working paper
2018

Caste in Class: Evidence from Peers and Teachers

Javier García-Brazales

September 2020

CENTRO DE ESTUDIOS
MONETARIOS Y FINANCIEROS
Casado del Alisal 5, 28014 Madrid, Spain
www.cemfi.es

CEMFI Working Paper No. 2018
September 2020

Caste in Class: Evidence from Peers and Teachers

Abstract

Differences in academic achievement across Indian castes are both large and persistent. I make use of rich individual data to explore how class caste composition affects academic progress as well as the mechanisms in place. Benefiting from exogenous assignment of students to classes and teachers, I find that a one-percentage point increase in the proportion of low-caste class-mates leads to a fall of around 2% of a standard deviation in the mathematics score and to much smaller effects in English. This phenomenon is mediated through lower effort exerted by the students, which itself emanates from the students' worsened perception about the extent to which their teachers value them. This non-cognitive channel, which has not been previously identified in the peer effects literature, suggests that the use of a fairly malleable input such as more open and receptive teachers among low-caste students would be an appropriate policy response.

JEL Codes: I24, J15, J24.

Keywords: Castes, peer effects, non-cognitive skills, India.

Javier García-Brazales
CEMFI
javier.garcia@cemfi.edu.es

Acknowledgement

The author gratefully acknowledges the funding received from the Fundación Ramón Areces. The data used in this publication come from Young Lives, a 15-year study of the changing nature of childhood poverty in Ethiopia, India, Peru and Vietnam (www.younglives.org.uk). Young Lives is funded by UK aid from the Department for International Development (DFID). The views expressed here are those of the author. They are not necessarily those of Young Lives, the University of Oxford, DFID or other funders.

1 Introduction

One of the distinctive features of the Indian society is its caste system. It is a hereditary construction that plays a role in about every aspect of daily life and comes hand-in-hand with large inequalities in multiple dimensions between low and high castes (Kijima, 2006). One of them is school achievement. Similarly to the white-black gap in the United States, sizable differences in school enrollment and test scores between high and low castes have been present for decades and, despite a recent narrowing, remain strong nowadays (e.g. Bertrand et al., 2010). Given the importance of understanding the roots of this gap in order to design optimal policies that can reduce the existing academic differences and the inequalities in income and wealth across castes that perpetuate later in life, the lack of formal research on the relative academic circumstances of low and high castes and on their dynamic evolution constitutes an important gap in the literature (Munshi, 2019).

One such policy, launched five decades ago, consisted in putting in place the world's largest affirmative action program to ensure that lower castes would have access to a fixed percentage of reserved seats in university and in public servant positions.[1] A series of recent papers shows that this initiative has been partly successful: Bagde et al. (2016) find that these programs do bring a higher proportion of low-caste students into college education at engineering schools while Bhattacharjee (2016) claims that this is also true for Other Backward Castes. Khanna (2016) shows that the beneficial effects for low-caste students participating in these programs extends to their ability to find jobs.

While these findings shed some light on the extent to which low-caste students benefit from affirmative action in terms of targeting and proper matching of these policies as described in Munshi (2019), formal research around the potential catching-up experienced by low-caste students during the course of the studies and the mechanisms through which these policies work is very scarce. In particular, an important question remains unexplored: what is the role of classroom mix-caste composition on academic and non-cognitive abilities of both lower and higher caste individuals, and how do teachers play a role in moderating or exacerbating these effects?[2] Although some work has postulated that positive peer effects favoring disadvantaged groups should be present when mixed with higher-caste students (Kochar et al., 2008) this is not ex-ante obvious, particularly given the

[1]Originally, these policies targeted the lowest castes (Scheduled Castes and Scheduled Tribes), but were subsequently expanded to Other Backward Castes. This expansion was recommended by the Mandal Commission of 1980, and was implemented in college in 2006. Since the objective of these policies is to bring the caste composition in higher education closer to the overall one in the society, caste-reserved seats amount to around 50% of the available ones: 15% for Scheduled Castes, 7.5% for Schedules Tribes and 27% for Other Backward Castes.

[2]To the best of my knowledge, only two contemporaneous papers in the peer effects literature have had access to changes in teacher practices as a response to class composition: Fruehwirth and Gagete-Miranda (2019) and Gong et al. (2019). While Fruehwirth and Gagete-Miranda (2019) focuses on a very different setting (kindergarten students in the US), Gong et al.'s interest is in peers' gender, which may likely operate very differently from caste effects.

existing evidence on lower performance under negative stereotypes (e.g. Hanna and Linden, 2012) and on unequal teacher treatment of different castes (e.g. Rawal et al., 2010).

While the affirmative action programs just described are targeted at the tertiary level, caste-mixing is not uncommon at earlier stages. The objective of this paper is to answer the above question by being the first one to feature classroom caste composition into an academic production function at middle-school. I do this by exploiting the 2016 Indian School Survey module contained within Oxford University's Young Lives dataset. It collects cognitive and non-cognitive information at the beginning and at the end of one academic year for all the students in all the classes in Grade 9 within a close-to-representative set of schools in the Indian state of Andhra Pradesh. Importantly, it identifies classmates and teachers and provides detailed individual socio-demographic information of both groups. Moreover, it collects information on the procedure followed by the schools when deciding the student composition of each section[3]. This wealth of information is crucial for exploring potential channels as well as for overcoming the well-known difficulties in proper identification of peer effects that have plagued the literature (Manski, 1993).

Investigating the exact mechanisms is important. For example, it may be that low caste status is correlated with lower family investments on education that can spillover to classmates, or that teachers respond to the caste composition of their students so that there is an optimal assignment of students to teachers based on caste. Knowing which channels are operating may contribute to closing the achievement gap. Surprisingly, the lack of evidence on peer effects and teacher effectiveness in India is not exclusive to the caste dimension - for example, although the Government recently favored a large push to foster the presence of female teachers, nothing was known at the time of the implementation about how matched teacher-student gender could help females to close the existing gap in scores (Muralidharan and Sheth, 2016). This highlights the extent to which the Indian educational context is under-researched. This paper aims at making progress along several dimensions.

First, it focuses on the unexplored topic of caste peer effects and looks into their role in cognitive and non-cognitive performance at school. This allows me to provide novel evidence on academic disparities across castes not only statically at a given point in time, but also dynamically over one academic year as well as and on the factors affecting the evolution of the caste gap. This is relevant, among other reasons, because optimal class formation can make a sizable difference in the world's largest school system - over 230 million students enrolled up to Grade 12[4].

Second, it complements the existing research discussed above on the effectiveness of affirmative action programs by taking into consideration the effects of caste composition on the academic progress of the different caste subgroups within a classroom as well as teachers' behavioral re-

[3] I use the terms class and section interchangeably.

[4] Figure reported in the 2013 All India Education Survey.

sponses. Given that changes in class composition arise mechanically in affirmative action programs, understanding their effects on both students benefiting from these programs and those that were not admitted through the policy is relevant when evaluating the overall effectiveness of the programs.

Third, the richness of my data allows me to carefully explore the potential channels that may be behind caste peer effects. These include, *inter alia*, non-cognitive responses from the students and teachers' changes in behavior and/or teaching style. Standard datasets for developed countries lack this information, let alone for developing ones. This has severely limited the exploration of the full set of potential mechanisms through which peer effects operate, hence leaving room for alternative channels to be in place even when the authors find evidence for the presence of a specific one (e.g. Lavy and Schlosser, 2011). Providing this novel look into the student and teacher factors that significantly contribute to the students' human capital accumulation is important in light of the small academic progression and the large disparities in academic trajectories observed in the Indian context (Singh, 2016).

Fourth, my identification of peer effects is arguably credible and straightforward. My data provide information on the assignment procedure of students to classes. This allows me to work with a subsample of classrooms to which the allocation of students is plausibly exogenous (i.e. headmasters state that the assignment was "random" or "by gender"). I verify this through detailed balance checks of baseline characteristics. This quasi-randomized setup that I exploit is uncommon in educational settings across the world, particularly in middle-school[5]. Because class effects may be different at different ages, being among the first papers that credibly explores them for middle school is already a relevant contribution. Importantly in terms of external validity, I show that the qualitative results found for my preferred subsample also hold when expanding the analysis to classrooms with other, less exogenous, allocation procedures.

By using school fixed effects to account for selection into schools, and by showing that teachers do not select into classes either, my estimation strategy overcomes the main threats to identification. Moreover, having access to scores both at the beginning and at the end of the year allows for estimating value-added regressions, which are more informative of the role of the current peers and also allow to control for selection by ability or past investments on the child (Muralidharan and Sheth, 2016). The fact that the exams are multiple-choice and externally distributed and graded alleviates concerns of differential grading across castes[6]. Furthermore, unlike most peer effects papers that make use of random assignment to peers, which focus on a single school (e.g. Sacerdote,

[5] Exceptions are South Korea (Kang, 2007) and China (e.g. Carman and Zhang, 2012; Gong et al., 2018),

[6] This avoids the need of using class fixed effects to only resort to within-class variation in performance as in Fairlie et al. (2014).

2001; Zimmerman, 2003), my data were originally designed to be almost representative[7] of the predominantly rural state of Andhra Pradesh. For this reason, I observe schools with different types of ownership (e.g. governmental, private aided, private unaided...) located in both rural and urban areas. This yields results that are arguably more externally valid.

Lastly, the dataset matches students to classmates and instructors, which allows not only to consider teachers as a mechanism through which peer effects may operate, but also for a narrower definition of peers compared to the studies that use variation within schools in peer composition over repeated cross-sections and that are constrained to define as peers all the students in the same school and grade[8]. In my preferred specification, and following the existing evidence of close network formation within castes (e.g. Brown, 2017; Lowe, 2018), I define peers in an even finer manner: same-caste classmates. This choice, although potentially misclassifying some same-caste classmates as peers, has the benefit of overcoming selection into peers conditional on students being in the same class and also avoids the need for modelling a first stage of friendship formation. In particular, improving on the industry-standard excessively-broad definitions of peers is important to mitigate the downward bias arising from such measurement error (Arcidiacono et al., 2012).

My paper contributes to several strands of the literature. Most broadly, it connects with the large literature on peer effects in educational settings (e.g. Hoxby, 2000; Ammermueller and Pischke, 2009), in particular those exploring contextual peer effects arising from peers' gender and racial composition (e.g. Hanushek et al., 2009; Gong et al., 2019) in a (quasi-) randomized setting (e.g. Kang, 2007; Carrell et al., 2009), and to the much more scarce literature that refers to developing countries (e.g. De Melo, 2014; Gong et al., 2018).

Instead of the widely discussed gender and racial composition, it investigates a particular feature of the Indian society, the caste system, that has not been previously explored in the context of academic peer effects. In this sense, it is closer to a broader literature looking into caste networks (e.g. Banerjee et al., 2013; Lowe, 2018), interactions across castes (e.g. Munshi and Rosenzweig, 2016; Field et al., 2016), school performance under caste stereotyping (Hoff and Pandey, 2006), and the effect of class interactions between rich and poor children in India (Rao, 2019). Furthermore, the availability of non-cognitive data allows not only for exploring the effects of peers on non-cognition, but also the investigation of potential mechanisms. This is important because, in spite of the wealth of papers on peer effects, these two aspects have largely been obviated due to data limitations (as argued in, for instance, Lavy et al., 2011; Gong et al., 2018). In fact, my results suggest a new channel in the literature of peer effects: a fall in the confidence that students have in that their teachers care for and value them (albeit the teachers do not claim to behave in this

[7]It is representative of the 95% non-wealthiest households in the province (pro-poor approach).

[8]In reality one expects the majority of interactions with peers to occur at the class level and, most likely, at an even finer level.

way) leads them to exert less effort at school. Hence, this paper also speaks to the literature on the education production function (a recent review can be found in Jackson et al., 2014) and on teacher effectiveness, which has been often referred to as the most important school input (e.g. Burgess, 2016). In particular, I provide novel evidence on the role of teacher practices and behaviors in affecting students' attitudes with respect to school, especially for stereotyped students (e.g. Hanna and Linden, 2012; Burgess and Greaves, 2013).

The main finding is that classes with a higher proportion of low-caste students progress significantly less over one academic year, particularly in mathematics, for which a one percentage point increase in the percentage of low-caste students is associated with a fall of 2% of a standard deviation in the progression of scores. This effect is present above and beyond those generated by other socio-demographic characteristics correlated with caste and is robust to the inclusion of a rich set of controls for peer characteristics featuring, among others, wealth, baseline cognition and non-cognition, health, and parental education. An exploration of the channels suggests that these effects come from behavioral responses of the students, who feel less valued by their teachers and lower their academic aspirations. Importantly, this finding that (the perception of) prejudices by students matters for performance may apply to contexts other than castes, such as female performance in STEM subjects being affected by lower expectations on their capabilities. When separating the effects by child's caste, lower-caste students do not suffer as much as higher-caste students when mixed with a higher proportion of low-caste students, which lends support in an outside-the-lab context to Hoff and Pandey (2006)'s work on performance under negative stereotyping. Finally, while non-linearities seem to be present, other heterogeneous effects are not.

