Familiarity Does *Not* Breed Contempt: Diversity, Discrimination and Generosity in Delhi Schools

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Abstract

I exploit a natural experiment in India to identify how mixing wealthy and poor students in schools affects social preferences and behaviors. A policy change in 2007 forced many private schools in Delhi to meet a quota of poor children in admissions. This led to a sharp increase in the presence of poor children in new cohorts in those schools, but not in older cohorts or in other schools. Exploiting this variation, and using a combination of field and lab experiments, administrative data and test scores, I study impacts on three classes of outcomes: (i) prosocial behavior, (ii) social interactions and discrimination, and (iii) academic outcomes. First, I find that having poor classmates makes wealthy students more prosocial and generous. They become more likely to volunteer for a charity at school, more generous towards both rich and poor students in dictator games, and choose more equitable distributions of payoffs in the lab. Second, having poor classmates makes wealthy students discriminate less against poor children, measured by their teammate choice in an incentivized sports contest. Consistent with this, they become more willing to socialize with poor children outside school. Third, I find marginally significant negative effects on test scores in English, but no effect on Hindi or Math. Overall, I conclude that mixing in schools had substantial positive effects on the social behaviors of wealthy students, at the cost of negative but arguably modest impacts on academic achievement. To shed light on mechanisms, I exploit idiosyncratic assignment of students to study groups and find that the effects on social behaviors are largely driven by personal interactions between wealthy and poor students, rather than by changes in teacher behavior or curriculum.

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1 Introduction

Schools are de facto segregated across social and economic lines in many countries. Much research has examined the effects of such segregation on learning outcomes.[1] But desegregation and affirmative action efforts have historically been motivated not only by concerns about disparities in educational outcomes, but also by the argument that diversity in schools benefits society by positively influencing inter-group attitudes and social behavior (Schofield 1996). Yet empirical evidence on such effects is exceedingly scarce. More generally, little is known about how social preferences and behaviors are shaped, and whether they can be influenced by policy. This question is of particular interest in diverse and polarized societies, where the costs of social divisions are well documented.[2]

I focus on a particular dimension of diversity - economic status - and seek to answer the following question: What effect do peers from poor households have on students from relatively wealthy families? I assemble a data set of about two thousand students in fourteen schools in Delhi, India and use a combination of field and lab experiments, tests of learning and administrative data to measure the following outcomes: (i) generosity and prosocial behavior; (ii) tastes for socially interacting with or discriminating against the poor; and (iii) learning and classroom behavior.

My first econometric strategy exploits the plausibly exogenous staggered timing of a policy change that required elite private schools to offer places to poor students. This causes a sharp discontinuity across cohorts in the presence of poor students. In most schools, cohorts beginning schooling in 2007 or later have many poor students, while older cohorts are comprised exclusively of rich students. However, a small control group (about 4%) of elite private schools are entirely exempt from the policy for historical reasons, while another handful (6%) of schools complied with the policy a year late - in 2008 instead of 2007. I can therefore identify the effect of the presence of poor students (the "treatment")

[1] Buchmann and Hannum (2001) and Karsten (2010) report measures of educational segregation or stratification in a number of countries. Hattie (2002) and Van Ewijk and Sleegers (2009) provide meta-analyses of the effects of segregation on inequalities in learning.

[2] A large empirical and theoretical literature links greater social diversity, inequality and polarization with conflict (Esteban and Ray 2011), collective action problems (Bardhan et al. 2007), low levels of public good provision (Miguel and Gugerty 2005), political instability (Alesina and Perotti 1996) and diminished economic growth (Easterly and Levine 1997).

by comparing both within schools (comparing treated and untreated cohorts) and within cohorts (comparing treated and untreated schools) using a difference-in-differences regression model. This approach identifies the average effect on wealthy students of adding poor children to their classroom - an important estimate for policy.

The second econometric strategy isolates the role of personal interactions between rich and poor students by exploiting idiosyncratic variation in peer groups *within* the classroom. Some schools in the sample use alphabetic order of first name to assign students to group-work and study partners. In these schools, the presence of a poor child with a name alphabetically close to a given rich student provides plausibly exogenous variation in personal interactions with a poor student. This allows me to distinguish between changes occurring due to personal interactions between students, and the effects of other possible changes at the classroom level, say in teacher behavior or curriculum.

My first finding is that having poor classmates makes students more prosocial, as measured by their history of volunteering for charitable causes at school. The schools in my sample occasionally offer students opportunities to volunteer or fundraise for affiliated charities. One such activity involves attending school on two weekend afternoons to help fundraise for a charity for disadvantaged children. I collect attendance records from such events and find that having poor classmates increases the share of volunteers by 10 percentage points (se 2.5%) on a base of 24%, while having a poor study partner increases volunteering by an imprecisely estimated 13 percentage points (se 9%).

To complement the field measure of prosocial behavior, I invite students to participate in a set of dictator games in the lab. Their incentivized choices in the games show that having poor classmates makes them behave in a more generous and egalitarian way. Treatment students share 45% (se 7%) or about 0.45 standard deviations more than control students when offered the chance to share money with an anonymous poor recipient at another school. But importantly, they are also 27% (se 5%) more generous when paired with other *rich* students. These effects are driven largely by increases in the share of students choosing a 50-50 split of the endowment. Consistent with this, I find increases in separate experimental measures of egalitarian preferences. Thus, exposure to poor students does not just make students more charitable towards the poor. Instead, it affects generosity and notions of fairness more generally.

The second finding is that economically diverse classrooms cause wealthy students to discriminate less against other poor children outside school. I measure discrimination using a field experiment in which participants select teammates for a relay race. By having participants choose between more-athletic poor students and less-athletic rich students, I create a tradeoff between ability and social similarity. Ability was revealed in a first stage using individual sprints, and the reward offered for winning the relay race was randomly varied across students. This provides exogenous variation in the price of discrimination. I find that when the stakes are high - Rs. 500 ($10), about a month's pocket money for the older students - only 6% of wealthy students discriminate by choosing a slower rich student over a faster poor student. As the stakes decrease, however, I observe much more discrimination. In the lowest stakes condition (Rs. 50), almost a third of students discriminated against the poor. But past exposure to poor students reduces discrimination by 12 percentage points. I structurally estimate a simple model of taste-based discrimination and find that wealthy students dislike socially interacting with a poor teammate relative to rich one by an average of Rs. 34, about two days worth of pocket money. Having poor classmates reduces this distaste by 30%.[3]

To shed light on the observed reduction in discrimination, I conduct a separate experiment to directly measure tastes for social interactions. Preferences for interacting with members of other social groups provide a natural foundation for taste-based discrimination. To measure such preferences, I invite students to attend a play date at a school for poor students, and elicit incentivized measures of their willingness to accept. I find that having poor classmates makes students more willing to attend the play dates with poor children. In particular, it reduced the average size of the incentive they required to attend the play date by 19% (se 3%). Having a poor study partner affects "willingness to play" by a similar

[3]The observed behavior is inconsistent with a simple model of statistical discrimination. When students are asked which of the prospective teammates is most likely to win the relay race, they invariably (98%) select the fastest student in the ability revelation round, similarly across treated and control classrooms. This implies that a substantial number of students chose a rich teammate despite believing that he is *less* likely to help them win the race. This suggests that taste-based discrimination dominates in this setting.

amount.

Having established the effects of having poor classmates on social preferences and behaviors, I turn attention to impacts on learning and classroom discipline. A traditional concern with integrating disadvantaged students into elite schools is the potential for negative peer effects on academic achievement. To evaluate this concern, I conduct tests of learning in English, Hindi and Math, and collect teacher reports on classroom behavior. I detect a marginally significant but meaningful decrease of 0.09 standard deviations in wealthy students' English language scores, but find no effects on their Hindi or Math scores, or on a combined index over all subjects. This pattern of findings is consistent with the measured achievement gap between poor and wealthy students, which is largest in English – perhaps because wealthy students are more likely to speak English at home. And while teachers do report higher rates of disciplinary infractions by wealthy students in treated classrooms, the increase comprises entirely of the use of inappropriate language (that is, swearing), as opposed to disruptive or violent behavior. My third finding is thus of negative but arguably modest effects on academic achievement and discipline.

For each of the outcomes above, I compare the effects of the two types of variation: across-classroom variation in the presence of poor students, and within-classroom variation driven by assignment to study groups. This sheds light on mechanisms underlying the results by teasing apart the effect of direct personal interactions from the impact of other changes such as those in teacher behavior or curriculum. I find that personal interactions are an important driver of the overall effects. For example, having a poor study partner alone explains 70% of the increase in "willingness to play" with a poor child, and 38% of the increase in generosity towards the poor. This likely underestimates the importance of personal interactions, since students surely also interact with other poor classmates outside their study groups.

This paper relates to four bodies of work in economics. First, a recent literature studies whether interaction reduces inter-group prejudice. Most closely related are Boisjoly et al. (2007) and Burns et al. (2013), who find that being randomly assigned a roommate of a different race at college increases inter-racial social interactions in later years.[4] Second, this paper relates to research on the effects of desegregation and (more generally) peer effects in education. Evidence on peer effects in learning is mixed, with impacts on non-academic outcomes such as church attendance and drug and alcohol use a more robust finding (see Sacerdote 2011 for a review). Consistent with this, I find substantial effects on prosocial behavior and discrimination, but mixed and overall modest effects on test scores. A third connection is to the growing literature on how distributional social preferences are shaped, for example by exposure to violent conflict (Voors et al. 2012) and the ideology of one's college professors (Fisman et al 2009). I add to this literature by showing that peers at school can also shape social preferences. Finally, this paper relates to research on the economics of discrimination. I contribute to this literature by showing evidence of taste-based discrimination in an experiment (albeit in a non-market setting amongst students), and more importantly by showing that past exposure to out-group members causally reduces such discrimination.[5]

My findings are also of relevance to policy makers: the policy I study will shortly be extended throughout India under the Right To Education Act, with consequences for many of India's 400 million children. This policy is controversial, with legal battles over its legitimacy reaching India's Supreme Court. Opponents have prominently argued that any gain for poor children will come at a substantial cost to the existing clientele of private schools. Proponents have responded that diversity will benefit even rich students by providing them with "a clearer idea of the world".[6] While we must be cautious in extrapolating from elite private schools in Delhi to the rest of India, my findings provide some support for each side of the debate. A radical increase in diversity in the classroom did have modest negative impacts on the academic achievement and behavior of advantaged students. But it also

[4]Outside of economics, a long tradition of related work in social psychology following Allport (1954) generally documents a negative correlation between inter-group contact and prejudice, but suffers from both selection problems and a reliance on stated attitudes rather than observed behavior as outcome measures (Pettigrew and Tropp 2006).

[5]Scholars have investigated the extent to which discrimination exists and matters in the labor market using audit studies (Bertrand and Mullainathan 2004), quasi-experiments (Goldin and Rouse 2000) and experiments (List 2004, Mobius and Rosenblat 2006).

[6]The Indian Express, April 13 2013. http://www.indianexpress.com/news/learning-curve/936084/

made them substantially more generous and prosocial, more willing to socially interact with poor children, and less likely to discriminate against them. A full accounting of the effects of economic diversity in schools on privileged students should consider all these effects.

The rest of this paper is organized as follows. In Section 2, I describe the policy change underlying the natural experiment. Section 3 discusses the two econometric strategies and addresses possible challenges to identification. Section 4 reports impacts on the first class of outcomes, prosocial behavior and generosity. Section 5 describes the experiments and results relating to discrimination and social interaction. Section 6 reports effects on learning and discipline. Section 7 briefly interprets and contrasts the key results, drawing out the main themes and clarifying this paper's contributions. Section 8 summarizes and discusses shortcomings and avenues for future research.

2 Background and Policy Experiment

In this section, I describe a policy change which forced most elite private schools in Delhi to offer places to poor children, thus integrating poor and wealthy students in the same classrooms. I briefly describe how the timing of the policy change varied across schools, as well as key features of the selection process for both poor and wealthy students. In particular, poor students are selected using randomized lotteries, while wealthy applicants are selected using a transparent scoring system, which does not allow the use of baseline test scores or ability measures.

Delhi - like most cities in India - has a highly stratified school system. Public schools and a growing number of low-fee private schools serve the large population of urban poor. Relatively expensive 'elite' private schools cater to students from wealthy households.[7] These types of schools differ widely in affordability, school inputs and acceptance rates. Public schools are free, and students are typically guaranteed admission to at least one public school in their neighborhood. In contrast, elite private schools as I define them charge tuition fees in excess of Rs. 2000 per month (approximately $40, 25% of median monthly household

[7] A loosely defined middle class typically sends it's children to private schools intermediate in their price and exclusivity to public and elite private schools.

consumption in 2010), and are vastly over-subscribed. Private schools in my sample report average acceptance rates of 11%, and monthly fees of up to Rs. 10,000.[8]

Policy Change. Many private schools in Delhi – including over 90% of the approximately 200 elite private schools – exist on land leased from the state in perpetuity at highly subsidized rates. A previously unenforced part of the lease agreement required such schools to make efforts to serve "weaker sections" of society. In 2007, prompted by the Delhi High Court, the Government of Delhi began to enforce this requirement. It issued an order requiring 395 private schools to reserve 20% of their seats for students from households earning under Rs. 100,000 a year (approximately $2000). Schools were not permitted to charge the poor students any fees; instead, the government partially compensated the schools. Decades after most of these private schools were founded, the policy change forced open their doors to many relatively poor children.

Two features of the policy change are particularly important for my analysis: (i) schools were not permitted to track the students by ability or socioeconomic status. Instead, they were required to integrate the poor students into the same classrooms as the rich, and (ii) the policy only applied to new admissions, which occur almost exclusively in the schools' starting grades (usually preschool). Thus, the policy did not change the composition of cohorts that began schooling before 2007.

