# Bridge Classes and Peer Networks among Out-of-school Children in India

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**Abstract**

We estimate the effects of peer networks on the enrollment and attendance patterns of children in a community-based education program in India. The program is open to all out-of-school children, and we randomly assign a subset of the eligible children to be actively encouraged to participate. This active encouragement increases participation among selected children by 30 percentage points, allowing us to measure the indirect effects of their treatment on their peers' and siblings' participation. Using a detailed questionnaire to measure the various ties between children, we are able to contrast the degree to which participation is casually transmitted through different types of peer relationships. Having a treated friend increases participation by about 20% of the main effect, but there is no evidence that additional treated friends affects participation. The effect of treated friends comes primarily from bilateral ties, where both the child and his friend indicate that they spend time with each other.

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# 1 Introduction

Enrollment levels in most developing countries are much lower than those of their more developed counterparts. The millennium development goals call for universal primary enrollment by 2015 (UN, 2008a). While, significant gains have been made towards achieving this goal, many children are still out of school. Between 1999 and 2006, for example, the United Nations estimates that the number of un-enrolled primary-aged children fell by 30 million. However, 73 million children still do not attend a formal educational program (UN, 2008b).

There are many potential reasons why children do not attend school. In many areas of the world, educational resources are simply scarce, and children must travel significant distances to take advantage of them. Some localities still charge user fees that create financial barriers to enrollment for low-income families. Even in areas with similar accesses to schools, enrollment rates vary by household. To better understand the causes of low enrollment, more research is needed on the family and individual decision processes surrounding the choice to engage in academic activities.

We focus on estimating the causal effect of peer relationships on participation in a specially designed program targeted at out-of-school children in India. The program provides community-based classes designed to provide educational inputs to children not attending school. Using a randomized controlled trial, we randomly assign a fraction of out-of-school students in a suburban area to be actively recruited to participate in the program. All out-of-school children in the sample are allowed to participate in the program, but instructors of the program make a particular point of recruiting and encouraging the enrollment and attendance of children selected for treatment. This includes discussing the program with parents, visiting children prior to the start of each day's activities to bring the child to class, and making an active effort to retain the children if they stop participating.

In the our context, peer effects could operate through several channels. First, there may be complementarities between friends in the effort it takes to attend or achieve in school. Children who attend classes (or their parents) could also provide information to their non-attending peers about the value of attending. Finally, children whose friends attend classes may be more likely to attend because they want to spend time with their friends.

Our active encouragement design is intended to increase participation among treated children,

and it does. We find that actively recruited children are 30 percentage points more likely to attend one of the classes at any point in time than non-treated children. On average, these children have daily attendance rates that are 12 percentage points higher than non-treated children. The child-level randomization provides variation in the density of treatment within children's networks, allowing us to directly measure the indirect effects of treatment through the children's peer networks. This is paired with a detailed survey designed to map out children's peer networks. The detailed friendship survey allows us to compare the effects of treating different types of peers and to compare the different mechanisms for peer interaction.

We find that treating the peers of children does have a causal effect on participation levels. Children with a treated friend were about 6% more likely to attend the classes, but there is little evidence that treating additional friends has an additional impact on attendance. However, we also find that only certain types of relationships mediate this causal effect. Bilateral ties (where both the child and the child's friend report each other as friends), have much higher impacts than unilateral ties. Having a treated sibling has a similar impact to having a treated friend.

Using the effects of peers' treatment status as an instrument for peer participation, we also provide instrumental-variables estimates of the effects of peer attendance on the child's own attendance. We find large and significant effects: a 10 percentage point increase in the mean attendance of treated friends results in a 2.7 percentage point increase in the child's own attendance. We find that the instrumental-variables estimates are similar to OLS estimates which simply regress attendance on friend's attendance.

Our study relates to the growing literature that seeks to identify the effects of peer networks on individuals' behavior. The empirical challenge of this research is to isolate the effects of an individual's peers from the confounding factors associated with endogenous social interaction (Manski, 1993 and Glaeser and Scheinkman, 2000). Papers that explicitly deal with these endogeneity issues do so in two ways. One group exploits the random assignment of individuals into shared environments, (Sacerdote, 2001; Zimmerman, 1999; Bayer, Pintoff, and Pozen, 2004), and the other strand exploits random variation in the treatment of individuals within an existing peer network to determine how the intervention affects treated individuals' peers (Miguel and Kremer, 2003; Duflo and Saez, 2003).

A growing literature measures peer effects amongh primary-aged children in developing countries. These studies generally fall into two categories. One set of studies looks at the effects of financial incentives on siblings of treated children. Filmer and Schady (2008) generally find no effect on siblings, while Barrera-Osorio et al (2008b) find that the absence of an effect is limited to siblings that are not engaged in academic activities. They find negative effects on academically engaged siblings suggesting that transfers cause families to consolidate resources behind treated children.

A second set of studies focus both on attendance decisions (Bobonis and Finan, 2008, Lalive and Cattaneo, 2004) and grades (Kremer, Miguel and Thornton, 2004). By estimating how a child's attendance decisions are affected the decisions of his or her peers, our study is most closely related to that of Bobonis and Finan (2008) and Lalive and Cattaneo (2004). Both studies use the Progresa intervention in Mexico to identify how children ineligible for the program were affected by the attendance decisions of those eligible for the program. These studies find that a 10-percent increase in the attendance of eligible children resulted in around 5-percent higher attendance of ineligible children.

Unlike the these other studies of peer effects, our study defines the peer group through surveys rather than through the other individuals in one's cohort in school. We do so for sevaral reasons. First, because we focus on out-of-school children, the cohort-based measure of a peer group is less well-defined.[1] Second, the survey-based design allows us to identify different types of networks chosen by the child. On the other hand, since our intervention was randomized at the child level, we do not study community-wide network effects estimated in these other studies.

The remainder of the paper is organized as follows. Section 2 describes the setting and the intervention. Section 3 outlines the research design and data collection procedures. Section 4 describes the empirical specifications we use. Section 5 provides our tests for the internal validity of the social network effects. Section 6 presents estimates of the social network effects of the program on attendance in the classes. Section 7 concludes.

[1] One possiblility would be to define the cohort based on the child's age group within a community. This measure would mirror studies of in-school children that define the peer group as the children within a school and grade. However, it is unclear how to determine the age range of the relevant peer group when children do not have structured interactions with other children of their exact age.

## 2 Background

The intervention was run by Pratham, a large, India-wide NGO specializing in basic literacy and numeracy skills for both in-school and out-of-school children. Pratham's Bridge Course program has been implemented for a number of years in various areas in India.

Pratham's approach centers around the involvement of specially-trained teachers who are recruited directly from the same communities as the program children. Unlike teachers in the formal schooling system who do not share a common background with the children they teach, Pratham's model is designed so that the teachers can relate to the situations of the children they teach (Banerjee et. al., 2007). These teachers are normally educated through 10th or 12th grade and are predominantly women. After they are recruited, selected teachers attend an intensive two-week program of training in Pratham's teaching methodologies. The teachers are paid the equivalent of $20 per month.

Pratham's Bridge Course program is designed to give out-of-school children the opportunity take informal classes for one year as a bridge to the formal school system. First, out-of-school children are identified in a community through a community census. Identified out-of-school children are then recruited for enrollment in the classes. Children are taught in groups of 20-25 students for three hours per day, six days per week. When children in the program are not regularly attending class, the teacher visits the child's home. At the end of one year, children and their families are assisted with enrollment in the local public schools.

