Limits to Experimental Evaluation and Student Attendance in Rural Indian Schools: A Field Experiment

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Abstract

This paper reports the results of a field experiment in schools for the children of migrant workers in rural north India. The experiment tested whether the "megaphone program" — an intervention using social psychology and focused on children's agency — would improve attendance among students enrolled in an NGO's primary schools. While standard impact evaluation econometrics initially suggests the program may have been weakly effective, operational data reveals that the program probably had no impact on student attendance.

Further results illustrate challenges for development policy and caveats about experimental evaluation. Teacher attendance, confirming previous findings, is a first-order problem for rural education. Impacts measured among capable implementers may have little validity when applied to less capable organizations. In some cases, donor and implementing organizations' commitment to delivering quality services is the most important obstacle to using impact evaluations to improve lives. More attention should be given to the question of how to attract organizations that are dedicated to improving their effectiveness to the service of deprived populations.

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1 Introduction

Estimates of the number of out of school children in India vary widely, but all are in the millions. One group that faces severe challenges to school participation is impoverished internal migrant workers. It is difficult to generalize about their lives; but at least in part due to the nature of their parents' work, migrant children are often excluded from school and other basic services.

This paper reports an experimental evaluation of a program intended to increase student attendance at NGO schools for the children of migrant workers. The megaphone program was designed to alter the children's social psychological environment. Possibly due to the NGO context, the program probably had no effect on student attendance, even though, by some measures of attendance, there initially appears to be a statistically significant difference in attendance between the treatment and control schools. In addition to presenting the findings of the experiment, the results section reviews some non-experimental findings about the operations of the schools that may have policy implications.

The discussion addresses why the intervention probably did not work, and suggests that further research be done about the role of social psychology and children's agency in school attendance. The experiment stresses the importance of teacher attendance to educating poor, rural children, and offers an addendum to the ongoing discussion about the external validity of impact evaluations: we should be wary of impact evaluations' applicability not only to scale-ups and government projects, but also to low-capacity NGO settings. Finally, this study concludes that there is not yet a role for experimental evaluation where service providers do not exist or do not invest in quality service delivery. First, more attention must be given to the question of how to attract quality service providers to populations as remote and deprived as the migrants in this study.

1.1 Context of the experiment

This study was conducted in a rural district of north India between October, 2008 and April, 2009. A single local non-governmental organization (NGO) managed all of the schools and teachers involved in the experiment. While the organization had low capacity, it was the only one serving implementing education programs for the three groups of migrant workers in the experiment: salt workers, fishermen and nomadic herders.[1] Of the 26 schools in the experiment, 14 served salt workers, 9 served fishermen, and 3 served herders.

Salt workers are predominately Hindu and live in cement, cloth or other improvised structures at marine salt pan worksites. Fishermen and nomadic herders are almost uniformly Muslim, and live in canvas tents. Fishing families spend the majority of the year living on the beach, while herders live in remote locations in the interior of the district. Though representatives of salt companies sometimes claim that the children who live in the pans attend school, there were no other schools available to the children in the experiment except the ones offered by the NGO.[2] The remoteness of the migrants' locations make government education and health services almost inaccessible and obtaining water and staple food sources a struggle.

2 Conceptual framework: Children's school attendance

2.1 Interventions for improving student attendance

Drèze and Kingdon (2001) observed that "relatively little is known about why so many Indian children are out of school" (1). While there unfortunately remains much to learn, investigation into interventions to improve children's school participation in developing countries has

[1] Though another organization had a few schools for salt workers' children, they seemed to be trying to turn them over to the organization that hosted the experiment.

[2] I met a few fisher and salt worker families that left older children in a village with extended kin in order for the child to attend school, but this is a rare exception rather than the norm. Coffey and Spears (2009), a demographic survey of salt worker families, found that only 3% of children under 16 were left in the village when their parents migrated.

been done.

Many recent evaluations of educational programs in developing countries have used randomized experiments. Implicitly or explicitly, almost all evaluated interventions attempt to influence parents' cost-benefit calculus over investing in children's human capital. Is schooling worth the direct and opportunity costs? More formally, a parent asks if

$$\mathrm{E}[W(S;I) - W(NS;I)] + B(S) > C(S), \tag{1}$$

where $W(S;I)$ is a child's adult welfare, as valued by the decision maker, if she goes to school given a certain level of school inputs, $W(NS;I)$ is a child's future welfare if she does not, $B(S)$ are the immediate benefits of sending her to school and $C(S)$ are the costs of schooling, including lost opportunity costs, such as work.

Roughly, the interventions can be grouped into four categories: those targeting $W(S;I)$ by increasing I, those aimed at increasing the value of $W(S;I)$ in expectation; those providing immediate incentives for participation by increasing B, and those reducing costs (lowering C). Table 2 presents a representative summary of recent evaluations, organized according to this framework.

While this essay's experiment focused on student attendance, previous evaluators have recognized that one strategy to increase student participation is to promote teacher attendance. Targeting teacher attendance targets both I and $C(S)$, since teaching is a primary input in schooling, and since students who walk to school and find the teacher absent have paid costs of time and energy. An influential study by Duflo et al. (2008) showed that external monitoring and incentives can be effective in getting more teachers to attend school. Researchers provided a randomly selected group of rural teachers with tamper-proof cameras to document their own attendance, and made pay dependent on teacher attendance. While the number of students in classes in the treatment group did not increase, teacher attendance did, and so did the amount of time students spent in school. The authors estimated that

the intervention increased the amount of child-days of education by about one-third.

Though some of these interventions are inexpensive, particularly deworming, cameras and providing statistics about the returns to education, all of them require some financial resources and administrative capacity on the part of the implementing organization — to say nothing of a commitment to improvement. As section 5.3 will discuss, due to the heterogeneity among NGOs, these results may not be directly transferable.

2.2 School attendance, social psychology, and agency: The megaphone program

In the framework of equation 1 — into which almost all randomized attendance interventions fit — schooling decisions are made as a cost-benefit analysis, by parents. While these factors are clearly relevant, they may not fully explain school attendance, and may not highlight the strategies most likely to succeed in the hands of all NGOs. Social pyschological factors surely complement rational choice to influence attendance, and children themselves might be, at least in part, responsible for their own school participation.

2.2.1 Social norms, self-signaling, social proof and habits

Social psychological factors have been shown to influence behavior in a wide variety of settings. Four concepts from social psychology inspired the intervention used in this experiment: social norms, self-signaling, social proof and habits.

First, Cialdini and Trost (1998) define social norms as "rules and standards that are understood by members of a group, and that guide and/or constrain social behavior without the force of laws" (152). They specify three types of social norms: descriptive, which inform us "how others act" in similar situations; injunctive, which tell us "what others denounce;" and subjective, which send us behavioral messages based "on our own internal standards and sanctions for good conduct" (162).