The rest of the paper is organized as follows. Section 2 describes the data used and demonstrates that there is exogenous assignment of students and teachers to classes within my restricted sample. Section 3 describes the empirical strategy that I employ to estimate the causal impact of caste class composition on students' cognitive evolution, while Section 4 compiles the main results obtained from it. Section 5 investigates potential mechanisms that may be operating behind my findings and quantifies their relative importance. Section 6 briefly explores heterogeneous effects and non-linearities while Section 7 verifies the robustness of my results. Section 8 concludes.

2 Context and Data

2.1 Cultural and Schooling Context

Hindus' social stratification into four hierarchical classes (varnas) intimately linked to the four main professions has been in placed over three millennia. Within these classes there are thousands of castes (jatis). Although being precise about an individual's jati is crucial when exploring social networks, when evaluating broader phenomena and policies (e.g. discrimination, affirmative action programs) the usual level of aggregation relies on a hierarchical system dating back to the British colonial times that aimed at simplifying the system by distinguishing between: i) Scheduled Castes (SC), Scheduled Tribes (ST), Other Backward Castes (OBC), and General Castes (GC). While the first three classes have been traditionally disadvantaged, there are differences across them. Notably, STs are more socially and geographically isolated while OBCs constitute a more recent addition to the classification and tend to occupy a middle-ground between SC-STs and GCs (Deshpande et al., 2014). As conventional in the literature (e.g. Hnatkovska et al., 2013), throughout the analysis I will use the term "low caste" to refer to individuals belonging to SC or ST.

Given the rural nature of a significant part of my sample it is important to understand castes' geographical patterns. Data from the Rural Economic and Demographic Survey (REDS) shows that, for the average Indian village size of 340 households, 30 of them will belong to a given caste. In recent years there has been a governmental focus to provide a physical school within walking distance in rural areas. This, coupled with the geographical dispersion of castes, leads to some degree of caste segregation across schools, particularly in the mixing of GC and low-caste students (mostly ST). However, cross-caste classroom sharing is not uncommon - and it is often accompanied by discrimination in access to resources and treatment against low-caste students as discussed below. Section 2.2 will quantify the extent of caste-mixing within my estimating sample.

The preceding paragraph highlights the importance of taking into account selection of students into schools when exploring peer effects. I deal with this by including school fixed effects, which also accounts for varying school quality. However, it may still be possible that there is selection of students into classes based on unobservables. There are no country-wide regulations regarding how headmasters should distribute students across classes. As a result, several methods are used: by randomization, by ability, by last name, by language of instruction, etc. In the next section I explain how I make use of a question asking headmasters about the way in which each section's composition was decided to ensure that my empirical analysis is performed only for the subset of sections where such composition is plausibly exogenous. One last remark is that once individuals are assigned to a section they remain with the same classmates for all the subjects in the current academic year, which are usually delivered by subject-specific teachers.

2.2 Data

My data come from the Young Lives study led by Oxford University, which followed two cohorts of children (aged 1 and 7 by the time of the first round) across five different waves from 2002 to 2016 in four countries (India, Peru, Ethiopia, and Vietnam). Apart from collecting data for this panel, a complementary study, called "School Survey", was conducted in 2016 to better understand the importance of key educational inputs. To do so, it surveyed a subset of the Young Lives participants as well as all the students in their same school and grade (Grade 9).[9] In this paper I only make use of the 2016 Indian School Survey.

This dataset is unique in that it not only provides information on basic socio-demographic characteristics such as gender, age, academic history, family wealth, parental education, and caste, but it also features rich cognitive and non-cognitive information. Mathematics and English tests were taken both at the beginning and at the end of the academic year. This permits the estimation of value added models (that control for baseline cognition), which have a clearer interpretation of the role of each input over a well-defined period of time and also allow to control for ability and past investments on the children (Singh, 2015). Importantly, since the baseline cognition is computed at the beginning of the year (and not at the end of the previous academic year), we overcome concerns of differential extents of knowledge losses over the holidays (Borman and Boulay, 2004). Moreover, because the exams are standardized and evaluated by objective external examiners there are less concerns about the comparability of scores and grading criteria across schools and classes (which would then be compounded by differential grading depending on student caste as found by Hanna and Linden, 2012). The availability of the detailed set of individual characteristics (particularly wealth and non-cognition) is crucial for the credibility of the conditional exogeneity assumption of caste composition (Luke and Munshi, 2011).[10]

Cognitive data for mathematics and English come from multiple-choice low-stakes tests of 40 and 50 questions, respectively. I standardize the first-round scores to have a mean of 500 and a standard deviation of 100. To make round 2 scores directly comparable, I follow Moore et al. (2017) in using round 1's mean and standard deviation to standardize the scores from the second round. In doing so we observe an increase of 30 points in the average mathematics score and a much more modest one of 6 in English. For this reason, and because peer effects are much weaker in size (and not statistically significant) for English, throughout the text I focus on mathematics, but the English results are available upon request.

Young Lives also obtained numerous measures of non-cognitive performance. In the first round we have information about a series of questions that elicit the interest of each child in reading, her academic aspirations, and the parental interest in the academic performance of the kid. I find their principal component and make use of them as proxies for academic interest, both on the part of the child and of the parents.[11] These measures, often unavailable in standard datasets, will prove

[9] All the sampled schools are located in Andhra Pradesh. It is the tenth most populous Indian state, with an economy based on agriculture and livestock and a relatively low Human Development Index among the Indian states (0.65 in 2018).

[10] As mentioned before, although difference across classes have traditionally been large, there has been a sizable process of convergence along many dimensions such as consumption or income (Munshi, 2019).

[11] Throughout the paper I obtain several variables from extracting the principal component of various survey questions (importantly, most times only one eigenvalue greater than 1 is found). This reduces measurement error (Fruehwirth and Gagete-Miranda, 2019). More details on the variables used are provided in Appendix C.

important in reinforcing the plausibility of exogenous random student assignment addressed in the balance checks as well as the conditional exogeneity assumption in the main analysis.

In the second round a wider range of questions were asked about motivation to do well at school, interest in mathematics and English, academic aspirations, students' perceptions on whether the teacher of each subject is encouraging and open with questions and doubts, cares about whether students understand the material, how frequently the teacher revises homework, etc. Because these questions are not directly comparable to the baseline non-cognitive measures that I use in the balance check and as controls in the main analysis, I am not able to explore their longitudinal evolution as I do for test scores. However, given the absence of significant differences in the first-round non-cognitive measures across individuals with different proportions of low-caste classmates, these second-round measures should still be very informative about potential mechanisms of peer effects, as existing differences at round 2 are likely to have arisen during the observed academic year. For most of these dimensions there are several survey questions aiming at the same issue, so I apply principal component analysis to obtain their main component (further details are provided in section C). In section 5 I will be more specific about the exact variables that I use.

One particular focus of my paper is in exploring whether teachers can play a moderating or exacerbating role in caste peer effects. For example, they may reduce the learning speed of all the students in the class if they were to think that low-caste students are less able to make progress. I elaborate more on the channels and theories related to teachers in the Mechanisms section (5). For now, I highlight that we have ample measures of socio-demographic characteristics of the teachers such as gender, caste, age, experience as instructors, number of days involved in professional training, and membership in a teacher association. Moreover, apart from the students' perception of the teacher's behavior mentioned in the previous paragraph, the second round of the survey also elicited direct information on how much the teachers believe that they can affect students' performance, the importance of background and gender to succeed in school, how much time they devote to preparing their classes, whether they discuss with other faculty members how to more effectively work with students, how often they meet with students' parents, how much time they devote to class preparation, etc. This provides valuable information to discern among possible channels that may be in place.

A final but crucial piece of information provided in the data is the way students were allocated to their respective classes as reported by the schools' faculty members. They had the possibility to name up to three different methods[12]. In my main (and preferred) estimating sample, I disregard those schools with only one section (as consistent with the inclusion of school fixed effects in

[12]The options were: a) there is only one section; b) randomly; c) alphabetically; d) by ability; e) by language; f) by gender; g) by date of enrollment; h) by other method.

the empirical approach) and those sections that were formed based on either ability or language of instruction (which likely reflect unobserved heterogeneity across students). There are 9,820 students in the original sample (8,308 are observed in both rounds, which is needed to observe end-of-year scores). Among the original group, 5,368 were not assigned by ability nor by language. Imposing the presence of more than one exogenously-allocated section in the school reduces the sample to 1,921. On the one side, this subsample has the largest potential to provide unbiased estimates of the effects of the proportion of low-caste classmates on academic achievement. On the other hand, it comes at the expense of a significant decrease in the sample size, which might limit the external validity of my findings. For this reason, I will show that my main qualitative results also hold when including every school with at least two sections, irrespective of their student assignment procedure (i.e. I include sections formed based on ability and/or language).[13]

Apart from ensuring the quasi-random allocation of students to classes within schools as mentioned in the paragraph above, I carry out the following additional sample restrictions. I focus on students who report grades in both the first and the second round, since our interest is in the evolution of such scores. Out of the 1,921 students in my subsample, 1,556 took both mathematics tests (and 1,620 both English exams). They constitute my main estimating sample (but I compare and discuss the findings when using less stringent definitions below). Although there is a set of maintained controls across specifications, in some specifications additional controls or different outcomes will be considered, which results in small variations in the number of observations.

Finally, in order to obtain peers' characteristics I compute the average value among the peers (to be defined below) within the class for each student, leaving herself outside (i.e. leave-out means). I use all students whose characteristics for each variable are available in round 1, irrespective of whether they attrite or not in round 2. One important benefit of my data is the clear linkage of classmates and teachers. Given that there is no question of nomination of friends, I work with two definitions of peers. The first one includes everybody else in the class, as it is standard in the literature.[14] The second (and preferred) one defines peers as only those students within the class that share the same caste as the respondent. This is in line with existing evidence that in-caste networks and preferences towards same-caste individuals are strong in India (e.g. Banerjee et al., 2013; Brown, 2017). This is particularly relevant in rural areas and educational settings where SC and ST students often do not have access to the same infrastructures and resources at school (e.g. toilets, water, and food), are often clustered in the seating arrangement of the classroom, and are not treated equally (and sometimes abused) by higher-caste students and teachers (e.g. Munshi,

[13]Relying on single-section schools, although feasible (there is still within classroom variation in the proportion of low-caste peers), is likely inadvisable since such variation is purely mechanical. On average, the discarded one-section schools have slightly less wealthy students (wealth index of 0.47 vs. 0.51), less educated parents (1.81 vs. 2.14 years of education) and more low-caste students (41% vs. 33%).

[14]Alternatively, and depending on the research design, researchers may instead use everybody else in the same grade.

2019).

Importantly, despite the use of two different definitions of peers, my main independent variable is always measured as the proportion of low-caste students (SC and ST) inside the class (excluding the student herself). This is in line with the literature on peer effects, as it addresses the question of how having a higher proportion of classmates featuring a specific pre-determined characteristics (here, low caste status) affects a student's outcomes.

2.3 Descriptive Statistics

Table 1 compiles the main descriptive statistics about the students obtained from the baseline survey (round 1). They are computed from the 1,921 students for whom we have identified their class formation as exogenous. They are distributed across 58 sections in 27 schools (23 with two sections and 4 with three). Fourteen of these schools are located in rural areas. Class size ranges from 21 to 56 with a mean of 39 and a median of 40.[15] Some key features to highlight from Table 1 are the slight over-representation of females (59% females), the ample heterogeneity in parental education, that fathers are slightly more educated than mothers, that a sizable proportion of students (16%) has repeated a grade, and that 42% of the students in my sample are either SC or ST. Finally, regarding teachers' characteristics (as displayed in Table B.1), mathematics teachers are young (37 years on average) but with ample teaching experience (11 years) and are slightly male-dominated (60%). They tend to be employed full-time (80%) although only 50% have permanent contracts. Finally, they are well-qualified (98% completed tertiary studies and above 90% hold at least a bachelor's degree in education) and their caste composition in the sample is consistent with the distribution observed for the whole population.

Additionally to socio-demographic characteristics, it is relevant to provide descriptive information about the cognitive performance of the students. As mentioned above, the baseline scores have been normalized to have a mean of 500 and a standard deviation of 100 prior to sample selection. Figure A.1 shows the clear shift to the right in the distribution of mathematical performance across all students between the first and second rounds. Breaking down these distributions by caste in Figures A.2 and A.3 we observe that they follow the expected ordering based on caste "highness". Note, however, that there are also portions of clear overlapping (e.g. the worst-performing GC students do worse than the best-performing SC students). What is more, OBC and GC achieved the largest increments in mathematics (both absolute and relative), hence increasing the gap with the lower castes. In English the overall increase in scores is much smaller, and it is actually driven

[15] In my estimating sample there are 5 state government schools (public schools that do not impose academic fees), 9 tribal/social welfare schools, 9 private unaided schools (fully private schools that do not receive public funds and so charge relatively large fees to their students) and 4 private aided ones (private schools where fees are much smaller since they receive governmental funds to pay the teachers' salary).

