)	(0.561)	(0.008)	(0.008)	(1.108)	(0.898)	(1.030)	(0.886)
Explained GAPs										
Black										
Socio-Emotional Skills		-1.246***		-0.880***		-0.008***		-1.156***		-0.811***
		(0.186)		(0.132)		(0.002)		(0.243)		(0.175)
Reading Performance		-6.071***		-2.739***		-0.022***		-7.103***		-3.625***
		(0.541)		(0.264)		(0.002)		(0.899)		(0.482)
Math Performance		-2.515***		-4.366***		-0.020***		-2.811***		-5.125***
		(0.254)		(0.401)		(0.002)		(0.378)		(0.645)
SES		-0.341		-0.545**		-0.008*		-0.545		-1.186***
		(0.245)		(0.221)		(0.004)		(0.439)		(0.425)
Total		-10.173***		-8.530***		-0.057***		-11.615***		-10.746***
		(0.814)		(0.657)		(0.006)		(1.275)		(1.079)
Girl										
Socio-Emotional Skills		1.263***		0.053		-0.003		0.639**		0.042
		(0.202)		(0.183)		(0.003)		(0.310)		(0.275)
Reading Performance		6.467***		2.926***		0.028***		5.862***		2.991***
		(0.351)		(0.189)		(0.002)		(0.552)		(0.304)
Math Performance		0.028		0.045		-0.003***		-1.497***		-2.730***
		(0.140)		(0.241)		(0.001)		(0.227)		(0.393)
SES		0.235		0.354***		0.008***		0.034		0.107
		(0.145)		(0.134)		(0.002)		(0.253)		(0.249)
Total		7.993***		3.377***		0.029***		5.038***		0.410
		(0.547)		(0.455)		(0.005)		(0.840)		(0.727)
Non-cog. skills	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Reading	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Math	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
SES	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	13,301	13,301	13,301	13,301	13,301	13,301	7,330	7,330	7,330	7,330