Variation in timing. I divide elite private schools into three categories based on their response to the policy change. (i) *Treatment schools* were subject to the policy, and complied with it in the very first year. In these schools, cohorts admitted in 2007 or later have many poor students, while older cohorts comprise exclusively of wealthy students. This group includes about 90% of all elite private schools. (ii) *Delayed treatment schools* were also subject to the policy, but failed to comply in the first year - either because they expected the policy to be overturned in court, or because they felt the order was issued too late for them to modify their admission procedures. These schools complied with the policy a year later, in 2008, following a court ruling upholding the policy. This group comprises about 6%

[8] Parents of the wealthy students in the elite private schools I study apply to 8.8 schools on average and are offered admission to 1.8 of them. An article in the Indian Express memorably lamented that gaining admission to preschool in an elite private school in Delhi is harder than getting into Harvard.

of all elite private schools. *Control schools* are the 4% of elite private schools which were not subject to the policy at all, typically because they were built on land belonging to private charitable trusts or the federal government instead of the state government. In control schools, therefore, all cohorts comprise exclusively of rich students. The important point, discussed in detail in the next section, is that while schools are not randomly assigned to treatment, delayed treatment and control status, variation in the presence of poor children exists both within schools (across cohorts) and within cohorts (across schools).

Selection of Poor Students. If the seats for poor children are over-subscribed, schools are required to conduct a lottery to select beneficiaries. Conditional on applying to a given school, poor students are thus randomly selected for admission. While applications are free, they do involve the time costs of filling out and submitting the application form, and of obtaining documentation of income. Within the universe of eligible households, applicants are thus likely to be positively selected on their parents' preferences for their education, knowledge of the program, and their ability to complete the necessary paperwork. Since the children themselves are between 3 and 4 years of age when applying, it is less likely that their own preferences are reflected in the decision to apply.

The key point for this paper is that while the poor students may not be a representative sample of poor children in Delhi, they are without doubt from a very different economic class than the typical wealthy student in an elite private school. Figure 1 shows that the income cutoff of Rs. 100,000 per year is around the 45^{th} percentile of the household income distribution, and the average poor student in my sample is from the 25th percentile. In contrast, the typical rich student in the sample is from well above the 95th percentile of the consumption distribution. In the US, a corresponding policy change would see students from households making $23,000 a year attend the same schools as those making $200,000 a year.

Selection of wealthy students. The admissions criteria used by elite private schools to select wealthy (fee-paying) students are strictly regulated by the government, and publicly declared by the schools themselves. Schools rank applicants using a point system, with the greatest weight placed on distance to the applicant's home and whether an older sibling is already enrolled in the school. Other factors include a parent interview, whether parents are alumni, and gender (a slight preference is given to girls). Importantly, schools are prohibited from interviewing or testing students before making admissions decisions. Thus, it is difficult for schools to screen applicants on ability.[9] The overwhelming majority of admissions to elite private schools occur in preschool (students aged 3-4), which is the usual starting grade. New students are typically only admitted to higher grades when vacancies are created by transfers, which are rare: 1.7% per year in my sample.

3 Econometric Strategies

Between February 2012 and September 2013, I conducted field and lab experiments, and gathered test scores and administrative data on 2032 students in 14 elite private schools in Delhi. The sample consists of 9 treatment schools, 2 delayed treatment schools and 3 control schools, recruited as part of a larger project studying a variety of learning and behavioral outcomes.[10] Within each school, I constructed a random sample of wealthy (that is, fee-paying) students in the four cohorts who began preschool between 2005 and 2008, stratified by classrooms within schools. Given the timing of the policy change, these students were in grades 2 (cohort of 2008) through 5 (cohort of 2005) at the time of data collection.

Using this data, I exploit two types of variation to identify the effects of poor students on their rich classmates: whether or not poor students are present in a particular cohort and school, and idiosyncratic variation in interactions with poor students within the classroom.

3.1 Variation within schools and cohorts

The first approach identifies the average effect of having poor students in one's classroom. Recall that in treatment schools, wealthy students in grades 2 and 3 are "treated" with poor

[9]Schools may, of course, use parent interviews to judge the ability of applicants. But parents cannot easily provide schools with credible information about student ability in the interviews, since the child is typically under 4 years of age and has no prior schooling.

[10]I contacted a total of 16 schools. Two of these schools (one control and one treatment school) declined to participate. The 16 schools were selected partly for convenience, but also to cover all 12 education districts of the Delhi Directorate of Education, while oversampling control and delayed treatment schools and satisfying my criteria for being elite schools (monthly fees exceeding Rs. 2000). All schools were guaranteed complete anonymity in exchange for participating.

classmates, while grades 4 and 5 have no poor students. In delayed treatment schools, only grade 2 is treated, while grades 3-5 are untreated. And in control schools, grades 2-5 are all untreated. Restricting the sample to rich students, I estimate the following difference-in-differences specification by OLS:

$$Y_{igs} = \alpha + \delta_s + \phi_g + \beta \text{TreatedClassroom}_{gs} + \gamma X_{igs} + \varepsilon_{igs} \tag{1}$$

where Y_{igs} denotes outcome Y for student i in grade g in school s; X is a vector of controls, δ_s are school fixed effects, ϕ_g are grade or cohort fixed effects and ε_{igs} is a student specific error term. TreatedClassroom$_{gs}$ is the treatment indicator; it equals one if grade g in school s contains poor students, and is zero otherwise. β is thus the average effect of having a poor classmate, and is the key parameter to be estimated. I cluster standard errors at the grade-by-school level, since this is the unit of treatment. With 14 schools and 4 grades (2 through 5) per school in the sample, this results in 56 clusters. As a robustness check, I also cluster standard errors at the school level. Given the small number of schools ($k = 14$), I use the wild-t cluster bootstrap method of Cameron, Gelbach and Miller (2008).

Note that average differences in outcomes across schools are permitted; they are controlled for by the school fixed effects. Thus, I do not assume that treatment, delayed treatment and control schools would have the same average outcomes without treatment. Similarly, average differences across cohorts (or grades) are controlled for using cohort fixed effects. This is important, given the possibility of age effects in social behaviors and preferences, as shown by Fehr et al. (2008) and Almas et al. (2012). I only utilize variation within schools (comparing students in different cohorts) and within cohorts (comparing students in different schools).

The identifying assumption is that in the absence of treatment, the *gaps* in outcomes across the different types of schools would be the same across treated and untreated grades. This would be violated if, for example, even in the absence of the policy, treatment schools had (say) better teachers than control schools in grades 2 and 3, but not in grades 4 and 5.

Challenges to Identification. This identification strategy faces the following potential challenges, each of which I briefly address below. (i) Wealthy students may select into control schools based on their affinity for poor children. (ii) Treatment and delayed treatment schools have fewer seats for wealthy students after the policy change, which might increase the average ability of admitted students. (iii) There may be spillovers between treated and untreated grades within treated schools, and (iv) The policy may cause an increase an class size, which could directly affect outcomes.

The concern most relevant to estimating effects on social outcomes is that students might sort across the different types of schools based on their affinity for poor children.[11] In practice, this mechanism is of limited concern for the following reasons. First, it is difficult for parents to be picky, since acceptance rates at elite schools are low (about 10%) and less than 5% of such schools are control schools. Transfers between elite schools are also rare; control schools report very few open spaces in grades other than preschool each year.[12] Second, as a robustness check, I can restrict attention to students who had older siblings already enrolled in the same school. These students are likely to be less selected, both because parents might prefer to have both children in the same school, and because younger siblings of a current student are much more likely to be offered admission to the school. I show that none of the main results substantially change when restricting the sample in this way. Finally, the second identification strategy I describe below is entirely exempt from this concern, since it does not rely on variation across schools.

The main concern with estimating effects on academic outcomes is that the policy change may force treatment schools to become more selective when admitting wealthy students. And indeed, while the share of poor students in the incoming cohorts is around 18%, total cohort size only increases by 5%.[13] This implies that fewer wealthy students are accepted into treated private schools after the policy change. If schools select students based on

[11]For example, a parent who particularly dislikes the thought of his son sitting next to a poor child might try extra hard to have him enroll in one of the few control schools. Or students who find that they particularly dislike their poor classmates might transfer to a control school in later years.

[12]Additionally, I find that the number of applications to control schools relative to treatment schools does not increase after the policy change, suggesting that the policy change did not increase overall demand for the control schools amongst wealthy parents.

[13]The target of 20% reservation was not always met in the early years of the program.

academic ability, this would mechanically raise the average quality of admitted wealthy students, and bias my estimate of the effect on learning outcomes. I can deal with this concern as above - by restricting attention to the less-selected younger siblings of previously enrolled students, and by relying on the second identification strategy. However, it is also worth emphasizing that the schools are prohibited from testing or interviewing prospective students in starting grades. Since preschool applicants are between 3 and 4 years old, schools also have no prior test scores available while making their decisions. Thus, it is difficult for schools to screen applicants based on ability.

Spillovers between grades are likely minimal, since students spend over 85% of the school day exclusively with their assigned classmates, and little time interacting with students in other classrooms of the same grade, let alone students in other grades. To the extent that any spillovers do exist, they would bias against finding effects. And finally, class sizes increase by only 5% after the policy change. It is therefore unlikely that changes in class size could be important drivers of any effects.

The econometric strategy described above identifies the overall effect on wealthy students of having poor students integrated into their classrooms. This effect would be one important input to any evaluation of the costs and benefits of such programs. However, it tells us little about the mechanisms underlying any effects. In particular, it does not separate the effect of increased personal interactions between rich and poor students from other plausible classroom-level changes such as teachers spending more time teaching students about inequality and poverty.

3.2 Idiosyncratic variation within classrooms

The second approach uses membership in the same "study groups" as a proxy for personal interactions between students. Students in my sample spend an average of an hour a day engaged in learning activities in small groups of 2-4 students. Examples of such activities include collaborative craft projects, role playing exercises, and recitation. I collect data on study group membership in each school, and determine whether each student i has *any* poor children in his study group. I denote this binary measure by $HasPoorStudyPartners_i$.

In 8 of the 14 schools, students are assigned to study groups by alphabetic order of first name ($SchoolUsesAlphaRule = 1$). In the remaining schools, groups are either frequently reshuffled by teachers, or no systematic assignment procedure is used ($SchoolUsesAlphaRule = 0$). I obtain class rosters, and sort them alphabetically to compute whether each student i is immediately followed or preceded by a poor student. I denote this measure by $HasPoorAlphabeticNeighbor_i$. I then estimate the following regression by two-stage least squares:

$$Y_{icgs} = \alpha + \nu_{cgs} + \beta_1 \text{HasPoorStudyPartners}_i + \gamma X_i + \varepsilon_{igs} \tag{2}$$

where Y_{icgs} denotes outcome Y for student i in classroom c in grade g in school s, ν_{cgs} is a classroom fixed effect, and $\text{HasPoorStudyPartners}_i$ is instrumented for using $SchoolUsesAlphaRule_s * HasPoorAlphabeticNeighbor_i$.

This identification strategy isolates the effect of personal interactions between rich and poor students. Identification comes entirely from within treated classrooms in treatment and delayed treatment classrooms, and average differences across classrooms (or schools and cohorts) are controlled for using classroom fixed effects. Thus, this strategy is not subject to concerns about the sorting of wealthy students across different types of schools, or changes in class size or teacher behavior.

Note that this approach does not require that poor and rich students have a similar alphabetic distribution of names. Nor do I assume that a rich student's first name has no direct effect on his outcomes. Instead, I only utilize variation in study groups predicted by the differential effect of alphabetic ordering in schools which use versus do not use such ordering to assign study groups. In the most aggressive specification, the individual level controls include first letter of first name, and thus directly control for any average differences across alphabetic order of names.

Figure 2 graphically reports the first stage of this regression. It shows that in the schools which report using alphabetic order to assign study groups, having a name alphabetically adjacent to at least one poor student substantially increases the probability of having at least one poor study partner, from about 40% to 90%. In contrast, alphabetic adjacency has no effect in other schools. Table 1 provides the first stage regression, and reports that the instrument is strong, with an F-statistic of over 40.

4 Generosity and Prosocial Behavior

Common sense and empirical evidence suggest that human beings care about others, and about fairness. Economists have argued for the importance of such "social preferences" in domains including charitable donations (Andreoni 1998), support for redistribution (Alesina and Glaeser 2005), voter turnout (Edlin et al. 2007), and labor markets (Akerlof 1984, Bandiera et al. 2005). Researchers have measured social preferences in the field using behaviors like charitable giving and public goods provision (DellaVigna 2009), and in the lab using dictator games (Kahneman et al. 1986), where the participant (the "dictator") typically decides how to split a pot of money between himself and an anonymous recipient.[14]

Recently, scholars have begun to investigate how social preferences are shaped by life experiences, including education (Jakiela et al. 2010), the ideology of one's college professors (Fisman et al. 2012), political violence (De Voors et al. 2012) and macroeconomic conditions (Fisman et al. 2013). Researchers have also begun to trace the emergence of social preferences in children, where egalitarian preferences are seen to emerge around age 4-8 (Fehr et al. 2008), while more sophisticated notions of fairness emerge in adolescence (Almas et al. 2010).

In this section, I estimate how having poor classmates affects the prosocial behavior of wealthy students. I measure such behavior in two ways: in the field using administrative data on volunteering for charities, and in the lab using dictator games. I first find that wealthy students are substantially more likely to volunteer for a charity if they have poor classmates in school. Next, I show that the increase in prosocial behavior is also visible in the

[14] More sophisticated versions of dictator games might vary the identity of the recipient (Hoffman et al. 1996) or vary the exchange rate at which money can be transferred between the dictator's endowment and the recipient (Andreoni and Miller 2002). Choices made in such lab games have been shown to predict real-world behavior such as charitable donations (Benz and Meier 2008), loan repayment (Karlan 2005) and voting behavior (Finan and Schechter 2012).

lab. Having poor classmates makes wealthy students more generous in dictator games. This increased generosity is partly driven by the wealthy students displaying more egalitarian preferences over monetary payoffs.

4.1 Prosocial Behavior - Volunteering for Charity

I begin by studying prosocial behavior in a setting familiar to students in elite private schools in Delhi. All the schools in my sample provide students with occasional opportunities to volunteer for charities. One such activity common across the schools involves spending two weekend afternoons in school to help fundraise for a charity serving disadvantaged children. The task itself is to help make and package greeting cards, which are then sold to raise money for the charity. Participation in these events is strictly voluntary; only 28% of students choose to attend. Volunteering activities thus serve as a natural real-world measure of prosocial behavior.

