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Familiarity Does Not Breed Contempt: Generosity, Discrimination, and Diversity in Delhi Schools[†]

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I exploit a natural experiment in Indian schools to study how being integrated with poor students affects the social behaviors and academic outcomes of rich students. Using administrative data, lab and field experiments to measure outcomes, I find that having poor classmates makes rich students (*i*) *more prosocial, generous, and egalitarian; and* (*ii*) *less likely to discriminate against poor students, and more willing to socialize with them. These effects are driven by personal interactions between rich and poor students. In contrast, I find mixed but overall modest impacts on rich students' academic achievement.* (*JEL* C90, D31, I21, I24, O15, Z13)

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 equity concerns, but also by the argument that diversity in schools benefits society by positively influencing intergroup attitudes and social behavior (Schofield 1991). 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.

I focus on a particular dimension of diversity (economic status) and seek to answer the following question: what effects do peers from poor households have

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

on students from relatively wealthy families?[2] I assemble a dataset of over 2,300 students in 17 schools in Delhi, India, and use a combination of field and lab experiments, administrative data, and tests to measure the following outcomes: (i) generosity, fairness, 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 free 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 percent) of elite private schools are entirely exempt from the policy for historical reasons, while another handful (6 percent) 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") 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 having poor students in 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 number of poor children with names alphabetically adjacent to a given rich student provides plausibly exogenous variation in personal interactions with a poor student.[3] 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. Moreover, since the variation is at the individual level (rather than school-by-grade) level, concerns about the limited number of schools in the sample may loom less large for the second strategy.

My first finding is that having poor classmates makes students more prosocial, as measured by their history of volunteering for charitable causes at school. These schools occasionally offer opportunities to students to volunteer or fund-raise for select charities. One such activity involves attending school on two weekend afternoons to help fund-raise 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 13 percentage points (standard error 2.6 percent) on a base of 24 percent, while having a poor study partner increases volunteering by an estimated 15 percentage points (standard error 8 percent).

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 powerfully shapes fairness preferences. Treatment students share 44 percent (standard error 6 percent) or about 0.43

[2] The variation in the data does not allow me to identify the effect on the poor students of being integrated with wealthier students.

[3] Other schools in the sample do *not* use alphabetic order to assign groups. In such schools, I can measure whether having a name alphabetically adjacent to a poor student predicts outcomes even in the absence of increased personal interaction. I find no evidence of such selection effects.

standard deviations more than control students when offered the chance to share money with an anonymous poor student at another school. But, importantly, they are also 24 percent (standard error 5 percent) 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 toward the poor. Instead, it affects fundamental notions of fairness and generosity.

The second finding is that economically diverse classrooms cause wealthy students to discriminate less against other poor children even outside of 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 trade-off 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 percent 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 one-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 35, about two days' worth of pocket money. Having poor classmates reduces average distaste to just Rs 2.6.

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 individuals from other social groups provide a natural foundation for taste-based discrimination. To measures 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 percent (standard error 3 percent). Having a poor study partner affects "willingness to play" by a similar 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 marginally significant but meaningful decreases in wealthy students' English language scores, but find no effects on 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 mixed 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 isolating the effect of direct personal interactions.[4] I find that personal interactions are an important driver of the overall effects. For example, having a poor study partner alone explains 70 percent of the increase in "willingness to play" with a poor child, and 38 percent of the increase in generosity toward the poor. This likely underestimates the importance of personal interactions, since students surely also interact with other poor classmates outside their study groups.[5]

This paper relates to four bodies of work in economics. First, an active recent literature studies whether interaction reduces intergroup prejudice. Most closely related are Boisjoly et al. (2006); Burns, Corno, and La Ferrara (2016); Carrell, Hoekstra, and West (2018); and Finseraas and Kotsadam (2017), who find that being randomly assigned a roommate of a different race at college or at a military academy reduces interracial prejudice in later years.[6] 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 small literature on how social preferences and prosocial behavior are shaped, for example by exposure to violent conflict (Voors et al. 2012), the ideology of one's college professors (Fisman, Kariv, and Markovits 2009), or early-childhood education and mentoring interventions (Cappelen et al. 2016, Kosse et al. 2016). 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 (see Charles and Guryan 2011 and Bertrand and Duflo 2017 for reviews). I contribute to this literature by showing evidence of and quantifying taste-based discrimination in a field experiment (albeit in a non-market setting),

[4] Conceptually, these two types of variation could have very different effects. For example, it could be that forced integration at the classroom level causes group identity to become more salient and worsens prejudice, but that intense personal interaction defuses the prejudice. Or that teachers provide prosocial messaging at the classroom level, improving attitudes, while personal interaction actually causes friction and worsens attitude.

[5] This result echoes Slavin and Madden (1979), who study school practices which improve race relations in the United States, and conclude that cooperative personal interaction (playing together on sports teams) is most effective.

[6] These papers build on a long tradition of related work in social psychology on intergroup contact theory following Allport (1954), which generally documents a negative correlation between intergroup contact and prejudice (Pettigrew and Tropp 2006), but suffers from issues of selection and self-reported outcomes. More recently, Paluck, Green, and Green (2018) scour the vast literature on intergroup contact, and identify 27 studies (including those cited above) where contact with a minority group was randomized. Their meta-analysis also concludes that contact substantially reduces prejudice, with a pooled effect size of 0.39 standard deviations. None of the studies they review specifically study the effect of *economic* desegregation, the development of fairness or egalitarian preferences, or trade-offs in terms of economic efficiency or learning outcomes. Relatively few collect revealed preference measures as outcomes, with Marmaros and Sacerdote (2006); Burns, Corno, and La Ferrara (2016); and Scacco and Warren (2018) being important exceptions.

and by showing that past exposure to out-group members causally reduces such discrimination.[7]

My findings are also of relevance to policymakers: the policy I study is being extended throughout India under the Right To Education Act, with consequences for over 300 million school-age 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."[8] 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 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 I, I describe the policy change underlying the natural experiment. Section II discusses the two econometric strategies and addresses possible challenges to identification. Section III reports impacts on the first class of outcomes, prosocial behavior, and generosity. Section IV describes the experiments and results relating to discrimination and social interaction. Section V reports effects on learning and discipline. Section VI summarizes the results and discusses shortcomings and avenues for future research.

I. 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 rich and poor 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.[9] 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,

[7] See Bertrand and Duflo (2017) and Paluck and Green (2009) for reviews of interventions to reduce discrimination, including exposure to role models (Beaman et al. 2009), cognitive and behavioral debiasing strategies (e.g., Lai et al. 2014, Devine et al. 2012), training in perspective taking (Lustig 2003), and introducing anonymity into selection processes (Goldin and Rouse 2000; Krause, Rinne, and Zimmermann 2012).

[8] "Learning Curve," *The Indian Express*, April 13, 2012 (http://archive.is/YrjLu).

[9] A loosely defined middle class typically sends its children to private schools intermediate in their price and exclusivity to public and elite private schools.

elite private schools as I define them charge tuition fees in excess of Rs 2,000 per month (approximately \$40, 25 percent of median monthly household consumption in 2010), and are vastly over-subscribed. Private schools in my sample report average acceptance rates of 11 percent, and monthly fees of up to Rs 10,000.[10]

Policy Change.—Many private schools in Delhi, including over 90 percent of the approximately 200 elite private schools, exist on land leased from the state (decades ago) 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 percent of their seats for students from households earning under Rs 100,000 a year (approximately \$2,000). 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 percent 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 percent of all elite private schools. *Control schools* are the 4 percent 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). Online Appendix Table A1 reports some summary statistics, such as annual tuition and class size, for the different types of schools.

[10]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 an elite private school in Delhi is harder than getting into Harvard. This author's CV provides corroborative evidence.

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 three and four 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 forty-fifth percentile of the household income distribution, and the average poor student in my sample is from the twenty-fifth percentile. In contrast, the typical rich student in the sample is from well above the ninety-fifth percentile of the consumption distribution. In the United States, 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.[11]

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.[12] The overwhelming majority of admissions to elite private schools occur in preschool, 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 percent per year in my sample.