Gurgaon is a small city just outside of Delhi. It was selected as a location for the expansion of the program because of its large population of out-of-school children. As part of Delhi's urban sprawl, there has been an explosion in the population of Gurgaon over the past 10 years, both among wealthy and poor households.

## 3 Research Design

The research strategy comprised four main components. First, eligible out-of-school children within Gurgaon were identified through a child census of localities selected for treatment. This was then followed by a detailed survey of the children's friendship networks. Third, based on the information

collected in the census, 25 out-of-school children per area were selected to be actively recruited into the program. Finally, we collected detailed participation data on all of the children until the classes closed.

### 3.1 Household Census

The initial survey work began in July of 2006. Through initial community visits, 17 communities ("bastis") with large numbers of out-of-school children were identified. Surveyors then visited these communities to conduct a household census. The census was conducted to map the community, to identify individual out-of-school children, and to collect basic demographic information on all of the children in the bastis. In addition, simple oral reading and math tests were administered to all children between 6 and 14 years old. These tests were identical to those commonly used in Pratham during its large-scale testing activities (e.g., ASER, 2008). The reading test evaluated children on a 0-4 scale, while the math test evaluated children on a 0-3 scale. During the census, supervisors also identified potential teachers from these bastis to teach the bridge classes.

Summary statistics from the household census are calculated in Column (1) of Table 1. Almost half of the children were reported to be out of school, and the vast majority expressed interest attending the bridge classes. Very few children were reported to be working outside of the household (1.4%). The majority of the children surveyed were migrants.

Column (2) restricts the sample to out-of-school children whose parents expressed interest in the bridge course. While these children were similar in age and gender as those in the overall population of the bastis, substantially more were migrants (97% vs. 68%). Not surprisingly, out-of-school children also had much lower test scores than the overall population.

The main purpose of the census was to identify all children eligible for attending the Bridge Course classes and who could then constitute the sample of children for the purposes of the experiment. In order to be eligible, children had to meet two criteria: 1) they had to be currently out-of-school, either having dropped out or never attended, and 2) their parents had to indicate an interest in sending their children to participate in the classes. Overall, 47 percent of children were identified as being out-of-school, and of these children 96 percent had parents that indicated a willingness to send them to the classes. Finally, to be included in the sample, children also had

to complete the friendship survey.

### 3.2 Friendship Survey

After the household census had been conducted, surveyors returned to conduct a social networks survey of the out-of-school children whom families identified as interested in the program. The purpose of this survey was to provide a detailed description of the types of relationships formed by children in our sample. We sought to elicit variation along several dimensions. First, we measure the degree of emotional connection to provided peers. Second, we identify familial/sibling relationships between peers. And finally, we measure the reciprocal nature of these relationships.

The friendship survey consisted of two sections: 1) an open-ended section in which the children could identify their own friends, and 2) a closed-ended section in which children were asked if they were friends with a group of 10 pre-selected out-of-school children. The point of the different sections is to vary the immediacy with which a child recalls a friend's name. Child are rarely able to provide a immediate list of their friends. Instead, some friends are usually easily identified while other friends are identified only through subsequent discussion. Since most friendship network questions only ask for a single list of peers, we sought to identify whether these first remembered peers were more likely to causally mediate participation then friends who were only identified with varying levels of prompting.

The open-ended section consisted of four prompts for the children to identify their friends:

1. Who do you play with?
2. Who do you talk with?
3. Who do you roam around with?

These descriptions of activities were created through informal focus groups with children in the communities designed to elicit their descriptions of how they spent their time with their peers. The questions were also piloted extensively to ensure that the children were understanding the questions as intended. For each of the questions, the surveyor also asked how much time per day and days per week the child spent doing the activity with the identified friend. Ninety-four percent of the children's friends were identified through this first identification request.

For the second level of prompting, children were given a list of popular games played in these communities (e.g., hopscotch, stick-ball), and asked if they played these games with any children not already listed. These games were again identified through the informal focus groups and were designed to act as a more concrete version of the previous activity questions. Six percent of the total peers identified in the open-ended section were given after this additional prompting.

Finally, to measure emotional attachment, the children were asked to identify their best friend, and a friend they would talk to about a problem. Children were allowed to chose more than one friend that fit either category, but in practice very few did. In total, children identified 32 percent of their friends to be "best friends," 32 percent to be friends with whom they discussed problems, and 26 to be "best friends" with whom they discuss their problems.

The closed-ended section contained a list of 10 out-of-school children living near the surveyed child.[2] The child was asked if he knew each potential friend, as well as whether he played with, talked with or roamed around with this potential friend. The hours per day and days per week spent in each activity was also recorded. If a child appeared on the closed-ended section and had already been given as part of the open-ended section, that child was skipped.

Out of the 1889 children identified as out of school and interested in the program as of the household census, 1303 (69%) were successfully interviewed during the friendship survey. Attrition between the household survey and the census was largely due to out-migration during the period between the two surveys, as well as difficulty in locating the children during the day when the surveyors visited. Column (3) of Table 1 displays the summary statistics for those children who completed the friendship survey. As per these observable characteristics, the children were broadly similar to the larger population of out-of-school children who completed the census.

More detailed information from the friendship survey is summarized in Table 2. Column (1) summarizes information from friends identified in either the open- or closed-ended section.[3] On average, the children identified 4.5 friends. Out of these friends, two were also out of school and completed the friendship survey. Because about 40% of children in the randomized sample were

[2] As part of the census, each household was assigned a number. Numbers were assigned in the order the households were visited, in increasing order as the surveyors moved along one side of the street or lane. Thus, a household was geographically close to other houses with similar numbers. The children for the closed-ended questions were selected based on the nearest 10 children to the surveyed child, by house number.

[3] In the closed-ended section, a friend is identified as someone the child indicates spending time with.

assigned to the treatment group, on average each child completing the friendship survey had .9 friends from the treatment group. Columns (2) and (3) summarize information from the open- and closed-ended sections, respectively.

In addition to identifying the child's friends through their descriptions, we also construct a measures of bilateral friendship. Children are considered to be bilateral friends if both children listed the other as a friend in response to one of the survey questions. On average 1.1 friends were in the sample and fell into this category.

### 3.3 Randomization

The randomization included only children from the research sample. All children identified in the baseline survey whose parents expressed a willingness to participate and who completed a friendship survey were included. One class (each class comprised one teacher) was assigned to the 17 localities for every 60 children in the sample. For every 60 children, 25 children were selected for active recruitment. The normal Pratham class includes 25 children and based on the projected participation rates from actively recruited and non-actively recruited children, we anticipated that this should yield about 25 attending children per class. In practice, the actual number of children per class ranged from 43 to 90 students since the number of children was rarely divisible by 60. The resulting fraction of students chosen for treatment per basti therefore ranged from about 25% to 58%, with 42% of children overall assigned to the treatment.

The treatment consisted of actively recruiting children to attend the classes. Before the classes began, treated children were notified when and where the class would be held. In addition, class teachers periodically re-visited the homes of treated children who were not attending to remind them of class and to walk with them to class when necessary. Children not assigned to the treatment group were free to attend the classes as they wished. These children simply did not have the benefit of active recruitment.