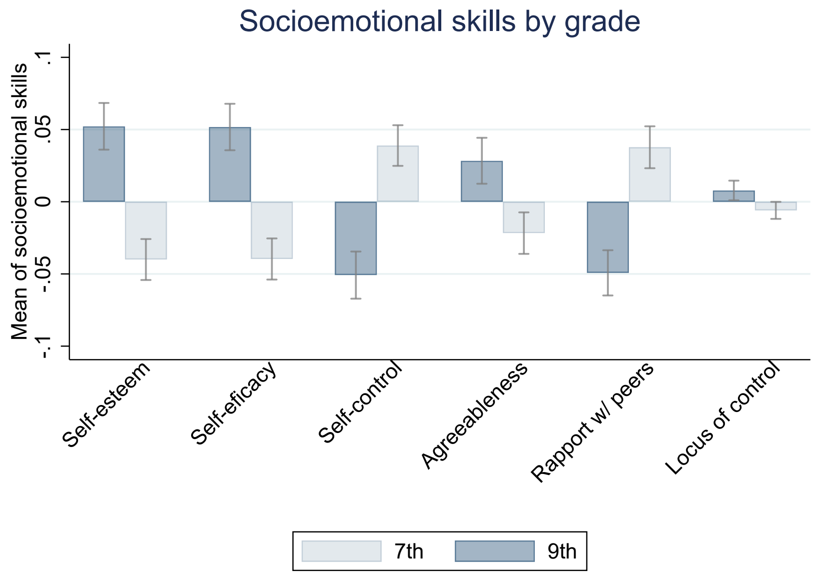


Figure 2: Socio-emotional skills and grade

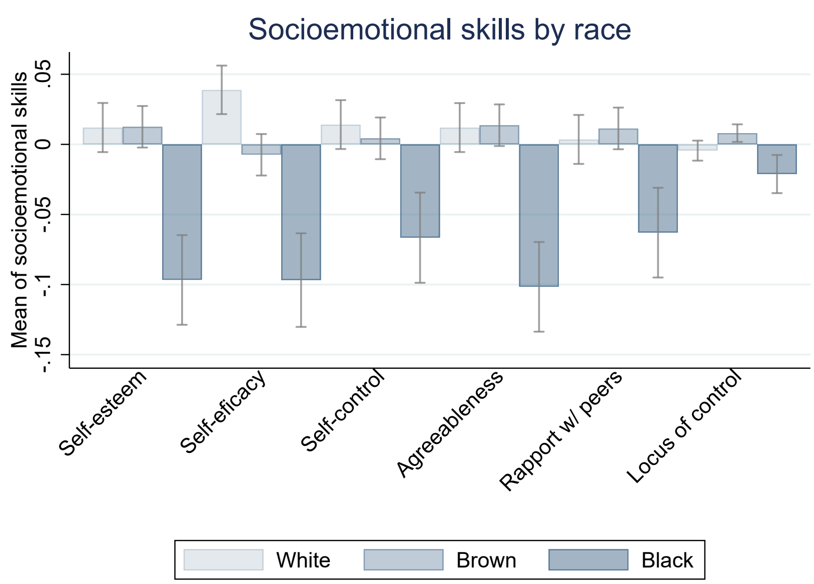


Figure 3: Socio-emotional skills and race

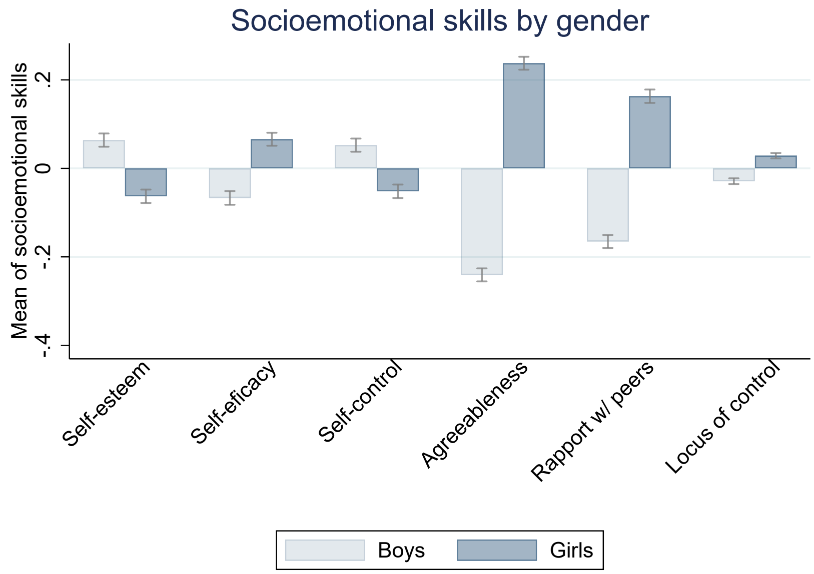


Figure 4: Socio-emotional skills and gender