II. Econometric Strategies

Between March 2012 and March 2014, I conducted field and lab experiments, and gathered test scores and administrative data on 2,362 students in 17 elite private schools in Delhi. The sample consists of 11 treatment schools, 2 delayed treatment schools, and 4 control schools, recruited as part of a larger project studying the returns

[11] There is a reason this paper focuses on economic status rather than caste. First, only a small share of the poor students in the schools are from the most disadvantaged castes. Thus, the policy does not necessarily generate a substantial increase in caste diversity in the schools I study. Second, caste does not actually appear to be a salient social category among students in my sample. In pilot work, I found that most students in grades 2 through 5 can precisely identify which of their classmates are poor. But only a few can categorize their classmates by caste.

[12] 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 four years of age and has no prior schooling.

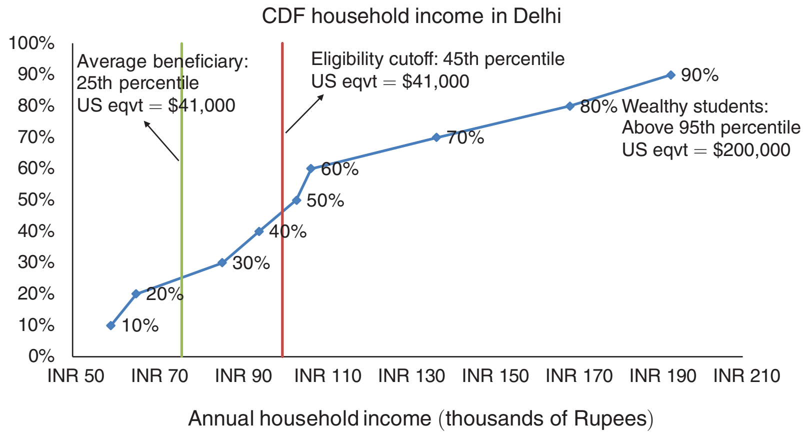

FIGURE 1. PROGRAM ELIGIBILITY AND THE HOUSEHOLD INCOME DISTRIBUTION IN DELHI

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.

to private education in India.[13] Within each school, I constructed a representative sample of wealthy (that is, fee-paying) students in the four cohorts who began preschool between 2005 and 2008. 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 these 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.

A. *Variation within Schools and Cohorts*

The first approach identifies the average effect of having (about 20 percent) poor students in one's classroom.[14] Recall that in treatment schools, wealthy students in grades 2 and 3 are "treated" with poor classmates, while grades 4 and 5 have no poor students. In delayed treatment schools, only grade 2 is treated, while grades 3 through 5 are untreated. And in control schools, grades 2 through 5 are all untreated. Restricting the sample to rich students, I estimate the following difference-in-differences specification by ordinary least squares (OLS):

$$(1) \qquad Y_{igs} = \alpha + \delta_s + \phi_g + \beta \, TreatedClassroom_{gs} + \gamma X_{igs} + \varepsilon_{igs},$$

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

[13] I contacted a total of 19 schools. Two of these schools (one control and one treatment school) declined to participate. The 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 2,000). All schools were provided anonymity in exchange for participating.

[14] Due to the rules against tracking, poor students are distributed fairly evenly across classrooms within grades. Thus, I cannot exploit variation in the precise share of poor students across classrooms.

student-specific error term. $TreatedClassroom_{gs}$ is the treatment indicator: it equals 1 if grade g in school s contains poor students, and is 0 otherwise. The β term is thus the average effect of having a poor classmate, and is the key parameter to be estimated. The vector of individual controls includes age, gender, whether the student's family owns a car, and whether the student commutes to school using a private (chauffeured) car.

Inference.—I cluster standard errors at the grade-by-school level, at which treatment varies. For robustness, I also report p-values from standard errors clustered at the school level. Given the small number of schools ($k = 17$), I use the wild cluster bootstrap-t method of Cameron, Gelbach, and Miller (2008). I also report p-values from two types of permutation tests: one at the school-by-grade level (randomly shuffling the treatment dummy at the school-by-grade level), and the other at the school level (randomly shuffling whether schools are labeled as control, treatment, or delayed treatment, and accordingly assigning treatment status to students). The conclusions are largely consistent across these different methods.

Identification.—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, Bernhard, and Rockenbach (2008) and Almås et al. (2010). 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 dislike for poor children. (ii) Treatment and delayed treatment schools have fewer seats for wealthy students after the policy change, which might mechanically 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.[15] 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

[15] 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.

low (about 10 percent) and less than 5 percent 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.[16] 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 cause 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 percent, total cohort size only increases by 5 percent.[17] This implies that fewer wealthy students are accepted into treated private schools after the policy change. If schools select students based on 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 three and four 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 percent 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 percent 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.

[16] 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.

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

B. *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 to 4 students. Examples of such activities include collaborative craft projects, and working on math problems or reading comprehension. 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 10 of the 17 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:

$$(2) \qquad Y_{icgs} = \alpha + \nu_{cgs} + \beta_1 HasPoorStudyPartners_i + \gamma X_i + \varepsilon_{igs},$$

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 $HasPoorStudyPartners_i$ is instrumented for using $SchoolUsesAlphaRule_s \times HasPoorAlphabeticNeighbor_i$.

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. Moreover, while the conclusions from the difference-in-differences strategy end up being robust to different levels of clustering and permutation tests, this within-classroom strategy utilizes individual-level (or more precisely study-group level) variation, so is less subject to concerns about the limited number of schools in the sample.

Note that this approach does not require that poor and rich students have a similar alphabetic distribution of names. It also allows for the possibility that rich students with names alphabetically adjacent to poor students might be different to begin with: any such preexisting differences should also be reflected in the schools which do *not* use alphabetic order to assign study groups. In practice, I do not find that alphabetic adjacency predicts outcomes in schools which do not utilize alphabetic assignment rules. Finally, it is worth highlighting that the schools all use first names to assign groups; none use surnames, which reflect subcastes and would tend to group students of the same caste together.

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 percent to 90 percent. In contrast, alphabetic adjacency has no effect in other schools. Table 1 provides the

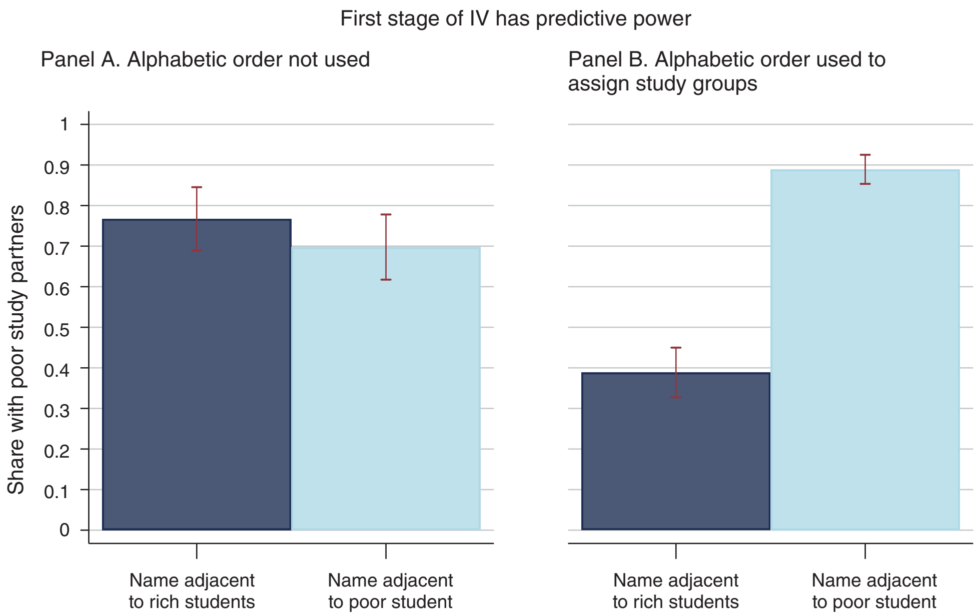

FIGURE 2. FIRST STAGE OF INSTRUMENTAL VARIABLE

Notes: 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. Ninety-five percent confidence intervals around mean.