Once classes began, they were run following the standard Pratham bridge class model, using the same teaching methods and materials used elsewhere. Because of the time taken by extra data collection activities, classes began at the end of November and were scheduled to run until the next school year began in April. Children were thus exposed to a shorter cycle than was customary.

### 3.4 Participation Measures

Our main outcome of interest is the degree to which students participated in the bridge classes. The participation data is taken directly from the attendance rosters of the class instructors. However, we were very concerned with the quality of this data. As a result, we employed a team of monitors charged with directly overseeing that these records were kept and kept accurately. The monitors visited each class twice per week. They checked that the class was running, ensured that the teachers were actively recruiting the students selected for recruitment, and double checked the attendance rosters.

For the analysis, we focus on two measures of attendance. First, we measure whether or not a child ever attended a bridge course class for even a single day. Second, we measure the days a child attended as a fraction of the number of total days the bridge class in that location was open. The former measure captures whether or not the child ever chose to experience the class or considered participating regularly while the latter metric measures intensity of participation.

## 4 Statistical Models

We primarily use three models in the following analysis. First, we use a simple difference estimator to measure the comparability of the treatment and control groups and the direct treatment effect. Second, we use a simple linear regression model to estimate the relationship between the treatment of a child's friends and child's participation rates. Third, we use instrumental variables to estimate the causal effects of peer attendance on the child's own attendance.

We estimate the simple difference estimator by estimating the following linear model using ordinary least squares:

$$y_{ib} = \alpha + \beta T_{ib} + \gamma Z_b + \kappa X_{ib} + \varepsilon_{ib} \tag{1}$$

The variable $y_{ib}$ is the characteristic of interest (participation rates or demographic characteristics) for child $i$ in basti $b$. The variable $T_{ib}$ is an indicator variable for whether or not a child was selected for active recruitment, and the coefficient $\beta$ is the estimated difference between children selected for active recruitment and those not selected for treatment. The variables $Z_b$ are locality fixed effects which must be included to account for the differential probabilities of selection in

each locality. The variable $X_{ib}$ is a vector containing demographic characteristics of each student at baseline. This includes the child's age, number of siblings in the household, the child's work status, the child's baseline reading score, and the child's math score. This model is primarily used in column (4) of Table 3 and in Table 4.

The friendship model is similar to the simple difference estimator in equation (1), but is used to measure the correlation of $y_{ib}$ with treatment status of friends. The following linear equation is estimated using ordinary least squares:

$$y_{ib} = \alpha + \beta T_{ib} + \delta F_{ib} + \lambda S_{ib} + \gamma Z_b + \kappa X_{ib} + \varepsilon_{ib} \tag{2}$$

As in equation (1), $y_{ib}$ is the variable of interest (demographic characteristics or participation measures). The new variable $F_{ib}$ is the number of treated friends a child has. This term enters either as dummy variables or linearly. The other new variable, $S_{ib}$, is the number of treated out-of-school friends a child has. This variable must be included in the specification because children with more out-of-school friends will mechanically have more treated friends. If the propensity to have out-of-school friends is correlated with the outcome of interest, then the resulting coefficient on the fraction of treated friend will be biased. This specification is the primary model used to estimate the treatment effects and is used in Tables 5 through 8.

Finally, we use a third model to directly estimate the affect of children's peers' participation on their own participation. This model uses equation (2) as a first-stage specification for an instrumental-variable regression of the child's participation level on the average participation level of his or her peers. The first stage takes the following form:

$$p_{ib} = \alpha + \delta F_{ib} + \lambda S_{ib} + \gamma Z_b + \kappa X_{ib} + \varepsilon_{ib} \tag{3}$$

The variable $p_{ib}$ is the average participation level of the child's peers. The independent variables are similar to those in equation (2) and include a indicator variable for whether or not a child has a treated friend and the number of treated friends as well as a indicator variable for whether a child has a friend in the sample and the number of in-sample friends a child has. The equation also includes demographic characteristics and locality indicator variables. The second-stage regression

is specified as follows:

$$y_{ib} = \alpha + \beta T_{ib} + \mu p_{ib} + \lambda S_{ib} + \gamma Z_b + \kappa X_{ib} + \varepsilon_{ib} \tag{4}$$

The variable $y_{ib}$ is the child's average participation level, and the variables measuring the degree of treatment of the child's peers $F_{ib}$ are excluded and used as instruments. This model is used in Tables 9 and 10.

# 5 Internal Validity

In order to provide a valid measure of the effects of peer networks, the research design must satisfy two criteria. First, the treatment assignment must have created a balanced sample—those children assigned to receive the active recruitment must be similar to those not assigned to receive the active recruitment, and the fraction of a child's treated friends must not be correlated with observable characteristics. Second, those children chosen to receive active recruitment must attend at a higher rate than those children not chosen to attend. The difference in participation rates then generates an increase in participation whose causal effects we can trace through the children's various peer networks.

## 5.1 Baseline Composition

Table 3 explores the balance of observable characteristics across treatment categories. The first four columns illustrate the differences between children assigned to the treatment group and those assigned to the control group. As shown in Column (4), no observable characteristic is significantly different between the two groups.

Because the treatment status of the child's friends is a key dependent variable, Columns (5)-(7) of Table 3 check the balance of obserbables by the number of treated friends. Each column reports the results of the regression of the characteristic on the number of treated open-ended friends, best friends, or closed-ended friends, controlling for the total number of friends in each category in the study. As with the basic treatment-control categories, observable characteristics are generally balanced between the number of treated friends. Among all three columns, three coefficients

are significant at the 10 percent level, but this is not surprising given the 27 coefficients in these columns.

### 5.2 Effect of the Treatment

Table 4 shows the results of linear regressions of class attendance on assignment to the treatment group. We use two outcome measures: 1) whether the child attended the class on any day, and 2) the percentage of days the child attended the classes. Overall, 24% of children in the randomized sample attended the classes, and average attendance was 10% over the course of the program. The effect of treatment on attendance is large and highly significant: treated children were around 31% more likely to attend the classes at all, and 13% more likely to attend the classes on a given day. Addition of controls, basti dummies and clustered standard errors changes these estimates little.

Aside from the treatment-control attendance patterns, several observations are worth noting. While boys and girls attended the classes in equal numbers, younger children were significantly more likely to attend. Second, there were no differences in attendance by initial test score. Finally, no children who were reported to be working at the baseline attended the classes, although their parents initially expressed interest. Because the bridge classes are held during the day, a working child would likely have to alter his work behavior in order to attend. It is possible that this change would have been too costly for families with working children.

## 6 Results

### 6.1 Combined Unilateral and Bilateral Ties

As a first look at the causal effects of treated friends, we estimate equation (3) using a set of dummy variables indicating the number of treated friends in both the open- and closed-ended sections. We flexibly control for the number of friends in the study using dummy variables, and for the child's location using a set of basti dummies. Using the binary indicator for any class attendance as the outcome, Figure 1 plots the coefficients on the dummy variables for the number of treated friends, where the omitted category is no treated friends. Because of the small number of children with 4 or more treated friends, the forth category combines children with four or more friends.

As shown by the figure, having a single treated friend has a small and statistically insignificant impact on attendance, and each additional friend does not substantially change the coefficients. Figure 2 repeats this exercise using percentage attendance as the outcome. The general pattern of the coefficients is similar: having any treated friends has a small impact on attendance, but there is little evidence of a monotonic relationship as the number of treated friends increases.