Table 1: Descriptive Statistics: Students

Variable	Mean	Std. Dev.	Observations
Female	0.588	0.492	1,914
HH size (count)	5.187	1.889	1,911
Repeater	0.164	0.371	1,915
Enrolled before age 7	0.974	0.158	1,913
Mother education: no schooling	0.247	0.431	1,736
Mother education: primary	0.207	0.405	1,736
Mother education: upper primary	0.129	0.335	1,736
Mother education: high school	0.227	0.419	1,736
Mother education: junior college	0.101	0.301	1,736
Mother education: higher education	0.089	0.285	1,736
Father education: no schooling	0.164	0.370	1,701
Father education: primary	0.167	0.373	1,701
Father education: upper primary	0.096	0.294	1,701
Father education: high school	0.228	0.420	1,701
Father education: junior college	0.166	0.373	1,701
Father education: higher education	0.179	0.383	1,701
Wealth index (0-1 index)	0.568	0.219	1,921
SC	0.189	0.392	1,921
ST	0.238	0.426	1,921
OBC	0.372	0.484	1,921
GC	0.200	0.400	1,921

All variables are dummies unless specified otherwise by a small catch-up of the lower castes.

In terms of caste-mixing, the mean proportion of low-caste students across the 58 sections in my sample is 40%. 19 sections have students from all four caste groups and 35 have at least one GC and one low-caste student. Traditionally, lower interactions have existed between GC and ST students, the former due to their higher status and the latter due to their more frequent geographical isolation. In particular, 13 sections do not have any GC student and 23 classes any ST student. 4 and 5 sections feature only GC and ST students, respectively. These figures suggest that, although caste-mixing in educational settings may sometimes be limited, its presence is still frequent at the middle-school level.

3 Estimation Strategy

3.1 Basic Approach

The main empirical strategy draws from the widely used linear-in-means specification to estimate Equation 1:

$$y_{ics2} = \alpha + \beta_0 y_{ics1} + \beta_1 X_{ics1} + \beta_2 \overline{X}_{-ics1} + \beta_3 PLC_{-ics1} + \lambda_s + \epsilon_{ics2}, \tag{1}$$

where y refers to our outcome of interest (primarily test scores), ics denotes person i in class c in school s and 1 indicates baseline information (i.e. at the beginning of the school year, while 2 denotes at the end-of-year retake).[16]

PLC measures the proportion of low-caste students present in the class, excluding the person of reference. Our coefficient of interest is hence β_3, which captures the effect of a higher proportion of low-caste classmates on the gain in schooling outcomes over one academic year. It is therefore an "exogenous" effect, as it arises from background characteristics of the students, and not from their achievement (i.e. "endogenous effect"). The existing literature has focused on exogenous effects stemming from gender and race. I add to this literature by focusing on a crucial socio-economic element of India: the caste system.

As it is well-known, estimation of exogenous effects is threatened by several factors. First, direct comparisons of students across schools is likely to yield biased results due to selection into schools. If present, this would mean that unobserved determinants of a student's achievement would likely also be correlated with her classmates' average characteristics. A common example in this respect is parental interest for better schooling. Dealing with this issue requires the introduction of unit-level fixed effects at a higher level of aggregation than the one at which peer effects are measured. In my case, since I define peers at the classroom level, I include school fixed effects[17].

[16] Another general benefit of value-added models is that, in the absence of random assignment to peers, they can control for unobserved inputs such as ability or family interest for school that might otherwise bias the results (Pivovarova, 2013).

[17] Note that school fixed effects are crucial in accounting for households' sorting into geographical areas.

The addition of school fixed effects does not, however, account for potential sorting of students across sections. For instance, it could be the case that more involved parents lobby the school principal to place their children into certain classes so that they can benefit from better teachers or peers. The literature has often exploited plausibly exogenous changes in peers' background characteristics across cohorts within schools. This requires tracking schools over years and forces the researchers to define peers at a rather broad level: same-grade students. Fortunately, I have information on the way sections were formed. This will allow me to focus on the schools that exogenously assign students to classes, which not only overcomes this concern but also allows me to consider peers at a finer level (i.e. the classroom). This also means that identification is achieved from variation in the percentage of low-caste classmates within schools across classes and within classes across students.

The last identification concern, which is specific to my context, is that it may that the proportion of low-caste students is proxying for other characteristics that are correlated with low status. To tackle this I take advantage of the richness of my dataset to control for the most pressing concerns. In particular, I account for wealth differences (signaled as the most relevant confounder in studies on caste by Luke and Munshi, 2011) by controlling for a wealth index[18]. Additional covariates are, among others, parental education, health status and, importantly, academic aspirations of both parents and children. These inclusions should reduce concerns about the proportion of low-caste students potentially capturing background characteristics other than caste itself. This validity is further reinforced by the stability of the point estimates across specifications with different sets of controls.

To strengthen this argumentation I extend my analysis by allowing for endogenous peer effects, i.e. I include peers' average scores at baseline. If lagged performance is a strong predictor of current achievement (as I show to be the case below), it should account for hard-to-observe individual characteristics such as past investments or effort. One aspect to bear in mind, however, is that baseline achievement may be correlated across students if they were classmates in the previous academic year. Although I do not have information on the trajectory of the academic groupings, this should not be an issue under random classroom assignment (Pivovarova, 2013). In any case, I instrument peers' baseline achievement with newly-arrived peers' scores (I have information on the exact year when each student arrived to the school and hence I can identify the newcomers in the survey year) in the spirit of Imberman et al. (2012) and Pivovarova (2013). The relevance condition is prone to be satisfied since these new students are part of the peer group. The exclusion restriction is likely to hold without systematic (and unobservables-driven) placement of new students to

[18] I compute the wealth index as the proportion of affirmative responses to a series of wealth-related questions to the household, such as the ownership of a fridge or a car. This is similar in spirit to the consumer durables portion within the wealth index constructed by YL for their main longitudinal survey (Kumra, 2008).

sections.

Turning back to Equation 1, X is a vector of individual controls including gender, household size, an indicator of belonging to SC/ST, an indicator of being a repeater, another one for having enrolled at school prior to age 7[19], six categories of paternal education, and the wealth index. These are maintained controls across all regressions. $\overline{X}$ contains the leave-out-means of the same variables so as to control for peers' characteristics.

Importantly, computing these leave-out means requires precisely defining the set of peers within the classroom. In the absence of friendship nominations, the literature has defined all other classmates (or all other students in the same grade) as peers. In my main analysis I make use of the idiosyncrasy of the Indian caste peer groups in that castes are hereditary, well-defined, and rule almost all aspects of social interactions, particularly in rural areas, to better identify peers as same-caste classmates.[20] One additional benefit is that the percentage of low-caste peers is pre-determined, so its corresponding slope coefficient should not be biased because of common unobserved shocks (Guryan et al., 2009). Defining all classmates as peers is left as a robustness check.

The stability of the results will also be verified by introducing additional covariates (both for the individual and the peers - i.e. in X and in $\overline{X}$) such as child's interest in reading, academic aspirations, health, and parental interest in the child's academic performance. School fixed effects, represented by λ_s, are featured to account for selection into schools. ϵ is the error term. I cluster the errors at the class level in order to account for possible correlation of the outcomes among classmates and provide wild-bootstrapped p-values (Davidson and Flachaire, 2008) for selected estimates.

The main specification is appealing in the sense that it provides an average effect of the role of classmates caste composition across all castes as well as for allowing for the separate identification of β_3 and the school fixed effects. Having said this, it is relevant to gain more detailed insights into potential heterogeneous effects based on caste. For this I estimate:

$$\begin{aligned} y_{ics2} = \alpha + \alpha_0 y_{ics1} + \alpha_1 X_{ics1} + \alpha_2 \overline{X}_{-ics1} + \gamma PLC_{-ics1} + \\ + \sum_{k=1}^{K} \theta_k Caste_{icsk} + \sum_{k=1}^{K} \beta_k PLC_{-ics1} * Caste_{icsk} + \lambda_s + \epsilon_{ics2}, \end{aligned} \tag{2}$$

where everything is as in Equation 1 except that now we have the additional interaction between belonging to a low caste and the proportion of low-caste peers. In fact, Equation 2 accounts for

[19]The usual enrollment age in primary education is 6 years. I allow for up to one year of delay when constructing this variable so that any first enrollment after age 7 is considered to be a delayed one.

[20]In other words, in my preferred specification $\overline{X}$ is computed as the leave-out mean among same-caste students in the class. In robustness exercises they are computed as the leave-out means among all classmates. PLC_{ics} is always computed as the leave-out mean of low-caste students in the class.

the more general case in which, instead of separating castes into high and low ones, all four classes are considered separately: i.e. $k \in \{SC, ST, OBC, GC\}$ and Caste are indicator variables for the student belonging to each of the four categories.

3.2 Validating the Identification Assumption: Balance Checks

Prior to turning to our main analysis, it is important to verify the presence of exogenous assignment. We perform balance checks of students' class assignment by regressing a large set of individual characteristics on the proportion of low-caste classmates (excluding the own observation). Table 2 demonstrates that, conditional on school fixed effects, the proportion of low-caste peers is plausibly as good as randomly assigned, as there are no significant statistical differences across a large number of observable characteristics featuring individual ones (age, gender, repeater and dropout status, academic aspirations and interest in reading), family background (wealth, parental education, number of books at home, household size), selection into the school (no difference in the main reason for attending the specific school being proximity to home, absence of academic fees nor teaching quality), and initial mathematics score[21]. Finally, there are no systematic differences in terms of individual caste (point estimate of 0.000, p-value of 0.879; unreported in the table). For this last case I include as additional control the proportion of low-caste students at the grade level (i.e. across all sections in Grade 9, excluding the own observation) since sampling without replacement will bias my estimates downward (Guryan et al., 2009).

Most existing peer effects papers do not take into account the role of teachers in mediating the school gains due to lack of data on their characteristics and teaching practices. However, even if one controls for selection into schools and benefits from exogenous assignment of students to classes within schools, it may still be the case that the headmasters assign certain teachers to classes with a specific composition of students within a grade, or across grades. For example, schools might try to place a female teacher in female-dominated groups, or to assign the most energetic teachers to Grade 9 if they believe that Grade 9 students need this type of teachers the most. For this reason, I also explore whether teachers' observable characteristics are correlated with their assigned students' observed characteristics. In particular, I exploit the fact that the schools in my estimating sample have at least two sections to follow Antecol et al. (2014) in estimating:

$$y_{cs1} = \alpha + \beta_1 \overline{X}_{cs1} + \lambda_s + \epsilon_{ics1}, \tag{3}$$

where y_{cs1} stands for observable teacher characteristics at baseline and $\overline{X}_{cs1}$ contains average characteristics of the students in the class (also at baseline). If there is indeed no systematic

[21] Table B.2 follows Kang (2007) in checking whether average peer characteristics are correlated with individual ones, which would be a sign of a potential lack of exogenous assignment of students to peers. I do so for peers' baseline score in mathematics. No individual characteristic is significantly correlated with it and the null in a joint F-test is not rejected with a p-value of 0.350.

Table 2: Class Composition Balance Checks: Proportion of Low-Caste Students and Individual Characteristics

Panel A

VARIABLES	(1) Age	(2) Female	(3) Ever Repeater	(4) Enrolled Before 7	(5) Ever Dropped Out	(6) Child's Health	(7) Wealth Index	(8) Books at Home	(9) Household Size
% SC and ST kids in class excluding student i	0.001	-0.005	-0.001	-0.0004	-0.0004	-0.0005	0.001	0.004	0.010
	[0.901]	[0.219]	[0.597]	[0.522]	[0.799]	[0.948]	[0.302]	[0.637]	[0.151]
Observations	1,914	1,914	1,915	1,913	1,914	1,921	1,921	1,912	1,911
R-squared	0.132	0.481	0.115	0.055	0.053	0.564	0.472	0.089	0.106

Panel B

	(1) Paternal Education	(2) Maternal Education	(3) Parental Interest School	(4) Reason Attending: School Near Home	(5) Reason Attending: No Fees	(6) Reason Attending: Good Teaching	(7) Child's Reading Interest	(8) Child's Academic Aspirations	(9) Math Score Round 1
% SC and ST kids in class excluding student i	0.001	0.000	-0.009	-0.000	0.000	-0.003	0.004	-0.009	-0.884
	[0.805]	[0.984]	[0.250]	[0.692]	[0.532]	[0.498]	[0.388]	[0.120]	[0.122]
Observations	1,701	1,736	1,914	1,914	1,914	1,914	1,907	1,914	1,804
R-squared	0.137	0.165	0.498	0.129	0.298	0.330	0.101	0.109	0.475

Regressions of individual characteristics on the proportion of low-caste classmates, excluding oneself. All regressions include school fixed effects with standard errors clustered at the class level. Ordered probits are estimated when the dependent variable is parental education, number of books at home, and academic aspirations. Wild-bootstrapped p-values in brackets. *** p<0.01, ** p<0.05, * p<0.1

sorting of teachers into sections, we should not find significant correlations between the class' characteristics and those of the teachers.

Table B.3 in the appendix shows that class composition, once school fixed effects are controlled for, is not systematically related to a large variety of mathematics teachers' characteristics including age (column 1), years of experience at the current school (2), gender (3), total years of experience as a teacher in their professional career (4), an indicator of being a full-time employee (5), academic qualification (6), and an indicator for enjoying a permanent job contract (7). The very few cases where they do show some statistical significance it is marginally at 10% and in patterns that would not actually suggest the presence of an strategic selection of teachers. F-tests of joint significance of all the explanatory variables are not able to reject the null that all of them have zero effect on teacher's characteristics.