TABLE 1—FIRST STAGE OF IV
DEPENDENT VARIABLE: INDICATOR FOR HAVING AT LEAST ONE POOR STUDENT IN ONE'S STUDY GROUP

	Has poor study partner (1)
(Name adjacent to poor student) × (school uses alphabetic rule)	0.487
	(0.0374)
Constant	0.104
	(0.424)
Observations	790
F-statistic	169.0

Notes: Standard errors in parentheses. This table reports the results from a linear probability model regressing an indicator for whether the student has at least one poor study-group partner on the excluded instrument school and grade dummies, and a vector of second-stage control variables (age, gender, whether the student's family owns a car, and whether the student uses a private (chauffeured) car to commute to school). The *F*-statistic corresponds to a Wald test of a coefficient of zero on the instrument.

first-stage regression, and reports that the instrument is strong, with an *F*-statistic of over 169.0.

III. 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), and labor markets (Akerlof 1984; Bandiera, Barankay, and Rasul 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.[18]

Recently, scholars have begun to investigate how social preferences are shaped by life experiences, including education and mentoring (Jakiela, Miguel, and te Velde 2015; Cappelen et al. 2016; Kosse et al. 2016), the ideology of one's college professors (Fisman, Jakiela, and Kariv 2012), political violence (Voors et al. 2012), macroeconomic conditions (Fisman, Jakiela, and Kariv 2015), and psychotherapy (Blattman, Jamison, and Sheridan 2017). 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, Bernhard, and Rockenbach 2008), while more sophisticated notions of fairness emerge in adolescence (Almås 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 having poor classmates also makes wealthy students more generous in dictator games. This increased generosity is partly driven by the wealthy students displaying more egalitarian preferences in the lab.

A. *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 fund-raise 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 percent 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. Panel A of Figure 3 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; before the policy change, all the schools had similarly prosocial students. However, wealthy students

[18] 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). Scholars have also studied the effect of varying the identity of the recipient (Hoffman, McCabe, and Smith 1996).

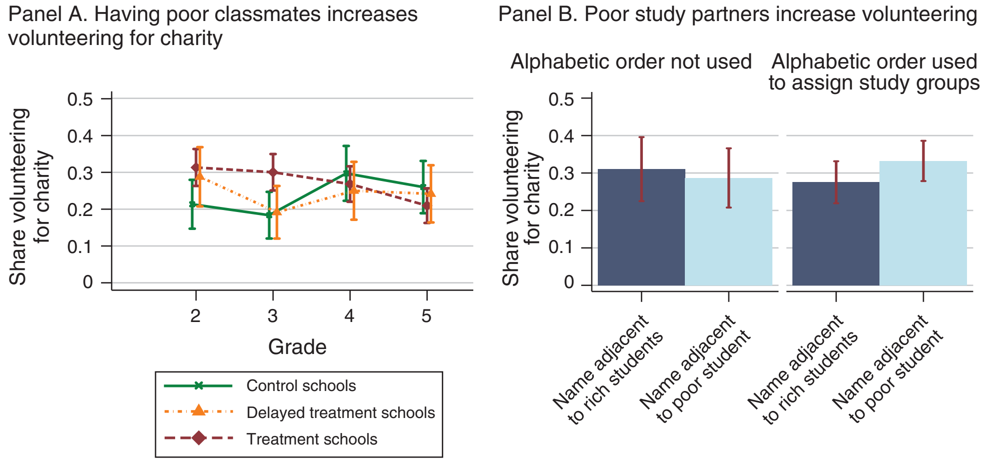

Figure 3. Volunteering for Charity

Notes: Panel A plots the raw share of wealthy students who participate in voluntary charitable fundraising activities in school, separately by grade and type of school. Error bars plot 95 percent confidence intervals for the mean. Panel B 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.

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 which causes the increase in wealthy students' prosocial behavior.[19]

Panel B shows the effect of having a poor study partner, and conveys the essence of the instrumental variable strategy. It plots 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 close 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 magnitude to the effects. Column 1 reports the main difference-in-differences estimate and shows that having poor classmates increases volunteering by 13 percentage points (standard error 2.6), an increase of 55 percent or 0.30 standard deviations over the volunteering rate in control classrooms. The effect remains highly significant clustering at the school level ($p = 0.004$) using the wild cluster bootstrap-t of Cameron, Gelbach, and Miller (2008), as well in a permutation test at the school-by-grade level ($p = 0$) or at the school level ($p = 0.002$). Column 2 reports the same specification estimated on the restricted sample of students who had older siblings in the same school at the time of admission. The results are similar and

[19] Poor students are less likely to volunteer than the rich. This might be due to underlying differences in prosocial preferences, but may also be explained by differential costs of participating. For example, poor parents might find it difficult to bring their children to school outside of the usual school hours.

Table 2—Volunteering for Charity
Dependent Variable: Indicator for Volunteering for Charity

Specification:	DiD	DiD	IV	DiD + IV
Sample:	Full sample	Younger sibs	Treated class	Full sample
	(1)	(2)	(3)	(4)
Treated classroom	0.130	0.102		−0.00931
	(0.0258)	(0.0315)		(0.0715)
Has poor study partner			0.149	0.200
			(0.0798)	(0.0778)
Controls	Yes	Yes	Yes	Yes
Fixed effects	School, grade	School, grade	Classroom	School, grade
p-value (CGM)	0.004	0.006	–	–
p-value (permute school × grade)	0	0	–	–
p-value (permute schools)	0.002	0.03	–	–
Control mean	0.237	0.246	0.240	0.237
Control SD	0.425	0.431	0.428	0.425
Observations	2,364	1,348	790	2,364

Notes: Standard errors in parentheses. This table reports results from linear probability models for the likelihood of volunteering for a charity. Column 1 reports difference-in-differences estimates of the effect of having poor students in one's classroom, with school fixed effects and grade fixed effects. Standard errors are clustered at the school-by-grade level. The first p-value reported in the table is instead calculated with clustering at the school level ($k = 17$) using the wild-cluster bootstrap-t of Cameron, Gelbach, and Miller (2008). The second p-value reported in the table comes from a randomization inference procedure which permutes treatment at the school-by-grade level. The third p-value comes from a randomization inference procedure which instead permutes the schools labeled as control, treatment, and delayed treatment schools, and accordingly permutes treatment indicators. Column 2 reports the same specification as column 1, but restricts the sample to students who have older siblings enrolled in the same school. Column 3 reports IV estimates of the effect of having a poor study partner, incorporating classroom fixed effects, and instrumenting for having a poor study partner with alphabetic proximity interacted with whether the school utilizes alphabetic order to assign study groups. Robust standard errors are reported. Column 4 reports a specification estimating both the classroom level effect using the difference-in-differences term and an additive effect of having a poor study partner, with standard errors clustered at the school-by-grade level. Individual *controls* used throughout include gender, age, whether the student's family owns a car, and whether the student uses a private (chauffeured) car to commute to school.

not statistically different: an increase in volunteering of 10 percentage points (standard error 3.1). 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 14.9 percentage points (standard error 7.9), an imprecisely estimated increase of 62 percent.

B. *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. I first use two dictator games to measure their generosity toward poor and rich recipients. Next, I use a set of three binary-choice dictator games to study egalitarian preferences in particular.

Design.—Within each school, students were assigned to specific experimental sessions conducted in small groups of six to ten students at a time. The sessions mixed students across grades, and were held in a separate room during a regular school day. Each student played two sets of games, with a short break between sets. The order of the games was randomized within each set, across sessions.