Table 5 presents more parametric estimates of the effect of treated friends. Each column presents the results of a regression of attendance on different functions of treated friends, controlling for the same function of the number of friends in the study. As noted above, the number of friends in the study must be controlled for because the number of treated friends is only random conditional on the number of friends in the study. Columns (1) and (4) use an indicator for any friends treated as the independent variable, combining both friends identified in the open- and closed-ended portions of the survey. In column (1) estimated coefficient on the treated friend is 0.063, indicating that having a treated friend makes the child 6.3 percentage points more likely to attend the class. This coefficient is significant at the 5% level.[4] Note that this coefficient is approximately 20% of the magnitude of the direct effect. In column (4), where the percent of classes attended is the outcome of interest, the estimated coefficient is 0.041, approximately one-third of the direct effect.

Columns (2) and (4) of Table 5 add linear terms for the number of treated friends and the number of friends identified in the study. In these specifications, the coefficients on the treated friend dummies change slightly and are no longer significant. The coefficients on the linear terms are very small, negative and not at all significant. These results suggest that there is little effect of treated friends on attendance beyond the first treated friend.

While the number of treated friends may be related to a child's attendance behavior, it may be the *percentage* of the child's network that it treated which is more directly to behavior. If the child has a large number of friends, for example, having a small number of treated friends may not affect attendance, because he can simply substitute his time away from the treated friends. Columns (3) and (6) of Table (5) include the percentage of the child's friends that are treated as a dependent variable. Note that in order to ensure proper identification, the independent variable is the percentage of friends *in the study* who are treated. In addition, because the denominator only

[4] Because of the small number of clusters (17), significance level uses the t-distribution with 17-2 degrees of freedom (Cameron, Miller and Gelbach, 2007).

includes the number of friends in the study, only children with friends in the study are included in these regressions The regressions show that there is no incremental effect of the percentage of the child's network treated on the child's own attendance, conditional on any friends treated.

Table 6 breaks out the social network effects by whether the friend was identified in the open- or closed-ended section of the friendship survey. Because the friends identified in the open-ended section were volunteered by the child, these ties are more salient to the child and could therefore have stronger effects on the child's attendance. Our results show that this was not the case. We regress attendance on a dummy for whether the child had a friend identified in the open-ended section, whether the child had a friend identified in the closed-ended section, and the interaction of the two variables, where the child had a friend identified in both sections. Thus, if a child had a friend identified in both sections, the overall effect of treated friends equals the sum of the three coefficients. We control for the corresponding variables indicating whether these friends were in the study. The difference between the open-ended and closed-ended dummies equals the relative effect of having either an open-ended or closed-ended friend treated. As shown by columns (1) and (4), the difference between the two effects is small and switches signs between the regression with a binary outcome and the one with a continuous measure of attendance.

We also test whether having a treated best friend increases the likelihood that the child attends the classes. We do so by including a dummy for the treatment status of the best friend, and the corresponding indicator for whether the best friend was in the study. Note that best friends were selected form a subset of open-ended friends, so the coefficient on the binary indicator represents the incremental effect of a treated best friend relative to a tretaed open-ended friend. The results of this regression are presented in columns (2) and (4). In both cases the estimated coefficients are positive but insignificant. While the estimated coefficient is large in the case of the binary attendance outcome, we cannot conclude that having a treated best friend has a significant impact on attendance relative to a treated open-ended friend.

### 6.2 Bilateral Ties

Table 7 presents the effects of bilateral ties–children who listed each other as friends in the friendship survey. We first examine this relationship using binary variables for any bilateral friend treated.

For reference, columns (1) and (5) show the overall effects of any friend treated reported columns (1) and (4) of Table 5. Columns (2) and (6) add dummies for whether there was a bilateral friend treated.[5] The coefficient on this variable therefore represents the difference between having a unilateral friend treated and having a bilateral friend treated. In the regression using the binary outcome, the estimated coefficient on the bilateral friend dummy is .107 , and is significantly different from the unilateral friend treatment category at the 1% level. Similarly, the corresponding coefficient in the regression using percent attendance is large and highly significant. The small and insignificant coefficients on the friend treated dummies suggests that unilateral friends have no impact on attendance.

Turning to the incremental effects of additional treated bilateral friends, columns (3) and (7) repeat columns (2) and (5) of Table 5 for reference. Columns (4) and (8) add variables for the number of treated bilateral friends to show the differences between additional treated unilateral and addtional treated bilateral friends.The regressions show that additional treated bilateral friends have strong effects on attendance relative to treated unilateral friends: an additional treated bilateral friend results in a 9.5% increase in the likelihood of any attendance, and a 6.6% increase in the percent of classes attended. Surprisingly, the inclusion of the number of treated bilateral friends results in a *negative* coefficient on the overall number of friends, which becomes significant at the 5% level in the specification using percent attendance as an outcome.

### 6.3 Siblings

Table 8 examines whether having treated siblings impact attendance in the classes. Column (1) regresses the binary attendance measure on an indicator for any treated sibling. The estimated coefficient is very similar to the effect of having a treated friend, and suggests that having a treated sibling increases participation by 6.6%. Column (3) repeats the estimation using the continuous measure of attendance as the outcome. The estimated coefficient is 0.042, almost identical to the coefficient on any friend treated from column (4) of Table 5.

Columns (2) and (4) estimate attendance as a function of the number of siblings treated. In

[5]The category of "any bilateral friend" includes friends who were listed in *either* section (open- or closed-ended) by both children. For example, a friend could have been listed in the open-ended section by the first child, and the first child could have been identified in the closed-ended section by the second child.

both specifications, the coefficient on the number of treated friends variable is relatively large but not significant. Although the point estimates are substantially larger than the incremental effects of friends estimated in Table 5, we cannot reject the null that additional treated siblings have no effect on attendance.

### 6.4 Effects of Peer Attendance

One of the main purposes of this experiment is to identify the effect of children's peers'participation levels on the participation levels of the children. In our experiment, we manipulate the degree to which children are actively encouraged to participate, allowing us to use the variation in peer attendance attributable to peer treatment in order to estimate this effect. As shown in Table 4, active recruitment does change children's participation levels. We can therefore use the model specified in equations (3) and (4) to estimate the effect of children's peers' participation on the participation levels of children themselves.

Table 9 estimates this model using the children's bilateral friends. Column (1) estimates the first-stage equation (3). Consistent with Table 4, the average participation levels of the bilateral friends is highly correlated with the number of bilateral friends receiving the treatment. Column (2) contains the reduced-form regression within this specification. This estimation is similar to the one in Table 5, column (4) but includes only bilateral friends. Although the coefficients are not individually significant, they are jointly significant at the 5% level (p-value = 0.023).

Column (3) contains the two-stage least-squares estimates using the equation in column (1) for the first-stage regression. The results demonstrate that the participation of a child's peers has a large impact on the child's own participation. The magnitude shows that a ten percentage point increase in the average participation of a child's bilateral friends increases the child's participation level by 4.2 percentage points. Effectively, an increase in participation by a child's peers increases the child's participation by 42 percent as much.