4 Main Analysis

4.1 Overview

In this section I present the main results for the role of caste composition in academic and non-cognitive development. Their credibility is reinforced by the exogenous assignment of students and teachers to classes just demonstrated and by the inclusion of an extensive set of covariates that controls for alternative peers' characteristics that could be driving the results, such as wealth, cognition and/or school interest and academic aspirations.

An initial exploratory look at the main correlates of academic performance is provided in Table B.4. As expected, there is large persistence in scores across rounds. While individuals with more educated fathers perform better, repeaters and children with more precarious health do worse. Importantly, low-caste individuals obtain, on average, one fifth of a standard deviation lower score in mathematics than their non-low-caste counterparts, while the difference in English is much smaller and not statistically significant.

4.2 The Role of Peers in the Evolution of Mathematics Scores

In this section I build upon the previous results to estimate value-added models in which I introduce peers' caste composition and peers' characteristics into the regressions (i.e. I estimate Equations 1 and 2). Column 1 in Table 3 reports the results only controlling for baseline mathematics score and school fixed effects. There is a negative and very significant correlation between the proportion of low-caste students in the section (excluding the own observation) and round 2's mathematics score, even after accounting for initial conditions (round 1's score). In subsequent specifications we will verify the robustness of this result to the inclusion of controls that account for potential confounding sources of this correlation.

Column 2 controls for our maintained set of individual characteristics (gender, household size, indicators for low-caste, repeater status and being enrolled before 7, parental categorical education, wealth index, and child's interest in reading). The point estimate remains remarkably similar, as would be consistent with an exogenous assignment of students to classes. In column 3 I introduce the following peers' characteristics: gender, paternal education, enrollment prior to age 7, household size, repeater status and wealth index. Wealth index has been signaled as the crucial characteristic to control for when studying the effects of caste (Munshi, 2019). Results do not change.

In column 4 I exploit the richness of my data by including non-cognitive attributes of peers: their parents' interest in their child's academics. This is important as it might be that low-caste students systematically have parents with less interest in their performance so that the effects that we find are driven by this and not by caste itself. However, this possibility is not supported by the data. Column 5 additionally controls for peers' baseline mathematics score (this inclusion will be further discussed in Table 4). The point estimate for our coefficient of interest does not change significantly, indicating that the results do not arise because higher proportions of low-caste students also bring worse academic abilities that eventually generate the smaller increase in cognition over the academic year. Finally, column 1 in Panel B further controls for peers' reading interest with no noticeable change. Overall, the consistency in the size of the point estimate to the inclusion of such an extensive set of controls suggests that our initial set of maintained individual controls is able to render the conditional exogeneity assumption of the proportion of low-caste students in class credible.

In terms of economic magnitude, a one percentage point increase in the percentage of low-caste classmates leads to a decrease in the one-year academic gains of about 1.6% of a standard deviation. The economic size of this estimate is consistent with those found in the literature. For example, a meta-analysis for developing countries found that a 10 percentage-point increase in the proportion of minority students decreased the classes' performance in 1.8% of a standard deviation (Van Ewijk and Sleegers, 2010)[22]. The reasons why my findings are in the upper-end (but very similar in size to Gong et al. (2019) who exploit a similar empirical strategy for China) probably relate to contextual differences i.e. addressing rural areas within a developing country where social norms are particularly strong, defining peers in a finer manner than the usual grade-level one (where effects are most likely diluted), estimating different econometric models (I look at value added), and having access to exogenous assignment of peers.

Columns 2 and 3 in Panel B present results from estimating Equation 3 when aggregating SC and ST into a "low caste" indicator and when treating each group separately, respectively. This allows us to see that high-caste students obtain on average higher second-round scores even after controlling for baseline scores (negative and significant estimate for "SC or ST Caste" in column 2 and negative and significant SC and ST estimates in 3, where the omitted category is GC) and that the uninteracted proportion of low-caste students remains significantly negative. Moreover, a higher proportion of low-caste classmates is less detrimental for lower-caste students (positive interaction in column 2), which is driven by SC (column 3). In the next section we will explore potential mechanisms behind these findings.

In Column 4 I provide the results for English for completeness. As mentioned, the importance of caste is much more limited both at the individual level and in terms of peers composition. All the analyses presented in the text for mathematics are replicated for English and available upon request.

4.3 Instrumental Variable Results

The analysis in Table 3 has focused on schools with multiple sections and exogenous assignment of students and teachers to classrooms. This choice maximizes the potential to obtain an unbiased estimate of our causal effect of interest but discards a fraction of the available observations. In order to increase the representativeness of the sample, in Table 4 I work with all multiple-section schools: 83 schools and 189 sections[23]. Since for this new sample the added observations may have

[22] No specific analysis for the Indian case was available to the authors.

[23] Given the larger number of clusters I do not report wild-bootstrapped standard errors.

been allocated based on previous academic achievement, there is potentially sorting into classrooms within schools. One way of dealing with this is controlling for peers' baseline scores. I do this in columns 1 and 2, which are the counterparts of columns 3 and 5 in Table 3's Panel A. The previous qualitative results hold but the point estimate falls in absolute value. This finding suggests that, if anything, in lack of exogenous assignment of students to sections there is an upwards bias in the estimate of my main coefficient, which runs against the idea that the negative effects in my preferred specification are driven by unobserved characteristics correlated with low-caste status.

However, as explained above, it is still possible that peers' baseline achievement is correlated with some determinants of current individual performance if students were already peers in the previous academic year. I therefore instrument peers' baseline cognition with the incoming students' average academic ability. The first stage is significant at the 1% level and yields a Kleibergen-Paap F-statistic for weak instruments of 58.[24] The unbiased estimate of the percentage of low-caste students in column 4 remains significant and increases in (absolute) economic magnitude with respect to the first two columns to -1.1, as expected. This local average treatment effect suggests that caste status indeed has its own-effect beyond the one arising from its usual correlates and that the results found for the restricted sample seem to have a wider external validity. For completeness, column 3 reproduces the analysis in column 2 for the same subsample as the one used in column 4 (which has a smaller sample size due to the absence of incoming students in some sections.)

5 Mechanisms

In this section I explore potential channels through which a higher proportion of low-caste classmates could hinder the academic progression of the class. The richness of the data on non-cognitive and behavioral responses of both students and teachers (featuring information on class atmosphere, students' behaviors and non-cognition, and teachers' practices and perceptions) sets this dataset apart by allowing for an ample evaluation of different mechanisms. This can speak to the role of teacher practices as inputs in the production function of cognition (e.g. Jackson et al., 2014) and to the importance of students' beliefs about their teachers' views of their abilities and life/academic prospects and about the class environment - which are relevant factors determining schooling choices even in developing countries[25].

[24]The exclusion restriction requires new students' baseline achievement to only affect the second round achievement of their peers through the overall peer quality. In Table B.5 in the Appendix I show that those individual characteristics of newcomers that were observable to the headmasters prior to their arrival to the school (such as gender, parental education and wealth) are not systematically related with the characteristics of the peers to which they are assigned upon arrival.

[25]46% of the students in the survey reported that the most important reason why they chose their school was because of good teacher quality and school care, which includes a positive school atmosphere.

Table 3: Caste Peer Effects in Academic Outcomes: Restricted Sample

Panel A

	(1)	(2)	(3)	(4)	(5)
VARIABLES			Math Score Round 2		
Math Score, Round 1	0.526***	0.511***	0.507***	0.507***	0.506***
	(0.041)	(0.040)	(0.041)	(0.041)	(0.041)
Percentage SC and ST kids in class	-1.717***	-1.751***	-1.656**	-1.698**	-1.633**
	(0.430)	(0.464)	(0.499)	(0.511)	(0.542)
	[0.001]	[0.005]	[0.045]	[0.034]	[0.048]
Maintained Controls	No	Yes	Yes	Yes	Yes
Main Peers' Characteristics	No	No	Yes	Yes	Yes
Peers' Non-cognition	No	No	No	Yes	Yes
Peers' Baseline Cognition	No	No	No	No	Yes
Observations	1,556	1,382	1,359	1,358	1,358
R-squared	0.658	0.670	0.675	0.676	0.676

Panel B

	(1)	(2)	(3)	(4)
VARIABLES	Math Score Round 2	Math Score Round 2	Math Score Round 2	English Score Round 2
Math Score, Round 1	0.506***	0.503***	0.506***	
	(0.041)	(0.041)	(0.040)	
English Score, Round 1				0.431***
				(0.037)
Scheduled Caste			-51.285***	
			(12.774)	
Scheduled Tribe			-49.063**	
			(25.032)	
Other Backward Class			-15.924	
			(10.355)	
Percentage SC and ST kids in class	-1.552*	-1.741**	-2.061**	0.025
	(0.555)	(0.532)	(0.503)	(0.156)
	[0.081]	[0.046]	[0.014]	[0.901]
SC or ST Caste	-16.697**	-39.356***		-4.547
	(7.629)	(10.407)		(3.900)
Low Caste*Perc. Low Caste Classmates		0.565*		
		(0.247)		
		[0.052]		
SC*Perc. Low Caste Classmates			0.870**	
			(0.308)	
			[0.023]	
ST*Perc. Low Caste Classmates			-0.409	
			(0.327)	
			[0.243]	
OBC*Perc. Low Caste Classmates			0.319	
			(0.320)	
			[0.327]	
Observations	1,358	1,358	1,358	1,408
R-squared	0.676	0.677	0.679	0.768

All regressions are run on the multiple section with exogenous assignment sample and include school fixed effects with standard errors clustered at the class level. 54 classrooms (our clustering unit) are used in the estimation. Column 1 in Panel A only controls for baseline mathematics score and the proportion of students of a different caste with respect to the student's one. For the remaining regressions, the maintained controls are: gender, household size, indicators for low-caste, repeater status and being enrolled before 7, parental categorical education, wealth index, and child's interest in reading. Additional controls are added across columns as indicated in the text. Wild-bootstrapped p-values in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$ (based on wild-bootstrapped p-values only for the signaled variables)

5.1 Description and Presence of Potential Mechanisms

Due to the demanding data requirements, few papers have tried to uncover the mechanisms at work. Lavy and Schlosser (2011) was the first one, and considered eight different categories pertaining to the classroom and school environments (e.g. "transparency, fairness, and feedback", or "instilling of capacity for individual study",) while more recently Gong et al. (2018) distinguishes between teacher behavior, class behavior, and student behavior. I instead separate the possible mechanisms according to whether they involve a change in the endogenous decisions of the agents or not, since this will have different implications for how to best address the existing peer effects in practice. In particular, I distinguish between:

1. **Purely Compositional Effects:** It may be that different castes posses different characteristics that, by themselves, will not affect optimal choices of other individuals (i.e. effort) but that correlate with lower performance. For example, low-caste students may be less-wealthy or may misbehave more frequently. Hence, in the latter case, a higher proportion of low-caste students may not influence the individual behavior of other students (if bad behavior is not imitated by other students), but instead simply mechanically increase the number of disruptive peers, which results in the slower progression in classes populated by more low-caste students.

2. **Endogenous Effects:** Class composition may instead directly affect the decisions taken by either/both, teachers and students:

   - Teachers' Practices Adaptation: Class composition may lead teachers to re-optimize how they use inputs or change the way that they perceive their students.
   - Students' Endogenous Effects: The traditional view in the peer effects literature is that effort decisions of the students may be affected. Apart from this, I also consider whether other endogenous outcomes such as interest in schooling or academic aspirations might be affected.

Table 5 starts by exploring whether students' perceptions about the importance of schooling and the extent to which their mathematics teachers have high expectations for them and facilitate their learning are affected by class composition. In all student-level regressions in this section I control for gender, household size, low-caste, having enrolled prior to age 7 and repeater indicators, paternal categorical education, reading interest, and wealth index of both the individual and his/her peers. Across the board there are negative point estimates for the percentage of low-caste classmates for the child's drive to work hard to please the teacher (column 1), for the child's perception about whether the teacher has high expectations for him/herself (column 2) and encourages learning

Table 4: Caste Peer Effects in Academic Outcomes: All Multiple-sections Sample

	(1)	(2)	(3)	(4)
VARIABLES	Math Score Round 2			
Math Score, Round 1	0.505***	0.506***	0.507***	0.530***
	(0.023)	(0.023)	(0.028)	(0.031)
Percentage SC and ST kids in class	-0.752***	-0.746***	-0.964***	-1.096***
	(0.215)	(0.233)	(0.303)	(0.307)
Peers' Math score, Round 1	1.550**	1.370*	1.756	-0.916
	(0.741)	(0.707)	(1.068)	(1.604)
All Multiple-section Schools	Yes	Yes	No	No
IV Sample	No	No	Yes	Yes
IV Estimation	No	No	No	Yes
Observations	4,391	4,367	2,808	2,808
R-squared	0.594	0.596	0.596	0.594

Columns 1 and 2 use all schools with multiple sections, irrespective of the student assignment procedure followed. Column 2 controls additionally for peers' baseline cognitive and non-cognitive characteristics. Columns 3 and 4 are estimated on the subset of multiple-section schools where new students arrive to multiple sections during the academic year in which the survey was collected. All regressions include school fixed effects with standard errors clustered at the class level. The maintained controls are: gender, household size, indicators for low-caste, repeater status and being enrolled before 7, parental categorical education, wealth index, and child's interest in reading. Columns 2-4 additionally control for peers': proportion of females, paternal education, enrollment age, parental interest in schooling, household size, wealth index, interest in reading, proportion of repeaters and health status. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

through being nice with students' questions (column 3), cares that students understand (column 4) and about what students say (column 1 in Panel B) and congratulates them when deserving it (column 2). Given the large amount of outcomes explored, I standardize the outcomes to have a mean of zero and a standard deviation of one and obtain their simple average for each student. The result of using this measure as my outcome variable is reported in Column 3 in Panel B. It confirms the finding that having a higher percentage of low-caste classmates is strongly negatively correlated with having positive views towards teachers degree of care towards them and their learning.