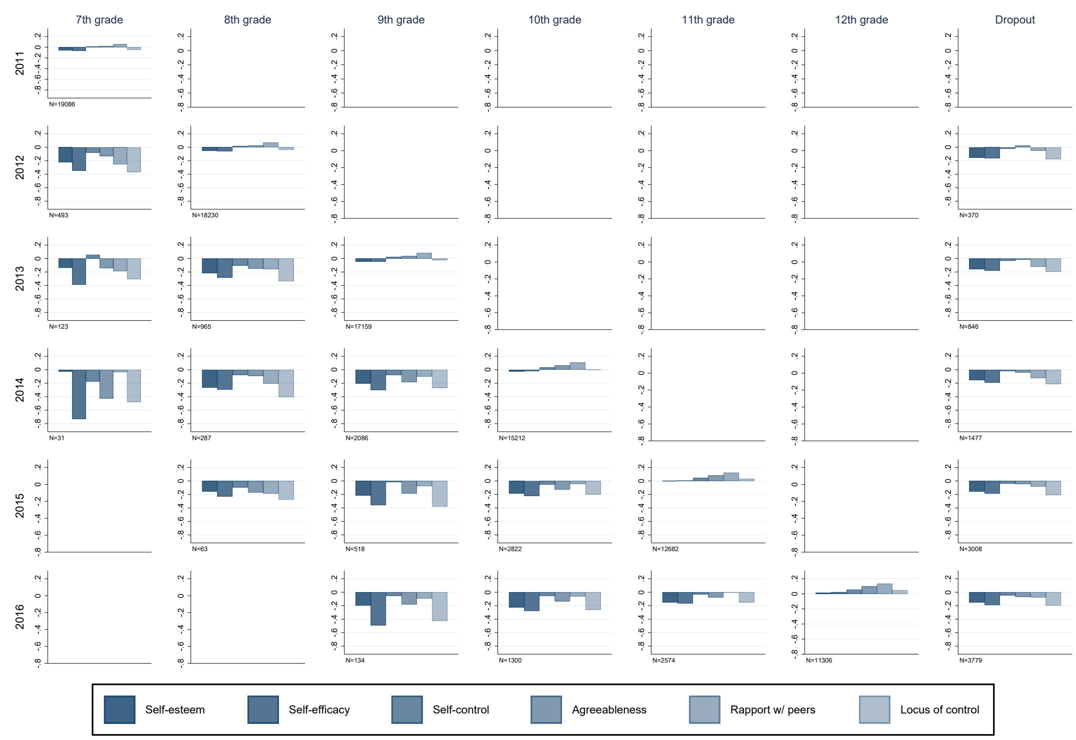


Figure 5: Grade attrition and socio-emotional skills - 7th grade

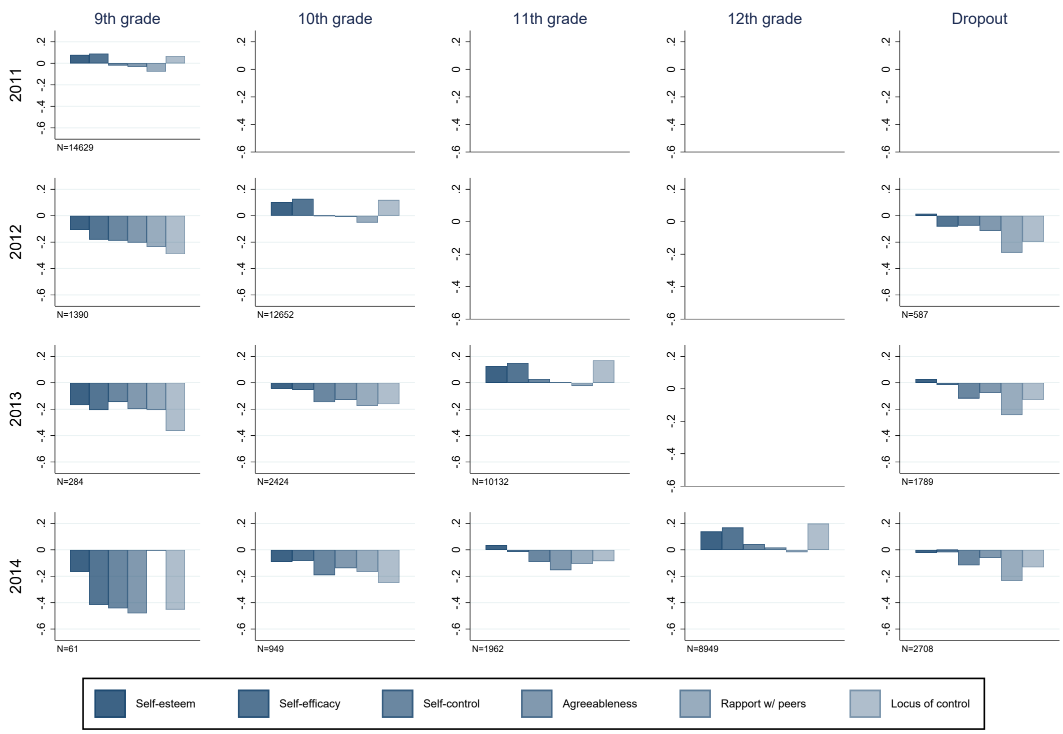
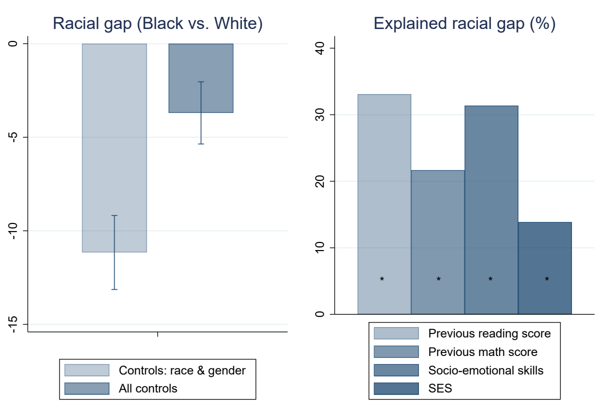
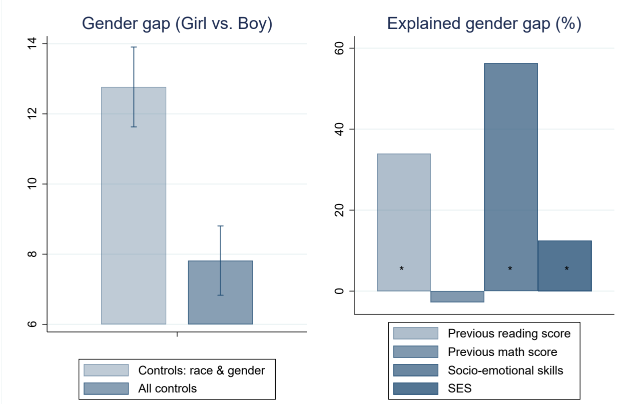