I collect administrative data on attendance at these volunteering events, and apply both econometric strategies described in the previous section to identify the effects of poor students on their wealthy classmates. Figure 3a graphically depicts the difference-in-differences strategy, plotting the share of students volunteering by grade and school type. The graph shows that wealthy students in grades 4 and 5 - which have no poor students - have similar volunteering rates across the three types of schools (control, treatment and delayed treatment). This suggests that the control schools are not especially bad at teaching prosocial behavior; in cohorts unaffected by the policy change, all the schools have similarly prosocial students. However, wealthy students in treatment schools volunteer substantially more in grades 2 and 3 - precisely the grades which contain poor students. The same pattern is evident for delayed treatment schools, which are only treated in grade 2. This pattern of volunteering behavior suggests that it is having poor classmates that causes the increase in wealthy students' prosocial behavior.

Figure 3b shows the effect of having a poor study partner, and conveys the essence of the instrumental variable strategy. It shows the share of volunteers, separately by whether or not the wealthy student's name is alphabetically adjacent to at least one poor student in his class roster. The graph shows that wealthy students with names adjacent to a poor student are more likely to volunteer for the charity - but only in those schools which use alphabetic order to assign study groups. This result suggests that it is having a poor student in one's study group - and therefore personally interacting with a poor student - that causes an increase in prosocial behavior.

The regression results in Table 2 confirm these findings, and attach a precise magnitude to the effects. Column 1 reports the main difference-in-differences estimate and shows that having poor classmates increases volunteering by 10 percentage points (se 2.4), an increase of 43% or 0.25 standard deviations over the volunteering rate in control classrooms. The effect remains highly significant ($p < 0.01$) when standard errors are clustered at the school level using the wild cluster bootstrap method of Cameron et al. (2008). Column 2 reports the same specification estimated on the restricted sample of students who have older siblings in the same school. The results are similar and not statistically different: an increase in volunteering of 7 percentage points (se 3.0). Column 3 reports the instrumental variable estimate of the effect of having a poor study partner. It shows that having at least one poor study partner causally increases volunteering by 13 percentage points (se 8.6), an imprecisely estimated increase of 53% over students without any poor study partners.

4.2 Dictator Games

To complement the field measure of prosocial behavior, and to better understand any changes in social preferences, I invite students to play dictator games in a lab setting. In particular, I use two dictator games to measure their generosity towards both poor and rich recipients. In the first game, students are knowingly paired with an anonymous poor student at a school catering solely to poor children. In the second game, instead, students are paired with an anonymous rich student at a control school catering exclusively to rich students.

Protocol. Within each school, I randomly selected about 30 students from each of grades 2 through 5. I invited these students to experimental sessions conducted in a separate room in their school during a regular school day. Each student was provided with two numbered and colored envelopes, corresponding to the two dictator games. Each envelope contained a decision sheet and a photograph and brief description of the recipient's school. The only difference between the two games was the identity of the recipient. In one game, the recipient was an anonymous student in a school catering to disadvantaged children. In the other, the recipient was an anonymous student in an elite private school which caters to wealthy students. In each case, the experimenter read the description aloud to the students, and also asked them to carefully look at the materials themselves. Students were informed that they would play two such games, and that one of the two games would be selected for implementation through a coin toss.

The decision sheet contained a table with rows corresponding to the possible splits of the endowment. The student's allocation was in the left column, and the recipient's endowment in the right column. The allocations summed to 10 rupees, with only integer values permitted. Students were asked to circle their desired allocation. As a check, they also filled in blanks stating how much they would receive, and how much the recipient would receive. After the game was verbally described to the students, the experimenter answered any questions they had. The game proceeded only once the students appeared to understand the procedure very well, and were able to correctly state how much they would receive by circling each row. Students then played the two games, in an order randomly varied by experimental session. After subjects made each decision, they placed the decision sheet and descriptive materials back in the envelope, closed it and returned it to the experimenter. Then, as promised, a coin was flipped to determine which choice would be implemented.

Students next completed a brief survey to debrief, and a set of additional games and questions for a related research project. At the end of this period, sealed envelopes were returned to students containing their chosen payoff. Students then had the option to use their payoff (and any other money they may have had) to purchase candy from a small store set up by the experimenter.

Poor Recipient. I find that having poor classmates and interacting with them in study groups makes wealthy students substantially more generous towards poor recipients. Fig 4 shows the results graphically, while Table 3 provides numerical estimates. Having poor classmates increases the average amount shared with a poor recipient by 12 percentage points (se 1.9), an increase of 45% or 0.45 standard deviations over the average giving in control classrooms. The results are very similar for the reduced sample of younger siblings (Column 2). The instrumental variable estimates of Column 3 show that having at least one poor study partner partner causally increases giving by 7 percentage points (se 3.1), an increase of 22%.

Rich Recipient. Figure 5 plots the corresponding results for the amounts shared with rich recipients. They show a very similar pattern to the results for poor recipients, albeit with slightly smaller effect sizes. Table 4 reports that having poor classmates increases giving to wealthy recipients by 27% (se 5%), while having a poor study partner increases giving by a less precisely estimated 42% (se 23%).

This increase in generosity towards rich students appears puzzling at first. Why would exposure to poor children make one more generous towards rich children? Figure 6 provides a hint by plotting the distribution of giving in the two games, separately for students in treated and untreated classrooms. The figures show a distinct increase in the probability of sharing exactly 50% of the endowment with the recipient. This raises the intriguing possibility that interacting with poor classmates makes wealthy students more egalitarian over monetary payoffs.

Egalitarian Preferences. To explore this possibility further, I use a set of three simple dictator games designed to identify whether subjects dislike unequal allocations.[15] Each game poses dictators with a binary choice between more and less equal distributions of payoffs. The less equal option provides a higher personal payoff (in the "equality game") or a higher sum of payoffs for the two recipients (in the two "disinterested" dictator games). The payoffs in the games are listed in the table below.[16]

	More equal option	Less equal option
Equality Game	Dictator=5, Recip =5	Dictator=6, Recip =1
Disinterested Game 1	Recip A = 4, Recip B = 4	Recip A = 8, Recip B = 3
Disinterested Game 2	Recip A = 4, Recip B = 4	Recip A = 12, Recip B = 0

Table 5 reports that students with poor classmates are consistently more likely to pick

[15] These games are adapted from Charness and Rabin (2002).

[16] Note that the games themselves were presented without labels, and the order of the two options was randomized.

the more equal outcome. Column 1 shows that treated students are 9 percentage points more likely to reduce their own payoff by choosing (5,5) over (6,1) in the equality game, compared to a base of 55% in the control group. And when choosing allocations for two anonymous recipients (holding their own payoff fixed) in the disinterested dictator games, they are 12 percentage points more likely to choose (4,4) over (8,3) and 14 percentage points more likely to pick (4,4) over (12,0).

Considering the set of dictator game results together, I conclude that having poor classmates does not simply make students more charitable towards the poor. Instead, it makes them more generous overall, and in particular makes them exhibit more egalitarian preferences over monetary payoffs. This is an important point, suggesting that interacting with poor children in school does not just make students more favorably inclined towards the poor. Rather, they behave as if more fundamental fairness preferences have changed.

The dictator game measures were entirely independent of the field observations of volunteering behavior described previously. Putting together the findings of increased generosity in the lab and increased volunteering in the field thus substantially strengthens my conclusion that being exposed to poor children in school makes wealthy students more prosocial.

5 Social Interactions and Discrimination

Discrimination is a pervasive and important phenomenon in labor markets (Goldin and Rouse 2000, Bertrand and Mullainathan 2004), law enforcement (Persico 2009), residential location choice (Becker and Murphy 2009) and other contexts. Theories of discrimination are of two main types: taste-based discrimination, reflecting an innate animosity towards individuals from a particular group (Becker 1957), and statistical discrimination, which results from imperfect information about productivity or ability (Phelps 1972, Arrow 1973, Aigner and Cain 1977).

Tastes for social interactions provide a natural foundation for taste-based discrimination. But social interaction models also explain features of residential patterns (Schelling 1971), collective action (Granovetter, 1978), job search (Beaman and Magruder 2012) and the marriage market. Changes in willingness to interact with members of other social groups are therefore a potentially important outcome of diversity in schools. Indeed, theory suggests that even small changes in these tastes can lead to large differences in aggregate outcomes, since social interaction models often feature multiple equilibria (Card et al. 2008).

In this section, I estimate how having poor classmates in school affects rich students' willingness to socially interact and work with other poor children in teams, or conversely to discriminate against them. I design two novel experiments to measure these outcomes. The first is a team selection field experiment designed to estimate taste-based discrimination using exogenous variation in the price of discrimination. The second experiment elicits students "willingness to play" - the cost they attach to attending a play date with poor children.

5.1 Team-Selection Field Experiment

Design. The main idea of the team-selection experiment is to create a tradeoff for wealthy students between choosing a high ability teammate (and thus increasing their own expected payoff) versus choosing a lower-ability teammate with whom they would prefer to socialize. The team task I used in the experiment was a relay race, a task which was familiar to all the students, and in which ability is easily revealed through times in individual sprints. In addition to running the relay race together, participants were required to spend time socializing with their teammates.

The experiment was conducted on the sidelines of a sports meet featuring athletes from two elite private schools - one a treatment school, and the other a control school. The participants in the experiment were not the athletes themselves, but were instead drawn from the large contingents of students who were present to support their teams. Note that attendance in this supporting role was compulsory for students in both schools; the attendees were not a selected set of cheerleaders. In addition to students from the two elite private schools, I invited selected students from a public school catering to relatively poor students to participate in the experiment. These students were selected for having a particular interest in athletics.

The experiment proceeded in four stages.

Randomization. First, students were randomized to different sessions (separately by gender) with varying stakes for winning the subsequent relay race - either Rs. 500, Rs. 200 or Rs. 50 per teammate for winning the race. 500 rupees are approximately a month's pocket money for the oldest students in the sample, so the stakes are substantial. Within each session, students were asked to mix and introduce themselves to each other for about fifteen minutes. This ensured that students were able to accurately identify the difference in the social groups that the various participants belonged to. School uniforms made group membership salient, and debriefing suggested that students were quickly able to identify that the students from the public school were relatively poor, while the students from the private schools were wealthy. At the end of this phase, the following three stages were described to the students, and the experiment proceeded.

Ability Revelation and Team Selection. Students watched a series of one-on-one sprints, designed to reveal each runner's ability. In most cases, one runner was from the public school, while the other was from one of the private schools. However, some pairs included two students from private schools, or two from the public school. After each sprint, the rank (first or second) and times of the two runners were announced. Participants then privately indicated on a worksheet which of the two students they would like to have in their two-person team for a six-team relay race.

Choice Implementation and Relay Race. After the sprints were complete, six students were picked at random in each session to participate in the relay race, and one of their choices was randomly selected for implementation. The relay races were conducted and prizes were distributed as promised.

Socializing with Teammate. After the prizes were distributed, students were required to spend two hours socializing with their teammate. They were provided with board games and could also use playground equipment. However, they were not permitted to play in larger groups. This part of the experiment was described to the participants in advance, so they knew that their interactions with their selected teammate would exceed the few minutes spent on the relay race itself.

Reduced Form Results. The first reduced form finding is significant discrimination against the poor on average. I classify a wealthy student as having discriminated against the poor if he or she chooses a lower ability (i.e. slower) rich student from another school over a higher ability poor student from the public school.[17] Averaging over the different reward conditions, participants discriminate 19% of the time. These are not just mistakes, since the symmetric mistake of "discriminating" against a rich student occurs only 3% of the time. And when participants are choosing between two runners from the same (other) school, they pick the slower runner only 2% of the time. Thus, only poor students competing against rich students are systematically discriminated against.

The second finding is that discrimination decreases as the stakes increase. In the control school, 35% of choices exhibit discrimination against the poor in the Rs. 50 condition, but this falls to 27% when the reward is Rs. 200, and only 5% in the highest stakes condition of Rs. 500. This result is shown by the solid line in Figure 7, which I interpret as a demand curve for discrimination.

The third and most important finding is a reduction in discrimination from having poor classmates and study partners. Figure 7 shows that for each level of stakes, wealthy students with poor classmates are less likely to discriminate against the poor. In addition, the slope of the demand curve for discrimination is higher for students with poor classmates. Figure 8(a) depicts the difference-in-differences estimates graphically by plotting rates of discrimination by school and grade. In the treatment school, discrimination is substantially lower than in the control school in the treated grades 2 and 3, but not in grades 4 and 5. Figure 8(b) instead depicts the reduced form of the IV strategy, plotting rates of discrimination by whether the student has a name alphabetically adjacent to a poor students. Consistent with the difference-in-differences result, the figure shows that students with names close to a poor student (and therefore a higher likelihood of having a poor study partner) discriminate less.

Regression estimates are reported in Table 6. Column 1 shows that having a poor

[17] I do not consider a choice to be discriminatory if it involves choosing ones own schoolmate over a higher-ability poor student, since participants may prefer to partner with children they already know.

classmate reduces discrimination by 12 percentage points (se 5).[18] This effect is comparable to the 11 percentage point reduction in discrimination caused by increasing the stakes from Rs. 50 to Rs. 200 (an increase of about $3). Column 2 shows that having poor classmates has the biggest effect on discrimination in the lowest stakes condition (a 25 percentage point reduction). Column 3 reports the IV result that having a poor study partner reduces discrimination by 14.7 percentage points (se 8.8).[19]

The observed behavior is more consistent with taste-based discrimination than with statistical discrimination. When a separate sample of students is asked which of the two runners is more likely to be in the winning relay race, 98% pick the faster student. This implies that many students prefer a wealthy teammate even though they believe he makes them *less* likely to win, a fact inconsistent with a simple model of statistical discrimination. This is not surprising, since the experiment was designed with the intention of measuring taste-based discrimination. The clear signals of ability provided by the sprints additionally make statistical discrimination unlikely. And the fact that participants are forced to actually spend time socializing with their teammates - as is often the case when hiring colleagues or employees - provides a natural setting for taste-based discrimination.

Since the experiment does not simulate a market with a wage offered to teammates, I cannot directly test the classic prediction of taste-based discrimination that only the marginal employer's tastes matter. However, the fact that about 35% of control students discriminate in the low stakes condition suggests that at least a third of wealthy students dislike having a poor teammate (relative to a wealthy teammate).

Model and Structural Estimation. The reduced form results provide evidence of a reduction in discrimination. But they do not inform us of the magnitude of the distaste that wealthy students have for partnering and socializing with a poor child, nor how much this is changed by having poor classmates. In order to estimate these quantities, I structurally estimate a simple model of discrimination.