In the first set, students sequentially played two standard dictator games. In each game, the student was endowed with Rs 10, and could choose to share any integer amount (including zero) with an anonymous recipient from another school. In one game, the recipient was an anonymous student in a school catering to poor children. In the other, the recipient was an anonymous student in an elite private school which caters to wealthy students. Students were provided photographs and the name of the recipient's school, intended to make the economic status of the recipients clear. In debriefing surveys, students clearly received the message: over 90 percent of students correctly identified which recipient was poorer. After completing both games, one game was randomly selected to be implemented.

Next, students played three simple dictator games designed to identify whether subjects dislike unequal allocations.[20] Each game posed dictators with a binary choice between more and less equal distributions of payoffs. The less equal option provided 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. All recipients in these games were anonymous schoolmates of the participants. The games themselves were presented without labels, and the order of the games was randomized across sessions. After completing all three games, one game was randomly selected to be implemented.

	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

At the end of the experimental session, a sealed envelope was returned to each student containing their 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.

Results.—

Poor Recipient: I find that having poor classmates and interacting with them in study groups makes wealthy students substantially more generous toward poor recipients. Figure 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 (standard error 1.7), an increase of 45 percent or 0.45 standard deviations over the average giving in control classrooms. The results are very similar for the less-selected sample of younger siblings (column 2). Both clustering at the school level and permutation tests at the school-by-grade level or at the school level lead to p-values of 0. The instrumental variable estimates of column 3 show that having at least 1 poor study partner causally increases giving by 10.6 percentage points (standard error 4.9), an increase of 32 percent.

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

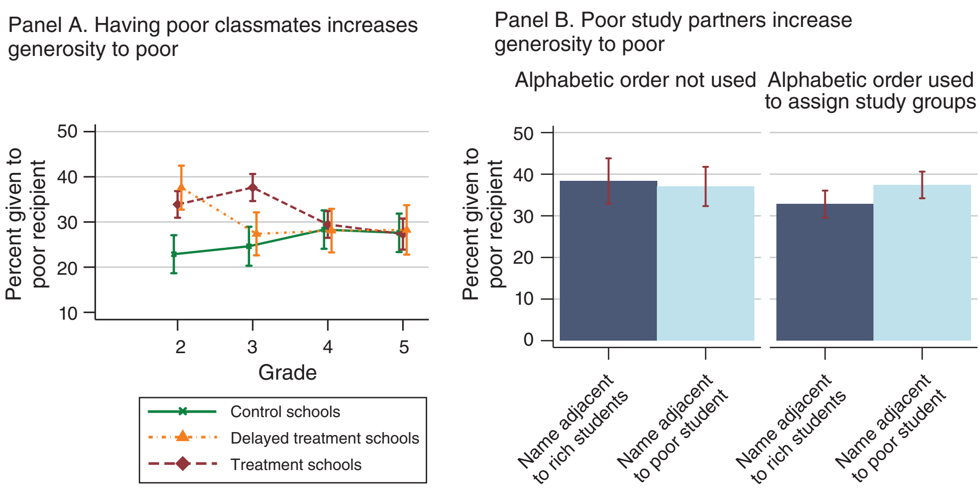

Figure 4. Giving to Poor Recipients in Dictator Game

Notes: Panel A plots the share given to a poor recipient against grade, separately by type of school. Ninety-five percent 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. Panel B 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.

Table 3—Generosity Toward Poor Students
Dependent Variable: Share Given to Poor Recipient in Dictator Game (Percent)

Specification:	DiD	DiD	IV	DiD + IV
Sample:	Full sample	Younger sibs	Treated class	Full sample
	(1)	(2)	(3)	(4)
Treated classroom	11.92	12.31		6.061
	(1.747)	(1.959)		(3.479)
Has poor study partner			10.59	8.402
			(4.874)	(4.005)
Controls	Yes	Yes	Yes	Yes
Fixed effects	School, grade	School, grade	Classroom	School, grade
p-value (CGM)	0	0	–	–
p-value (permute school × grade)	0	0	–	–
p-value (permute schools)	0	0	–	–
Control mean	27.34	26.60	32.88	27.34
Control SD	27.49	26.96	27.68	27.49
Observations	2,362	1,346	790	2,362

Notes: Standard errors in parentheses. This table reports regression results for share of the endowment given in the dictator game when matched with a poor recipient. Column 1 reports difference-in-differences estimates of the effect of having poor students in one's classroom, with school fixed effects and grade fixed effects. Standard errors are clustered at the school-by-grade level. The first p-value reported in the table is instead calculated with clustering at the school level ($k = 17$) using the wild-cluster bootstrap-t of Cameron, Gelbach, and Miller (2008). The second p-value reported in the table comes from a randomization inference procedure which permutes treatment at the school-by-grade level. The third p-value comes from a randomization inference procedure which instead permutes the schools labeled as control, treatment, and delayed treatment schools, and accordingly permutes treatment indicators. Column 2 reports the same specification as column 1, but restricts the sample to students who have older siblings enrolled in the same school. Column 3 reports IV estimates of the effect of having a poor study partner, incorporating classroom fixed effects, and instrumenting for having a poor study partner with alphabetic proximity interacted with whether the school utilizes alphabetic order to assign study groups. Robust standard errors are reported. Column 4 reports a specification estimating both the classroom level effect using the difference-in-differences term and an additive effect of having a poor study partner, with standard errors clustered at the school-by-grade level. Individual *controls* used throughout include gender, age, whether the student's family owns a car, and whether the student uses a private (chauffeured) car to commute to school.

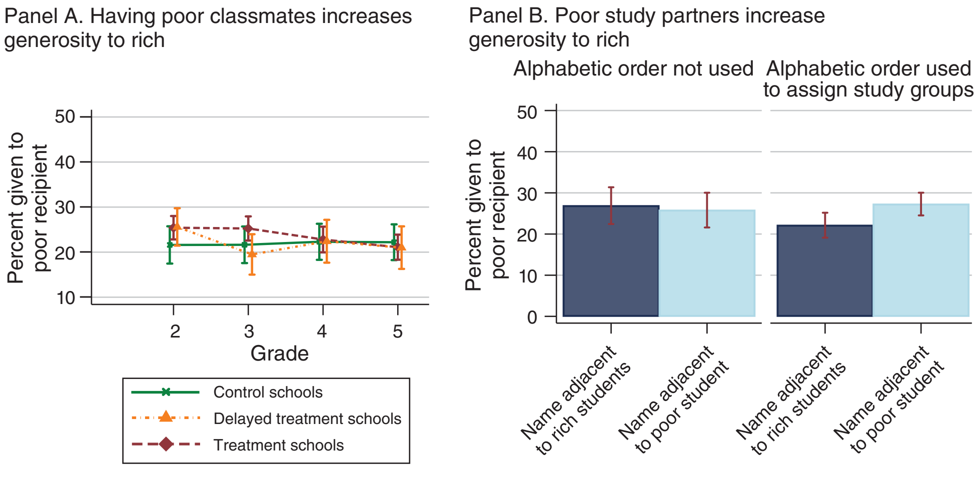

Figure 5. Giving to Wealthy Recipients in Dictator Game

Notes: Panel A plots the share given to a wealthy recipient against grade, separately by type of school. Ninety-five percent confidence intervals (unclustered) for the mean are included. Panel B 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.

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 5.1 percentage points (standard error 1.1), an increase of 24 percent, while having a poor study partner increases giving by 8.8 percentage points (standard error 4.1).

Poor students on average give less than their wealthy classmates, both to poor recipients (Rs 2.4 compared to Rs 3.6 given by rich classmates), and to rich recipients (Rs 1.8 compared to Rs 2.5). Thus, the increased generosity of the treated rich students is not explained by their simply imitating their poor classmates.

Digging deeper, Figure 6 plots 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 percent of the endowment with the recipient. This raises the intriguing possibility that exposure to poor classmates makes wealthy students more egalitarian in the lab.