Figure 6: Grade attrition and socio-emotional skills - 9th grade

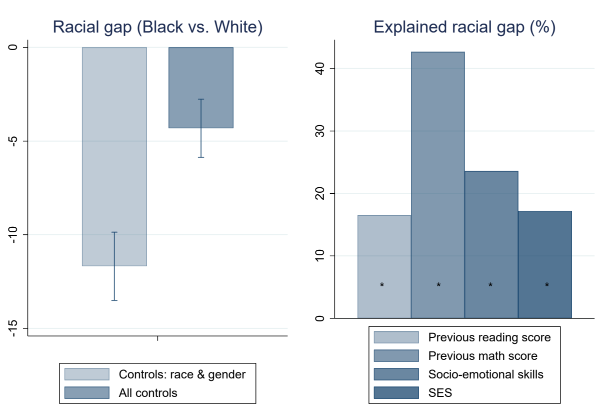
(a) Racial GAP - Reading



(b) Gender GAP - Reading



(c) Racial GAP - Math



(d) Gender GAP - Math

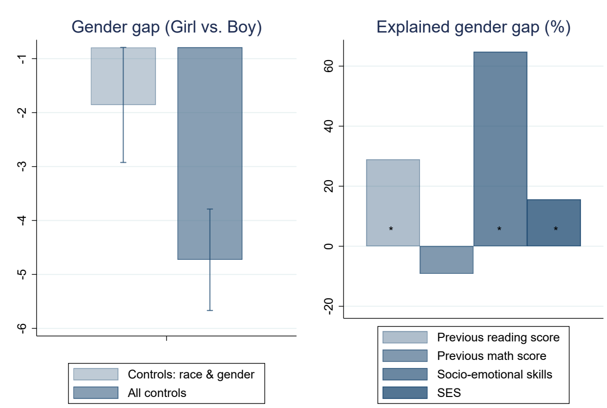
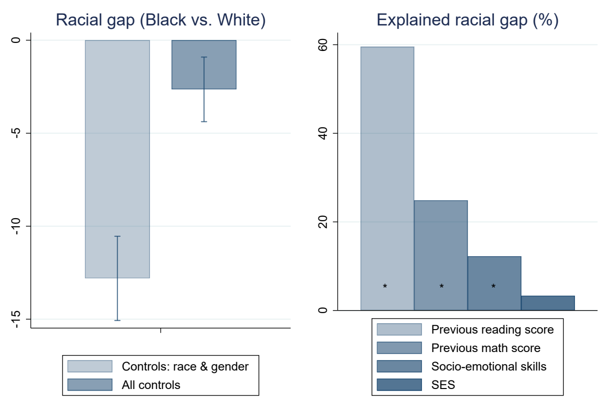
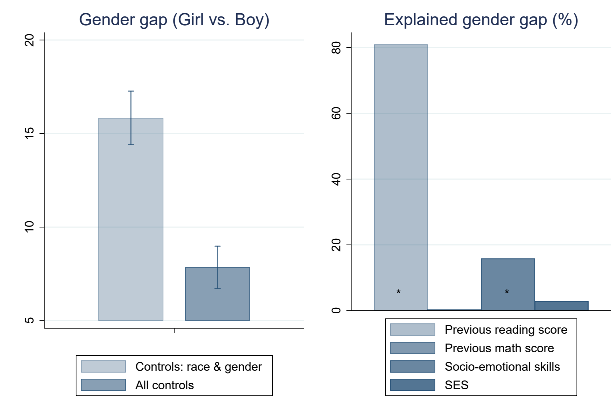