[18]Since the discrimination experiment has wealthy students from only two schools, I do not attempt to cluster standard errors at the school level. Instead, I report unclustered standard errors and, as a robustness check, cluster at the school-by-grade level (8 clusters) using the wild cluster bootstrap method.

[19]The treatment school uses alphabetic order to assign study partners. Since the sample for this experiment does not include any other treatment schools which do not use such a rule, I directly use alphabetic adjacency to a poor student as the instrument for having a poor student in one's study group.

Model. Suppose the decision-maker has expected utility:

$$U_t = p_t M + S_t \tag{3}$$

where p_t is the probability of winning the race with teammate t, M is the monetary reward for winning the race and S_t is the utility from socially interacting with teammate t. I assume that teammates are of two types, $t \in \{R, P\}$, where R denotes a rich student and P denotes a poor student.

Then, she chooses the rich teammate if

$$p_R M + S_R > p_P M + S_P$$

$$\Leftrightarrow S_R - S_P > (p_P - p_R)\, M$$

In the absence of a particular distaste for having a poor teammate, $S_P = S_R$. And in the absence of statistical discrimination, rich and poor students with the same performance in the sprint would be perceived to be equally able, $p_P = p_R$. Define $D_{poor} \equiv S_R - S_P$ as the distaste for interacting with a poor student (relative to a rich student), and $\delta_{poor} \equiv p_P - p_R$ as the perceived *increase* in probability of winning from having a poor teammate, provided the poor student won the ability-revelation sprint. Then, the decision-maker discriminates against a poor student if:

$$D_{poor} > \delta_{poor} M \tag{4}$$

Similarly, in the case where the rich student wins the sprint, we can define the increase in probability of winning from choosing the rich teammate, δ_{rich}.

In order to estimate the model, I impose the following distributional assumption: D_{poor} is distributed normally with mean μ_D^T and standard deviation σ_D^T, separately for students from treated classrooms ($T = 1$) and untreated classrooms ($T = 0$). Consistent with the fact that 98% of students state that the winner of the ability sprint is more likely to win the relay race (regardless of whether the winner was rich or poor), I additionally impose the assumption of no statistical discrimination, $\delta_{poor} = \delta_{rich} \equiv \delta$.

Then, the parameters to be estimated are: (i) μ_D^1 and μ_D^0, the average distaste for having a poor teammate amongst students with and without poor classmates, respectively; (ii) σ_D^1 and σ_D^0 , the standard deviations of the distribution of distaste; (iii) δ, the increase in probability of winning from choosing the teammate who won the ability-revelation sprint.

I estimate these parameters using a classical minimum distance estimator. Specifically, the estimator solves $\text{Min}_\theta\, (m(\theta) - \hat{m})' \, W \, (m(\theta) - \hat{m})$, where $\hat{m}$ is a vector of the empirical moments and $m(\theta)$ is the vector of theoretically predicted moment for parameters θ. The weighting matrix W is the diagonalized inverse of the variance of each moment; more precisely estimated moments receive greater weight in the estimation.

The moments for the estimation are the following: (i) The probability of discriminating against a higher-ability poor student, separately by stakes $M \in \{50, 200, 500\}$ and treatment status $T \in \{0, 1\}$, and (ii) The probability of discriminating against a higher-ability rich student, by stakes M and treatment status T. The empirical moments are simply shares of students observed to discriminate in each condition, estimated by an uncontrolled OLS regression.

Identification. All 5 parameters are jointly identified using the 12 moments. The intuition for the identification is straightforward. Conditional on δ, the exogenous variation in the stakes M pins down the mean μ_D and standard deviation σ_D of the distribution of distaste D. Conditional on the distribution of D, the perceived increase in probability of winning from choosing a high-ability teammate (δ), is identified from comparing the probabilities of choosing (say) the poor student when he wins versus loses the ability-revelation sprint.

Estimates. The lower panel in Table 7 reports the empirical and fitted values of the moments. The model overall does a good job of fitting the moments, with the exception of slightly over-predicting discrimination against the poor in the lowest stakes condition. Table 7 also reports the structural estimates of the parameters. The perceived increase in probability of winning from choosing a high-ability teammate is imprecisely estimated, $\delta = 0.08$ (se 0.1). Students without poor classmates are estimated to feel an average distaste for having a poor teammate of μ_D^0 =Rs. 37 (se Rs. 4.4), with a standard deviation σ_D^0 = Rs. 6 (se Rs. 1.9). In contrast, treated students are estimated to have a substantially lower distaste of μ_D^1 =Rs. 26 (se Rs. 4.8) and a similar standard deviaion, σ_D^1 = Rs. 5 (se Rs. 2.1). The difference in average distaste of Rs. 11 is significant at the 10% level, and constitutes a 30% reduction relative to students without poor classmates.

5.2 Willingness to Play

To shed more light on the observed reduction in discrimination in the team-selection experiment, I directly test wealthy students' tastes for socially interacting with poor children. I do so by inviting them to play dates at neighborhood schools for poor children. The play dates were motivated as an opportunity to make new friends, and involved two hours of games and playground activities. In order to measure tastes, I elicited incentivized measures of wealthy students' willingness to accept to attend these play dates. I find that having poor classmates in school makes wealthy students substantially more willing to play with other poor children.

Protocol. First, students were informed in school about the play dates. The play date was presented to them as an opportunity to make new friends in their neighborhood. The host school was named and described, and the experimenter showed the students a photograph of the school. The play dates all occurred on a weekend morning, and the students were informed about them approximately two weeks before the play date.

After answering students questions about the planned play dates, I elicited their willingness to accept - the payment they required - to attend the play date.[20] I employed a simple Becker-Degroot-Marschak mechanism, where students were presented with a decision sheet showing possible levels of payments for attending the play date. For each such price level, they were asked to indicate whether they would like to attend the play date. Then, a

[20]Pilot work revealed that students generally find the play dates unattractive – nearly all students expressed a negative willingness to pay. This is unsurprising, given that the play dates were held on weekends, which are surely a precious part of a child's week. The opportunity cost of attending anecdotally included watching television and playing with existing friends.

numbered ball was drawn from a bag, and their decision corresponding to that price was implemented. In particular, if they had indicated they would like to attend for the drawn price, their name was written down on a list, and they were provided an invitation form to take home to their parents.[21] The entire procedure was first explained to the students verbally, and then they played three practice rounds, at which point they appeared to understand the decision well.

Results. The key finding is that students become more willing to socialize with poor children if they already have poor classmates. Figure 9 shows the results of the two identification strategies graphically. Panel (a) plots average willingness to accept by school type (control, treatment and delayed treatment) and grade. For treatment schools, willingness to accept is lower than control schools only in the treated grades 2 and 3, but not in the untreated grades 4 and 5. A similar pattern is visible for delayed treatment schools, in which only grade 3 is treated. A lower willingness to accept indicates greater willingness to socially interact with poor children. Figure 10 plots the resulting supply curves for attending the play date, separately for students with and without poor classmates.

Figure 9(b) depicts the IV strategy graphically, by plotting average willingness to accept by whether the wealthy student has a name alphabetically adjacent to any poor children. We see that having an alphabetic neighbor only increases willingness to play in schools which use the alphabetic assignment rule. This suggests that personally interacting with a poor student through assignment to a common study group reduces a wealthy students' distaste for interacting with other poor children.

Finally, Table 8 reports numerical estimates of the effects, using the specifications discussed in Section 3. I find that having poor classmates decreases willingness to accept (i.e. increases willingness to play) by Rs. 7 (se 1.1) on a base of Rs. 37, a decrease of 19%. The effect is highly significant ($p<0.01$) even when clustering standard errors at the school level, and the result is similar in the restricted sample of younger siblings. Having a poor study partner reduces willingness to accept by 24% (se 9%).

[21] Parents, of course, had the ability to veto their childrens' choice to attend the play date, and did so in about 35% of cases. Since I wish to isolate the child's tastes rather than the parents, I use the elicited willingness to pay (or accept) as the outcome measure. Using actual attendance of play dates as an alternative outcome, I find similar but muted effects.

In contrast, I find no effects on willingness to attend play dates with rich students. In August 2013, I conducted a parallel experiment in a smaller sample as a placebo test. Students now had the opportunity to spend two hours playing with other wealthy students from a control private school. While the estimates are less precise due to a smaller sample size, Online Appendix Table 2 reports no average effect on willingness to play with rich students.

6 Academic Outcomes

One concern with integrating disadvantaged students into elite schools is that wealthy students' academic outcomes may suffer as a result. This concern is motivated by the large literature studying peer effects in education, which has sometimes found substantial peer effects (Hoxby 2000, Hanushek et al. 2003) and at other times no evidence that peers affect test scores (Angrist and Lang 2004, Imberman et al. 2009). Classroom disruptions by poorly disciplined students have been proposed to an key mechanism underlying any negative effects (Lazear 2001, Lavy and Schlosser 2011, Figlio 2007). Indeed, principals in the schools I studied reported being particularly concerned about classroom disruptions and learning. In this section, I therefore turn attention to estimating the impact of poor students on the learning and classroom discipline of their wealthy peers.

6.1 Learning

To measure effects on learning, I conduct simple tests of learning in English, Hindi and Math. With the assistance of teachers in a non-sample school, I first assembled a master list of questions from standard textbooks for grades 1 through 7. Students in each grade were asked to answer a set of questions considered appropriate for their grade, and a smaller set of questions at lower and higher grade levels. The test was designed to be quick and easy to implement, and therefore provides a somewhat coarse measure of learning. Nonetheless, it provides comparable test scores across different schools in the absence of any existing system of standardized testing in primary schools. I normalize the test score in order to provide standardized effect sizes.

I find that poor students do worse than rich students on average, but with substantial heterogeneity. Poor students score 0.32 standard deviations (s.d.) worse than wealthy students in English, 0.12 s.d. worse in Hindi and 0.24 s.d. worse in Math. The lower average learning levels of poor students make the possibility of negative peer effects very real. But the variance in poor students' test scores is similar to that of wealthy students; there is thus plenty of overlap in the distributions of academic achievement. For example, poor students have weakly higher scores than 40% of their wealthy study partners even in English.

Table 9 reports regression estimates of the effects of poor students on their wealthy classmates' test scores. The first two columns show a small and insignificant effect on an equally weighted average of standardized scores in the individual subjects. I also consider effects on each subject in turn. Most importantly, I estimate a 0.09 standard deviation reduction in average test scores in English in treated classrooms, significant at the 10% level. The coefficient on the IV regression of English scores on having a poor study partner is also negative, although quite imprecisely estimated. In contrast, I find no effects of poor classmates on wealthy students' test scores in Hindi or Math. The online appendix reports similar effects for the likely less-selected subsample of younger siblings.

Considering the results for the different subjects together, the overall pattern is one of mixed but arguably modest effects on academic achievement. The only negative effect is on English scores. This is consistent with English being the subject with the largest achievement gap between rich and poor students, perhaps due to the fact that poor students almost exclusively report speaking only Hindi at home. But substantial learning gaps also exist in Math and (to a lesser extent) Hindi, and yet I detect no negative peer effects in those subjects. These latter non-effects are consistent with those of Muralidharan and Sundararaman (2013), who find no effects on the achievement of existing students in private schools in rural India when initially lower-achieving voucher-recipients enter their schools.[22]

[22]But note that Muralidharan and Sundararaman (2013) study a quite different setting. I study the effects on quite wealthy students attending elite private schools in urban Delhi, while they study the effects on students attending relatively modest private schools in rural Andhra Pradesh, where the social and economic disparity between the existing and incoming students is likely much smaller.

An additional mechanism, supported by anecdotal reports from teachers, is that the presence of poor students causes conversations between students to shift more from English to Hindi, which might well reduce wealthy students' fluency in English. However, I find no evidence of a significant increase in Hindi test scores.

6.2 Discipline

To measure classroom discipline, I ask teachers to report whether each student has been cited for any disciplinary infractions in the past six months[23]. I find that 22% of wealthy students have been cited for the use of inappropriate language (that is, swearing) in school, but only about 6% are cited for disruptive or violent behavior. Poor students are no more likely than rich students to be disruptive in class, but they are 12 percentage points more likely to be reported for using offensive language.

Table 10 reports regression estimates of the effects of poor students on disciplinary infractions by their wealthy classmates. The results suggest that having poor classmates increases the share of wealthy students reported for using inappropriate language by 7.5 percentage points (se 3.7). Having a poor study partner causes an even larger increase of 10 percentage points (se 6), an increase of about 45%. In contrast, I find precisely estimated zero effects on the likelihood of being cited for disruptive or violent behavior.

The finding that poor students do not make their wealthy classmates more disruptive – and indeed are no more disruptive than wealthy students themselves – is consistent with the absence of negative peer effects on Hindi and Math scores. In the context I study, concerns about diversity affecting test scores through indiscipline appear to be unwarranted. In contrast, the effects on inappropriate language use are substantial, although possibly without implication for learning and future academic achievement.

[23]This question was asked at the end of the academic year

7 Discussion

Section 4 established that having poor classmates caused increases in prosocial behavior by wealthy students. A key finding was that generosity in the lab increased not only towards poor recipients, but also towards the rich. In addition, having poor classmates made wealthy students systematically choose more equal distributions of payoffs in dictator games. These findings suggest that what occurred was not just an increase in charitable feelings towards the poor, but instead a deeper change in notions of fair or desirable behavior towards others. This result contributes to a recent literature studying the factors that influence social preferences[24], and provides the first evidence that peers at school can shape a student's social preferences.

Section 5 showed that having poor classmates causes a reduction in discrimination by wealthy students against the poor, and increases their "willingess to play" with poor children. However, a smaller complementary experiment measuring tastes for attending play dates with *rich* children found no effects. Unlike the generalized increase in generosity in the dictator games, then, the effect on tastes for new social interactions is directed: familiarity breeds fondness. This set of results contributes to the economics literature on inter-group contact, and is most closely related to recent research using randomized roommate assignments by Burns et al. (2013), who measure effects on racial prejudice using psychological tests, and effects on trust using lab experiments. I additionally use a field experiment to measure discrimination, directly elicit tastes for social interaction using play dates, and study effects on prosocial behavior and academic outcomes.[25]

For both classes of social behaviors, I find that personal interactions between wealthy and poor students are an important driver of the results. Scaling the estimated effect of having a poor study partner by the relevant share of wealthy students with poor partners (68%), I find that having a poor study partner alone explains 70% of the overall classroom-level increase in willingness to play with a poor child, and 38% of the increase in generosity towards the poor. These are likely underestimates of the importance of personal interactions,

[24] Jakiela et al. (2010), Voors et al. (2012) and Fisman et al. (2012)

[25] Unlike much of the related literature in social psychology (Pettigrew and Tropp, 2006), I provide causal rather than correlational evidence, and use incentivized behavior to measure outcomes.

since membership in study groups underestimates total personal interactions.