Egalitarian Preferences: Table 5 reports that students with poor classmates are consistently more likely to pick the more equal outcome. Column 1 shows that treated students are 8.6 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 54 percent in the control group. And when choosing allocations for 2 anonymous recipients (holding their own payoff fixed) in the disinterested dictator games, they are 12.2 percentage points more likely to choose $(4,4)$ over $(8,3)$ and 12.3 percentage points more likely to pick $(4,4)$ over $(12,0)$. IV estimates show similar direction of effects, although with limited precision: increases in

Table 4—Generosity Toward Wealthy Students
Dependent Variable: Share Given to Wealthy Recipient in Dictator Game (Percent)

Specification: Sample:	DiD Full sample (1)	DiD Younger sibs (2)	IV Treated class (3)	DiD + IV Full sample (4)
Treated classroom	5.150 (1.100)	4.741 (1.662)		−2.739 (2.360)
Has poor study partner			8.858 (4.140)	11.32 (3.021)
Controls	Yes	Yes	Yes	Yes
Fixed effects	School, grade	School, grade	Classroom	School, grade
p-value (CGM)	0	0.022	–	–
p-value (permute school × grade)	0	0.0005	–	–
p-value (permute schools)	0.0095	0.0495	–	–
Control mean	21.74	21.67	20.24	21.74
Control SD	25.21	25.93	22.44	25.21
Observations	2,362	1,346	790	2,362

Notes: Standard errors in parentheses. This table reports regression results for share of the endowment given in the dictator game when matched with a rich recipient. Column 1 reports difference-in-differences estimates of the effect of having poor students in one's classroom, with school fixed effects and grade fixed effects. Standard errors are clustered at the school-by-grade level. The first *p*-value reported in the table is instead calculated with clustering at the school level ($k = 17$) using the wild-cluster bootstrap-*t* of Cameron, Gelbach, and Miller (2008). The second *p*-value reported in the table comes from a randomization inference procedure which permutes treatment at the school-by-grade level. The third *p*-value comes from a randomization inference procedure which instead permutes the schools labeled as control, treatment, and delayed treatment schools, and accordingly permutes treatment indicators. Column 2 reports the same specification as column 1, but restricts the sample to students who have older siblings enrolled in the same school. Column 3 reports IV estimates of the effect of having a poor study partner, incorporating classroom fixed effects, and instrumenting for having a poor study partner with alphabetic proximity interacted with whether the school utilizes alphabetic order to assign study groups. Robust standard errors are reported. Column 4 reports a specification estimating both the classroom level effect using the difference-in-differences term and an additive effect of having a poor study partner, with standard errors clustered at the school-by-grade level. Individual *controls* used throughout include gender, age, whether the student's family owns a car, and whether the student uses a private (chauffeured) car to commute to school.

probability of choosing the more equal option of 5.5 (standard error 8.6), 13.8 (standard error 8.7), and 10.9 (standard error 5) percentage points respectively for the 3 games. Online Appendix Table A2 shows similar results for the less-selected sample of younger siblings.

Interpretation.—Considering the set of dictator game results together, I conclude that having poor classmates does not simply make students more charitable toward the poor. Instead, it makes them more generous overall, and in particular makes them exhibit more egalitarian preferences over monetary payoffs. This result is conceptually different from the usual "contact hypothesis" story of exposure reducing prejudice. Instead, we see that personal interactions with poor classmates shapes fundamental and disinterested social preferences regarding fairness and equality.

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.

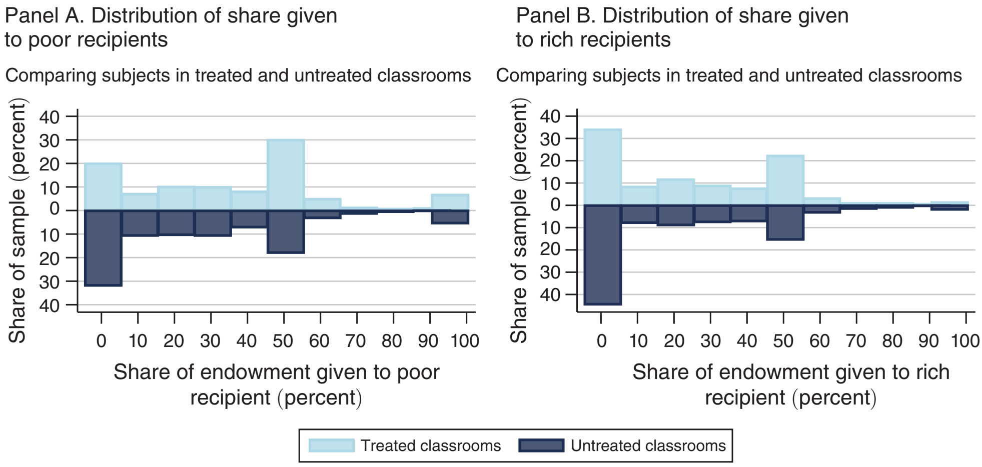

Figure 6. Distribution of Giving in Treated and Untreated Classrooms

Notes: Panel A shows the distribution of giving by wealthy students to poor recipients, separately for whether they have poor classmates (red bars) or not (blue bars). Panel B shows the same results for giving to wealthy recipients instead of poor recipients.

Table 5—Egalitarian Preferences
Dependent Variable: Indicator for Choosing the More Egalitarian Option in a Binary Choice Dictator Game

	Equality game (5, 5) versus (6, 1)		Disinterested game 1 (0, 4, 4) versus (0, 8, 3)		Disinterested game 2 (0, 4, 4) versus (0, 12, 0)	
Specification:	DiD	IV	DiD	IV	DiD	IV
Sample:	Full sample	Treated class	Full sample	Treated class	Full sample	Treated class
	(1)	(2)	(3)	(4)	(5)	(6)
Treated classroom	0.0863		0.122		0.123	
	(0.0486)		(0.0616)		(0.0293)	
Has poor study partner		0.0554		0.138		0.109
		(0.0863)		(0.0875)		(0.0500)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	School, grade	Classroom	School, grade	Classroom	School, grade	Classroom
p-value (CGM)	0.132	–	0.19	–	0	–
p-value (permute school × grade)	0.001	–	0	–	0	–
p-value (permute schools)	0.0985	–	0.022	–	0.0015	–
Control mean	0.538	0.616	0.473	0.536	0.774	0.872
Control SD	0.499	0.487	0.499	0.500	0.418	0.335
Observations	2,364	790	2,364	790	2,364	790

Notes: 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. Columns 1 and 2 report shares choosing (5, 5) over (6, 1). Columns 3 and 4 report shares choosing (0, 4, 4) over (0, 8, 3). Columns 5 and 6 report shares choosing (0, 4, 4) over (0, 12, 0). Odd-numbered columns report difference-in-differences estimates of the effect of having poor students in one's classroom, incorporating school fixed effects and grade fixed effects. In these columns, standard errors are clustered at the school-by-grade level. The first p-value reported in the table is instead calculated with clustering at the school level ($k = 17$) using the wild-cluster bootstrap-t of Cameron, Gelbach, and Miller (2008). The second p-value reported in the table comes from a randomization inference procedure which permutes treatment at the school-by-grade level. The third p-value comes from a randomization inference procedure which instead permutes the schools labeled as control, treatment, and delayed treatment schools, and accordingly permutes tre Even-numbered columns report IV estimates of the effect of having a poor study partner, incorporating classroom fixed effects, and instrumenting for having a poor study partner with alphabetic proximity interacted with whether the school utilizes alphabetic order to assign study groups. Robust standard errors are reported. Individual controls used throughout include gender, age, whether the student's family owns a car, and whether the student uses a private (chauffeured) car to commute to school.

IV. Social Interactions and Discrimination

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

Tastes for social interactions provide a natural foundation for taste-based discrimination, but are also important in models of residential patterns (Schelling 1971), collective action (Granovetter 1978), job search (Beaman and Magruder 2012), and the marriage market. 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. Changes in willingness to interact with members of other social groups are therefore a potentially important impact of integrated schools.

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 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.