The above discussion on the students' perceptions towards the teachers' roles and actions are important because students are likely to act upon their views, regardless of whether they truly reflect reality or not. I investigate whether this is the case in Table 6, where I explore changes in students' interest towards the subject of mathematics, in the first two principal components (PCA) eliciting how hard the student studies for the subject, and in measures of the degree to which they believe that hard work should be devoted to improve as a person, whether hard work will pay off in the future, and how this translates into academic aspirations (years of schooling they wish to attain). Again, negative signs for low-caste composition emerge across all these dimensions and are condensed in the z-score provided in Panel B's column 3.

The above results consistently suggest that students sharing the classroom with a higher proportion of low-caste students tend to (negatively) change their views about the extent that mathematics teachers care about them and their progress (recall that there were no baseline differences in non-cognitive measures in Table 2) and that this leads them to lower their interest in schooling, their efforts and their aspirations. This is consistent with abundant qualitative evidence on low-caste students regretting the lack of attention on the part of their teachers and their putting down of their abilities and interests towards education (e.g. Thorat et al., 2010).

However, such changes in teachers' behaviors may simply reflect students' perceptions and not be actual ones. In Table 7 I explore this possibility by looking at whether class composition can affect teacher's happiness with her life (which is likely to affect her attitudes in class and teaching quality), whether she is more likely to be above the median of my sample in minutes per day devoted to preparing classes, whether she spends less time meeting and discussing with parents or dealing with misbehavior. For this I regress such outcomes on the mean characteristics of the students in the section. There are no significant differences across classes with different proportions of low-caste students. This may suggest that students' perceptions about their teachers attitudes do not fully correspond with reality, although these findings may also arise from teachers not reporting accurately their own behavior under social desirability concerns. Note, however, that lack of power arising from the relatively small number of available sections will limit our potential to uncover conclusive results

Table 5: Channels I: Students' Perceptions

Panel A

	(1)	(2)	(3)	(4)
VARIABLES	Child Works Hard To Please Teacher	Teacher Has High Expectations	Teacher is Nice with Questions	Teacher Cares Students Understand
Percentage Low Caste	-0.012	-0.003	-0.017**	-0.023**
	(0.006)	(0.003)	(0.005)	(0.006)
	[0.217]	[0.591]	[0.022]	[0.029]
Observations	1,325	1,349	1,346	1,332
R-squared	0.194	0.090	0.120	0.108

Panel B

	(1)	(2)	(3)
VARIABLES	Teacher Cares About What Students Say	Teacher Congratulates Students	z-Score
Percentage Low Caste	0.001	-0.019**	-0.009**
	(0.004)	(0.005)	(0.003)
	[0.843]	[0.025]	[0.028]
Observations	1,340	1,349	1,359
R-squared	0.133	0.120	0.154

All regressions include school fixed effects with standard errors clustered at the class level. All outcome variables are individually reported by the students regarding their own behavior (whether they choose to work hard to please their teacher in Panel A's column 1) and their perceptions of their teachers behaviors and attitudes (columns 2-4 in Panel A and 1-2 in Panel B). The maintained controls are: gender, household size, indicators for low-caste, repeater status and being enrolled before 7, parental categorical education, wealth index, baseline mathematics score, child's interest in reading (and the peers' values for these variables). Wild-bootstrapped p-values in brackets. *** p<0.01, ** p<0.05, * p<0.1 (based on wild-bootstrapped p-values only for the signaled variables)

Table 6: Channels II: Students' Behavioral Responses

Panel A				
	(1)	(2)	(3)	(4)
	Child's Interest in Mathematics	Child Works as Hard as Possible (PCA 1)	Child Works as Hard as Possible (PCA 2)	Child Works Hard to Improve as a Person
Percentage Low Caste	-0.029**	-0.012	-0.010	-0.016*
	(0.009)	(0.009)	(0.006)	(0.007)
	[0.034]	[0.420]	[0.226]	[0.094]
Observations	1,284	1,262	1,262	1,214
R-squared	0.141	0.128	0.223	0.164

Panel B			
	(1)	(2)	(3)
VARIABLES	Child Works Hard for her Future	Academic Aspirations	z-Score
Percentage Low Caste	-0.015*	-0.009	-0.008**
	(0.006)	(0.007)	(0.002)
	[0.091]	[0.417]	[0.013]
Observations	1,304	1,575	1,575
R-squared	0.153	0.263	0.203

All regressions include school fixed effects with standard errors clustered at the class level. All outcome variables are individually reported by the students regarding their own behavior. Columns 2 and 3 include the first two components from a principal component analysis. The maintained controls are: gender, household size, indicators for low-caste, repeater status and being enrolled before 7, parental categorical education, wealth index, baseline mathematics score, child's interest in reading (and the peers' values for these variables). Wild-bootstrapped p-values in brackets. *** p<0.01, ** p<0.05, * p<0.1 (based on wild-bootstrapped p-values only for the signaled variables)

Table 7: Channels III: Mathematics Teachers' Behavioral Responses

VARIABLES	(1) Teacher is Happy with her Life	(2) Above Median Time for Class Preparation	(3) Total Time Devoted Discussing with Parents	(4) Total Time Dealing with Student Misbehavior
Percentage Low Caste	-0.017	-0.006	0.053	-0.122
	(0.027)	(0.006)	(0.037)	(0.093)
	[0.603]	[0.283]	[0.111]	[0.130]
(mean) Female	0.883*	-0.017	-0.748	-7.004***
	(0.525)	(0.123)	(0.699)	(1.924)
(mean) HH Size	-0.494**	0.104	-0.165	2.356**
	(0.241)	(0.076)	(0.406)	(0.935)
(mean) Repeater	-0.282	0.142	-0.466	1.058
	(1.076)	(0.264)	(2.289)	(5.076)
(mean) Maternal Education	-0.441	-0.357*	2.608*	0.616
	(0.453)	(0.194)	(1.397)	(3.393)
(mean) Paternal Education	-0.215	0.307**	-2.074	-1.302
	(0.776)	(0.135)	(1.576)	(2.132)
(mean) Wealth Index	1.706	-1.000	1.968	4.494
	(2.045)	(0.839)	(7.883)	(10.265)
Observations	57	58	57	58
R-squared	0.847	0.879	0.927	0.969

All regressions include school fixed effects with standard errors clustered at the class level. All outcome variables are mathematics teachers' self-reported behaviors and situations. Explanatory variables are the average characteristics of the students in the section that they teach. Wild-bootstrapped p-values in brackets. *** p<0.01, ** p<0.05, * p<0.1 (based on wild-bootstrapped p-values only for the signaled variables)

We move on now to further accounting for the role of teachers. We have shown that exogenous allocations of teachers to classes also seems to be present. Hence, if endogenous effects on teachers' behaviors were absent, the inclusion of teacher characteristics should not change our results. I explore this avenue in Table 8 column 1. The additional teacher controls are gender, years of experience, highest education and caste. If our main results are driven by endogenous effects on the side of the teacher (partly discarded from our discussion of Table 7) one would expect that they are stronger for teachers who are more prone to think that a students' background is an important determinant of her academic progression. Columns 2 (and 3) include an indicator signaling that the mathematics teacher agrees or strongly agrees to the statement that a student's background (gender) affects her learning of mathematics. The addition of these controls does not change the results.

Finally, purely compositional effects have been addressed in Section 4 through the addition of a rich set of individual and peer characteristics that significantly reduce the potential for unobserved characteristics of low-caste students to be driving the main results. Particularly noteworthy was the inclusion of peers' parental care for schooling, which accounts for the idea that parents who care more for school may have children with different characteristics along these lines and/or to have spillovers on school workers and students' behaviors. None of these alternative explanations seemed to be supported by the data. In unreported results I do not observe differential changes in school absences and disruptions across varying class compositions. Overall, my main results do not seem to be driven by purely compositional effects beyond caste itself.

5.2 Mechanisms' (and Caste Correlates') Relative Importance

The above section has pointed at the presence of a behavioral channel. Moreover, as discussed before, low-caste status is correlated with a series of socio-demographic characteristics. Although I have argued that caste has a separate effect above and beyond those from its correlated characteristics, it is of interest to quantify the extent to which each of the channels and correlated characteristics can account for our main effects of interest.

Gelbach (2016) provides a decomposition of the relative importance of each covariate that is independent of the order in which they are added. His approach, inspired by the standard formula defining the population omitted variable bias arising from excluding a relevant set of controls in an OLS regression, consists of two steps. In the first one I separately regress the two main mechanisms found above (the two different z-scores for changes in students' behavior and in students' perceptions used in column 3 of Panel B in Tables 5 and 6, respectively) on the leave-out mean of the proportion of low-caste students in the class, a gender indicator, the baseline mathematics score and an indicator of low caste. I proceed similarly using also as outcomes

Table 8: Channels IV: Teachers' Perceptions

	(1)	(2)	(3)
VARIABLES		Math Score Round 2	
Math Score, Round 1	0.490***	0.491***	0.494***
	(0.044)	(0.044)	(0.045)
Percentage Low Caste	-1.697*	-1.730*	-1.704*
	(0.603)	(0.635)	(0.608)
	[0.089]	[0.075]	[0.069]
Math Teacher Believes Background Affects Learning		-12.182	
		(13.440)	
Math Teacher Believes Gender Affects Learning			9.203
			(6.043)
Observations	1,202	1,185	1,185
R-squared	0.667	0.668	0.668

All regressions include school fixed effects. The maintained student controls are: gender, household size, indicators for low-caste, repeater status and being enrolled before 7, parental categorical education, wealth index, baseline mathematics score, child's interest in reading (and the peers' values for these variables). The maintained mathematics teacher controls are: gender, years of experience, highest education and caste. Column 2 additionally controls for whether the teacher agrees or strongly agrees with the statement that a student's background affects her learning in mathematics. Column 3 proceeds similarly regarding the importance of gender. Standard errors clustered at the class level. *** p<0.01, ** p<0.05, * p<0.1 (based on wild-bootstrapped p-values only for the signaled variables) potential correlates of low-caste status: household size, repeater status, enrollment prior to age 7, paternal education, wealth index and the student's interest in reading. I denote all these right hand side variables as "p". This yields, for each of these outcomes variables "j", an estimate of how the proportion of low-caste students shifts each of these variables (β_j).

In particular, the above regressions are as follows:

$$p^j_{ics1} = \beta_0 + \beta_1 y_{ics1} + \beta_2 X_{ics1} + \beta_j PLC_{-ics1} + \lambda_s + \epsilon_{ics1}, \tag{4}$$

where y is the math score at baseline and X_{ics1} includes gender and low caste indicators.

In the second step I run my main specification including all the above outcome variables as controls. This will yield estimates for the separate effect of each of them on second-round scores: α_j (and one for the proportion of low-caste peers, α_M).

$$y_{ics2} = \alpha_0 + \alpha_1 y_{ics1} + \alpha_2 X_{ics1} + \alpha_M PLC_{-ics1} + \sum_J \alpha^j p^j_{ics1} + \lambda_s + \epsilon_{ics2}. \tag{5}$$

One can then quantify the relative importance (RI) by applying, separately for each variable j, the following ratio: $\text{RI}_j = \frac{\alpha^j \beta_j}{\alpha_M + \sum_J \alpha^j \beta_j}$. The denominator takes into account both the direct and indirect effects of the proportion of low-caste on second-round achievement while the numerator measures the size contributed by each of the indirect pathways considered.

The results show that all these variables can jointly explain 11% of the total effect of the proportion of low-caste classmates. While the channel of students' behavior is the main driver of the effect (3.3 percentage points - over a third of the joint magnitude), the change in students perceptions also accounts for a large fraction (1.1 percentage points). Among the correlates of low-caste status, being a repeater plays an important role (2 p.p.) while parental education and reading interest contribute 1 p.p. each. The remaining variables considered account for less than half of a percentage point. Hence, both the large relative importance of the behavioral channel and the relatively little importance of the correlates of low-caste status are in line with the highlighted mechanisms uncovered in Section 5.1 and with the argument that caste status has its own separate effect beyond the one arising from its correlates, respectively.