It is also worth discussing the magnitudes of the estimated effects. The effects on social behaviors appear to be large. For example, the 0.45 standard deviation increase in sharing with the poor is similar to the causal effect of a one standard deviation increase in test scores in Jakiela et al. (2010). The estimated 30% reduction in distaste for having a poor teammate (relative to a rich one) is similarly substantial. In contrast, the overall effect on test scores is small (-0.02 standard deviations), although the reduction in English language scores of 0.09 s.d. is economically meaningful and comparable to the strength of peer effects on reading scores in Hoxby (2000).

8 Conclusion

In this paper, I exploit a natural experiment in education policy in India to estimate how greater economic diversity in classrooms affects wealthy students. I assemble a variety of evidence from field and lab experiments, administrative data and tests of learning to reach three main findings. The first finding is that having poor classmates makes wealthy students more prosocial and concerned about equality, and thus more generous towards others. The second finding is that wealthy students become more willing to socially interact with poor children outside school, and thus exhibit less taste-based discrimination against the poor. The third finding is of mixed but overall modest impacts on academic outcomes, with negative effects on English language learning but no effect on Hindi or Math. Thus, my overall conclusion is that increased diversity in the classroom led to large and arguably positive impacts on social behaviors, at the cost of negative but modest impacts on academic outcomes.

One implication of this paper is that school policies involving affirmative action, desegregation and tracking should be evaluated not only on learning outcomes - which are of unarguable importance - but also on other economically important outcomes related to social behaviors. More generally, my findings support the view that increased interactions across social groups, perhaps especially in childhood, can improve inter-group behaviors.

This has implications for diverse and polarized societies, where the costs of social divisions are thought to be high. Finally, my findings are of relevance to the planned expansion of this policy across India over the next few years, where it will touch many of India's 300 million school children.

One limitation of this paper is that due to the recency of the policy experiment, it does not study important long-term outcomes and behaviors such as political beliefs, social interactions as adults, and marriage market choices. Another limitation is the very particular nature of the sample - wealthy students in elite private schools in Delhi. Finally, this paper does not examine the effects on poor students of attending elite private schools, which might have profound consequences for their academic achievement and life outcomes. In ongoing and future work, I hope to address these shortcomings, as well as examine other aspects of the impacts of diversity in schools.

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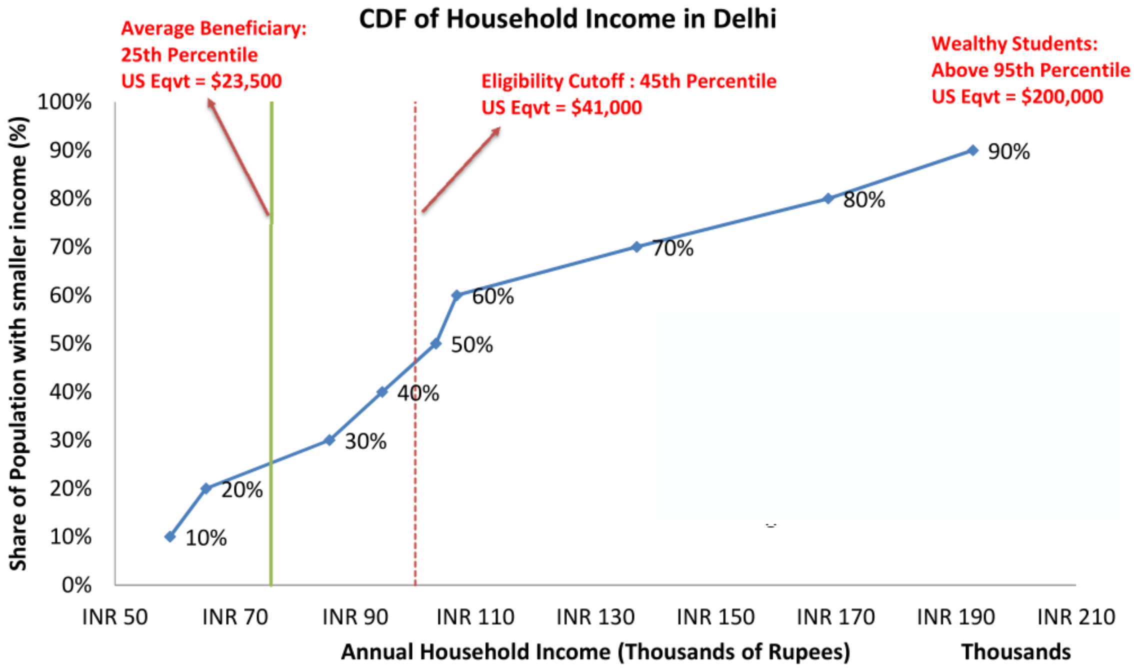
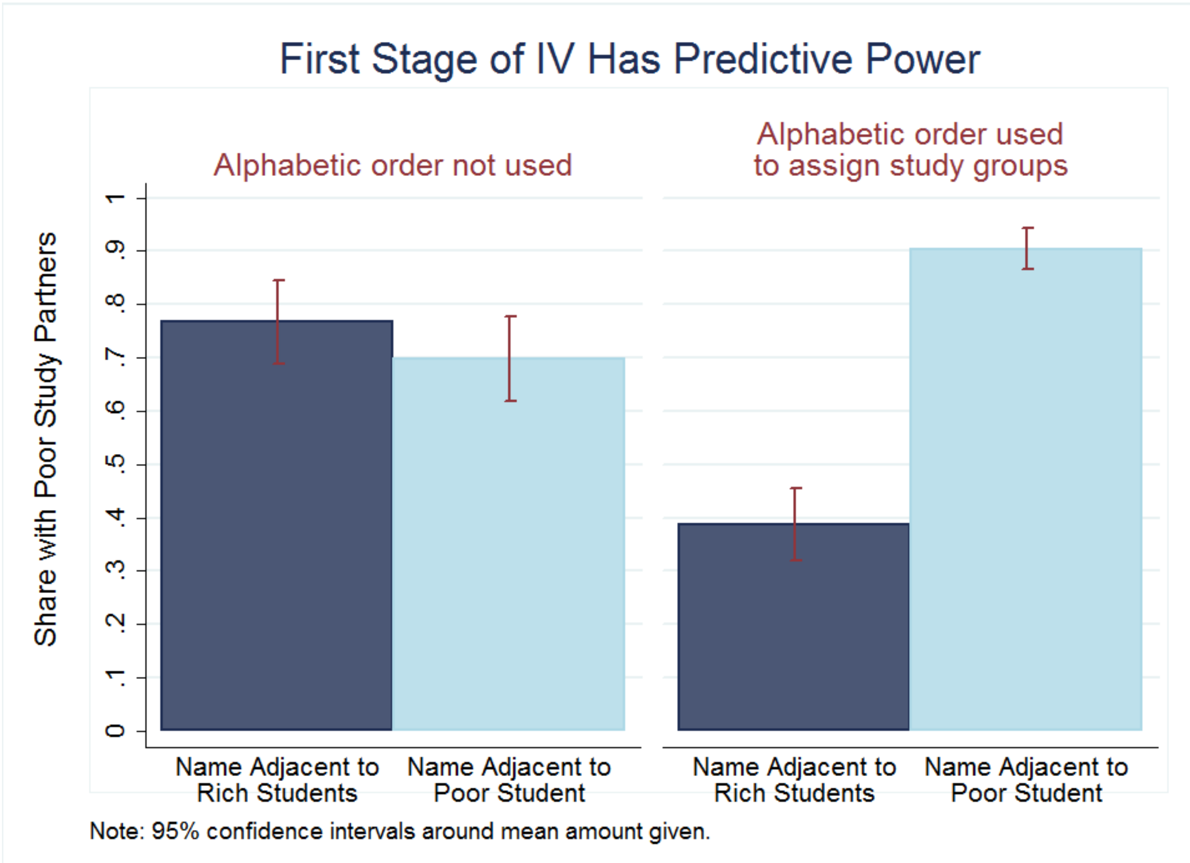
Figure 1. Program eligibility and the household income distribution in Delhi



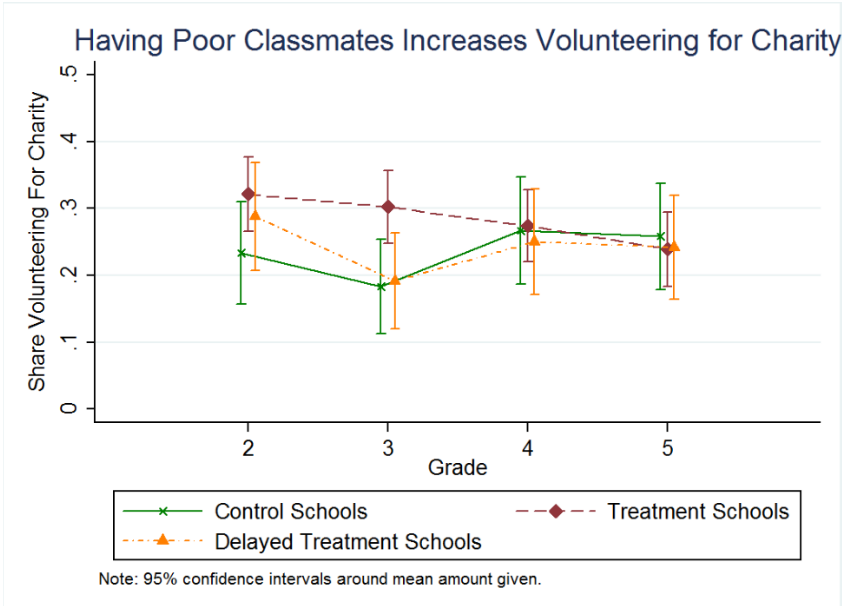
Note: This graph is based on the household consumption distribution reported in NSS-2010, with consumption amount converted to income levels using the ratio of household income to household consumption for urban Indian households reported in IHDS-2005.

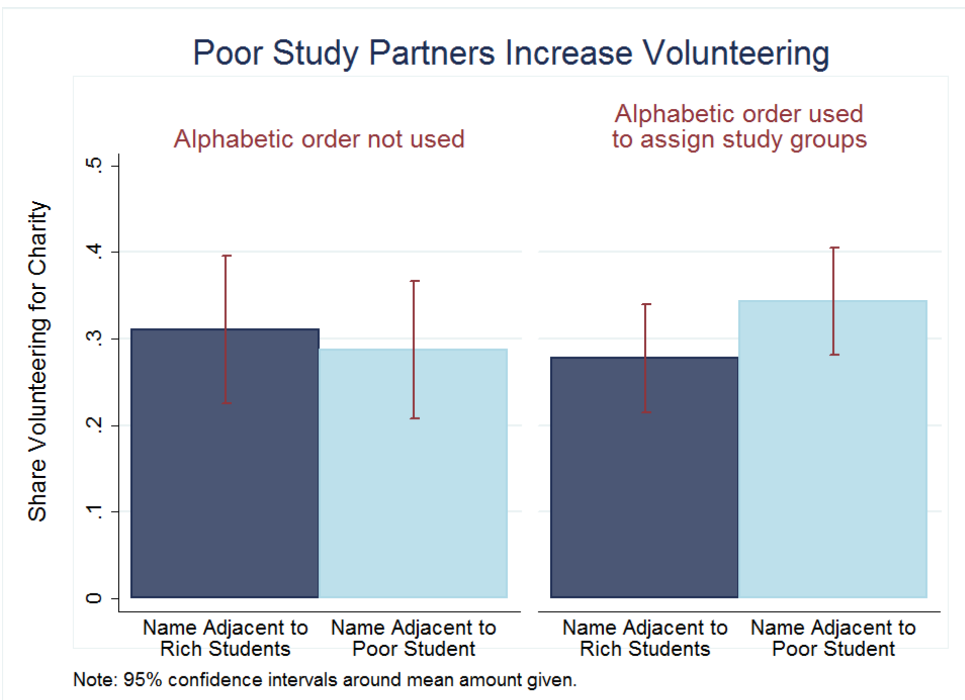
Figure 2. First Stage of Instrumental Variable



Note: Having a name alphabetically adjacent to a poor student predicts having a poor student in one's study group, but only in the schools which explicitly use alphabetic ordering