A. *Team-Selection Field Experiment*

Design.—The main idea of the team-selection experiment is to create a trade-off for wealthy students between choosing a high-ability teammate (and thus increasing their own expected payoff) or choosing a lower-ability teammate with whom they would prefer to socialize. The team task I use in the experiment is 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 are required to spend time socializing with their teammates, capturing a feature of many labor market settings.

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. Rs 500 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 15 minutes. This ensured that students were able to accurately identify the difference in the social groups to which the various participants belonged. 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. After each sprint, the rank (first or second) and times of the two runners were announced.

Decision Stage: After each such ability revelation sprint, students privately chose on a worksheet which of the two students they would like to have in their two-person team for a relay race. After the sprints were complete, six students were picked at random to participate in the relay race, and one of their choices was randomly selected for implementation.

Relay Race and Socializing with Teammate: The relay races were conducted and rewards were distributed as promised. After the rewards 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. The goal was to capture a realistic aspect of many jobs: that one must often spend time interacting with one's colleagues.

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.[21] Averaging over the different reward conditions, participants discriminate 19 percent of the time. These are not just mistakes, since the symmetric mistake of "discriminating" against a rich student occurs only 3 percent of the time in the few cases when the rich student wins the sprint. And when participants are choosing between two runners from the same (other) school, they pick the slower runner only 2 percent of the time. Thus, only poor students competing against rich students are systematically discriminated against.

[21] I exclude cases where one chooses between one's own schoolmates and others, since participants may prefer to partner with children they already know.

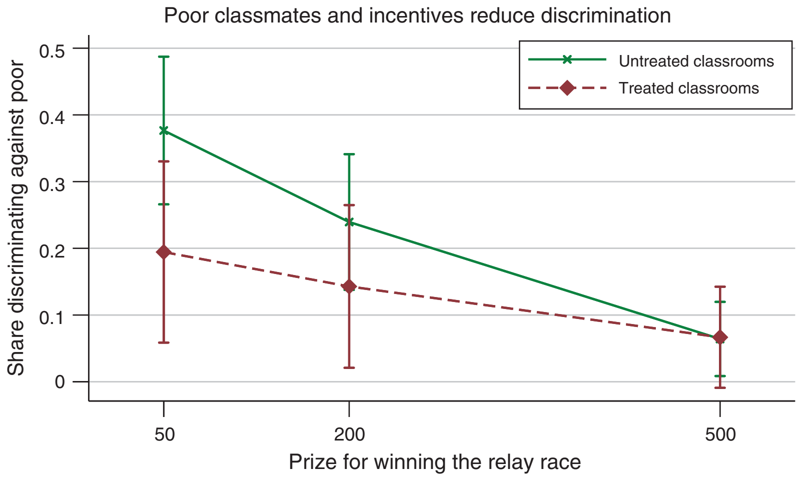

Figure 7. Demand Curve for Discrimination

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. Ninety-five percent confidence intervals around mean.

The second finding is that discrimination decreases as the stakes increase. In the control school, 35 percent of choices exhibit discrimination against the poor in the Rs 50 condition, but this falls to 27 percent when the reward is Rs 200, and only 5 percent 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. Panel A of Figure 8 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. Panel 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 classmate reduces discrimination by 12 percentage points (standard error 5).[22] 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

[22] Since the discrimination experiment has wealthy students from only two schools, I do not attempt to cluster standard errors at the school level, and urge caution in interpreting the standard errors.

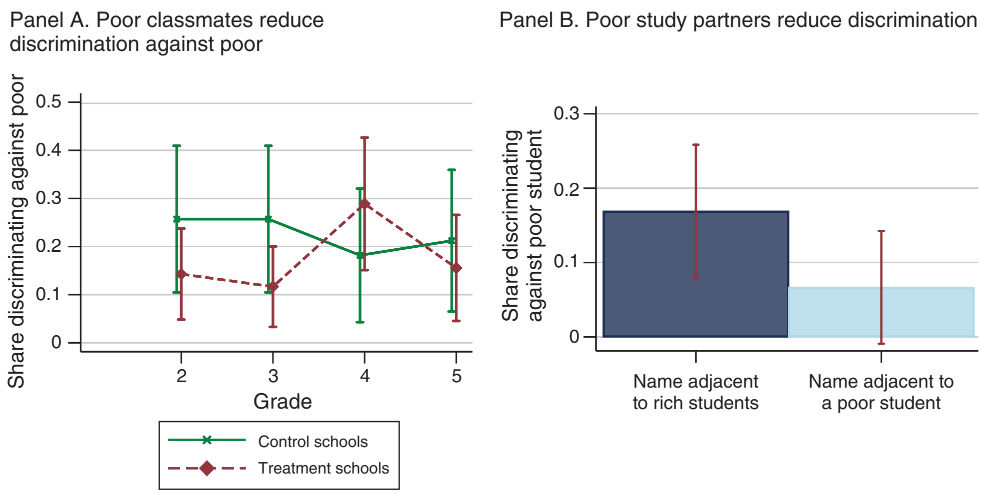

FIGURE 8. DISCRIMINATION AGAINST THE POOR

Notes: Panel A plots the share of wealthy students who discriminate against the poor (on the *y*-axis) by grade (on the *x*-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 percent confidence intervals (unclustered). Panel B plots discrimination rates by whether the participant has a name alphabetically adjacent to any poor students, only for the treatment school.

TABLE 6—DISCRIMINATION AGAINST POOR CHILDREN
DEPENDENT VARIABLE: INDICATOR FOR CHOOSING LOWER-ABILITY WEALTHY STUDENT OVER HIGHER-ABILITY POOR STUDENT

Specification: Sample:	DiD-1 Full sample (1)	DiD-2 Full sample (2)	IV-1 Treated class (3)	IV-2 Full sample (4)
Treated classroom	−0.157	−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) × (prize = Rs 200)		0.0853		
		(0.0667)		
(Treated classroom) × (prize = Rs 500)		0.186		
		(0.0939)		
Has poor study partner			−0.147	−0.118
			(0.0885)	(0.156)
(Poor study partner) × (prize = Rs 200)				−0.0337
				(0.210)
(Poor study partner) × (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
Observations	342	342	116	116

Notes: 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 student winning the first-round race. Columns 1 and 2 report difference-in-differences estimates of the effect of having poor students in one's classroom, incorporating school and grade fixed effects, with robust standard errors. Columns 3 and 4 report IV estimates of the effect of having a poor study partner, incorporating classroom fixed effects, with robust standard errors.

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 (standard error 8.8).[23]

The observed behavior is more consistent with taste-based discrimination than statistical discrimination (about running ability). When a separate sample of students is asked which of the two runners is more likely to be in the winning relay race, 98 percent 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.[24] 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 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.

Model and Structural Estimation.—The reduced-form results provide evidence of a reduction in discrimination. But they do not inform us of the precise 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.

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 about running ability, rich and poor students with the same performance in the sprint would be perceived to be equally able, $p_P = p_R$. Define $D \equiv S_R - S_P$ as the distaste for interacting with a poor student (relative to a rich student), and $\delta \equiv p_P - p_R$ is the *increase* in probability of winning

[23] 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.

[24] I mean statistical discrimination in the usual sense of beliefs about work ability (here, running speed). Of course, participants likely also have probabilistic beliefs about the utility from socially interacting with different partners. What I call taste-based discrimination here may well be statistical discrimination about the "niceness" of one's teammates (which is not what is typically meant by statistical discrimination).

from having a poor teammate, provided the poor student won the ability-revelation sprint.[25] Then, the decision-maker discriminates against a poor student if

$$D > \delta M. \tag{4}$$

In order to estimate the model, I impose the following distributional assumption: D is distributed according to a truncated-normal distribution (truncated at zero) with mean μ_D^T and standard deviation σ_D^T, separately for students from treated classrooms $(T = 1)$ and untreated classrooms $(T = 0)$.[26] 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; and (ii) σ_D^1 and σ_D^0, the standard deviations of the distribution of distaste. In addition, I assume that participants have accurate beliefs about the increase in probability of winning from choosing the faster teammate: this parameter is taken directly from the data on the relay races, $\delta = 0.3$ (winners are 30 percentage points more likely to win the relay races).