Focusing peoples' attention on social norms can be an effective way to influence their behavior. Kallgren et al. (2000) point out that individuals are more likely to responsibly dispose of trash when exposed to a relevant normative message prior to behaving. A study about environmental protection by Cialdini (2003) described how invoking only descriptive norms was more effective in encouraging pro-environmental behavior than invoking only injunctive norms, and that positive messages were more effective than negative ones.[3] Festinger (1954) argues that the norms that motivate our behavior the most are those which we consider shared with the people most like ourselves.

Second, self-signaling is the idea that "individuals come to know their own attitudes, emotions and other internal states partially by inferring them from observations of their own overt behavior" (Bem, 1972, 2). Cognitive dissonance theory suggests that people are uncomfortable when they become aware that they hold or display logically inconsistent beliefs; evidence for this theory is provided by Festinger and Carlsmith (1959). Combined with self-signaling, applications of cognitive dissonance theory may be useful for manipulating behavior. People who are encouraged to do something difficult or to publicly espouse an opinion that they do not initially believe may later become convinced that the behavior was worthwhile or the idea true.

Third, social proof, inferring that a behavior is advantageous because others are seen to do it, is like signaling the inference is made from others' behaviors. Cialdini et al. (1999) review the various experiments that have demonstrated the importance of social proof. One example is a study by Bandura et al. (1967) which found that children who were originally afraid of dogs behaved less fearfully after observing other children playing with dogs. Cialdini et al. (1999) find that social proof is more important for determining behavior in collectivistic societies than in individualistic ones.

Finally, social psychological research has shown that habits, defined by Wood and Neal

[3]Accordingly, telling youth that most teenagers behave responsibly with alcohol would be more effective in reducing alcohol abuse than telling them that too many teenagers abuse alcohol.

(2007) as "learned behavior to repeat past responses" (843), play an important role in our behavior. Habits often emerge from situations where a cue is repeated over time and is followed by a behavior triggered by that cue. Cues can be preceding actions, such as brushing one's teeth and taking a shower, or physical settings, such as arriving at the corner café and purchasing a coffee. According to Wood and Neal (2007), habits can help us implement goals by making processes more automatic and therefore more effortless, or they can distract us from goals, as in when contact with drug use-associated environmental stimuli induce recovering addicts' cravings.

2.2.2 Children's agency

As section 2 reviewed, experiments testing methods of promoting student attendance have generally focused on changing how parents weigh the costs and benefits of schooling. Additionally, theoretical models such as Udry (2006), imply that decisions about children's school attendance are made by *parents* based on the trade-offs presented by the present income generated by child labor and the future income generated by educated offspring. This section questions the assumption that parents are the sole actors in decisions about a child's schooling.

There is evidence that many poor parents may not be making the kind of calculated decisions about school attendance discussed in some literature. Banerjee et al. (2008) conclude that poor parents in rural India have relatively little information about the quality of their children's education and its possible effects on their lives. Nguyen (2008) reports similar findings from Madagascar. Levinson et al. (2007) suggests that work opportunities for children tend to be highly intermittent and uncertain, so parents may have difficulty anticipating the benefits to work.

In addition, children's own ideas about how they will spend their time may be overlooked. A child's desire to study rather than work, and her decisions about how to spend her free time, may play an important role in her attendance. Little empirical research has been done

into children's choices, though some economists, such as Levison (2000), suggest that research should begin to explore the role of children's agency in theories of economic reasoning.

One such study was recently completed. Berry (2009) experimentally tested whether giving children, rather than adults, incentives to attain a reading goal was more effective. Overall the treatments had an equal effect, but offering children a toy rather than offering their mothers an equally valued cash payment was more likely to encourage attainment of the reading goals in situations where parents had less influence over their children's schooling.

2.3 The megaphone program

The program gave randomly chosen schools a tin megaphone, which the children were to use to call one another to school. The children were also taught a song about going to school:

(translation)Big kids, little kids, boy and girls
We all go to school every day (2X)
We went to school yesterday, we'll also go today, and then tomorrow
We all go to school every day (2X)
C'mon kids, let's dash, let's run (2X)
Let's dash, let's dash, let's dash all you kids
Let's run, let's run, let's run all you kids!

The program was supposed to operate as follows: each day, at the end of school, a different child takes the megaphone home with her; the next day, before school, she walks around the settlement singing the song into the megaphone; she returns the megaphone to the teacher at school.[4]

The program drew substantially from the social psychology research described in section 2.2.1 and focused on children's agency. In the migrant worker settlements in the experiment, social norms about school attendance are very weak. A survey of salt pan workers found that

[4]The student whose responsibility it was to call the others to school was instructed to sing the song between his house and the school, rather than to arrive at the school and then make rounds of the settlement, so that he did not know whether the teacher was present or not. If the teacher was not present, he was to take the megaphone home and try again the next day.

94% of women had never been to school and only 28% of their husbands had ever attended (Coffey and Spears, 2009). In theory, the program tries to establish the social norm of school attendance by means of self signaling and social proof. First, the child who uses the megaphone makes a substantial effort to advertise to others that she is going to school. She concludes from that behavior that going to school is something that she enjoys and values, and starts to go more often. Second, the student offers social proof to her classmates and adults in the settlement that at least some of their neighbors go to school. Others infer by the advertisement of the child using the megaphone that school-going is a good idea.

Additionally, the song's normative messages — which are both positive and descriptive — are designed to encourage school going. Rather than lamenting how few children are literate or warning of the social and economic consequences of failing to go to school, they claim that school going is for everyone, everyday, and that it is fun.

Finally, the megaphone itself is a habit-inducing cue. The NGO staff mentioned that in the area where the experiment took place similar megaphones are used to gather people for a meeting. The megaphone might become a cue that induces the habit of setting out to meet one's friends and teacher at school.

In part, the megaphone program was chosen for its feasibility in organizations lacking human and financial resources. The program, which included the megaphone, a laminated sheet of instructions and song lyrics, and an erasable pen to keep track of which children had used the megaphone in a given week, cost only about five US dollars per school. A school could use the same megaphone and participation sheet for several years. The implementation involved teaching the students and teachers the song and how to use the megaphone. There was little threat of a staff member or teacher appropriating the materials for personal use or selling them.

3 The field experiment

The field experiment ran from October 2008 to April 2009. In October, I collected enrollment lists from the schools' coordinators.[5] The lists I used gave the name and sex of each child enrolled at a given school, as well as the teacher's name.[6] Every school for the children of migrant workers that the organization's coordinators reported to have a teacher during the month of October was included in the study. The megaphone program was introduced in treatment schools in the last two weeks of December, 2008. During January, February and the beginning of March, I monitored the implementation of the program in the treatment schools.[7]

3.1 Treatment assignment: Blocking and avoiding spillovers

I used blocking to assign schools to treatment and control groups. I blocked over the type of work the children's parents did, the school's main funder (in both cases well-known international donors), its coordinator (a staff member hired by the NGO to manage the school),[8]

[5]Enrollment in this organization's schools occurred when a coordinator went to the vicinity of the school and asked the surrounding families about their children. The names of the children, their sexes, and in some cases their approximate ages and grades (which a child might know if she had ever been enrolled in school or tested for grade level) were recorded. The families may or may not have been told that giving this information meant that their children were "enrolled" at the NGO's school. They may not have been told the name of the teacher or what times and days the school would be in session.
It is probable that many children who lived in the area of the school and were of appropriate age were left off the lists for a variety of reasons: the coordinator did not notice the child's house; her house was a far walk for the coordinator; the coordinator had already recorded what seemed to him to be an appropriate number of names; the child was not considered to be a school-going child by her family (perhaps she was disabled), etc. Though the NGO reported to donors that classes began in September, many coordinators had not produced enrollment lists by the end of October.
Thus, a child's "enrollment status" at one of this NGO's schools does not always accurately describe his or her relationship with the school.