Figure 3. Volunteering for Charity

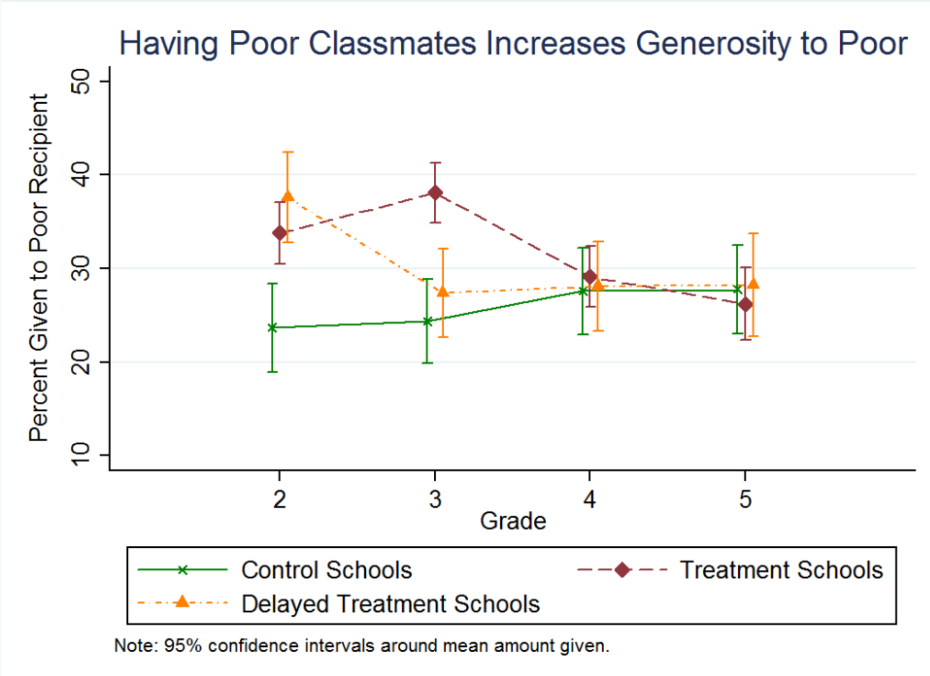


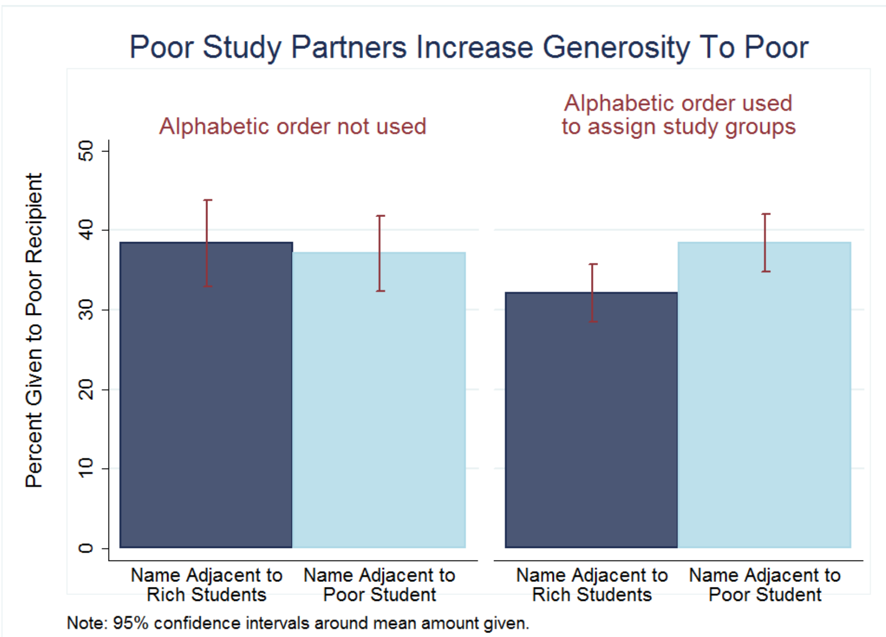


Note: The top panel plots the share of wealthy students who participate in voluntary charitable fundraising activities in school, separately by type of school. Error bars plot 95% confidence intervals (unclustered).

The bottom panel plots share volunteering by whether the subject has a name alphabetically adjacent to any poor students, separately by whether schools use alphabetic order to assign study groups.

Figure 4. Giving to Poor Recipients in Dictator Game.

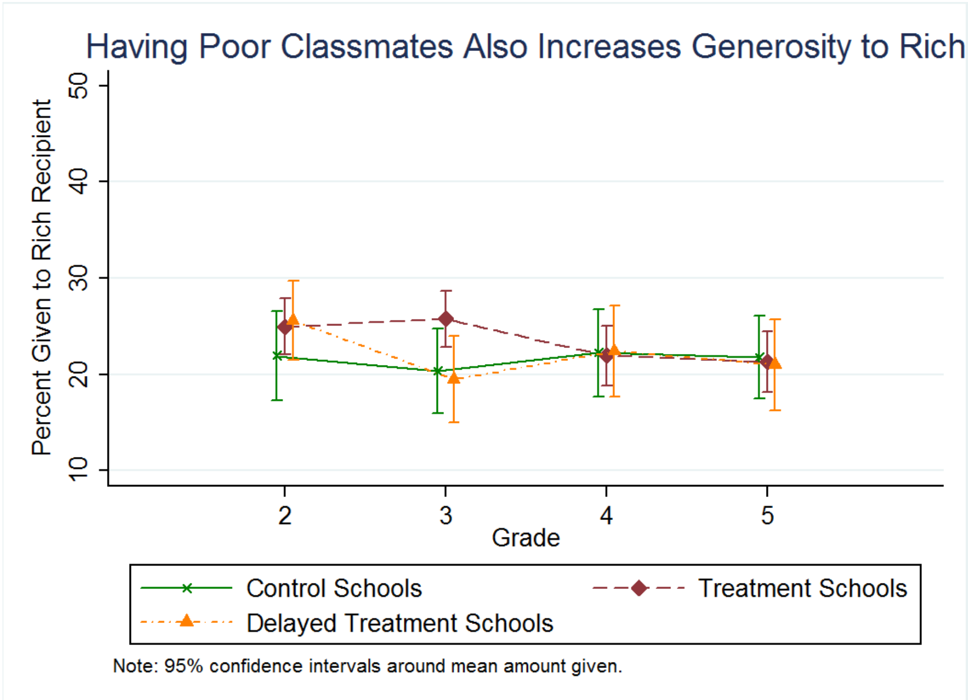


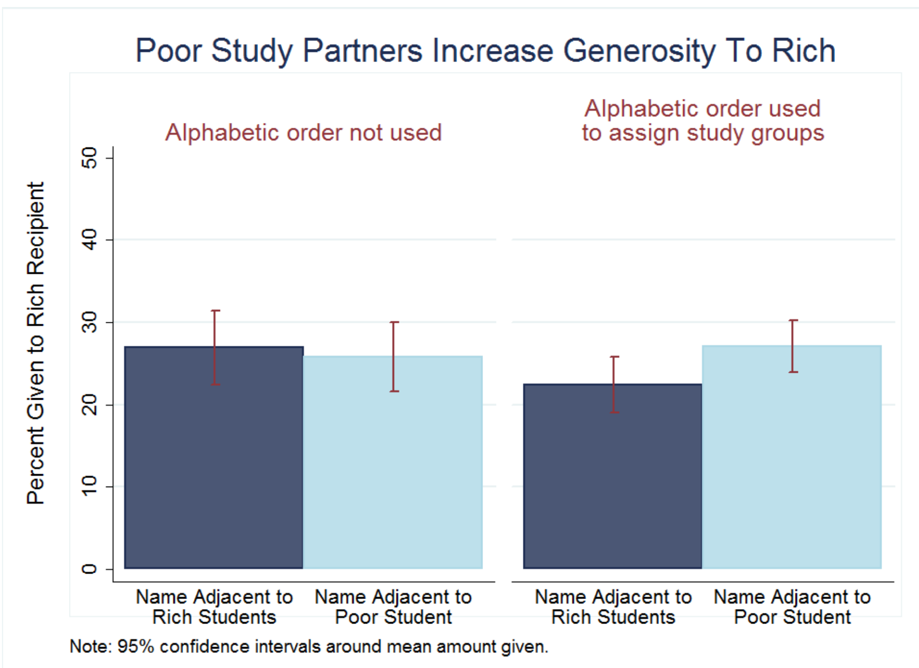


Note: The top panel plots the share given to a poor recipient against grade, separately by type of school. 95% confidence intervals (unclustered) for the mean are included. The figure shows that giving is higher in treatment and delayed treatment schools only in the treated grades.

The bottom panel plots the share of the endowment given to the poor recipient by whether the subject has a name alphabetically adjacent to any poor students, separately by whether schools use alphabetic order to assign study groups. This figure thus graphically depicts the reduced form regression of generosity to the poor on the excluded instrument.

Figure 5. Giving to Wealthy Recipients in Dictator Game.

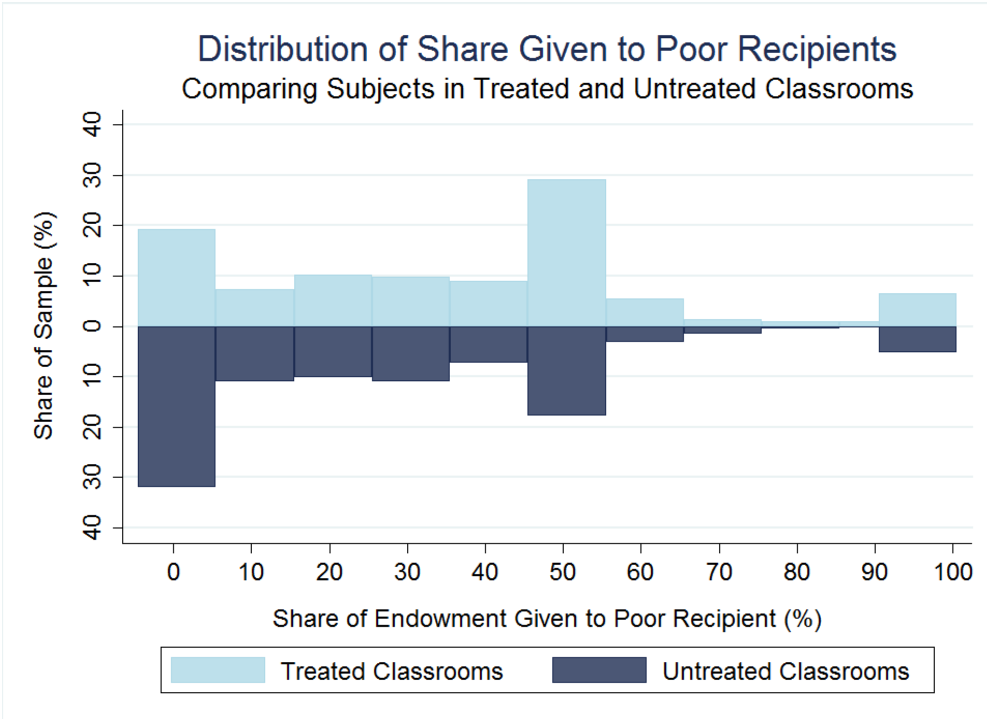


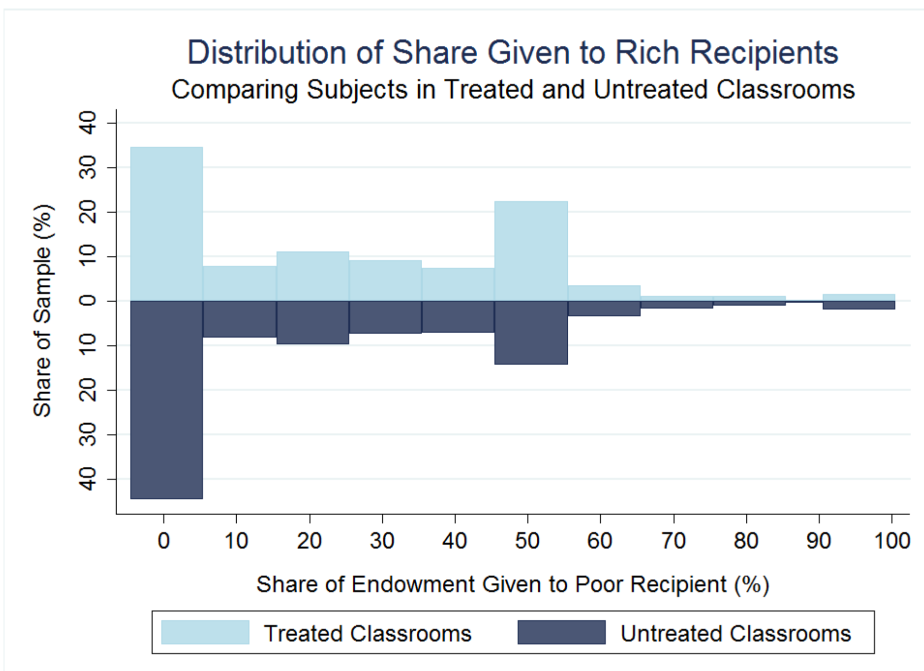


Note: The top panel plots the share given to a wealthy recipient against grade, separately by type of school. 95% confidence intervals (unclustered) for the mean are included.

The bottom panel plots the share of the endowment given to the poor recipient by whether the subject has a name alphabetically adjacent to any poor students, separately by whether schools use alphabetic order to assign study groups.

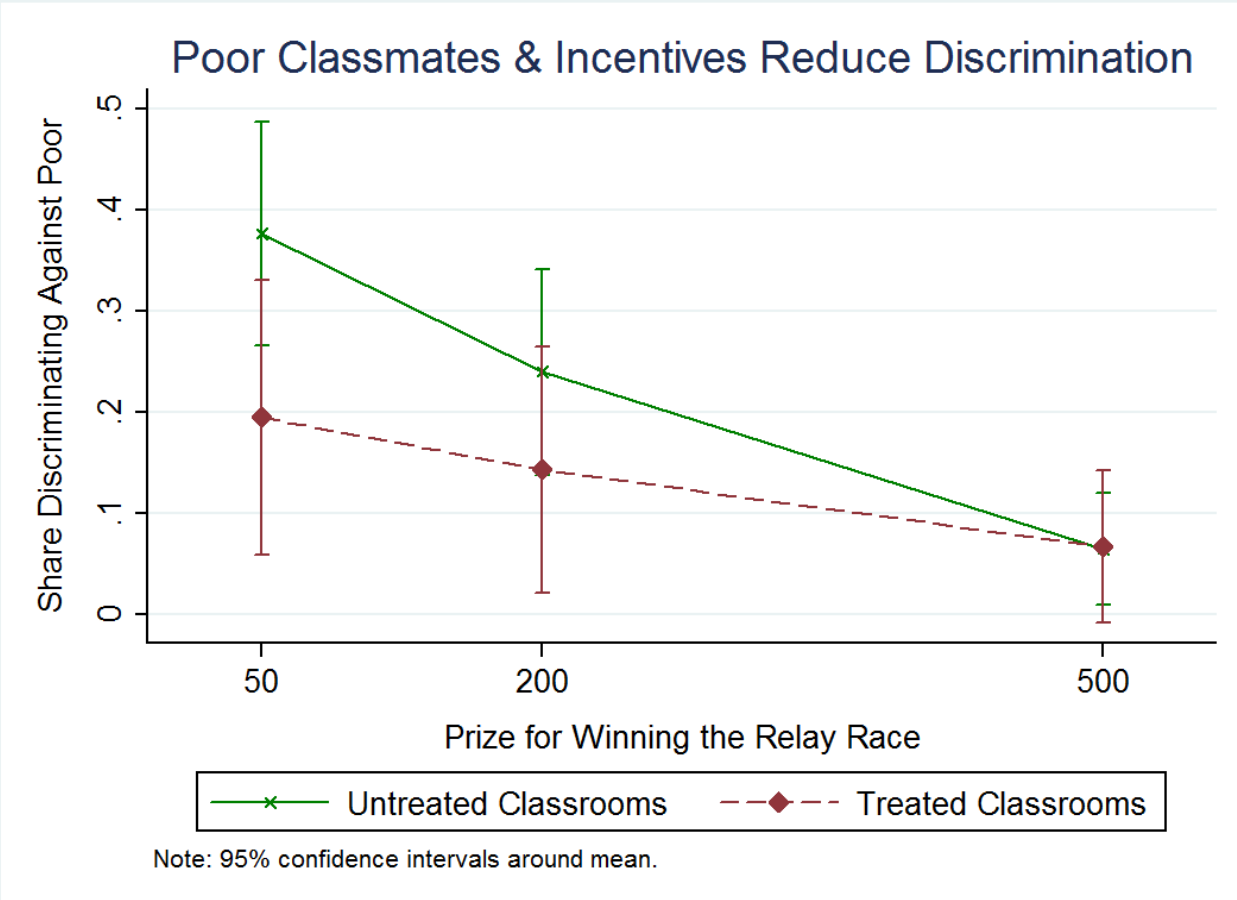
Figure 6. Distribution of Giving in Treated and Untreated Classrooms