6 Heterogeneity and Non-Linearities

Heterogeneous effects might be present both in terms of the role of classmates caste composition and also on the role of teachers as an input in the production of cognition. As part of my main analysis I have already explored heterogeneous effects by a student's individual caste. One obvious extension would be to see if females are differently affected. This is particularly relevant given the large and persistent gender gap in education in India (Muralidharan and Sheth, 2016) and the differences in non-cognitive responses across genders to being exposed to different conditions (e.g. Gong et al., 2019). In Table B.6 I explore whether this is the case by looking at heterogenous effects by gender (column 1) and by baseline cognitive ability - measured as a categorical variable after splitting the baseline mathematics score into quintiles (column 2). No differential effect across the students' gender or initial quality is found.

In column 3 I consider potential non-linearities by (somewhat arbitrarily) defining a categorical variable as follows[26]: Value 0 if low-caste make up 0%; Value 1 if low-caste make up 0-40%; Value 2 if 40-80%; Value 3 if 80-100%. As expected given our results for the continuous measure of the proportion of low-caste classmates, compared to the omitted category (no low-caste classmates), having higher proportions of low-caste classmates is strongly negatively correlated with second-round performance.

7 Robustness Checks

In this section I verify the robustness of my main results to different checks. My findings hold when modifying the definition of exogenous assignment of students to classes and when using randomly selected subsets of schools. Moreover, a placebo analysis that randomly assigns the percentage of low-caste classmates lends support to the conditional exogeneity assumption. Finally, I show that attrition between the first and second round did not affect differently classes with varying proportions of low-caste students.

7.1 Different Estimating Samples

In order to explore the sensitivity of my results to the sample restriction choices implemented I have already shown the consistency in the findings when making use of every multiple-section school in my sample, irrespective of its allocative mechanism of students to classrooms. This also holds under the proposed instrumental variable estimation. Additionally, I follow Gong et al. (2019) in re-running the main analyses only for randomly selected subsets of the schools in the estimating sample (I drop two of them each time and replicate this exercise 351 times - C^2_{27}). The (unreported) distribution of point estimates is centered around the ones in Table 3, which suggests that the main estimating sample is indeed composed of sections with exogenous selection.

7.2 Placebo Test

In order to verify that our main independent variable is not capturing the effects of some omitted variable, I conduct a placebo-like test that performs the thought experiment of assigning a contin-

[26]The qualitative results hold for alternative choices e.g. Alternative 1: Value 0 if 0-5%; Value 1 if 5-60%; Value 2 if 60-100%. Alternative 2: Value 0 if 0-5%; Value 1 if 5-40%; Value 2 if 40-80%; Value 3 if 80-100%.

uous treatment to every student, which is unusual in that placebo tests are typically implemented under binary treatment. This exercise has the additional benefit of delivering an empirical p-value for my main coefficient of interest without imposing asymptotic normality in its estimation.

I proceed as follows. I first compute the size of every section within a school's Grade 9 and the total number of low-caste students in them. I define the highest possible proportion of low-caste students in a class as the ratio between the total number of low-caste students across all sections and the size of the smallest section observed in the school (if the proportion is greater than 1, I cap it at 1). I then randomly draw a number from a uniform (0,1) for each school. I multiply this number times the highest possible proportion of low-caste students computed before. This will be my counterfactual proportion of low-caste students in the largest section in the school. Multiplying this proportion times the actual size of the largest section (and rounding to the nearest natural number) determines the total number of low-caste individuals in the counterfactual largest section. I then turn to the second largest section in the school and operate similarly with the only additional constraint that the number of low-caste students to distribute is the difference between the original total number of low-caste students and those that have been randomly (and counterfactually) allocated to the largest section. I replicate this exercise 1,000 times and find negative and statistically significant results for the parameter attached to the counterfactual proportion of low-caste students in around 7% of the iterations. As mentioned above, this figure can be understood as the p-value of the coefficient attached to the proportion of low-caste classmates without imposing asymptotic normality. This result reinforces our confidence that our main coefficient of interest is indeed conditionally exogenous.

7.3 Attrition

To check for non-random attrition within my estimating sample related to the percentage of low-caste students in the class, I estimate linear probability models where my dependent variable takes value 1 if the student did not take the mathematics exam in round 2 and zero otherwise. Table B.7 shows that the percentage of low-caste students does not correlate with the attrition of the students in the second round, regardless of whether we include controls or not (columns 2 and 3, respectively). In fact, the inclusion of key possible determinants of attrition such as wealth or being a repeater do not significantly correlate with attrition either nor changes the point estimate from column 2.

8 Conclusion

The pervasiveness of the caste system in the Indian society makes understanding cross-caste interactions of crucial importance. Those arising in educational settings might arguably be the most critical ones, as they will be pivotal in a wide range of late-life outcomes of the children. Surprisingly, there is no existing research on relative academic evolution across castes over a well-defined period of time nor on peer effects arising from class caste composition, which is fundamental for optimal grouping policies and for complementing the existing research on the widely-implemented affirmative action programs.

Making use of a unique dataset that provides rich information on class composition, individual characteristics of every student and their teachers, academic evolution over a year, and the process of class formation, I am able to more narrowly define peers and to cleanly identify caste peer effects by making use of a subset of classes where student allocation is plausibly exogenous. Moreover, balance checks demonstrate that there was no selection of teachers into classes either.

My results show that a higher proportion of low-caste students is detrimental for academic evolution. I uncover that this phenomenon operates through a behavioral channel on the part of the students not previously considered in the peer effects literature: students in classes with a higher proportion of low-caste students believe that teachers value them less, which leads them to lower their academic efforts. Interestingly, self-responses of the teachers in terms of their practices do not show any differences according to the caste composition of the class. This indicates that either the teachers are not accurately reporting their behaviors or students are not correctly assessing the teachers' perception about them. Importantly, my findings suggest that a higher proportion of low-caste students is not a reason to mechanically lower scores because of other characteristics across which castes differ, such as wealth. Instead, academic performance falls because this worsened perception discourages students to do well at school. Because these channels consist partly of endogenous decisions of the students, there is room for social multiplier effects.

Although my analysis is also able to describe which castes benefit more from higher proportions of classmates from each caste based on a heterogeneous effects analysis across castes, I refrain to make claims about optimal allocations of students to classes for several reasons. First, students' reactions to "optimal" allocations might be unexpected (Carrell et al., 2013). Moreover, fully describing the optimal decision would require more assumptions on the educational production and welfare functions. Finally, my analysis is mainly focused on academic outcomes, while caste mixing might have positive effects in other valuable dimensions that may overcome the negative academic effects. For example, Rao (2019) finds that mixing poor with rich students improves social behavior while inflicting relatively small academic damage.

Nevertheless, my findings do suggest that a simple policy to deal with the academic downside of caste diversity would be to assign teachers who will display less stigma concerns and more trust in the abilities of low-caste pupils to classes with higher percentages of these students. Another easy-to-implement policy would be to carry out informative sessions for teachers in which the equal potential that low-caste and high-caste students have for academic performance is emphasized - as shown in, for example, Hoff and Pandey (2006) for the case of Indian castes, in Dhar et al. (2018) for gender roles also in India, or in Chetty et al. (2016) for the case of poor students reallocating to wealthier areas in the US. This is in line with a wealth of studies suggesting that pedagogy on the side of the teachers is a key determinant of school progression (e.g. Banerjee et al., 2016) and that interventions focusing on pedagogy and governance tend to be more effective than simple increases in inputs (Glewwe and Muralidharan, 2016). Therefore, my paper is able to make innovative contributions on the role of caste in academic settings in India as well as to further explore the mechanisms of peers effects in middle school in a developing country with a credible empirical strategy that overcomes the key identification difficulties that have plagued the peer effects literature.

References

Ammermueller, Andreas and Jörn-Steffen Pischke, "Peer effects in European primary schools: Evidence from the progress in international reading literacy study," *Journal of Labor Economics*, 2009, *27* (3), 315–348.

Antecol, Heather, Ozkan Eren, and Serkan Ozbeklik, "The effect of teacher gender on student achievement in primary school," *Journal of Labor Economics*, 2014, *33* (1), 63–89.

Arcidiacono, Peter, Gigi Foster, Natalie Goodpaster, and Josh Kinsler, "Estimating spillovers using panel data, with an application to the classroom," *Quantitative Economics*, 2012, *3* (3), 421–470.

Bagde, Surendrakumar, Dennis Epple, and Lowell Taylor, "Does affirmative action work? Caste, gender, college quality, and academic success in India," *American Economic Review*, 2016, *106* (6), 1495–1521.

Banerjee, Abhijit, Esther Duflo, Maitreesh Ghatak, and Jeanne Lafortune, "Marry for what? Caste and mate selection in modern India," *American Economic Journal: Microeconomics*, 2013, *5* (2), 33–72.

_ , Rukmini Banerji, James Berry, Esther Duflo, Harini Kannan, Shobhini Mukherji, Marc Shotland, and Michael Walton, "Mainstreaming an effective intervention: Evidence from randomized evaluations of "Teaching at the Right Level" in India," Technical Report, National Bureau of Economic Research 2016.

Bertrand, Marianne, Rema Hanna, and Sendhil Mullainathan, "Affirmative action in education: Evidence from engineering college admissions in India," *Journal of Public Economics*, 2010, *94* (1-2), 16–29.

Bhattacharjee, Arpita, "Cast into Castes? Targeting persistent Caste-based inequalities with Affirmative Action," *Unpublished Manuscript*, 2016.

Borman, Geoffrey D and Matthew Boulay, *Summer learning: Research, policies, and programs*, Routledge, 2004.

Brown, Caitlin, "Social Eects on Schooling in Rural India," *Unpublished Manuscript*, 2017.

Burgess, Simon and Ellen Greaves, "Test scores, subjective assessment, and stereotyping of ethnic minorities," *Journal of Labor Economics*, 2013, *31* (3), 535–576.

Burgess, Simon M, "Human capital and education: The state of the art in the economics of education," 2016.

Carman, Katherine Grace and Lei Zhang, "Classroom peer effects and academic achievement: Evidence from a Chinese middle school," *China Economic Review*, 2012, *23* (2), 223–237.

Carrell, Scott E, Bruce I Sacerdote, and James E West, "From natural variation to optimal policy? The importance of endogenous peer group formation," *Econometrica*, 2013, *81* (3), 855–882.

_ , Richard L Fullerton, and James E West, "Does your cohort matter? Measuring peer effects in college achievement," *Journal of Labor Economics*, 2009, *27* (3), 439–464.

Chetty, Raj, Nathaniel Hendren, and Lawrence F Katz, "The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment," *American Economic Review*, 2016, *106* (4), 855–902.

Davidson, Russell and Emmanuel Flachaire, "The wild bootstrap, tamed at last," *Journal of Econometrics*, 2008, *146* (1), 162–169.

Deshpande, Ashwini, Rajesh Ramachandran et al., "How backward are the other backward classes? Changing contours of caste disadvantage in India," *Centre for Development Economics, Working Paper*, 2014, (233).

Dhar, Diva, Tarun Jain, and Seema Jayachandran, "Reshaping Adolescents' Gender Attitudes: Evidence from a School-Based Experiment in India," Technical Report, National Bureau of Economic Research 2018.

Ewijk, Reyn Van and Peter Sleegers, "Peer ethnicity and achievement: A meta-analysis into the compositional effect," *School Effectiveness and School Improvement*, 2010, *21* (3), 237–265.

Fairlie, Robert W, Florian Hoffmann, and Philip Oreopoulos, "A community college instructor like me: Race and ethnicity interactions in the classroom," *American Economic Review*, 2014, *104* (8), 2567–91.

Field, Erica, Seema Jayachandran, Rohini Pande, and Natalia Rigol, "Friendship at work: Can peer effects catalyze female entrepreneurship?," *American Economic Journal: Economic Policy*, 2016, *8* (2), 125–53.

Fruehwirth, Jane Cooley and Jessica Gagete-Miranda, "Your peers' parents: Spillovers from parental education," *Economics of Education review*, 2019, *73*, 101910.

Gelbach, Jonah B, "When do covariates matter? And which ones, and how much?," *Journal of Labor Economics*, 2016, *34* (2), 509–543.

Glewwe, Paul and Karthik Muralidharan, "Improving education outcomes in developing countries: Evidence, knowledge gaps, and policy implications," in "Handbook of the Economics of Education," Vol. 5, Elsevier, 2016, pp. 653–743.

Gong, Jie, Yi Lu, and Hong Song, "The effect of teacher gender on students' academic and noncognitive outcomes," *Journal of Labor Economics*, 2018, *36* (3), 743–778.

_ , _ , and _ , "Gender peer effects on students' academic and noncognitive outcomes: Evidence and mechanisms," *Journal of Human Resources*, 2019, pp. 0918–9736R2.

Guryan, Jonathan, Kory Kroft, and Matthew J Notowidigdo, "Peer effects in the workplace: Evidence from random groupings in professional golf tournaments," *American Economic Journal: Applied Economics*, 2009, *1* (4), 34–68.

Hanna, Rema N and Leigh L Linden, "Discrimination in grading," *American Economic Journal: Economic Policy*, 2012, *4* (4), 146–68.