[6]I had planned to include all of the schools for children of migrant workers that the NGO had included in its budget proposals to funders. However, this was not possible, since the organization had collected funds for several schools did not have teachers.

[7]This involved calling teachers in the experimental group to ensure they were using the megaphones and that their students knew the song and visiting schools to see if they were using the megaphones.

[8]While in October, the blocking balanced treatment and control schools by coordinator, by the end of the experiment this had become irrelevant, since many coordinators had quit. Others were absent for long stretches during the experiment; it was unclear whether these individuals were engaged by the organization or not. Still others were given non-school related responsibilities and in some cases new coordinators were hired.

the number of students on initial enrollment lists, the teacher's attendance in the first round and a subjective measurement of how well the school was working, which took into account the teacher's capability and dedication and the students' engagement. Within blocks, many of which were simply pairs, I divided schools into two clusters. A cluster might contain more than one school if schools are near one another, to ensure spillover effects do not contaminate the results. Spillovers from the megaphone program might have occurred if children from a control school learned the song from children in a treatment school or were exposed to children using the megaphones. In blocks, one cluster was randomly assigned to the treatment group and the other to the control group.

3.2 Data and measurement

Data was collected in four rounds, two before and two after the intervention. In each round, I collected data on whether each teacher assigned to the school was present,[9] how many boys and girls were in the classroom at the time of the visit,[10] whether or not the teacher was teaching at the time of arrival, and whether each child on the enrollment list was present when her name was called.[11] If a child was not present in the classroom when her name was called, she was counted as absent.[12] The lists were printed in a random order, though boys were grouped at the beginning and girls at the end. During the first and third rounds, I read the attendance lists from top to bottom and during the second and fourth rounds from bottom to top.[13]

[9] A teacher was counted as present if he was in the vicinity of the school and appeared to intend to spend time with students that day.

[10] Though I made efforts to close the classroom door (if the school had one) upon arrival, students sometimes came and went during the roll call. I counted the students immediately upon entering the room and again after the roll call, and averaged the results to get the $numberofstudents_{jt}$ variable.

[11] When I arrived at the school, I said to the students, in the state language,"Hello. My name is Diane. I would like to take attendance. When I say your name, please say, "Present, miss!" in a loud voice. Is that ok?" If the students did not understand the state language, I asked the teacher or driver to translate this into the local language.

[12] Some children were shy and did not answer when their names were called. Inevitably, their friends would inform me of their presence, which I confirmed with the children.

[13] I did this in case the children at the end of the list were more distracted or tired than those at the beginning and therefore less likely to answer when their name was called.

Because I wanted to measure the effect of the megaphone program on student attendance, and because student attendance is dependent on teacher attendance, I made two attempts to collect attendance data in each round in the schools where a teacher was not present on the first visit. Therefore, the minimum number of valid data collection visits to each school during the experiment was four, and the maximum number was eight. A data collection visit was considered valid if the teacher did not know about the visit in advance.

There are several measurement problems that may have affected the experiment. I do not believe that any of these challenge the internal validity of the experiment, but they do add noise to the results.

- **Working with a migrant population.** Several children on the enrollment lists moved to different worksites, becoming unable to attend the schools at which they were "enrolled" in October. New children moved to the worksites during the experiment and some attended school. When teachers told me that many of their students had moved, it was sometimes true and sometimes an excuse for why so few of the children whose names I was calling were present. I did not alter the original lists throughout the experiment, neither purging names of students who had possibly moved, nor adding the names of new students who joined. While this surely added noise to my measurement, I have no reason to believe that students' migration was differential across treatment and control groups.

- **School Attrition.** Of the 26 schools that were part of the experiment as of October, 8 had closed by the end of the experiment. One fourth of the 1046 students enrolled in the 26 schools were enrolled in a school that had closed. 4 of the closed schools were assigned to the treatment group.

- **Making surprise visits.** Due to time and transportation constraints, I was not able to visit the schools in a random order to take attendance. However, I tried to make my visits unpredictable to teachers and coordinators, often by not telling the driver where

we were going until the day of the visit. Nonetheless, there were several occasions when, being a one of perhaps two conspicuous non-Indians living in the district, I was spotted by an acquaintance on the road to a school, which may have resulted in a teacher being warned. In cases in which I suspected that the teacher was present because he had been warned, I discarded the data and made an extra visit to the school.[14] After an initial incident in which a teacher called his colleagues in nearby schools to warn them that I had visited his school and might be on my way to theirs,[15] I began leaving monitors with teachers who might warn others until I had finished the day's data collection. In spite of these efforts, it is possible that the difficulties of making surprise visits to the schools added substantial noise to the data.

- **Teacher turnover.** Teacher turnover was also an important problem. Of the 33 teachers assigned to work at the 26 schools in October, only 14 were still employed by the organization at the end of the experiment. For some schools, multiple teachers were hired and then dismissed (or quit of their own accord) during the six months, so a total of 43 teachers participated in the experiment. Through contact with the staff, I learned quickly when teachers came and went. Therefore, I was able to orient teachers hired at treatment schools to the program even after the formal implementation of the program. However, the teacher turnover, like teacher absence, was likely an important disruption to the children's schooling and also to the megaphone program.

3.3 Did treatment schools receive the treatment?

The four schools assigned to the treatment group that closed before the end of the experiment cannot be said to have received the treatment. Also, many of the nine treatment schools still

[14] One school was accessible only by a single, infrequent government bus. After my first surprise visit to the school, I became convinced that the teacher had asked the conductor or a daily passenger to warn him via cell phone when I was on my way to the school. Therefore, I hired an local man from outside the organization to collect data at this school.

[15] The data was discarded.

running at the end of the experiment did not use the megaphone as it was intended; only in three schools were students probably taking home the megaphone and using it regularly. However, the majority of students in all of the treatment schools could sing the song, either assisted by me and their teacher or on their own.

While it would be possible to say that the program did not work because the schools were not implementing it, the program was intended to be something that could be done in an NGO with low human and financial resources. The fact that the schools did not use the program, is, when deciding whether social psychology can help *these* schools, the failure of the program, not of the schools.