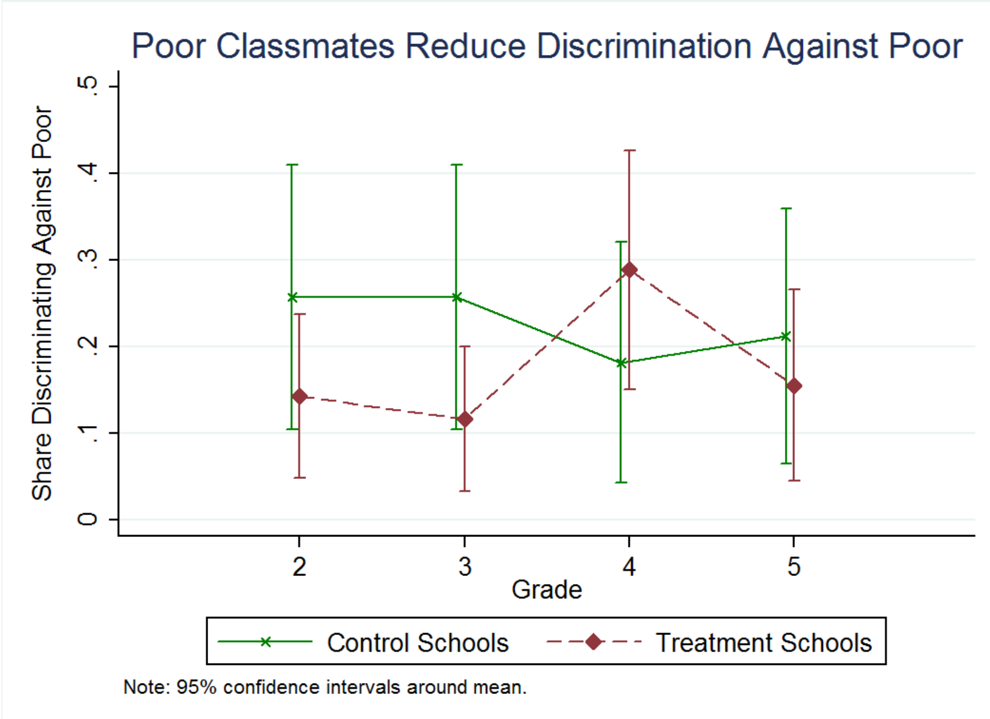
Note: The top panel shows the distribution of giving by wealthy students to poor recipients, separately for whether they have poor classmates (red bars) or not (blue bars). The bottom panel shows the same results for giving to wealthy recipients instead of poor recipients.

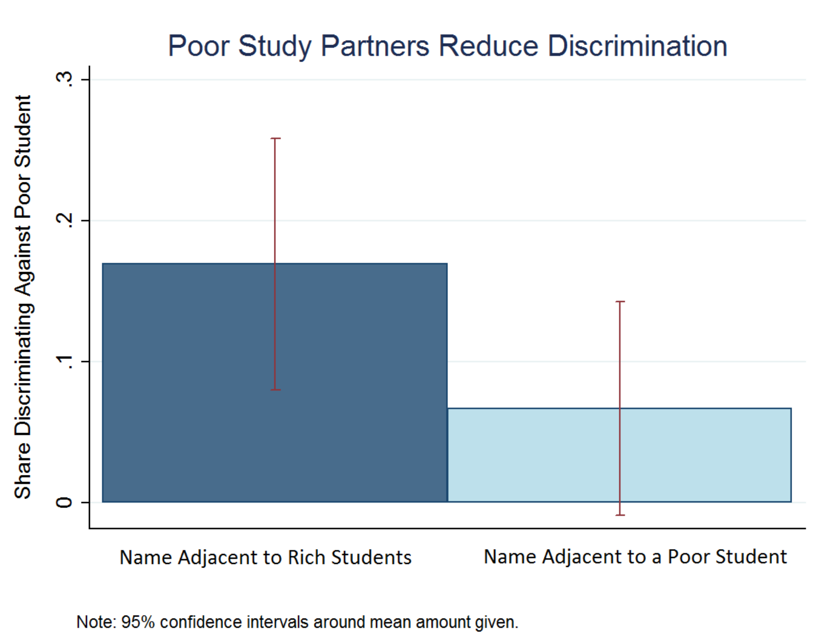
Figure 7. Demand Curve for Discrimination



Notes: This graph plots the share of wealthy students who discriminate against the poor (on the y axis) by the stakes of the decision, separately by whether the student has poor classmates (dotted red line) or not (solid green line). A student is classified as having discriminated against the poor if he chooses a lower-ability rich student over a higher-ability poor student.

Figure 8. Discrimination Against the Poor

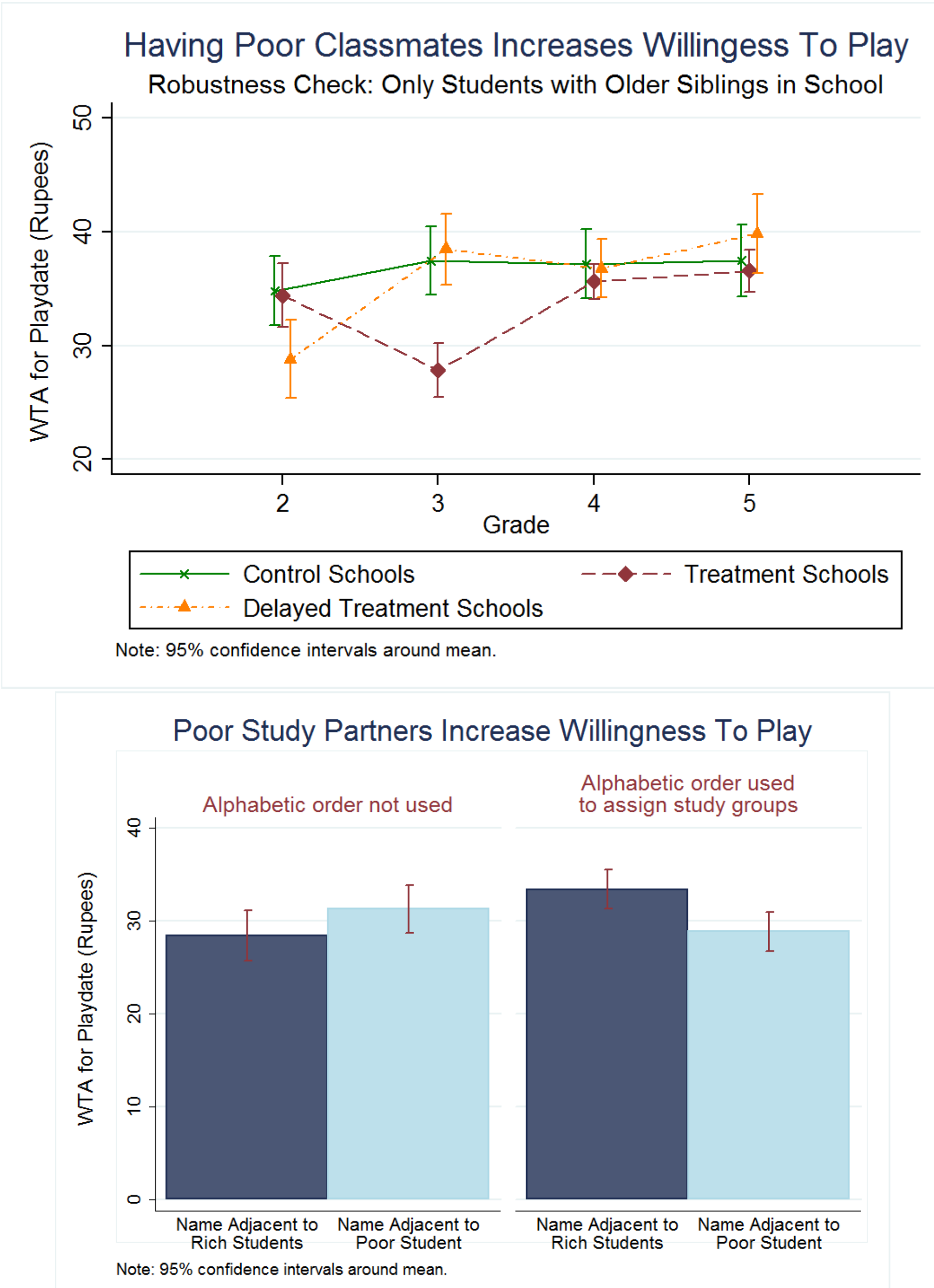




Notes: The top panel plots the share of wealthy students who discriminate against the poor (on the y axis) by grade (on the axis), separately by school type. The control school is represented by the solid green line, while the treatment school is represented by the dotted red line. Error bars plot 95% confidence intervals (unclustered).

The bottom panel plots discrimination rates by whether the participant has a name alphabetically adjacent to any poor students, only for the treatment school.

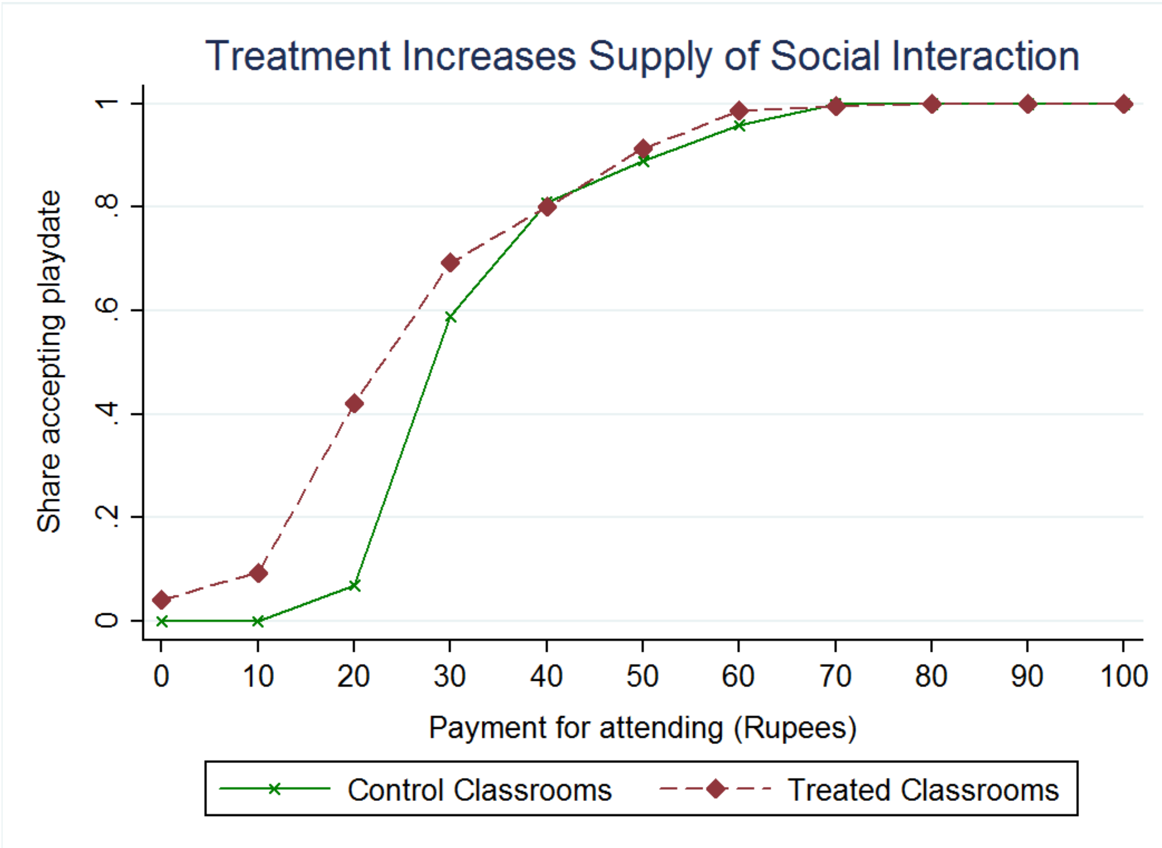
Figure 9. Willingness to Play



Note: The top panel plots wealthy students' average Willingess To Accept a play date with poor children, separately by type of school. Error bars plot 95% confidence intervals (unclustered).

The bottom panel plots "willingess to play" by whether the subject has a name alphabetically adjacent to any poor students, separately by whether schools use alphabetic order to assign study groups.

Figure 10. Supply Curve for Attending Play Date



Note: This graph plots the share of wealthy students willing to attend the play date with poor children (on the y axis) for each given level of payment for attending the play date (on the x axis). The solid green line represents this supply curve for wealthy students without poor classmates, while the dotted red line represents wealthy students who do have poor classmates.