We estimate the same specification for different measures of the child's peer network in Table 10. In each case, we use the model specified in equations (3) and (4), but change the measures of the number of treated peers and the number of in sample peers to match the type of peer relationship under consideration. We estimate the model for all friends (column 1), bilateral friends

(column 2), unilateral friends (column 3), and siblings (column 4). Turning first to column (1), the estimated effect of a 10 percentage point increase in the peers' participation levels results in a 2.7 percent increase in the child's participation level. In columns (2) and (3), the estimates show a sharp difference in the effects of bilateral and unilateral friends. As shown in the previous table, bilateral friends increase a child's participation by 4.2 percentage points for every 10 percentage point increase in average peer participation. However, unilateral friends have almost no effect on children's participation levels. In a combined regression, this difference is statistically significant at the 1% level (p-value $< 0.001$).

Finally, column (4) estimates the effect of siblings. As described before, this mechanism is likely very different than the treatment through friends alone because siblings share parents that could reinforce similar behavior or insist on different participation patterns. As show in Column (4) this effect is, in fact, very large. A child's participation level increases by 6.1 percentage points for every ten percent increase in participation among his or her siblings.

This experiment was designed to identify the effects of peers' participation and to avoid the possible selection effects that would occur if we simply estimate the correlation patterns between a child and his or her friends' participation. An obvious question is whether or not these estimates differ dramatically from what we would estimate had we not used the experiment. To construct these estimates, we use a subsample of our data including only children that were not treated and who had no treated peers. Just as in the estimates of equation (2), conditional on the number of in-sample peers the identification of this sample should be random. As a result, we estimate equation (4) using an OLS regression and not instrumenting for the participation levels of the peers. These estimates are displayed in Table 11. Quite surprisingly, all of the estimates are quite close to the instrumental variables estimates with the exception of the sibling effects which seem to be underestimated by about two-thirds. The inconsistency in the estimated sibling effects is a concern, but even considering that estimate, the estimates do suggest that in this sample and context the OLS estimate does not suffer from a significant positive bias due to the endogenous factors associated with peer network formation.

# 7 Conclusion

This paper examines the peer effects of the decisions of out-of-school to attend classes designed to teach basic literacy and numeracy skills. We study the child's attendance decision as a function of his own invitation to attend the class and invitations for members of his friendship network to attend. Through a within-community randomization of the invitation, we are able to generate exogenous variation in the fraction of a child's peer network that attends the classes. We measure peer effects through a broad survey which seeks to identify both the salience of the ties (through open-ended and closed-ended questions), the type of network (through bilateral and multilateral ties), and the effects of siblings.

We find that active recruitment increases participation by about 30 percentage points. Having a treated friend (either from the open-ended or closed-ended section of the survey) has an effect approximately equal to 6 percentage points, but there is no evidence that having more than one treated friend increases the likelihood of attending. There is some evidence that the strength of the ties does matter, in that unilateral ties have either a negligible or negative impact, while bilateral ties have large positive impacts. Having a treated sibling has an impact approximately equal to having a treated friend.

Using the peers' treatment status as an instrument for peer attendance, we find that friends' attendance has a large impact on the child's own attendance: an increase in the percentage of classes attended by a child's peers increases the child's attendance by 27 percent as much. The effects are strikingly similar to naive OLS estimates of this effect.

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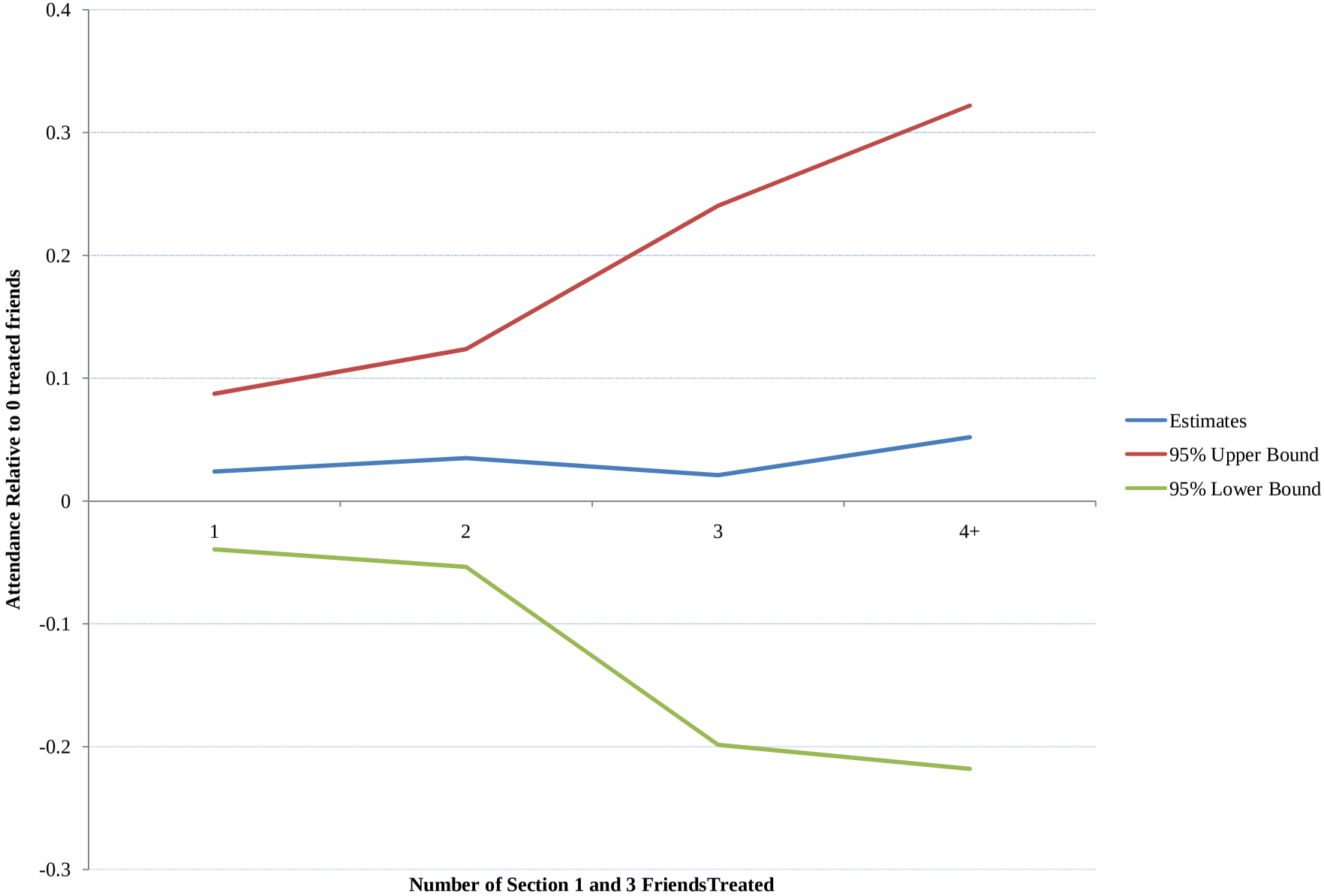
# Figure 1. Attendance 1/0 by Number of Friends Treated