Figure 7: Explained Racial and Gender Gaps in Contemporaneous Performance - 7th grade

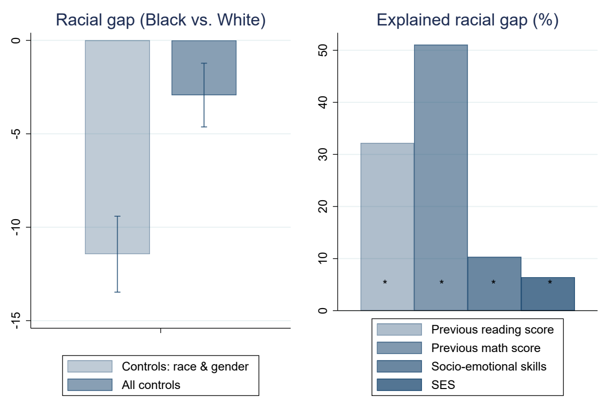
(a) Racial GAP - Reading



(b) Gender GAP - Reading



(c) Racial GAP - Math



(d) Gender GAP - Math

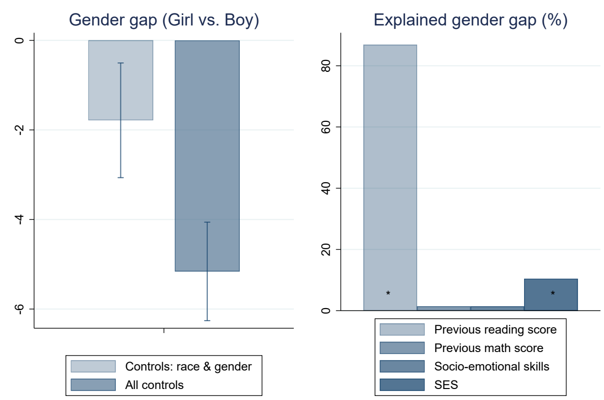
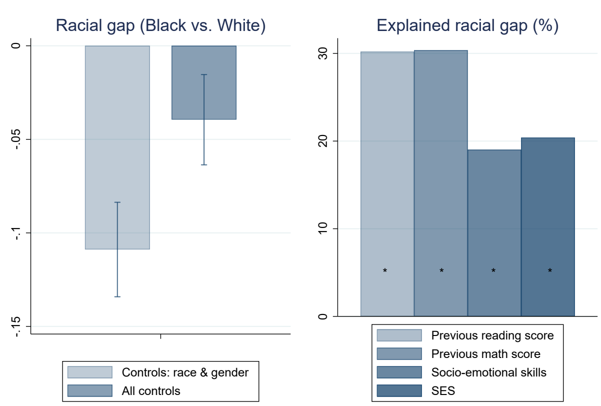


Figure 8: Explained Racial and Gender Gaps in Contemporaneous Performance - 9th grade

(a) Racial GAP - Normal School Path



(b) Gender GAP - Normal School Path

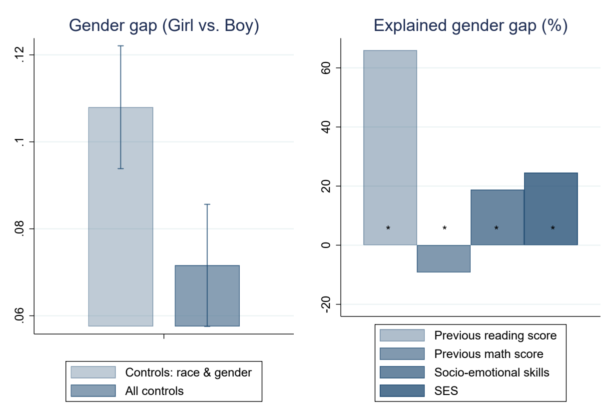
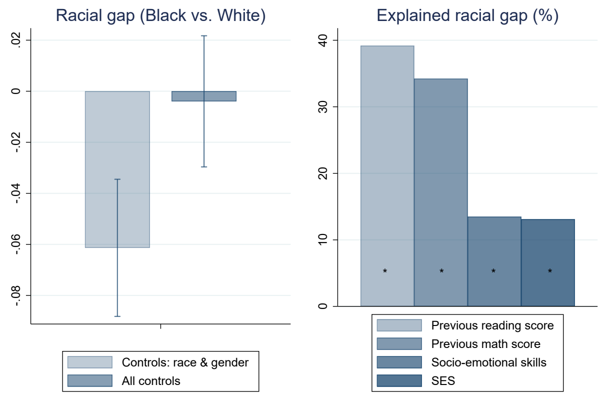