Table 1. First Stage of IV

	Dependent Variable: Indicator for having at least one poor student in one's study groups
Instrument	(1) Has Poor Study Partner
(Name Adjacent to Poor Student) x (School Uses Alphabetic Rule)	0.519*** (0.0808)
Constant	0.456*** (0.0793)
N	677
F-Statistic	41.25

Note: Standard errors in parentheses. This table reports the results from regressing an indicator for whether the student has a poor study group partner on the excluded instrument, the direct effect of having a name immediately adjacent to at least one poor student, school and grade dummies, and a vector of second stage control variables. The F statistic reports results of a Wald test of a zero coefficient on the excluded instrument.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2. Volunteering for Charity

	Dependent Variable: Indicator for Volunteering for Charity			
	(1)	(2)	(3)	(4)
Specification**:**	DiD	DiD	IV	DiD+IV
Sample:	Full Sample	Younger Sibs	Treated Class	Full Sample
Treated Classroom	0.105***	0.0715**		-0.0409
	(0.0244)	(0.0305)		(0.0766)
Has Poor Study Partner			0.130	0.208**
			(0.0866)	(0.0881)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade	School, Grade	Classroom	School, Grade
p-value (CGM)	< 0.01	0.06	.	.
Control Mean	0.242	0.220	0.245	0.242
Control SD	0.428	0.414	0.431	0.428
N	2017	1143	677	2017

Note. Standard errors in parentheses. This table reports results from linear probability models for the likelihood of volunteering for a charity. **Col 1** reports a difference-in-differences estimates of the effect of having poor students in one's classroom, with standard errors clustered at the school-by-grade level. The p-value reported in the table instead is calculated using clustering at the school level (k=14) using Cameron, Gelbach and Miller's wild-cluster bootstrap. **Col 2** reports the same specification as Col 1, but restricts the sample to students who have older siblings enrolled in the same school. **Col 3** reports IV estimates of the effect of having a poor study partner, and presents robust standard errors. **Col 4** reports a specification estimating both the classroom level effect using the difference-in-differences term and an additional effect of having a poor study partner, with standard errors clustered at the school-by-grade level. Individual **controls** include gender, age, mother's education, distance of student's home from school, and whether the student uses a private (chauffeured) car to commute to school.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3. Generosity towards Poor Students

	Dependent Variable: Share Given to Poor Recipient in Dictator Game (%)			
	(1)	(2)	(3)	(4)
Specification:	DiD	DiD	IV	DiD+IV
Sample:	Full Sample	Younger Sibs	Treated Class	Full Sample
Treated Classroom	12.22***	12.95***		8.747**
	(1.901)	(2.274)		(3.510)
Has Poor Study Partner			7.53**	12.08***
			(3.147)	(4.313)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade	School, Grade	Classroom	School, Grade
p-value (CGM)	< 0.01	< 0.01	.	.
Control Mean	27.12	26.75	33.77	27.12
Control SD	27.22	26.53	28.13	27.22
N	2015	1141	677	2015

Note. Standard errors in parentheses. This table reports regression results for giving in the dictator game when matched with a poor recipient. **Col 1** reports a difference-in-differences estimates of the effect of having poor students in one's classroom, with standard errors clustered at the school-by-grade level. The p-value reported in the table instead is calculated using clustering at the school level (k=14) using Cameron, Gelbach and Miller's wild-cluster bootstrap. **Col 2** reports the same specification as Col 1, but restricts the sample to students who have older siblings enrolled in the same school. **Col 3** reports IV estimates of the effect of having a poor study partner, and presents robust standard errors. **Col 4** reports a specification estimating both the classroom level effect using the difference-in-differences term and an additional effect of having a poor study partner, with standard errors clustered at the school-by-grade level. Individual **controls** include gender, age, mother's education, distance of student's home from school, and whether the student uses a private (chauffeured) car to commute to school.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4. Generosity towards Wealthy Students

	Dependent Variable: Share Given to Wealthy Recipient in Dictator Game (%)			
	(1)	(2)	(3)	(4)
Specification:	DiD	DiD	IV	DiD+IV
Sample:	Full Sample	Younger Sibs	Treated Class	Full Sample
Treated Classroom	5.861***	6.053***		2.867
	(1.135)	(1.742)		(2.801)
Has Poor Study Partner			8.342*	8.02**
			(4.479)	(3.866)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade	School, Grade	Classroom	School, Grade
p-value (CGM)	< 0.01	0.01	.	.
Control Mean	21.44	22.49	19.62	21.44
Control SD	25.08	26.07	22.56	25.08
N	2015	1141	677	2015

Note: Standard errors in parentheses. This table reports regression results for giving in the dictator game when matched with a rich recipient. **Col 1** reports a difference-in-differences estimates of the effect of having poor students in one's classroom, with standard errors clustered at the school-by-grade level. The p-value reported in the table instead is calculated using clustering at the school level (k=14) using Cameron, Gelbach and Miller's wild-cluster bootstrap. **Col 2** reports the same specification as Col 1, but restricts the sample to students who have older siblings enrolled in the same school. **Col 3** reports IV estimates of the effect of having a poor study partner, and presents robust standard errors. **Col 4** reports a specification estimating both the classroom level effect using the difference-in-differences term and an additional effect of having a poor study partner, with standard errors clustered at the school-by-grade level. Individual **controls** include gender, age, mother’s education, distance of student’s home from school, and whether the student uses a private (chauffeured) car to commute to school.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5. Egalitarian Preferences

Dependent Variable: Indicator for Choosing the more egalitarian option in a binary choice dictator game

	Equality Game (5,5) v (6,1)		**Disinterested Game 1** (0,4,4) v (0,8,3)		**Disinterested Game** 2 (0,4,4) v (0,12,0)	
	DiD Full Sample	IV Treated Class	DiD Full Sample	IV Treated Class	DiD Full Sample	IV Treated Class
	(1)	(2)	(3)	(4)	(5)	(6)
Treated Classroom	0.0933* (0.0525)		0.123*** (0.0476)		0.143*** (0.0321)	
Has Poor Study Partner		0.0516 (0.0908)		0.0648 (0.0718)		0.1052** (0.0517)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade	Classroom	School, Grade	Classroom	School, Grade	Classroom
p-value (CGM)	0.09	.	0.02	.	< 0.01	.
Control Mean	0.551	0.623	0.467	0.500	0.769	0.896
Control SD	0.498	0.486	0.499	0.501	0.422	0.306
N	2017	677	2017	677	2017	677

Note: Standard errors in parentheses. This table reports results of linear probability models of the likelihood of choosing the more equal or egalitarian of two options in three binary choice dictator games. Cols 1 and 2 report shares choosing (5,5) over (6,1). Cols 3 and 4 report shares choosing (0,4,4) over (0,8,3). Cols 5 and 6 report shares choosing (0,4,4) over (0,12,0). Odd numbered columns report difference-in-difference estimates of the effect of having poor students in one's classroom. In these columns, standard errors are clustered at the school-by-grade level, while the p-value reported in the table comes from clustering at the school level using Cameron, Gelbach and Miller's wild-cluster bootstrap. Even numbered columns report IV estimates of the effect of having a poor study partner, and present robust standard errors. Individual **controls** include gender, age, mother's education, distance of student's home from school, and whether the student uses a private (chauffeured) car to commute to school.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6. Discrimination Against Poor Children

Dependent Variable: Indicator for choosing lower-ability wealthy student over higher-ability poor student				
	(1) DiD-1 Full Sample	(2) DiD-2 Full Sample	(3) IV-1 Treated Class	(4) IV-2 Full Sample
Treated Classroom	-0.123**	-0.256***		
	(0.0466)	(0.0654)		
Prize = Rs. 200	-0.110**	-0.137**	-0.0582	-0.0415
	(0.0423)	(0.0540)	(0.0757)	(0.126)
Prize = Rs. 500	-0.250***	-0.314***	-0.126*	-0.101
	(0.0583)	(0.0498)	(0.0713)	(0.135)
(Treated Classroom) x (Prize = Rs. 200)		0.1153*		
		(0.0667)		
(Treated Classroom) x (Prize = Rs. 500)		0.186*		
		(0.0939)		
Has Poor Study Partner			-0.147*	-0.118
			(0.0885)	(0.156)
(Poor Study Partner) x (Prize = Rs. 200)				-0.0337
				(0.210)
(Poor Study Partner) x (Prize = Rs. 500)				-0.0510
				(0.227)
Fixed Effects	School, Grade	School, Grade	Classroom	Classroom
Control Mean	0.226	0.226	0.220	0.220
Control SD	0.419	0.419	0.418	0.418
N	342	342	116	116

Note: Standard errors in parentheses. This table reports results of linear probability models of the likelihood of discriminating: i.e. choosing a wealthy teammate despite the poor child winning the first round race. **Cols 1 and 2** report difference-in-difference estimates of the effect of having poor students in one's classroom, with robust standard errors. **Cols 3 and 4** report parallel IV estimates of the effect of having a poor study partner, with robust standard errors.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7. Structural Estimates

Estimates	Control		Treated	Difference
Mean distaste for poor teammate relative to rich (μ_D), in Rupees	37		26	-11*
	(4.4)		(4.8)	(6.5)
Std. Dev. of distaste for poor teammate relative to rich (σ_D), in Rupees	6		5	-1
	(1.9)		(2.1)	(2.8)
Boost in probability of winning from choosing high ability teammate (δ)		0.08		
		(.1)		

Moments (Probability of Discriminating	Against Poor		Against Rich	
	Empirical	Predicted	Empirical	Predicted
Control Students:				
Stakes = Rs. 50	35%	46%	4%	11%
Stakes = Rs. 200	27%	24%	3%	0%
Stakes = Rs. 500	5%	1%	2%	0%
Treated Students:				
Stakes = Rs. 50	20%	32%	6%	16%
Stakes = Rs. 200	15%	18%	4%	1%
Stakes = Rs. 500	6%	0%	2%	0%

Notes: Estimates from minimum-distance estimator using the moments shown, and weights given by the inverse of each moment's variance. Standard errors are in parentheses.

Table 8. Willingess to Play with Poor Children

	Dependent Variable: Willingness to Accept to Attend Play Date (Rupees)			
	(1)	(2)	(3)	(4)
Specification**:**	DiD	DiD	IV	DiD+IV
Sample:	Full Sample	Younger Sibs	Treated Class	Full Sample
Treated Classroom	-6.961***	-6.262***		-1.891
	(1.075)	(1.698)		(2.355)
Has Poor Study Partner			-7.878***	-7.226**
			(2.889)	(3.272)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade	School, Grade	Classroom	School, Grade
p-value (CGM)	< 0.01	0.02	.	.
Control Mean	36.84	40.02	32.08	36.84
Control SD	11.94	13.67	14.75	11.94
N	2017	1143	677	2017

Note: Standard errors in parentheses. This table reports regression results for wealthy students' minimum willingness to accept to attend a play date with poor children. **Col 1** reports a difference-in-differences estimates of the effect of having poor students in one's classroom, with standard errors clustered at the school-by-grade level. The p-value reported in the table instead is calculated using clustering at the school level (k=14) using Cameron, Gelbach and Miller's wild-cluster bootstrap. **Col 2** reports the same specification as Col 1, but restricts the sample to students who have older siblings enrolled in the same school. **Col 3** reports IV estimates of the effect of having a poor study partner, and presents robust standard errors. **Col 4** reports a specification estimating both the classroom level effect using the difference-in-differences term and an additional effect of having a poor study partner, with standard errors clustered at the school-by-grade level. Individual **controls** include gender, age, mother’s education, distance of student’s home from school, and whether the student uses a private (chauffeured) car to commute to school.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9. Test Scores in English, Hindi and Math

Dependent Variable: Normalized Test Score

	Combined		English		Hindi		Math	
Specification:	DiD	IV	DiD	IV	DiD	IV	DiD	IV
Sample:	Full	Treated	Full	Treated	Full	Treated	Full	Treated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated Classroom	-0.024		-0.087*		0.0260		0.037	
	(0.068)		(0.050)		(0.0632)		(0.0466)	
Has Poor Study Partner		-0.025		-0.066		-0.181		0.0146
		(0.132)		(0.135)		(0.172)		(0.083)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade		School, Grade	Classroom	School, Grade	Classroom	School, Grade	Classroom
p-value (CGM)	0.76	.	0.09	.	0.72	.	0.45	.
Control Mean	0	0.0466	0	0.0266	0	0.0372	0	0.0761
Control SD	0.594	0.628	1.000	1.127	1.000	1.021	1.000	0.974
N	2017	677	2017	677	2017	677	2017	677

Note: Standard errors in parentheses. This table reports effects on an index of test scores comprised of equally weighted normalized scores in English normalized test scores of wealthy students in English (Cols 3 and 4), Hindi (Cols 5 and 6), (Cols 7 and 8). Odd numbered columns report difference-in-difference estimates of the effect of having poor students in one's classroom. In these columns, standard errors are clustered at the school-by-grade level, while the p-value reported in the table comes from clustering at the school level using Cameron, Gelbach and Miller's wild-cluster bootstrap. Even numbered columns report IV estimates of the effect of having a poor study partner, and present robust standard errors. Individual **controls** include gender, age, mother's education, distance of student's home from school, and whether the student uses a private (chauffeured) car to commute to school.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10. Indiscipline

Dependent Variable: Indicator for being cited by teacher for *indiscipline* – either inappropriate language or disruptive behavior

	Cursing		**Disruptive Behavior**	
Specification:	DiD	IV	DiD	IV
Sample:	Full Sample	Treated	Full Sample	Treated
	(1)	(2)	(3)	(4)
Treated Classroom	0.0749**		-0.0181	
	(0.0368)		(0.0193)	
Has Poor Study Partner		0.1032*		-0.0251
		(0.0612)		(0.0488)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade	Classroom	School, Grade	Classroom
p-value (CGM)	0.06	.	0.624	.
Control Mean	0.219	0.231	0.0619	0.0613
Control SD	0.414	0.423	0.241	0.240
N	2017	677	2017	677

Note: Standard errors in parentheses. This table reports linear probability models for the likelihood of being cited by the class teacher for two types of indiscipline - inappropriate language (Cols 1 and 2) and disruptive behavior (Cols 3 and 4). Odd numbered columns report difference-in-difference estimates of the effect of having poor students in one's classroom. In these columns, standard errors are clustered at the school-by-grade level, while the p-value reported in the table comes from clustering at the school level using Cameron, Gelbach and Miller's wild-cluster bootstrap. Even numbered columns report IV estimates of the effect of having a poor study partner, and present robust standard errors. Individual **controls** include gender, age, mother's education, distance of student's home from school, and whether the student uses a private (chauffeured) car to commute to school.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$