3.4 Econometric strategy

The data contain two measurements of attendance, one counting the total number of students in the classroom, and the other indicating whether an individual student on the enrollment list was present. I use both measures of attendance to estimate the effect of having the program on attendance. I estimate:

$$attendance_{it} = \beta_0 + \beta_1 treatment_i + \beta_2 after_t + \beta_3 program_{it} + \epsilon_{it}, \tag{2}$$

where $attendance_{it}$ is a dummy variable for whether student i was present during time period t, $treatment_i$ is a dummy variable for whether student i was enrolled in a school assigned to the treatment group, $after_t$ is 0 before the implementation of the program and 1 after, and $program_{it}$ is 0 in periods one and two and 1 in periods three and four for treatment schools; and

$$numberofstudents_{jt} = \beta_0 + \beta_1 treatment_j + \beta_2 after + \beta_3 program_{jt} + \epsilon_{jt}, \tag{3}$$

where $numberofstudents_{jt}$ is the number of students present in school j at time period t, $treatment_j$ is a dummy variable for whether school j was assigned to the treatment group,

$after_t$ is 0 in periods one and two and 1 in periods three and four, and $program_{jt}$ is 0 if a school is not receiving the program and 1 if it is.

Both of these difference-in-differences equations use the random assignment to treatment and control groups to promote parallel trends in treatment and control schools.

I estimate several parameterizations of β_3, the effect of the program. I include "intent to treat" (ITT) regressions, in which "receiving the program" is defined as random assignment to the treatment group. In these regressions, $program_{it}$ and $program_{jt}$ are the interactions of $treatment_i$ and $treatment_j$, respectively, with $after_t$. Therefore, I call these variables $program^{RA}$. I include one ITT regression without fixed effects and one with individual student or school fixed effects and time period fixed effects. Even though the closed schools in the treatment group could not possibly be receiving the program, these results are included because they are the most conservative estimates of the effect of the program.

I also include regressions that address school closure during the experiment. In order to illustrate the well-known problem with retrospective analysis, I show the results of an OLS regression in which $program_i$ and $program_j$ are variables for whether the school was open and in the treatment group in the after period. Finally, I use assignment to the treatment group, $program^{RA}$, as an instrument for open schools that were assigned to the treatment group. This estimate is referred to as the effect of the "treatment on the treated" (TOT).

I present the results for each of the four types of estimation for both individual student attendance and the number of students in a class. Since I paid second visits to schools where the teacher was absent on the first visit, I use two measurements of each of these dependent variables as well. I use the term "maximum attendance" to refer to the combination of first and second visit attendance results.

4 Results

4.1 Balance in control and treatment groups

As shown in table 3, the randomization achieved balance in all measured school characteristics. In the second round of data collection, there was no statistically significant difference between the number of students in the class on the first visit, the maximum number of students in the class, the teacher attendance rate on the first visit and the maximum teacher attendance rate. Balance was not achieved in all student level characteristics. Though students were as likely to be present in the control as the treatment schools on the first visit, the maximum student attendance is statistically significantly lower in the treatment group than the control group. Additionally, students in the treatment schools were more likely to never be observed in school across all four rounds. This, like the lower maximum attendance in the treatment group in the second round, might bias the results against finding an effect of the megaphone program.

4.2 Effect of the program

Several ways of estimating the effect of the program in the first visit of a round suggest that the megaphone program had no effect on student attendance. Yet, at first glance it appears that the megaphone program may have had an effect on students' maximum attendance, which would imply that when a teacher was present a student in the treatment group was more likely to attend than a student in the control group. This result becomes stronger when likely sources of noise are eliminated. However, closer examination of the data reveal that this finding is based on a small difference in teacher attendance in the third and fourth rounds, as opposed to an increase in the average number of students in class. Taken together, the data do not support the conclusion that the megaphone program positively impacted student attendance.

4.2.1 Main results

Table 4 shows that the effect of random assignment to the program on an individual's attendance on the first visit of each round is essentially zero (column 1 and column 2). The effect is the same whether or not student and time period fixed effects are included. As might be expected, the effect estimated by retrospective OLS regression that uses actually experiencing the program as the *program* variable overestimates the effect (column 3). It does not account for the fact that some schools originally assigned to the treatment group closed, and have an attendance of zero, but are counted as not receiving the program. When random assignment to the treatment group is used as an instrument for actually having the program, the effect of the megaphone program on the individual student attendance on the first visit disappears (column 4).

Table 4 also reports the results on an individual student's maximum attendance (columns 5, 6 and 8). For each regression, the effect on $program^{RA}$ and *program* are statistically significant. The effect as estimated by the retrospective OLS regression is statistically significant but does not evidence of the program's success (column 7).

The regressions using the count of students in a class on the first visit as the dependent variable suggest that the megaphone program had no effect. The fixed effects and non-fixed effects ITT estimates are again the same (columns 9 and 10). Finally, using the maximum number of students present in a round as the dependent variable does not yield significant results in the ITT regression, the ITT regression with fixed effects nor in the regression using random assignment as an instrument for having the program (columns 13, 14 and 16).

Overall, the results seem to suggest that the program was unsuccessful, but the results from the equations that use individual students' maximum attendance are inconsistent with this trend. Since the program was only minimally implemented, it is doubtful the individual students' maximum attendance results have much import.

4.2.2 Omission of sources of noise

In order to determine whether the megaphone program indeed had an impact on student attendance when teachers were present, I tried omitting sources of noise from the regression. Table 5 compares the full sample ITT estimate presented in table 4 to the results of regressions that use restricted samples.

Three sources of noise are omitted: the first round data, students who never attended school and students from schools that eventually closed. The first round data is of lower quality than the other three rounds because teachers and staff were initially nervous about interacting with a foreigner and being part of an experiment, making my visits more anticipated during this round than the others.[16] Of the 1,046 children in the sample, 559, or 53%, were not present during any of the visits to their schools. This suggests that the NGO had essentially no relationship with them, so there would be little reason to believe that the megaphone program could improve their attendance. A similar argument could be made for students whose schools eventually closed.

Table 5 shows that even removing these sources of noise, individually and collectively, does not uncover a positive effect of the megaphone program on individual student attendance for the first visit of each round. However, the apparent effect of the megaphone program on individual students' maximum attendance does not disappear; if anything it appears to be greater.

4.2.3 Evidence that the megaphone program had no impact

Looking beyond standard experimental difference-in-difference estimates helps us best understand why the megaphone program seemingly had, but probably did not actually have, an effect on individual students' maximum attendance.

The attendance graphs in figure 1 correspond with the regression results. Figure 2 shows

[16] Also, because I was still learning about the roles of different staff members in the NGO and the locations of the schools, my visits in the first round were sometimes more conspicuous than I would have liked.

that treatment group teachers were always as likely or more likely than control group teachers to attend school. The difference in teacher attendance between the groups is more pronounced in the graph showing the maximum attendance than in the graph showing first visit attendance. This suggests that increased teacher maximum attendance might be driving the maximum attendance regression results. Figure 3 shows that when a teacher is present, there are not more students present in treatment schools than control schools. If anything, there are more students in control schools, although this is probably skewed by one very large school in the control group. Therefore,additional instances of maximum teacher attendance in the treatment group than in the control group seem to be driving the maximum attendance results. Two and three more teachers in the treatment group than in the control group were present for one of the two visits in the third and fourth rounds respectively. In the scheme of a six-month experiment, it seems unlikely that five teacher days could impact on the results. However, because of the school closures and very high absence rate, these five days were indeed influential.