Figure 9: Explained Racial and Gender Gaps in Normal School Path - 7th grade

(a) Racial GAP - Normal School Path



(b) Gender GAP - Normal School Path

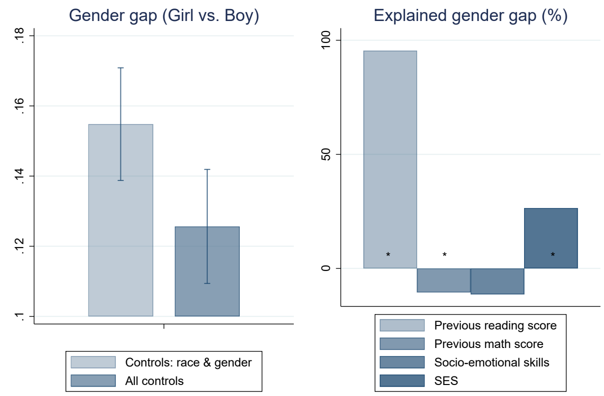
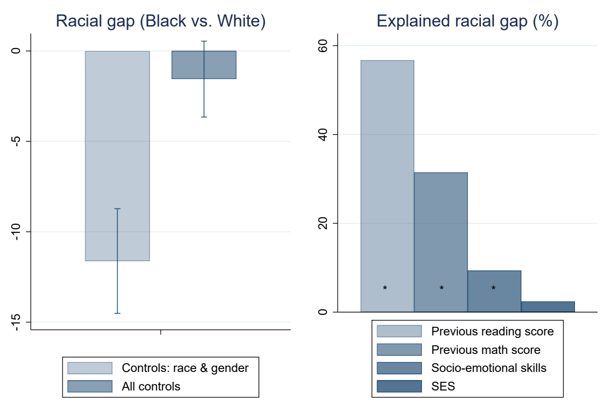
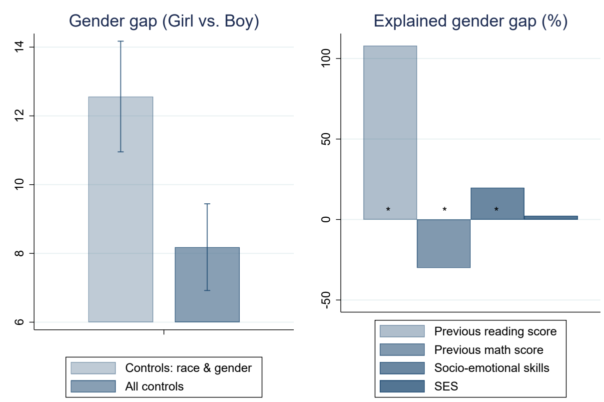


Figure 10: Explained Racial and Gender Gaps in Normal School Path - 9th grade

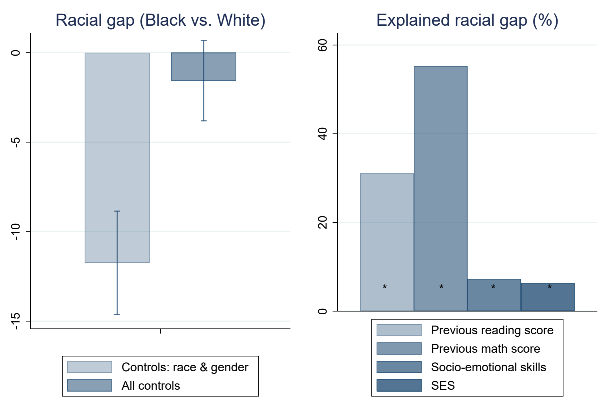
(a) Racial GAP in Future Performance - Reading