Hanushek, Eric A, John F Kain, and Steven G Rivkin, "New evidence about Brown v. Board of Education: The complex effects of school racial composition on achievement," *Journal of Labor Economics*, 2009, *27* (3), 349–383.

Hnatkovska, Viktoria, Amartya Lahiri, and Sourabh B Paul, "Breaking the caste barrier intergenerational mobility in india," *Journal of Human Resources*, 2013, *48* (2), 435–473.

Hoff, Karla and Priyanka Pandey, "Discrimination, social identity, and durable inequalities," *American Economic Review*, 2006, *96* (2), 206–211.

Hoxby, Caroline, "Peer effects in the classroom: Learning from gender and race variation," Technical Report, National Bureau of Economic Research 2000.

Imberman, Scott A, Adriana D Kugler, and Bruce I Sacerdote, "Katrina's children: Evidence on the structure of peer effects from hurricane evacuees," *American Economic Review*, 2012, *102* (5), 2048–82.

Jackson, C Kirabo, Jonah E Rockoff, and Douglas O Staiger, "Teacher effects and teacher-related policies," *Annu. Rev. Econ.*, 2014, *6* (1), 801–825.

Kang, Changhui, "Classroom peer effects and academic achievement: Quasi-randomization evidence from South Korea," *Journal of Urban Economics*, 2007, *61* (3), 458–495.

Khanna, Gaurav, "Does Affirmative Action Incentivize Schooling? Evidence from India," *Review of Economics and Statistics*, 2016, pp. 1–46.

Kijima, Yoko, "Caste and tribe inequality: evidence from India, 1983–1999," *Economic Development and Cultural Change*, 2006, *54* (2), 369–404.

Kochar, Anjini et al., "Can Schooling Policies Affect Schooling Inequality? An Empirical Evaluation of School Location Policies in India," *Suman Bery Barry Bosworth Arvind Panagariya*, 2008, p. 53.

Kumra, Neha, "An assessment of the Young Lives sampling approach in Andhra Pradesh, India," 2008.

Lavy, Victor and Analia Schlosser, "Mechanisms and impacts of gender peer effects at school," *American Economic Journal: Applied Economics*, 2011, *3* (2), 1–33.

_ , M Daniele Paserman, and Analia Schlosser, "Inside the black box of ability peer effects: Evidence from variation in the proportion of low achievers in the classroom," *The Economic Journal*, 2011, *122* (559), 208–237.

Lowe, Matt, "Types of contact: A field experiment on collaborative and adversarial caste integration," *Unpublished Manuscript*, 2018.

Luke, Nancy and Kaivan Munshi, "Women as agents of change: Female income and mobility in India," *Journal of Development Economics*, 2011, *94* (1), 1–17.

Manski, Charles F, "Identification of endogenous social effects: The reflection problem," *The Review of Economic Studies*, 1993, *60* (3), 531–542.

Melo, Gioia De, "Peer effects identified through social networks: Evidence from Uruguayan schools," Technical Report, Working Papers 2014.

Moore, Rhiannon, Obiageri Bridget Azubuike, P Prudhvikar Reddy, Caine Rolleston, and Renu Singh, "Young Lives School Survey 2016-17: Evidence from India," 2017.

Munshi, Kaivan, "Caste and the Indian economy," *Journal of Economic Literature*, 2019, *57* (4), 781–834.

_ **and Mark Rosenzweig**, "Networks and misallocation: Insurance, migration, and the rural-urban wage gap," *American Economic Review*, 2016, *106* (1), 46–98.

Muralidharan, Karthik and Ketki Sheth, "Bridging education gender gaps in developing countries: The role of female teachers," *Journal of Human Resources*, 2016, *51* (2), 269–297.

Pivovarova, Margarita, "Peer Effects in the Classroom: Evidence from New Peers." PhD dissertation 2013.

Rao, Gautam, "Familiarity does not breed contempt: Generosity, discrimination, and diversity in Delhi schools," *American Economic Review*, 2019, *109* (3), 774–809.

Rawal, Shenila, Geeta Kingdon et al., "Akin to my teacher: Does caste, religious or gender distance between student and teacher matter? Some evidence from India," Technical Report, Department of Quantitative Social Science-UCL Institute of Education 2010.

Sacerdote, Bruce, "Peer effects with random assignment: Results for Dartmouth roommates," *The Quarterly Journal of Economics*, 2001, *116* (2), 681–704.

Singh, Abhijeet, "Private school effects in urban and rural India: Panel estimates at primary and secondary school ages," *Journal of Development Economics*, 2015, *113*, 16–32.

_ , "Learning more with every year: School year productivity and international learning divergence," *Journal of the European Economic Association*, 2016.

Thorat, Sukhadeo, Nidhi Sadana Sabharwal, and Gowhar Yaqoob, *Caste and Social Exclusion: Issues Related to Concept, Indicators, and Measurement*, Indian Institute of Dalit Studies and UNICEF, 2010.

Zimmerman, David J, "Peer effects in academic outcomes: Evidence from a natural experiment," *Review of Economics and Statistics*, 2003, *85* (1), 9–23.

A Figures

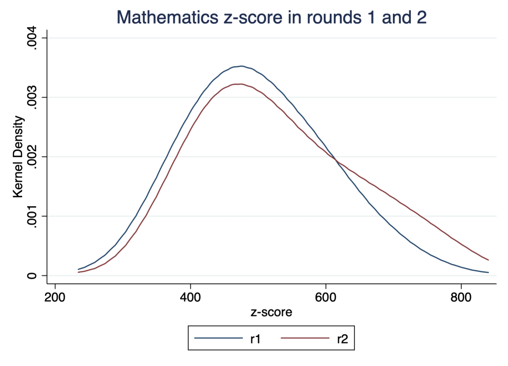

Figure A.1: Evolution in Mathematics Scores between Rounds 1 (r1) and 2 (r2)

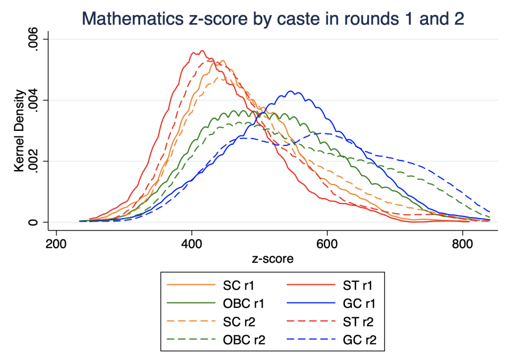

Figure A.2: Evolution in Mathematics Scores between Rounds 1 and 2 by Caste

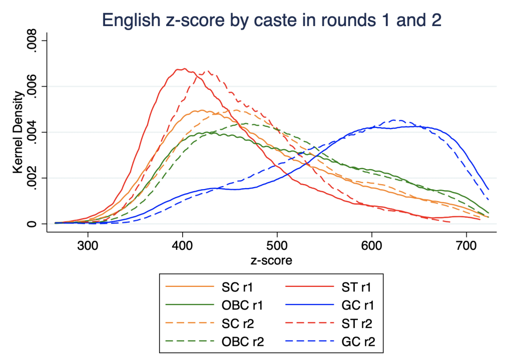

Figure A.3: Evolution in English Scores between Rounds 1 and 2 by Caste

B Appendix (For Online Publication)

Table B.1: Descriptive Statistics: Mathematics Teachers

Variable	Mean	Std. Dev.	Observations
Age (years)	37.179	10.248	56
Female	0.400	0.493	58
SC	0.143	0.353	56
ST	0.071	0.260	56
OBC	0.464	0.503	56
GC	0.321	0.471	56
Tenure at school (years)	6.241	6.213	58
Total tenure as teacher (years)	11.362	9.821	58
Full time contract	0.810	0.395	58
Permanent contract	0.534	0.503	58
Highest Education: High school graduate	0.036	0.189	55
Highest Education: College graduate	0.291	0.458	55
Highest Education: Master and above	0.673	0.474	55
Highest Qualification: Diploma in education	0.035	0.186	57
Highest Qualification: Bachelor in education	0.807	0.398	57
Highest Qualification: Master in education	0.123	0.331	57
Member teacher association	0.407	0.496	54

All variables are dummies unless specified otherwise

Table B.2: Balance Check Mathematics Score

	(1) Peers' Mathematics Score Round 1
Female	0.117
	(0.245)
Scheduled Caste	-0.452
	(0.535)
Scheduled Tribes	-0.859
	(0.782)
Other Backward Classes	0.317
	(0.504)
Repeater	0.045
	(0.146)
Father Never Enrolled	-0.181
	(0.152)
Female	0.117
	(0.245)
Scheduled Caste	-0.452
	(0.535)
Scheduled Tribes	-0.859
	(0.782)
Other Backward Classes	0.317
	(0.504)
Repeater	0.045
	(0.146)
Father's Highest Education: Primary School	0.114
	(0.112)
Father's Highest Education: Upper Primary School	0.113
	(0.116)
Father's Highest Education: High School	0.040
	(0.124)
Father's Highest Education: Junior College	0.144
	(0.168)
Father's Highest Education: Higher Education	0.181
	(0.152)
Wealth Index of the child	0.470
	(0.314)
Parental Interest in Child's School Performance	-0.046
	(0.064)
Child's Interest in Reading	0.072
	(0.047)
Any Health Problem	-0.159
	(0.130)
Observations	1,585
R-squared	0.894

Regression of peers' baseline mathematics score on own characteristics of the individual respondent. It additionally includes school fixed effects. A joint F-test of all the independent variables does not reject the null with a p-value of 0.35 (and a considerably larger one when wild-bootstrapping). Standard errors clustered at the class level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.3: Balance Check of Mathematics Teacher Assignment

VARIABLES	(1) Age (years)	(2) Current Tenure (years)	(3) Female	(4) Total Tenure (years)	(5) Full-Time Contract	(6) Highest Education (categorical)	(7) Permanent Contract
Female	0.098	0.942**	0.054	0.113	0.003	-0.002	-0.004
	(1.171)	(0.455)	(0.034)	(0.778)	(0.005)	(0.030)	(0.013)
HH Size	0.078	-0.019	-0.001	0.047	0.003	0.000	0.005*
	(0.053)	(0.014)	(0.001)	(0.046)	(0.002)	(0.003)	(0.003)
Repeaters	-0.146	0.427*	0.008	-0.013	-0.020	0.019	-0.035*
	(0.390)	(0.216)	(0.012)	(0.372)	(0.013)	(0.024)	(0.018)
Enrolled Before 7	0.165	-0.731	-0.055	0.016	0.002	0.023	0.015
	(0.916)	(0.453)	(0.035)	(0.661)	(0.005)	(0.029)	(0.013)
Maternal Education	0.036	-0.031	-0.004	0.047	0.003	0.008	-0.005
	(0.094)	(0.056)	(0.004)	(0.075)	(0.002)	(0.007)	(0.007)
Paternal Education	0.027	-0.006	0.004	-0.036	-0.001	0.002	0.000
	(0.072)	(0.059)	(0.003)	(0.062)	(0.001)	(0.005)	(0.001)
Wealth Index	0.144	-0.862	-0.048	0.404	0.074	-0.004	0.092*
	(1.293)	(0.521)	(0.035)	(1.144)	(0.047)	(0.034)	(0.054)
Low Caste	0.001	0.019	-0.001	0.005	0.005	0.006	0.000
	(0.080)	(0.021)	(0.001)	(0.069)	(0.003)	(0.005)	(0.006)
p-value F-test of joint significance	0.156	0.098	0.921	0.107	0.952	0.511	0.786
Observations	1,567	1,608	1,608	1,608	1,608	1,510	1,608
R-squared	0.884	0.908	0.959	0.910	0.934	0.835	0.882

All regressions include school fixed effects with standard errors clustered at the class level. Mathematics teacher's individual characteristics are regressed on individual students' characteristics. *** p<0.01, ** p<0.05, * p<0.1

Table B.4: Basic Determinants of Academic Performance

VARIABLES	(1) Math Score Round 2	(2) English Score Round 2	(3) Score Round 2
Math Score, Round 1	0.519***		
	(0.042)		
	[0.000]		
English Score, Round 1		0.424***	
		(0.041)	
		[0.000]	
Score Round 1			0.322***
			(0.075)
			[0.000]
Low Caste	-20.528**	-3.237	
	(7.780)	(3.597)	
	[0.036]	[0.380]	
Female	8.698	5.948	
	(8.912)	(4.775)	
HH Size	-0.892	-0.423	
	(1.359)	(0.621)	
Repeater	-12.310*	-2.764	
	(6.158)	(4.600)	
Father's Highest Education: Primary School	2.221	-2.488	
	(6.585)	(4.674)	
Father's Highest Education: Upper Primary School	16.037*	2.539	
	(8.748)	(4.942)	
Father's Highest Education: High School	-1.077	-6.911	
	(7.733)	(4.577)	
Father's Highest Education: Junior College	17.526**	0.694	
	(7.617)	(5.172)	
Father's Highest Education: Higher education	27.330***	2.183	
	(8.920)	(5.151)	
Reading Interest	-0.177	0.309	
	(2.339)	(1.184)	
Wealth Index	2.416	12.099	
	(12.234)	(8.104)	
Parental Interest Child's School Performance	3.010*	2.340**	
	(1.535)	(1.164)	
Number Health Problems	-7.389**	1.581	
	(2.967)	(1.818)	
Observations	1,308	1,370	2,419
R-squared	0.671	0.776	0.891