Considering that the program was aimed at students, and that there is little theoretical reason to believe that it would have an impact on teachers, the statistically significant results found for maximum attendance probably do not indicate success of the program.

4.3 A closer look at the schools for migrant workers

Although this experiment teaches us little about the role of social psychology in increasing school attendance in developing countries, some of the descriptive data collected about the functioning of the schools could be important for policy.

4.3.1 Teachers

In addition to the attrition of 30% of the schools, and the high teacher turnover (only 14 of an original 33 teachers were employed by the end of the six month period), teacher absence was high. On a given school day, the likelihood that a given teacher was present was about 50%.

This is absence rate is much higher than the absence rates found by Chaudhury et al. (2006) at Indian government primary schools,–25%, and at primary health care centers,–40%.

This absence rate cannot be attributed to the continued absence of half of the teachers; rather, the majority of teachers were absent a little more than half of the time. In only 6 of 26 schools, or 23%, was a teacher present on the first visit of all four rounds of data collection. Finally, teacher presence declined as the school year progressed, from about 58% in the first half to about 40% in the second.

4.3.2 Students

There were 1,046 names on the enrollment lists for the 26 schools. 46% of those were girls. This is probably a reflection of the skewed sex ratios among the children of migrant workers rather than of an unwillingness to enroll girls in school.[17] Girls were as likely to attend school as boys.

Of the 1,046 children in the sample, 559 or 53% were not present during any of the visits to their schools. While some of the children probably moved after the lists were made, most of the absence probably stems from a weak relationship between the schools and families. Student attendance was low throughout the school year. Counting as absent students whose teachers were absent or whose schools were closed, only 17% of students on the lists were in school on a given day during the course of the experiment. If we omit schools that were closed permanently (during periods in which they were recognized by the NGO's directors as closed), this figure rises to 20%.[18]

Like teachers' attendance, student attendance declined as the school year progress. In November, student attendance was approximately 22%. By the beginning of March, it had

[17] In a household survey of salt pan workers in the region, Coffey and Spears (2009) found that the 5 to 10 year old sex ratio was 112 boys to 100 girls.

[18] It is of note that among all three groups, there were many children who lived in the settlements close to a school who were not on the enrollment lists. It appeared that better off children were more likely to be on the lists than worse-off children, though all were very poor. There also many other settlements that had no schools.

fallen to 13%. It increased slightly to 17% in the end of March, possibly due to the fact that some students were preparing to take exams in government schools in April. Table 6 summarizes the changes in student and teacher attendance during the school year.

Very few students attended school regularly. Only 15 out of the 1,046 students on the enrollment lists were present on all 4 first visits. Each of these 15 students attended one of the 6 schools which had a teacher on the first visit of every round. 2 of these 6 schools had 4 students present on all four visits, the rest had either 2 or 1. This suggests that even those students whose teachers attended were not themselves regular school goers. Figure 4 shows how many students were present for each of four visits, three visits, two visits and one visit, or not at all.

5 Discussion

This experiment neither confirms nor disconfirms the role of social psychology and children's agency in school participation. While social psychological mechanisms and children's agency remain important areas of inquiry for education and development, the data of section 4.3 reveal that they are not the most pressing ones for the children in this experiment. Indeed, the most binding constraint to education for the children of migrant workers seems to have been teacher attendance, a problem which is difficult to address, due more to contextual and political reasons than to a lack of informative experimental results. Finally, the experiment provides evidence for some of the limitations of randomized evaluations cited by Deaton (2009), Ravallion (2009) and Rodrik (2008) and raise questions about development that cannot be addressed by randomized evaluations.

Table 1: Interpreting potential outcomes depends on intervention and NGO type

	intervention type	
	cost-benefit	social psychological
works in a capable NGO	confirms relevance of the cost-benefit framework	suggests relevance of psychological framework
fails in a less capable NGO	suggests NGO heterogeneity bias	no clear conclusion

5.1 Social psychology and children's agency in school participation

In light of the theory presented in section 2.2.1, it is probable that social psychological factors play a role in determining school participation. Additionally, the position of children to determine their own school participation is probably overlooked by researchers, though not by their parents.[19]

Regrettably, this experiment can neither confirm nor disconfirm the role of social psychology and children's agency in school participation because of the context in which it was carried out. Table 1 compares the logical consequences of testing different types of interventions in different NGO settings.

Most impact evaluations correspond to the top left box of the table: they test the effectiveness of cost-benefit interventions and are carried out in partnership with high quality NGOs. Many of these interventions have succeeded, and have demonstrated the relevance of the cost-benefit framework in development policy. As section 5.3 will discuss, given their incentives, researchers do not often choose to test interventions with low-capacity organizations. However, we can imagine that a cost-benefit intervention which succeeds in a high-capacity NGO, but fails in a low-capacity setting, would demonstrate the limit of the

[19]On one of the school visits, I spoke with the mother of a fishing family who expressed frustration at her son's truancy. The teacher for this settlement attended school fairly regularly, but apparently, her son did not. She focused on the boy's own decisions about school attendance when she said: "Sometimes he goes. Sometimes he doesn't. But he should go. We are poor people who don't even know how to sign our names. And he doesn't even go to school."

intervention's external validity.

The relevance of social psychology-based interventions to development policy is less established because there are fewer rigorous evaluations of such programs.[20] If the program were tested with a capable NGO, and were successful, social psychological interventions would have appeared deserving of further tests. The implications of this experiment, which tested an unproven intervention in a low-capacity organization, and failed, are unclear. Though we might expect any intervention to fail in such a setting, the program may have worked in a capable organization.

As Deaton (2009) observes, experimental research is best used to test theories that can be generalizable to a range of situations. Future experiments to uncover the role of social psychology and children's agency in school participation would certainly make such a contribution.

5.2 Teacher attendance: A binding constraint

In trying to improve school participation, I looked for an intervention that was both politically feasible, given the organization, and that showed consideration for external validity. This is to say, I did not want to design a program that almost certainly could not have happened in my own absence. Even with my advocacy, it is unlikely that the organization would have allowed an evaluation concerning teacher attendance — this may have signaled a major flaw in the performance to funders. It would not have implemented a teacher attendance program on its own. Such a program would likely have involved expensive inputs, such as providing transportation (in the form of a bicycle or motorcycle) to teachers, or would have required monitoring, of which the administrative staff was not capable. This is why, for instance, I could not replicate Duflo et al. (2008)'s camera program, a proven success in

[20] Dupas (2009) fails to find an impact of social psychological interventions on the price elasticity of demand for insecticide treated bed nets in rural Kenya. However, new research by shows that whether or not a child is encouraged to attend remedial classes may have an impact on her friends attendance (Berry and Linden, 2009).

another NGO's rural schools.

Targeting student attendance was more politically feasible than targeting teacher attendance and might, *a priori*, have been something that the NGO could have done on its own. The megaphone program was designed to be carried out by the students with only minimal participation on the part of the teachers (they were supposed to assign children to take the megaphone home each day). In principle, the other staff did not have to do any administration after the initial distribution. It seemed possible that the program might have a small impact on attendance on days when teachers were present.