(b) Gender GAP in Future Performance - Reading



(c) Racial GAP in Future Performance - Math



(d) Gender GAP in Future Performance - Math

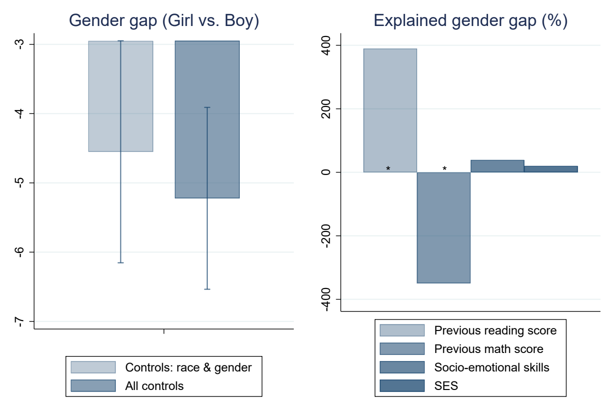
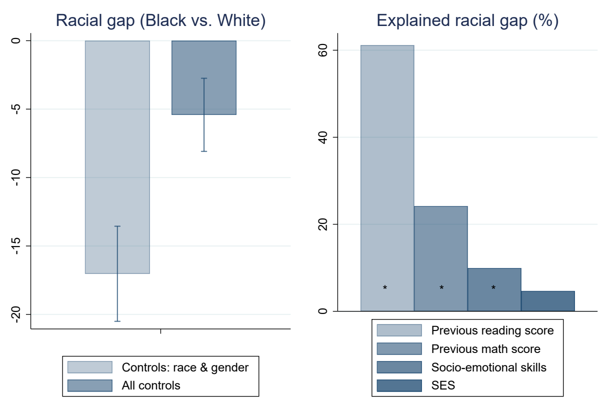
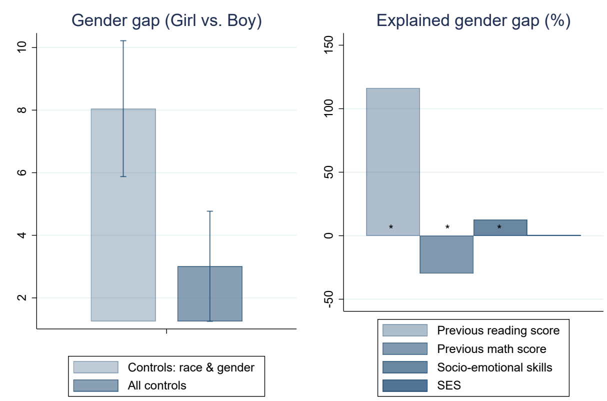


Figure 11: Explained Racial and Gender Gaps in Future Outcomes -7th grade

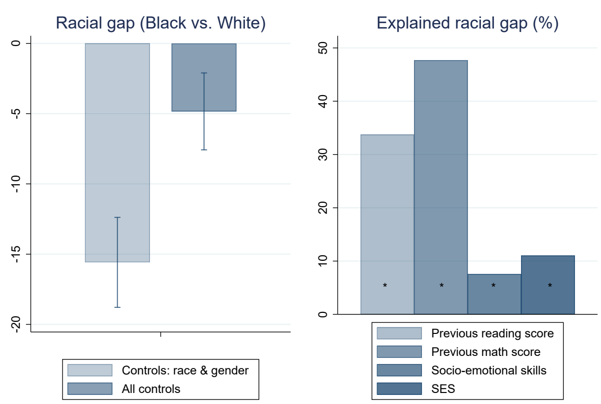
(a) Racial GAP in Future Performance - Reading



(b) Gender GAP in Future Performance - Reading



(c) Racial GAP in Future Performance - Math



(d) Gender GAP in Future Performance - Math

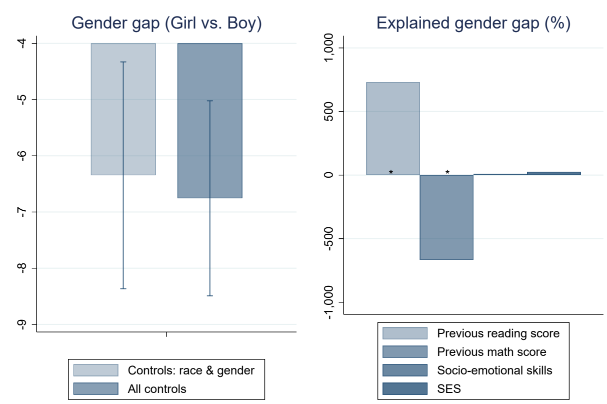


Figure 12: Explained Racial and Gender Gaps in Future Outcomes -9th grade