Columns 1 and 2 include school fixed effects. Column 3 includes individual fixed effects. Standard errors clustered at the class level. Wild-bootstrapped p-values in brackets. *** p<0.01, ** p<0.05, * p<0.1 (based on wild-bootstrapped p-values only for the signaled variables)

Table B.5: Determinants of New Students' Classroom Assignment

	Peer Characteristics								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Prop. Females	Enrolled < 7	Maternal Educ.	Health Problems	Parental Interest School	HH Size	Interest Reading	Repeater	Wealth Index
Math Score, Round 1	0.000	-0.000	0.000*	-0.000	-0.000	0.000	-0.000	-0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	0.071**	-0.012	-0.036	0.016	-0.197**	0.039	-0.036	0.043*	-0.015*
	(0.035)	(0.010)	(0.045)	(0.033)	(0.084)	(0.046)	(0.046)	(0.025)	(0.008)
HH Size)	-0.006	-0.001	-0.010	-0.002	0.003	-0.002	-0.006	0.001	-0.002*
	(0.004)	(0.001)	(0.009)	(0.006)	(0.011)	(0.007)	(0.010)	(0.003)	(0.001)
Low Caste	0.001	0.009*	-0.071	0.007	-0.104	-0.007	0.161***	0.012	0.001
	(0.012)	(0.005)	(0.045)	(0.033)	(0.063)	(0.034)	(0.057)	(0.018)	(0.006)
Repeater	0.032**	-0.009	-0.041	0.031	-0.058	0.008	-0.095*	0.031**	-0.008
	(0.012)	(0.008)	(0.038)	(0.028)	(0.043)	(0.046)	(0.050)	(0.013)	(0.006)
Enrolled Before 7	-0.049	0.032**	-0.210	0.076*	-0.158	-0.060	0.192	0.026	-0.009
	(0.048)	(0.014)	(0.134)	(0.044)	(0.133)	(0.095)	(0.129)	(0.030)	(0.015)
Maternal Education: Never been to school	-0.030	-0.017	-0.228	-0.023	0.197	0.081	0.096	0.056	-0.025*
	(0.041)	(0.015)	(0.147)	(0.055)	(0.124)	(0.081)	(0.165)	(0.035)	(0.014)
Maternal Education: Primary School	-0.038	-0.019	-0.195	-0.040	0.201	0.092	0.120	0.049	-0.014
	(0.044)	(0.015)	(0.146)	(0.057)	(0.125)	(0.083)	(0.161)	(0.033)	(0.014)
Maternal Education: Upper Primary School	-0.058	-0.013	-0.190	-0.078	0.378***	0.074	0.107	0.025	-0.006
	(0.051)	(0.016)	(0.154)	(0.061)	(0.143)	(0.093)	(0.195)	(0.034)	(0.014)
Maternal Education: High School	-0.006	-0.015	-0.151	-0.003	0.162	0.050	0.127	0.007	-0.009
	(0.034)	(0.014)	(0.138)	(0.050)	(0.117)	(0.075)	(0.134)	(0.029)	(0.014)
Maternal Education: Junior College	-0.034	0.011	-0.099	-0.051	0.173	-0.036	0.122	-0.020	-0.006
	(0.041)	(0.011)	(0.105)	(0.049)	(0.124)	(0.068)	(0.120)	(0.024)	(0.010)
Paternal Education: Never been to school	0.037	0.013	-0.034	0.051	-0.138	-0.055	0.069	-0.011	-0.008
	(0.026)	(0.008)	(0.107)	(0.059)	(0.084)	(0.092)	(0.113)	(0.023)	(0.009)
Paternal Education: Primary School	0.019	0.011	-0.077	-0.009	-0.128	-0.051	0.067	-0.017	-0.020**
	(0.024)	(0.007)	(0.103)	(0.052)	(0.088)	(0.071)	(0.101)	(0.018)	(0.008)
Paternal Education: Upper Primary School	0.017	0.002	-0.022	-0.036	0.016	-0.063	-0.059	-0.007	-0.019**
	(0.030)	(0.008)	(0.111)	(0.064)	(0.109)	(0.077)	(0.119)	(0.018)	(0.009)
Paternal Education: High School	0.029	0.019***	0.089	0.078	-0.199**	0.028	-0.023	-0.037*	0.002
	(0.026)	(0.007)	(0.093)	(0.052)	(0.085)	(0.059)	(0.106)	(0.022)	(0.008)
Paternal Education: Junior College	0.022	-0.020*	-0.020	0.070	-0.060	0.105	0.069	0.008	-0.007
	(0.037)	(0.011)	(0.076)	(0.046)	(0.081)	(0.094)	(0.070)	(0.017)	(0.008)
Wealth Index	0.069	0.014	0.075	0.165**	0.006	-0.142	-0.105	0.007	0.031
	(0.050)	(0.014)	(0.145)	(0.083)	(0.155)	(0.110)	(0.188)	(0.036)	(0.020)
Observations	413	413	413	413	410	413	412	413	413
R-squared	0.900	0.561	0.905	0.468	0.867	0.622	0.611	0.634	0.929

The average characteristics of peers are regressed on the characteristics of the incoming students. The sample is restricted to newly-arrived students in schools with multiple sections. This requirement is satisfied for 107 sections in 50 schools. Standard errors clustered at the class level. *** p<0.01, ** p<0.05, * p<0.1

Table B.6: Heterogeneous Effects and Non-linearities

VARIABLES	(1)	(2) Math Score Round 2	(3)
Math Score, Round 1	0.507***		0.512***
	(0.042)		(0.041)
Female	15.414	14.795	14.065
	(15.550)	(11.382)	(10.878)
Percentage SC and ST kids in class	-1.652**	-1.343	
	(0.592)	(0.566)	
	[0.042]	[0.165]	
Baseline Math Q2		39.964***	
		(13.904)	
Baseline Math Q3		71.068***	
		(16.959)	
Baseline Math Q4		96.510***	
		(15.087)	
Baseline Math Q5		140.109***	
		(18.491)	
Female*Perc. SC and ST	-0.090		
	(0.494)		
	[0.879]		
Baseline Math Q2*Perc. SC and ST		-0.260	
		(0.171)	
		[0.158]	
Baseline Math Q3*Perc. SC and ST		-0.382*	
		(0.207)	
		[0.089]	
Baseline Math Q4*Perc. SC and ST		-0.222	
		(0.222)	
		[0.361]	
Baseline Math Q5*Perc. SC and ST		-0.484*	
		(0.255)	
		[0.092]	
Perc. SC and ST: 0-40			-13.868
			(9.799)
			[0.283]
Perc. SC and ST: 40-80			-62.969**
			(11.034)
			[0.041]
Perc. SC and ST: 80-100			-49.988
			(17.024)
			[0.531]
Observations	1,358	1,358	1,358
R-squared	0.676	0.664	0.674

Regressions include school fixed effects and the following controls: gender, household size, indicators for low-caste, repeater status and being enrolled before 7, parental categorical education, wealth index, reading interest (at the individual and peer levels). Column 1 explores heterogeneous effects by gender and column 2 by baseline cognition after creating five quintiles based on the initial score for each caste group. Column 3 considers non-linear effects of the proportion of low caste students, where the omitted category is 0% of low-caste students. Standard errors clustered at the class level. Wild-bootstrapped p-values in brackets. *** p<0.01, ** p<0.05, * p<0.1 (based on wild-bootstrapped p-values only for the signaled variables)

Table B.7: Robustness: Alternative Estimating Sample and Attrition

VARIABLES	(1) Math Score Round 2	(2) Attrited	(3) Attrited
Percentage Low Caste Classmates	-2.335***	-0.000	-0.000
	(0.410)	(0.001)	(0.001)
	[0.002]	[0.824]	[0.839]
Observations	835	1,921	1,819
R-squared	0.611	0.111	0.115

All regressions include school fixed effects. Column 1 is the counterpart of column 1 in Table 3's Panel B with more strict sample restrictions. Compared to column 2 (no individual controls), column 3 additionally controls for gender, caste, parental education, household size, indicators for repeater status and having first enrolled before age 7, wealth index and interest in reading. Standard errors clustered at the class level. Standard errors clustered at the class level. Wild-bootstrapped p-values in brackets. *** p<0.01, ** p<0.05, * p<0.1 (based on wild-bootstrapped p-values only for the signaled variables)

C Variables Description

In this appendix I provide further details on the construction of certain variables.

- Wealth Index is computed as the proportion of positive answers to the questions of whether the household owns each of the following twelve items: bicycle; motorbike; television; electric fan; chairs; tables; telephone; fridge; car; air conditioning; computer; internet.
- The health categories considered are: sight problems; hearing problems; headaches; fever; stomach aches; other problems.
- Reading interest is the principal component of the following three items:
  - I read for fun outside the school.
  - I read things that I choose myself outside of school.
  - I read to find out things that I want to learn outside of school.
- Parental interest in their child's school performance is the principal component of the degree of agreement[27] to the following four items:
  - My parents (or someone else at home) asks me what I am learning at school.
  - I talk about my schoolwork with someone at home.
  - My parents (or someone else at home) makes sure that I set time aside for my homework.
  - Someone at home checks if I do my homework.
- Student working to please her teacher is the principal component of the following three items:
  - I want to do well in class to please my teacher.
  - I want to do well at school so that I can get praise from my teacher.
  - I do good work at school because I want to be recognised by my teacher.
- Student working hard to have better opportunities and outcomes in the future is the principal component of the following twelve items:
  - I study to increase my job opportunities for a good type of work in the future.
  - I am working hard in school to ensure that my future will be financially secure.
  - Making an effort in my studies is worth it because it will help me in the work I want to do later on.

[27] There are four possible categories: strongly disagree, disagree, agree, strongly agree. This categorical division is common to all attitudinal questions in the survey.

- I want to learn as much as I can in school to help me get good work in the future.
- I want to learn as much as I can in school to help me go on to college/university.
- I am working hard in school to help me gain admission to higher studies.
- Making an effort in my studies now is worthwhile because it will help me in my studies later on.
- Learning well in school will improve my work prospects and chances in the future.
- I am working hard in school to be able to get work in the future and support my family in the future.
- I want to do well at school to help my brothers and sisters in the future.
- Success in examinations will determine how successful I am in later life.
- Since success in the future is uncertain there is little use in learning very much.

• Student working hard to improve as a person is the principal component of the following ten items:

  - Studying gives me a lot of personal satisfaction.
  - I like studying because most of my subjects are really interesting.
  - I spend a lot of time working on topics I am interested in.
  - Keeping up with my studies helps to develop my character.
  - Learning is an important personal experience.
  - Learning in school teaches me to become self-disciplined.
  - I want to do well in school to show myself that I can learn new things.
  - I want to do well in my studies to show myself that I can learn difficult school work.
  - I work hard at school because I am interested in what I am learning.
  - Learning in school develops me as a person.

• Student works as hard as possible at school is the principal component of the following fourteen items:

  - I am willing to do my best in class.
  - I study hard for my tests in school.
  - If I put in enough effort I can succeed in my studies.
  - When studying, I work as hard as possible.
  - When studying, I keep working even if the material is difficult.

  - When studying, I try to do my best to acquire the knowledge and skills taught.
  - Setbacks in learning do not discourage me.
  - I am a hard worker in school.
  - I am diligent in my studies.
  - I am conscientious in my studies.
  - When studying, I want to do as little work as possible.
  - I want to do well in my studies, but only if the work is easy.
  - If the exercises in lessons are difficult I just don't do them.
  - I choose easy options in school so that I don't have to work too hard.

- Student math interest are the principal components of the following twelve items:
  - I look forward to my maths lessons.
  - I am interested in the things I learn in maths.
  - The things I learn in maths will be important to me in the future.
  - When I do maths, I sometimes get totally absorbed.
  - Because doing maths is fun, I wouldn't want to give it up.
  - Maths is important to me personally.
  - Studying maths gives me a lot of personal satisfaction.
  - I do extra work in maths topics that I like.
  - I find maths really boring.
  - Maths is an important part of the school programme.
  - I would rather spend my time on subjects other than Maths.
  - Learning maths is a waste of time.

- Teacher is open with questions is the principal component of the following three items:
  - If I don't understand something, I can ask my teacher to explain again until I do understand.
  - If I need help I can always ask my teacher.
  - When I ask a question my teacher will be nice to me.

- Teacher congratulates students for doing a good job is the principal component of the following two items:

- At least once a week my teacher will tell me that I have done good work.
- If I do something well, my teacher will come to me and congratulate me.

- Teacher cares about whether the students understand the material is the principal component of the following four items:
  - Every time my teacher explains something, we are asked whether we understand.
  - My teacher will always notice when I don't understand a topic and then come and help me.
  - My teacher always knows what I am doing.
  - My teacher will notice immediately if I am not concentrating and tell me to focus.

- Teacher cares about students is the principal component of the following two items:
  - My teacher never listens if I have something to ask or say.
  - If I raise my hand to share, my teacher will always want to know what I think about a topic.