It is now clear that the program's social psychological mechanisms could not impact school attendance if the schools were so rarely open.[21] Without more possibility for school attendance, there is no basis on which the children's norms and habits could be built. By definition, norm creation requires us to witness other people's behavior, and habits require repeated reinforcement, so teacher absence was a major obstacle to using social psychology to target children's decisions about school-going. The self-signalling and social proof mechanisms likewise required at least the possibility of going to class regularly. In this way, the experiment supports the idea proposed by Chaudhury et al. (2006) that service provider absence is a first order problem that cannot be "worked around."

5.3 When might experimental evaluation be able to help the migrant workers?

Unfortunately, identifying teacher absence as the binding constraint does little to tell us how to address the problem. Though a few were dedicated and some were uniquely positioned to attend school regularly (two lived only steps from their schools), most teachers faced

[21] In addition to high absence rates and teacher turnover, there were few officials school days during the year. In practice, many of the schools did not open until late October or November. Some closed in April like government schools, while others remained marginally open until the students, or the teacher, left the worksite. They were often closed for national holidays and religious holidays observed either by the students or the teacher.

the well-documented barriers to attendance that others in rural areas of poor countries also face: long walks in the sun, chronic illness and inadequate pay. The most important obstacle, though, was that no one responsible for the schools seemed to be invested in their regular functioning. One of the main funding agencies continued to write glowing reviews of the project when it had several indications that the programs were not working. Representatives of the other made such brief visits that they did not even visit the schools, focusing instead on another program it was funding. Neither verified, in any definitive way, that the enrollment figures in project proposals corresponded with student participation, yet they used the number provided by the NGO in their own fund-raising efforts. In this setting, several of the NGO staff misappropriated funds intended for teachers salaries and others maintained the charade of service provision; there was little incentive to compromise their own scanty returns to admit that poor children were not going to school. It is unlikely that experimental evaluation, which focuses on which procedures for effectiveness should be adopted by the willing, will help find the solutions to these problems.

Deaton (2009), Ravallion (2009) and Rodrik (2008) point out important problems with the current approach to impact evaluation. They suggest that impact evaluations often have low external validity — just because a program works in one place and time does not mean it will work in another. These authors also point out that most impact evaluations are done on a small scale, so their results may be invalid for large scale-ups, particularly when the scale-up might be done by a government, which has different incentives and administrative structures than an NGO. Additionally, for the reasons mentioned by Pritchett (2002), it is probable that governments will not be interested in evaluations of their own programs.

It also seems to be the case that the relevance of individual impact evaluations is limited by heterogeneity among NGOs. Cooley and Ron (2002) describe how "dysfunctional organizational behavior" in non-profits can arise, jeopardizing their effectiveness. They also point out that most NGOs have competing motivations, and many have been known to compromise their service delivery, even to the most needy, in response to internal and exter-

nal pressures. Certainly, the organization that hosted the megaphone project suffered from these, and other, constraints.

The external validity of impact evaluations is clearly reduced when we consider that replications of successful programs will often fail even in other NGOs. Duflo et al. (2008) discuss the tendency of randomized evaluations to "gold-plating:" giving the job of implementing the program being tested to the best staff at the best organization. We do not know how to avoid this bias in practice; the incentives that encourage researchers to "gold-plate" the programs they evaluate are strong. Working with capable organizations leads to successful interventions and to publications, working with incompetent organizations leads to failed projects, stress and inconclusive papers like this one. Nonetheless, the high incidence of poorly functioning organizations makes the problem of "NGO heterogeneity bias" an important one: impact evaluations are less valuable if their findings cannot be put into practice.

But, in places like the migrant worker settlements, where a lack of capacity only compounds the organizational structure's lack of investment in better schools, even more context-specific program evaluations may not be useful. More attention should be given to the question of how to attract the types of organizations that are interested in using tools like rigorous evaluation to ensure adequate service delivery to these deprived populations.

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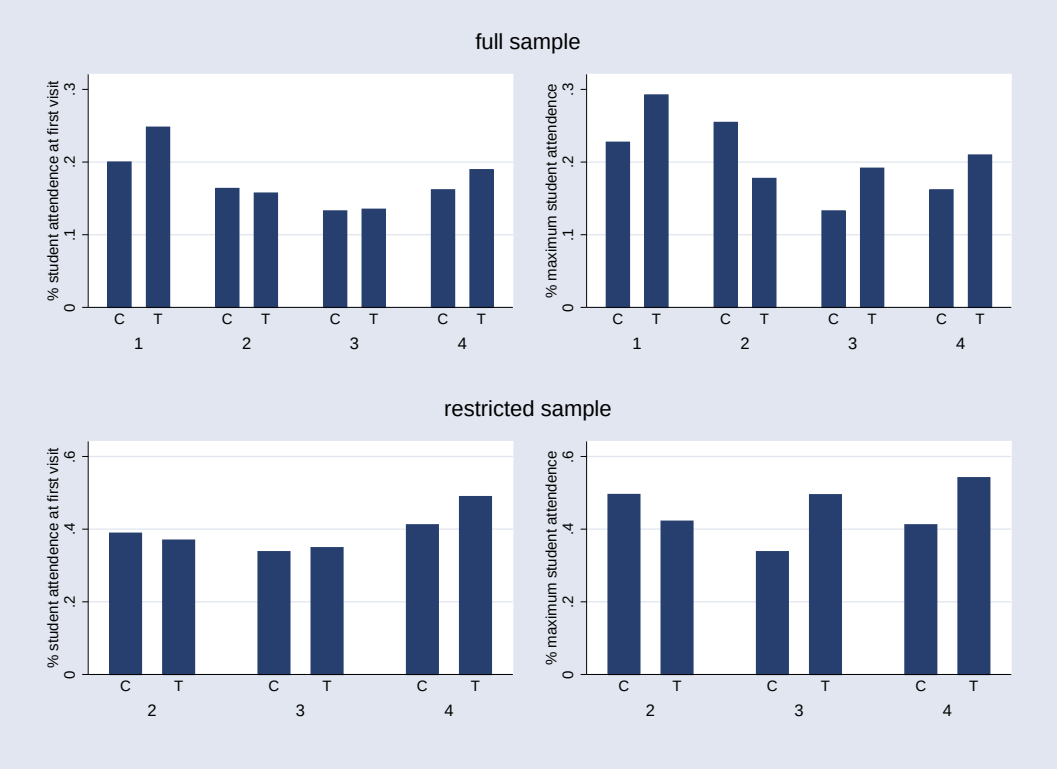

Figure 1: Student attendance by round and group

The restricted sample excludes data from round 1, students who were never observed at school and students who attended schools that closed.