I estimate the key parameters using a classical minimum distance estimator. Specifically, the estimator solves $\min_\theta \big(m(\theta) - \hat{m}\big)' W \big(m(\theta) - \hat{m}\big)$, 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 probability of discriminating against a poor student, separately by stakes $M \in \{50, 200, 500\}$ and treatment status $T \in \{0, 1\}$. The empirical moments are simply shares of students observed to discriminate in each condition.

Identification.—All four parameters are jointly identified using the six moments. The intuition for the identification is straightforward. The exogenous variation in the stakes $\delta \cdot M$ pins down the mean μ_D and standard deviation σ_D of the distribution of distaste D.

Estimates.—The lower panel of Table 7 reports the empirical and fitted values of the moments. The model overall does a very good job of fitting the moments. Table 7 also reports the structural estimates of the parameters. Students without poor classmates are estimated to feel an average distaste for having a poor teammate of $\mu_D^0 =$ Rs 35 (standard error Rs 5.2), but with substantial heterogeneity: a standard deviation $\sigma_D^0 =$ Rs 58 (standard error Rs 9.2). In contrast, treated students are estimated to have a substantially lower mean distaste of $\mu_D^1 =$ Rs 2.6 (standard error Rs 9.0), and with a smaller standard deviation, $\sigma_D^1 =$ Rs 14.6 (standard error Rs 28.0). The difference in average distaste of Rs 32.8 is significant at the 1 percent level.

[25] Note that nearly all sprints were won by the poor runner, consistent with the fact that the experiment invited athletic poor students to participate.

[26] Note that truncating the distribution at zero implies that no student exhibits a strict preference for interacting with poor children. In practice, discrimination against the two rich sprint-winners in the experiment was almost zero, consistent with this assumption.

Table 7—Structural Estimates

	Control	Treated	Difference
Estimates			
Mean distaste for poor teammate relative to rich (μ_D), in rupees	35.4 (5.2)	2.6 (9.0)	−32.8 (10.4)
SD of distaste for poor teammate relative to rich (σ_D), in rupees	58.5 (9.2)	14.6 (28.0)	43.8 (29.5)
Moments (probability of discriminating)		Empirical %	Predicted %
Control students			
Stakes = Rs 50		37.6	37.4
Stakes = Rs 200		23.9	23.8
Stakes = Rs 500		6.4	6.5
Treated students			
Stakes = Rs 50		19.4	18.3
Stakes = Rs 200		11.4	13.3
Stakes = Rs 500		6.7	6.3

Notes: Estimates from minimum-distance estimator using the moments shown, and weights given by the inverse of each moment's variance. The distaste for having a poor teammate is modeled as being drawn from a normal distribution truncated at zero, with a discrete mass at zero. Parameters reported are the *unconditional* mean and standard deviation of the estimated distaste. Standard errors are in parentheses.

B. *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, its location 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.[27]

After answering students' questions about the planned play dates, I elicited their willingness to accept (the payment they required) to attend the play date.[28] I employed a simple Multiple Price List elicitation, where students were presented with a decision sheet where each row showed a possible level 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 numbered ball was drawn from a bag,

[27] Due to logistical reasons, the play date experiment was only implemented in 14 of the 17 schools.

[28] 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, when the opportunity cost of attending anecdotally included watching television and playing with existing friends.

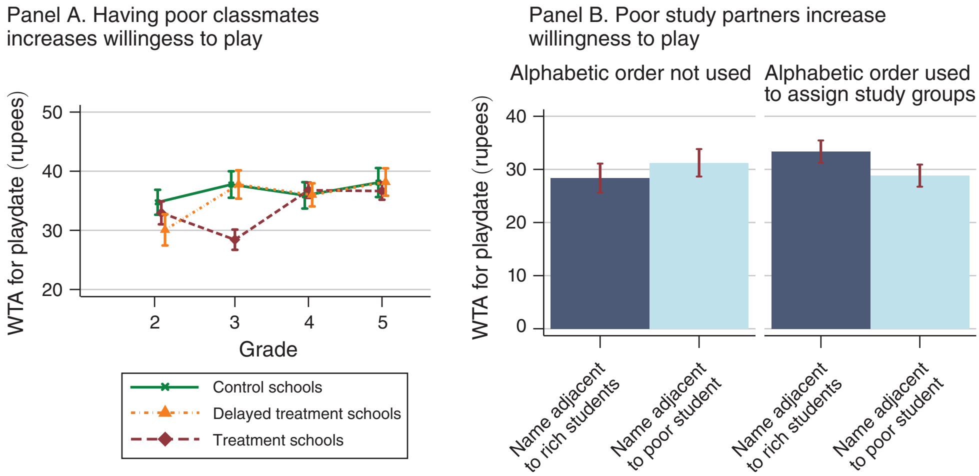

FIGURE 9. WILLINGNESS TO PLAY

Notes: Panel A plots wealthy students' average willingess to accept a play date with poor children, separately by type of school. Error bars plot 95 percent confidence intervals (unclustered). Panel B plots willingess to accept by whether the subject has a name alphabetically adjacent to any poor students, separately by whether schools use alphabetic order to assign study groups.

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.[29] 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.[30]

[29] Parents had the ability to veto their children's choice to attend the play date, and did so in about 40 percent of cases, with similar rates in the treatment and control groups. 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.

[30] Poor students are more willing to attend the play dates than their rich classmates: they have an average willingness to accept of Rs 12 compared to Rs 30 for their classmates. This may reflect a greater openness to interacting with other poor children, or might simply be an income effect: a given monetary incentive is likely more powerful for poor students. Regardless, one concern is that treated wealthy students are more willing to attend simply because they know that their poor classmates will be attending. This seems unlikely, since the elicitation is completed privately and simultaneously.

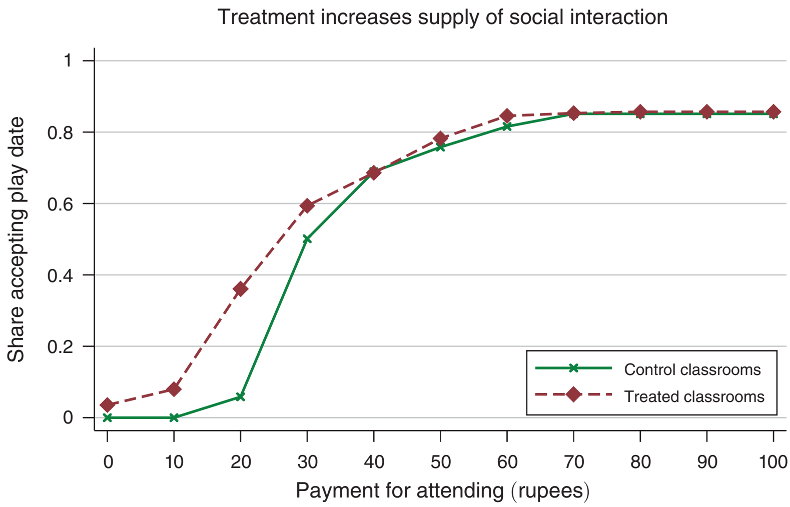

Figure 10. Supply Curve for Attending Play Date

Notes: 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.

Panel B of Figure 9 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 student's distaste for interacting with other poor children.

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

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 of three schools 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 A3 reports no average effect on willingness to play with rich students.