## Figure 2. Percent Attendance by Number of Friends Treated

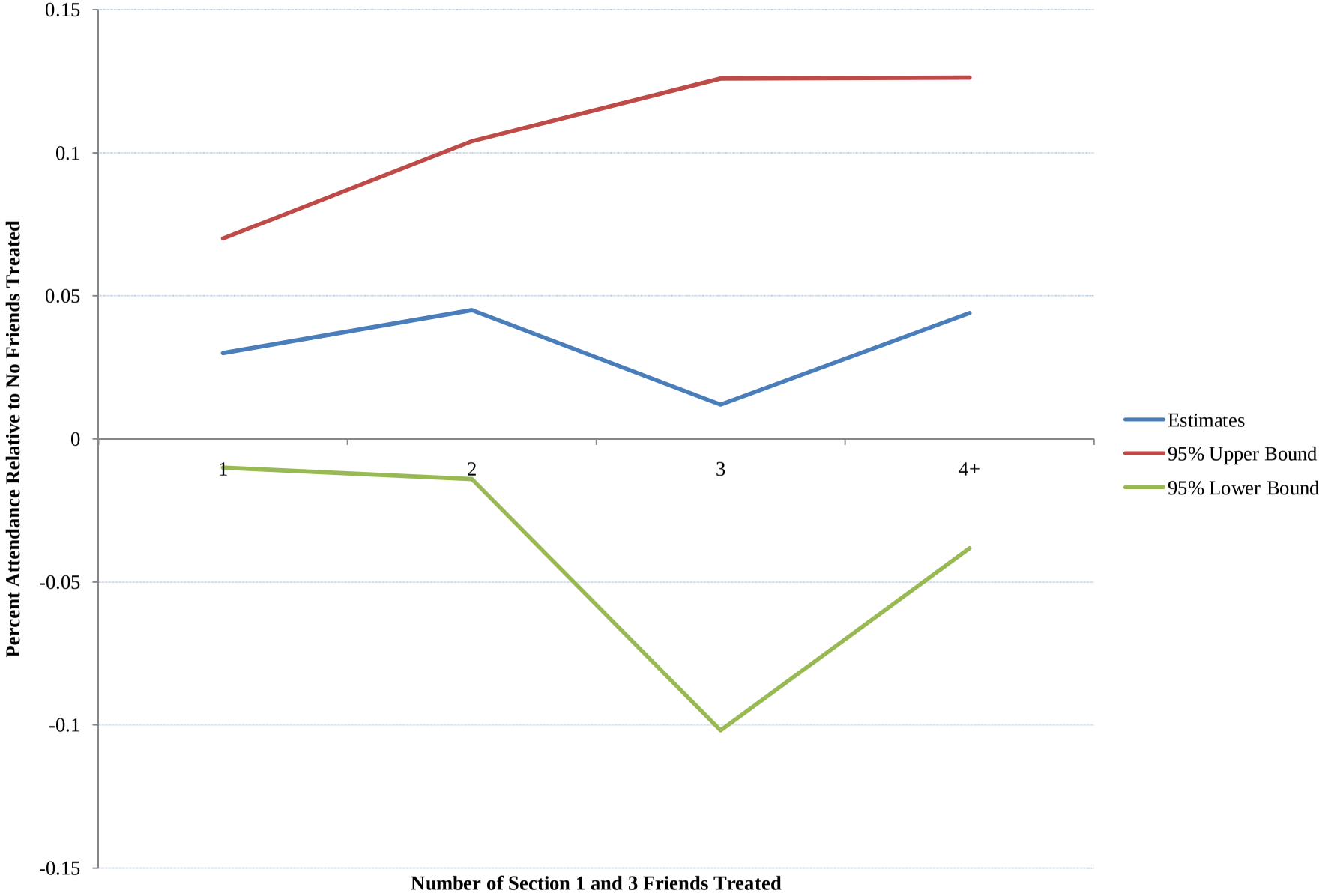


**Table 1. Sample Composition / Summary Statistics**

| | Census | Out of School | Friendship Survey |
|---|---|---|---|
| | (1) | (2) | (3) |
| Out of School | 0.470 | | |
| | (0.499) | | |
| Out of school, not interested | 0.021 | | |
| | (0.145) | | |
| Female | 0.463 | 0.489 | 0.492 |
| | (0.499) | (0.500) | (0.500) |
| Age | 9.100 | 8.692 | 8.554 |
| | (2.439) | (2.390) | (2.321) |
| Children 6-14 per household | 2.222 | 2.083 | 2.009 |
| | (1.110) | (1.064) | (1.021) |
| Migrant | 0.683 | 0.966 | 0.971 |
| | (0.465) | (0.182) | (0.168) |
| Working | 0.014 | 0.010 | 0.008 |
| | (0.116) | (0.097) | (0.087) |
| Reading | 1.287 | 0.256 | 0.222 |
| | (1.628) | (0.781) | (0.728) |
| Math | 0.780 | 0.140 | 0.121 |
| | (1.167) | (0.542) | (0.505) |
| Total Children | 4213 | 1889 | 1303 |

**Table 2. Summary of Friends**

| | Combined | Open Ended | Closed-Ended |
|---|---|---|---|
| | (1) | (2) | (3) |
| Number Listed | 4.534 | 2.878 | 1.617 |
| | (2.563) | (1.674) | (1.748) |
| Number in Study | 2.164 | 0.850 | 1.313 |
| | (1.920) | (1.031) | (1.540) |
| Number Treated | 0.904 | 0.346 | 0.556 |
| | (1.098) | (0.620) | (0.850) |
| Number of Bilateral Friends | 1.120 | 0.299 | 0.504 |
| | (1.313) | (0.590) | (0.851) |
| Number of Treated Bilateral Friends | 0.484 | 0.130 | 0.220 |
| | (0.752) | (0.370) | (0.504) |
| Number of Best Friends | | 0.934 | |
| | | (0.382) | |
| Number of Best Friends in Study | | 0.306 | |
| | | (0.464) | |
| Number of Treated Best Friends | | 0.137 | |
| | | (0.346) | |
| Number of Bilateral Best Friends | | 0.128 | |
| | | (0.334) | |
| Number of Treated Bilateral Best Friends | | 0.063 | |
| | | (0.244) | |
| Number of Siblings in Study | | 0.731 | |
| | | (0.832) | |
| Number of Treated Siblings | | 0.303 | |
| | | (0.547) | |

Sample includes all children who completed the friendship survey.
Cells represent the mean and standard deviation of the variables listed.
Friends represent both unilateral and bilateral ties unless otherwise indicated.
Bilateral friends are friends who indicated a tie with the child in the same category.