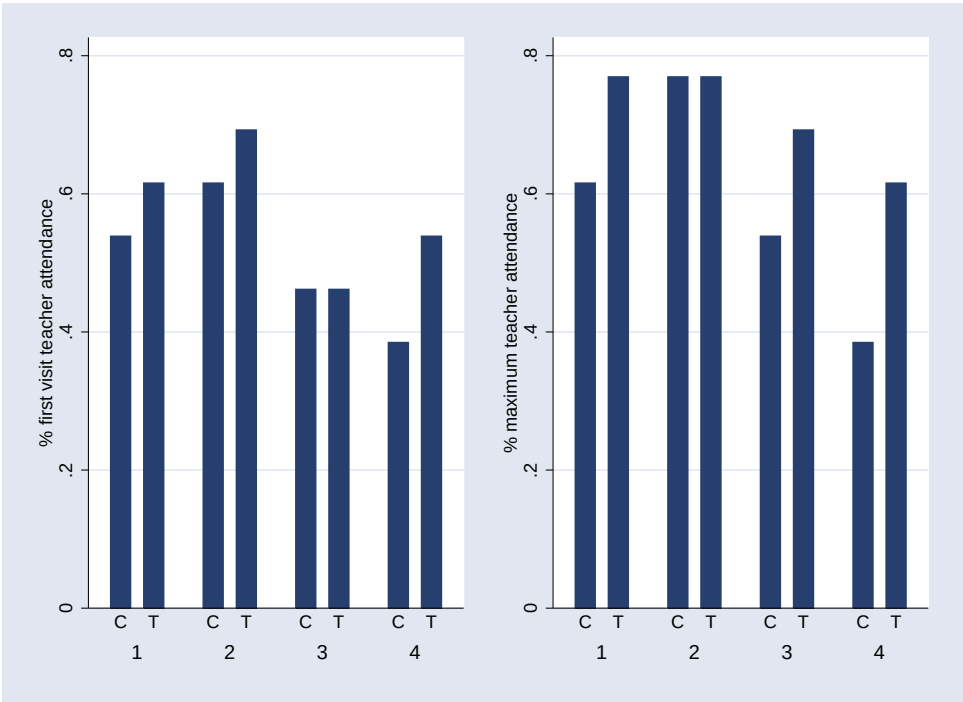

Figure 2: Teacher attendance by round and group

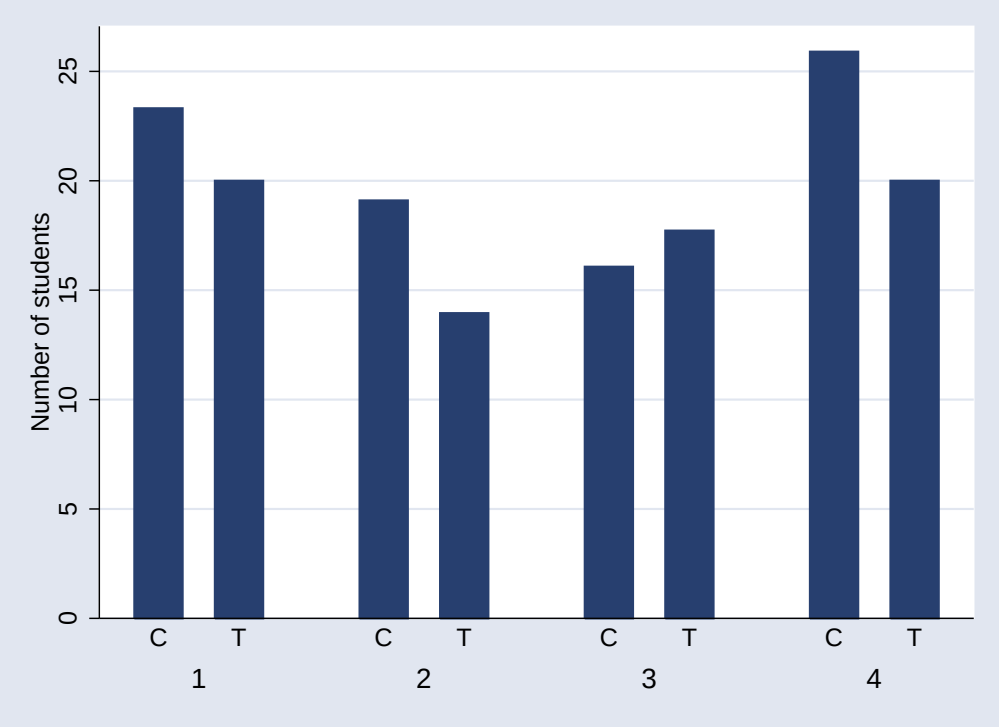

Figure 3: Number of students present, conditional on a teacher being present

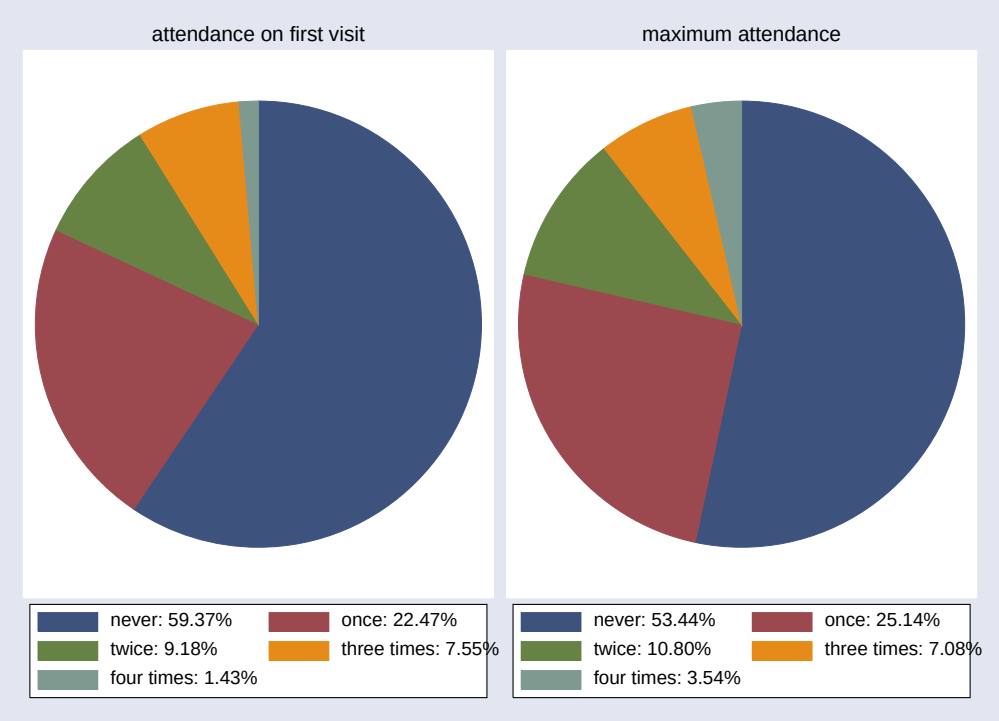

Figure 4: Distribution of longitudinal student attendance

Table 2: Summary of representative recent experimental evaluations in the cost-benefit framework