V. 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

Table 8—Willingness to Play with Poor Children
Dependent Variable: Willingness to Accept to Attend Play Date (rupees)

Specification:	DiD	DiD	IV	DiD + IV
Sample:	Full sample	Younger sibs	Treated class	Full sample
	(1)	(2)	(3)	(4)
Treated classroom	−7.009	−7.068		−1.891
	(1.097)	(1.678)		(2.361)
Has poor study partner			−7.864	−7.311
			(2.886)	(3.277)
Controls	Yes	Yes	Yes	Yes
Fixed effects	School, grade	School, grade	Classroom	School, grade
p-value (CGM)	0.002	0.002	–	–
p-value (permute school × grade)	0	0	–	–
p-value (permute schools)	0	0	–	–
Control mean	36.84	36.89	32.08	36.84
Control SD	11.94	11.96	14.75	11.94
Observations	2,017	1,143	677	2,017

Notes: Standard errors in parentheses. This table reports regression results for wealthy students' minimum willingness to accept to attend a play date with poor children. Column 1 reports difference-in-differences estimates of the effect of having poor students in one's classroom, incorporating school fixed effects and grade fixed effects. Standard errors are clustered at the school-by-grade level. The first *p*-value reported in the table is instead calculated with clustering at the school level ($k = 14$) using the wild-cluster bootstrap-*t* of Cameron, Gelbach, and Miller (2008). The second *p*-value reported in the table comes from a randomization inference procedure which permutes treatment at the school-by-grade level. The third *p*-value comes from a randomization inference procedure which instead permutes the schools labeled as control, treatment, and delayed treatment schools, and accordingly permutes treatment indicators. Column 2 reports the same specification as column 1, but restricts the sample to students who have older siblings enrolled in the same school. Column 3 reports IV estimates of the effect of having a poor study partner, incorporating classroom fixed effects, and instrumenting for having a poor study partner with alphabetic proximity interacted with whether the school utilizes alphabetic order to assign study groups. Robust standard errors are reported. Column 4 reports a specification estimating both the classroom level effect using the difference-in-differences term and an additive effect of having a poor study partner, with standard errors clustered at the school-by-grade level. Individual *controls* used throughout include gender, age, whether the student's family owns a car, and whether the student uses a private (chauffeured) car to commute to school.

found substantial peer effects on test scores (Hoxby 2000, Hanushek et al. 2003), and at other times no effect (Angrist and Lang 2004). Classroom disruptions by poorly disciplined students have been proposed to a key mechanism underlying any negative effects (Lazear 2001, Lavy and Schlosser 2011). 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.

A. *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

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	−0.0388		−0.169		0.0428		0.0099	
	(0.0434)		(0.0886)		(0.0769)		(0.0849)	
Has poor study partner		−0.004		−0.157		0.120		0.0243
		(0.111)		(0.199)		(0.165)		(0.179)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	School, grade	Classroom	School, grade	Classroom	School, grade	Classroom	School, grade	Classroom
p-value (CGM)	0.41	–	0.092	–	0.686	–	0.936	–
p-value (permute school × grade)	0.246	–	0.001	–	0.333	–	0.893	–
p-value (permute schools)	0.547	–	0.103	–	0.60	–	0.918	–
Control mean	0	−0.0212	0	0.0221	0	−0.0790	0	−0.00680
Control SD	0.595	0.641	1.000	1.076	1.000	1.039	1.000	1.007
Observations	2,364	790	2,364	790	2,364	790	2,364	790

Notes: Standard errors in parentheses. This table reports effects on normalized test scores of wealthy students in English, Hindi, and math. Odd-numbered columns report difference-in-difference estimates of the effect of having poor students in one's classroom, incorporating school fixed effects and grade fixed effects. In these columns, standard errors are clustered at the school-by-grade level. The first p-value reported in the table is instead calculated with clustering at the school level ($k = 17$) using the wild-cluster bootstrap-t of Cameron, Gelbach, and Miller (2008). The second p-value reported in the table comes from a randomization inference procedure which permutes treatment at the school-by-grade level. The third p-value comes from a randomization inference procedure which instead permutes the schools labeled as control, treatment, and delayed treatment schools, and accordingly permutes treatment indicators. Even-numbered columns report IV estimates of the effect of having a poor study partner, incorporating classroom fixed effects, and instrumenting for having a poor study partner with alphabetic proximity interacted with whether the school utilizes alphabetic order to assign study groups. Robust standard errors are reported. Individual *controls* used throughout include gender, age, whether the student's family owns a car, and whether the student uses a private (chauffeured) car to commute to school.

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 0.35 standard deviations (SD) worse than wealthy students in English, 0.10 SD worse in Hindi, and 0.20 SD 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 higher scores than 36 percent 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 0.17 standard deviation reduction in average test scores in English in treated classrooms ($p \approx 0.1$ when clustering or permuting at the school 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. Online Appendix Table A4 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 (2015), 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.[31] 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.

B. *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. I find that 20 percent of wealthy students have been cited for the use of inappropriate language (that is, swearing) in school, but only about 6 percent 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 percentage points (standard error 3.0; $p = 0.001$ when permuting at the school-by-grade level; $p = 0.1$ when permuting at the school level). Having a poor study partner causes an even larger increase of 13 percentage points (standard error 7.8), an increase of about 54 percent. 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.

[31] But note that Muralidharan and Sundararaman (2015) study a quite different setting: relatively modest private schools in rural Andhra Pradesh, where the social and economic disparity between the existing and incoming students is likely much smaller.

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	0.0705		−0.00982	
	(0.0298)		(0.0201)	
Has poor study partner		0.133		−0.0251
		(0.0780)		(0.0471)
Controls	Yes	Yes	Yes	Yes
Fixed effects	School, grade	Classroom	School, grade	Classroom
p-value (CGM)	0.032	–	0.67	–
p-value (permute school × grade)	0.0015	–	0.50	–
p-value (permute schools)	0.106	–	0.706	–
Control mean	0.203	0.244	0.0572	0.056
Control SD	0.403	0.430	0.232	0.230
Observations	2,364	790	2,364	790

Notes: 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 (columns 1 and 2) and disruptive behavior (columns 3 and 4). Odd-numbered columns report difference-in-differences estimates of the effect of having poor students in one's classroom, incorporating school fixed effects and grade fixed effects. In these columns, standard errors are clustered at the school-by-grade level. The first p-value reported in the table is instead calculated with clustering at the school level ($k = 17$) using the wild-cluster bootstrap-t of Cameron, Gelbach, and Miller (2008). The second p-value reported in the table comes from a randomization inference procedure which permutes treatment at the school-by-grade level. The third p-value comes from a randomization inference procedure which instead permutes the schools labeled as control, treatment, and delayed treatment schools, and accordingly permutes treatment indicators. Even-numbered columns report IV estimates of the effect of having a poor study partner, incorporating classroom fixed effects, and instrumenting for having a poor study partner with alphabetic proximity interacted with whether the school utilizes alphabetic order to assign study groups. Robust standard errors are reported. Individual *controls* used throughout include gender, age, whether the student's family owns a car, and whether the student uses a private (chauffeured) car to commute to school.

VI. 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 toward others. The second finding is that wealthy students become more willing to socially interact with poor children outside school, and thus exhibit less 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 these findings is that school policies involving affirmative action, desegregation, and tracking should be evaluated not only on learning outcomes, which are unarguably important, but also on other 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 intergroup behaviors. Finally, my findings are of relevance to the ongoing expansion of this policy across India.

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. Effects of integration might be quite different in other, more modest schools, where the initial social distance between the groups may be smaller, but which may also have fewer resources available for the incoming poor students. A third weakness is a failure to shed light on the specific conditions under which integration reduces prejudice, or instead backfires, as examined recently by Lowe (2018) in the context of inter-caste contact in India, and by an older non-experimental literature (e.g., Slavin and Madden 1979).

A final, glaring, omission is the inability to identify the effects on the poor students who potentially benefit the most from access to these elite schools. While an important body of research studies how attending selective schools or colleges affects the test scores or earnings of low-income students (e.g., Dale and Krueger 2002; Zimmermann 2014, 2017), we know much less about how social attitudes, skills, and behaviors change.

The expansion of the policy I study across India, typically utilizing school lotteries to select the poor students, provides a rich laboratory in which to study all of these questions in future research.

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