**Table 3. Treatment-Control Differences**

| | Total | Treatment | Control | Difference (treat.-cont.) | Number of Treated Open Ended Friends | Number of Treated Best Friends | Number of Treated Closed Ended Friends |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Female | 0.492 | 0.482 | 0.499 | 0.018 | -0.039 | -0.09+ | -0.025 |
| | (0.500) | (0.500) | (0.500) | (0.028) | (0.032) | (0.050) | (0.026) |
| Age | 8.554 | 8.647 | 8.487 | -0.160 | -0.253+ | -0.143 | 0.014 |
| | (2.321) | (2.367) | (2.287) | (0.130) | (0.147) | (0.233) | (0.118) |
| Number of siblings, age 6-14 | 2.009 | 2.002 | 2.014 | 0.013 | 0.030 | -0.059 | -0.028 |
| | (1.021) | (1.056) | (0.997) | (0.057) | (0.065) | (0.103) | (0.051) |
| Working | 0.008 | 0.013 | 0.004 | -0.009+ | -0.009 | -0.014 | -0.003 |
| | (0.087) | (0.113) | (0.063) | (0.005) | (0.006) | (0.009) | (0.004) |
| Reading | 0.206 | 0.222 | 0.194 | -0.029 | -0.010 | 0.024 | -0.004 |
| | (0.706) | (0.791) | (0.638) | (0.040) | (0.045) | (0.072) | (0.036) |
| Maths | 0.111 | 0.134 | 0.094 | -0.041 | 0.016 | 0.003 | 0.026 |
| | (0.483) | (0.541) | (0.436) | (0.027) | (0.031) | (0.049) | (0.025) |
| Number of Open-ended Friends Listed | 2.898 | 2.933 | 2.874 | -0.059 | -0.084 | -0.191 | 0.090 |
| | (1.662) | (1.609) | (1.700) | (0.094) | (0.098) | (0.168) | (0.084) |
| Number of Friends in Study | 0.855 | 0.869 | 0.846 | -0.023 | -- | -0.057 | 0.097 |
| | (1.033) | (1.022) | (1.040) | (0.058) | -- | (0.082) | (0.053) |
| Number of Matched Best Friends in Study | 0.308 | 0.316 | 0.301 | -0.015 | 0.034 | -- | 0.031 |
| | (0.465) | (0.466) | (0.465) | (0.026) | (0.023) | -- | (0.024) |
| Number of Closed-ended Friends Listed | 1.629 | 1.713 | 1.568 | -0.145 | 0.089 | 0.127 | 0.014 |
| | (1.749) | (1.794) | (1.715) | (0.099) | (0.111) | (0.177) | (0.034) |
| Number of Closed-ended Friends in Study | 1.322 | 1.399 | 1.267 | -0.132 | 0.098 | 0.162 | -- |
| | (1.542) | (1.593) | (1.503) | (0.087) | (0.098) | (0.156) | -- |
| Observations | 1303 | 544 | 759 | 1303 | 1303 | 1303 | 1303 |

Columns (1), (2) and (3) represent the sample means and standard deviations of the variables listed. Columns (4) through (7) represent the estimated coefficients and standard errors of regressions of the characteristic listed on the treatment variable indicated for each column.
+ significant at 10%; * significant at 5%; ** significant at 1%

**Table 4. Class Attendance**

| | Dependent Variable | | | | | |
|---|---|---|---|---|---|---|
| | Attendance 1/0 | | | Percent Attendance | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treatment | 0.312** | 0.313** | 0.321** | 0.125** | 0.125** | 0.128** |
| | (0.027) | (0.026) | (0.025) | (0.012) | (0.018) | (0.018) |
| Female | | | 0.001 | | | 0.007 |
| | | | (0.030) | | | (0.018) |
| Age | | | -0.024** | | | -0.008** |
| | | | (0.004) | | | (0.002) |
| Children 6-14 in Household | | | 0.011 | | | 0.005 |
| | | | (0.016) | | | (0.010) |
| Working | | | -0.187* | | | -0.074* |
| | | | (0.071) | | | (0.029) |
| Read | | | -0.011 | | | -0.012 |
| | | | (0.028) | | | (0.009) |
| Maths | | | -0.007 | | | 0.009 |
| | | | (0.030) | | | (0.015) |
| Constant | 0.107** | 0.107** | 0.286** | 0.050** | 0.050** | 0.100** |
| | (0.016) | (0.011) | (0.036) | (0.008) | (0.007) | (0.023) |
| Basti Fixed Effects? | NO | YES | YES | NO | YES | YES |
| Observations | 1303 | 1303 | 1303 | 1303 | 1303 | 1303 |
| R-squared | 0.131 | 0.161 | 0.184 | 0.119 | 0.119 | 0.129 |

Standard errors clustered by basti.
+ significant at 10%; * significant at 5%; ** significant at 1%

**Table 5. Friends Regressions**

| | Dependent Variable | | | | | |
|---|---|---|---|---|---|---|
| | Attendance 1/0 | | | Percent Attendance | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Friend Treated | 0.063* | 0.048 | 0.088 | 0.041* | 0.038 | 0.055 |
| | (0.025) | (0.038) | (0.071) | (0.019) | (0.023) | (0.040) |
| Number of Friends Treated | | -0.006 | | | -0.003 | |
| | | (0.027) | | | -0.012 | |
| Percent of Friends Treated | | | -0.076 | | | -0.029 |
| | | | (0.084) | | | (0.047) |
| Friend in Study | 0.091 | 0.077 | | 0.067* | 0.063* | |
| | (0.085) | (0.079) | | (0.028) | (0.029) | |
| Number of Friends in Study | | 0.012 | | | 0.004 | |
| | | (0.013) | | | (0.006) | |
| Treatment | 0.320** | 0.318** | 0.327** | 0.129** | 0.128** | 0.128** |
| | (0.025) | (0.025) | (0.031) | (0.019) | (0.019) | (0.022) |
| F-Stat: Treated Friends Have No Effect | 6.153 | 1.154 | 0.986 | 4.924 | 1.915 | 1.862 |
| P-Value | 0.025 | 0.34 | 0.394 | 0.041 | 0.18 | 0.187 |
| Observations | 1296 | 1296 | 1027 | 1296 | 1296 | 1027 |
| R-squared | 0.188 | 0.189 | 0.194 | 0.138 | 0.138 | 0.141 |

Standard errors clustered by basti.
Regressions include all variables in Table 4 and basti dummies as controls.
+ significant at 10%; * significant at 5%; ** significant at 1%

**Table 6. Friends Regressions by Type of Friend**

| | Dependent Variable | | | |
|---|---|---|---|---|
| | Attendance 1/0 | | Percent Attendance | |
| | (1) | (2) | (3) | (4) |
| Open-ended Treated | 0.057 | 0.022 | 0.028 | 0.021 |
| | (0.036) | (0.044) | (0.022) | (0.026) |
| Closed-Ended Treated | 0.05 | 0.048 | 0.044 | 0.042 |
| | (0.043) | (0.043) | (0.026) | (0.026) |
| Open and Closed-Ended Treated | -0.02 | -0.015 | -0.014 | -0.012 |
| | (0.067) | (0.066) | (0.036) | (0.036) |
| Best Friend Treated | | 0.066 | | 0.008 |
| | | (0.045) | | (0.021) |
| Open-ended in Study | -0.177 | -0.180+ | -0.142+ | -0.143+ |
| | (0.103) | (0.102) | (0.071) | (0.071) |
| Closed-ended in Study | 0.031 | -0.058 | 0.044+ | -0.011 |
| | (0.047) | (0.058) | (0.023) | (0.026) |
| Open and Closed-ended in Study | 0.191+ | 0.192+ | 0.140* | 0.140* |
| | (0.099) | (0.099) | (0.065) | (0.066) |
| Best Friend in Study | | 0.097+ | | 0.059** |
| | | (0.049) | | (0.016) |
| Treatment | 0.318** | 0.316** | 0.128** | 0.128** |
| | (0.024) | (0.025) | (0.019) | (0.019) |
| F-Stat: Treated Friends have no effect | 4.311 | 3.458 | 2.506 | 1.724 |
| P-Value | 0.021 | 0.032 | 0.096 | 0.194 |
| Observations | 1296 | 1296 | 1296 | 1296 |
| R-squared | 0.191 | 0.195 | 0.144 | 0.146 |

Standard errors clustered by basti.
Regressions include all variables in Table 4 and basti dummies as controls.
+ significant at 10%; * significant at 5%; ** significant at 1%