Study	Country	Program	Result
Manipulates E$[W(S;I)]$ by increasing I			
Case and Deaton (1999)	South Africa	(non-experimental) variation in the student-teacher ratio	lower student-teacher ratios increased enrollment
Banerjee and Kremer (2002)	India	extra teachers	girls' attendance improved
Kremer et al. (2003)	Kenya	students uniforms, textbooks and renovated classrooms at no cost to families	increased school attendance
Kremer et al. (2007)	Kenya	textbooks	no impact on student learning or attendance
Glewwe et al. (2008)	Kenya	bonuses for teachers whose students improved on standardized tests	test scores improved; teacher attendance, quantity of homework assigned and knowledge retention remained the same
Manipulates E$[W(S;I)]$ through expectations			
Banerjee et al. (2008)	India	encouragement of parental participation in education of village children and school monitoring	no impact on participation in Village Education Committees or on "teacher effort or learning outcomes"
Nguyen (2008)	Madagascar	information about the economic returns to education	improved attendance and achievement
Increases $B(S)$			
Alderman et al. (2002)	Uganda	school meals; take home rations to children who attend school	both improved attendance
Shultz (2004)	Mexico	cash transfers to parents whose children attended school	increased enrollment rates
Decreases $C(S)$			
Kremer and Miguel (2007)	Kenya	deworming medicine at the school level	improved school attendance
Kremer et al. (2008)	Kenya	merit scholarships to high performing girls	improved test scores and attendance for boys and girls

Table 3: Characteristics of treatment and control groups before the program

student characteristics	control	treatment	p
present at first visit	0.164	0.157	0.780
	(0.016)	(0.016)	
maximum presence	0.255	0.177	0.003
	(0.019)	(0.017)	
n	1046	1046	

school characteristics	control	treatment	p
number of students at first visit	9.731	9.731	1.000
	(3.399)	(2.308)	
maximum number of students	14.692	10.731	0.340
	(3.443)	(2.169)	
teacher presence at first visit	0.615	0.692	0.690
	(0.140)	(0.133)	
maximum teacher presence	0.769	0.769	1.000
	(0.122)	(0.122)	
n	13	13	

Standard errors in parentheses. The table uses data from round 2. Maximum presence, maximum attendance and maximum teacher presence count an individual as present if she was present for any visit in a round.

Table 4: Effect of the program by individual attendance and number of students in class

	students' attendance: first visit				students' attendance: maximum			
	ITT	ITT - FE	OLS	TOT	ITT	ITT - FE	OLS	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$program^{RA}$	-0.006	-0.006			0.056	0.059		
	(0.023)	(0.021)			(0.025)	(0.022)		
program			0.133	-0.008			0.202	0.083
			(0.020)	(0.033)			(0.021)	(0.035)
treatment group	0.021		-0.030	0.021	-0.006		-0.049	-0.006
	(0.017)		(0.013)	(0.017)	(0.019)		(0.014)	(0.019)
after	-0.035	-0.045	-0.083	-0.035	-0.094	-0.102	-0.134	-0.094
	(0.016)	(0.018)	(0.012)	(0.016)	(0.017)	(0.019)	(0.013)	(0.017)
c	0.182	0.223	0.206	0.182	0.241	0.258	0.261	0.241
	(0.012)	(0.010)	(0.010)	(0.012)	(0.013)	(0.011)	(0.011)	(0.013)
student FEs		✓				✓		
time period FEs		✓				✓		
first stage F				2676				2676
R^2	0.003	0.013	0.013	0.002	0.009	0.018	0.027	0.021
n	4184	4184	4184	4184	4184	4184	4184	4184

	number of students: first visit				number of students: maximum			
	ITT	ITT - FE	OLS	TOT	ITT	ITT - FE	OLS	TOT
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
$program^{RA}$	0.673	0.673			4.173	4.173		
	(4.853)	(3.205)			(4.678)	(2.840)		
program			9.826	0.927			13.530	6.028
			(3.485)	(6.975)			(2.936)	(6.520)
treatment group	0.269		-2.795	0.269	-1.192		-3.789	-1.192
	(3.317)		(2.510)	(3.317)	(3.157)		(2.388)	(3.157)
after	-2.115		-5.180	-2.115	-5.212		-7.808	-5.212
	(3.833)		(2.762)	(3.833)	(3.790)		(2.699)	(3.790)
c	11.423	18.798	12.955	11.423	14.519	17.981	15.818	14.519
	(2.649)	(1.903)	(2.325)	(2.649)	(2.587)	(1.598)	(2.261)	(2.587)
school FEs		✓				✓		
time period FEs		✓				✓		
first stage F				56.250				56.250
R^2	0.006	0.679	0.060	0.016	0.027	0.735	0.123	0.094
n	104	104	104	104	104	104	104	104

Robust standard errors in parentheses. "Students' attendance" measures linear probability of student attendance. Treatment on the treated (TOT) estimates instrument for *program* with $program^{RA}$.

Table 5: Effect on individual student attendance excluding sources of noise

	Student attendance on the first visit				
	ITT full sample	ITT omits $t = 1$	ITT omits never	ITT omits closed	ITT omits (2) - (4)
	(1)	(2)	(3)	(4)	(5)
program^{RA}	-0.006	0.021	-0.024	-0.004	0.063
	(0.023)	(0.028)	(0.044)	(0.030)	(0.059)
treatment group	0.021	-0.006	0.013	0.055	-0.019
	(0.017)	(0.003)	(0.032)	(0.022)	(0.048)
after	-0.035	-0.016	-0.070	-0.023	-0.014
	(0.016)	(0.019)	(0.028)	(0.019)	(0.041)
c	0.182	0.164	0.366	0.208	0.389
	(0.012)	(0.158)	(0.021)	(0.014)	(0.033)
R^2	0.003	0.000	0.016	0.005	0.002
n	4184	3138	1948	3112	1224

	Student attendance on the first or second visit				
	ITT full sample	ITT omits $t = 1$	ITT omits never	ITT omits closed	ITT omits (2) - (4)
	(6)	(7)	(8)	(9)	(10)
program^{RA}	0.056	0.130	0.109	0.040	0.217
	(0.025)	0.030	(0.045)	0.031	(0.060)
treatment group	-0.006	-0.077	0.059	0.067	-0.073
	(0.019)	(0.025)	(0.032)	0.023	(0.049)
after	-0.094	-0.107	-0.189	-0.049	-0.120
	(0.017)	(0.021)	(0.029)	0.019	(0.041)
c	0.241	0.255	0.485	0.234	0.495
	(0.013)	(0.019)	(0.021)	(0.014)	(0.034)
R^2	0.009	0.009	0.036	0.012	0.016
n	4184	3138	1948	3112	1224

Linear probability of student attendance. Robust standard errors in parentheses. Columns 2 and 7 omit the first period of data. Columns 3 and 8 omit students who were never observed attending school. Columns 4 and 9 omit students from schools that eventually closed. Columns 5 and 10 omit all three of these groups.

Table 6: Student and teacher attendance rates

round	time period	teacher attendance rate	student attendance rate
1	November	58%	22%
2	December	65%	16%
3	beginning of March	46%	13%
4	end of March	46%	17%