**Table 7. Bilateral Ties**

| | Dependent Variable | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Attendance 1/0 | | | | Percent Attendance | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Friend Treated | 0.063* | -0.012 | (0.048) | 0.036 | 0.041* | 0.004 | (0.038) | 0.032+ |
| | (0.025) | (0.027) | (0.038) | (0.037) | (0.019) | (0.011) | (0.023) | (0.018) |
| Bilateral Friend Treated | | 0.107** | | 0.008 | | 0.056** | | -0.003 |
| | | (0.033) | | (0.049) | | (0.016) | | (0.030) |
| Number of Treated Friends | | | -0.006 | -0.051 | | | -0.003 | -0.032* |
| | | | (0.027) | (0.030) | | | (0.012) | (0.012) |
| Number of Treated Bilateral Friends | | | | 0.095* | | | | 0.066** |
| | | | | (0.036) | | | | (0.023) |
| Friend in Study | 0.091 | 0.078 | 0.077 | 0.07 | 0.067* | 0.063* | 0.063* | 0.059+ |
| | (0.085) | (0.086) | (0.079) | (0.079) | (0.028) | (0.029) | (0.029) | (0.028) |
| Number of Friends in Study | | 0.02 | | 0.011 | | 0.005 | | 0.014 |
| | | -0.019 | | (0.033) | | -0.012 | | (0.015) |
| Bilateral Friend in Study | | | 0.012 | 0.011 | | | 0.004 | 0.009 |
| | | | -0.013 | -0.017 | | | -0.006 | -0.006 |
| Number of Bilateral Friends in Study | | | | -0.003 | | | | -0.015 |
| | | | | -0.026 | | | | -0.013 |
| Treatment | 0.320** | 0.315** | 0.318** | 0.315** | 0.129** | 0.126** | 0.128** | 0.127** |
| | (0.025) | (0.024) | (0.025) | (0.024) | (0.019) | (0.018) | (0.019) | (0.018) |
| F-Stat: Treated Friends have no effect | 6.153 | 6.345 | 1.154 | 5.604 | 4.924 | 6.093 | 1.915 | 4.61 |
| P-Value | 0.025 | 0.009 | 0.34 | 0.005 | 0.041 | 0.011 | 0.18 | 0.011 |
| Observations | 1296 | 1296 | 1296 | 1296 | 1296 | 1296 | 1296 | 1296 |
| R-squared | 0.188 | 0.198 | 0.189 | 0.203 | 0.138 | 0.146 | 0.138 | 0.152 |

Standard errors clustered by basti.
Regressions include all variables in Table 4 and basti dummies as controls.
+ significant at 10%; * significant at 5%; ** significant at 1%

**Table 8. Siblings Regressions**

| | Dependent Variable | | | |
|---|---|---|---|---|
| | Attendance 1/0 | | Percent Attendance | |
| | (1) | (2) | (3) | (4) |
| Any Treated Sibling | 0.068+ | 0.012 | 0.042+ | -0.016 |
| | (0.034) | (0.084) | (0.022) | (0.046) |
| Number of Treated Siblings | | 0.043 | | 0.055 |
| | | (0.063) | | (0.039) |
| Any Sibling in Study | 0.021 | -0.001 | 0.002 | 0.018 |
| | (0.031) | (0.060) | (0.020) | (0.033) |
| Number of Siblings in Study | | 0.028 | | -0.014 |
| | | (0.047) | | (0.026) |
| Treatment | 0.319** | 0.319** | 0.128** | 0.130** |
| | (0.024) | (0.023) | (0.018) | (0.018) |
| F-Stat: Treated Sibs have no effect | 3.915 | 1.98 | 3.771 | 2.3 |
| P-Value | 0.065 | 0.17 | 0.07 | 0.132 |
| Observations | 1296 | 1296 | 1296 | 1296 |
| R-squared | 0.187 | 0.188 | 0.134 | 0.136 |

Standard errors clustered by basti.
Regressions include all variables in Table 4 and basti dummies as controls.
+ significant at 10%; * significant at 5%; ** significant at 1%

**Table 9. Effect of Friend's Participation on Own Participation**
**Bilateral Friends**

| | Dependent Variable | | |
|---|---|---|---|
| | Avg Friend Attendance | Percent Attendance | Percent Attendance |
| | FS | RF | IV |
| | (1) | (2) | (3) |
| Avg Friend Attendance | | | 0.424** |
| | | | (0.114) |
| Any Friend Treated | 0.076* | 0.036 | |
| | (0.030) | (0.039) | |
| Number of Treated Friends | 0.061** | 0.023 | |
| | (0.012) | (0.028) | |
| Child Treated | | | -0.029+ |
| | | | (0.015) |
| Any Friend in Sample | -0.036** | (0.004) | (0.009) |
| | (0.010) | (0.014) | (0.007) |
| Number of Sample Friends | | | 0.119** |
| | | | (0.018) |
| Observations | 1296 | 1296 | 1296 |
| R-squared | 0.27 | 0.15 | 0.22 |

Standard errors clustered by basti.
Regressions include all variables in Table 4 and basti dummies as controls.
+ significant at 10%; * significant at 5%; ** significant at 1%

**Table 10. IV Estimates**
**Effect of Peers' Participation on Own Participation**
**by Peer Type**

| | Type of Peer | | | |
|---|---|---|---|---|
| | All Friends | Bilateral | Unilateral | Siblings |
| | (1) | (2) | (3) | (4) |
| Avg Friend Attendance | 0.273* | 0.424** | -0.067 | 0.616* |
| | -0.125 | -0.114 | -0.184 | -0.221 |
| Any Friend in Sample | 0.058* | -0.029+ | 0.019 | -0.026 |
| | -0.026 | -0.015 | -0.02 | -0.025 |
| Number of Sample Friends | 0.005 | 0.009 | 0.003 | 0.009 |
| | -0.003 | -0.007 | -0.006 | -0.016 |
| Child Treated | 0.126** | 0.119** | 0.127** | 0.124** |
| | -0.02 | -0.018 | -0.019 | -0.016 |
| Observations | 1296 | 1296 | 1296 | 1296 |
| R-squared | 0.2 | 0.22 | 0.12 | 0.25 |

Standard errors clustered by basti.
Regressions include all variables in Table 4 and basti dummies as controls.
+ significant at 10%; * significant at 5%; ** significant at 1%

**Table 11. OLS Estimates**
**Effect of Friend's Participation on Own Participation**
**by Peer Type**

| | Type of Peer | | | |
|---|---|---|---|---|
| | All Friends | Bilateral | Unilateral | Siblings |
| | (1) | (2) | (3) | (4) |
| Avg Friend Attendance | 0.204* | 0.402* | 0.044 | 0.224* |
| | (0.093) | (0.179) | (0.099) | (0.105) |
| Any Friend in Sample | 0.005 | 0.024 | -0.006 | 0.004 |
| | (0.011) | (0.016) | (0.021) | (0.040) |
| Number of Sample Friends | 0.006 | -0.032** | 0.021 | 0.01 |
| | (0.008) | (0.011) | (0.013) | (0.030) |
| Observations | 342 | 342 | 342 | 342 |
| R-squared | 0.11 | 0.15 | 0.08 | 0.12 |

Standard errors clustered by basti.
Regressions include all variables in Table 4 and basti dummies as controls.
+ significant at 10%; * significant at 5%; ** significant at 1%