Iron Deficiency Anemia and School Participation

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Poverty Action Lab Paper No. 7
March 2004

Poverty Action Lab
TRANSLATING RESEARCH INTO ACTION

Iron Deficiency Anemia and School Participation*

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Abstract: Iron-deficiency anemia is among the world's most widespread health problems, especially for children, but it is rarely studied by economists. This paper evaluates the impact of a health intervention delivering iron supplementation and deworming drugs to 2-6 year old children through an existing pre-school network in the slums of Delhi, India. At baseline 69 percent of sample children were anemic and 30 percent had intestinal worm infections. Sample pre-schools were randomly divided into groups and gradually phased into treatment. Weight increased significantly among assisted children, and pre-school participation rates rose sharply by 5.8 percentage points – a reduction of one-fifth in school absenteeism – in the first five months of the program. Gains are largest in low socio-economic status areas. Year two estimates are similar, but two methodological problems – sample attrition, and the non-random sorting of new child cohorts into treatment groups – complicate interpretation of the later results.

JEL Classification: C93, I12, I20, O15

* We thank Harold Alderman, Simon Brooker, Rick Hanushek, Matthew Jukes, David Lee, Steven Levitt, and Michael Kremer for helpful comments, and Rukmini Banerji, Janaki Desai, Namita Gupta, Radhika Iyengar, Shruti Nag, Vishal Seghal, Monica Singhal, and the Pratham Education Initiative and Niramaya Health Foundation field staff and management, for their generous support throughout. We are grateful to ICICI Bank for funding. Miguel acknowledges funding from The World Bank and from the U.C. Berkeley Center for Health Research, U.C. Berkeley Committee on Research, and the Hellman Fund. All errors remain our own.

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1. Introduction

Iron deficiency anemia is one of the world's most widespread health problems, especially among children: approximately 40 percent of children are anemic across various African and Asian settings (Hall et al 2001). Iron deficiency anemia leads to weakness, poor physical growth, and a compromised immune system – decreasing the ability to fight infections and increasing morbidity – and is also thought to impair cognitive performance and delay psychomotor development. Recent macroeconomic estimates suggest that the impact of iron deficiency anemia – through both physical and cognitive channels – could be as large as 4 percent of GDP on average in less developed countries (Horton and Ross, 2003). Through its impact on school participation and learning, anemia could also be central to understanding the intergenerational transmission of poverty. Yet there is little work by economists on the effects of anemia on economic development, and many existing non-experimental studies exploring the impact of anemia, and other dimensions of poor nutrition, on education are difficult to interpret due to the possibility of omitted variable bias.[1]

This study takes a step toward addressing this gap by evaluating the impact of a non-governmental organization (NGO) project in the slums of Delhi, India, which delivers iron supplementation and deworming drugs to 2-6 year old pre-school students. At baseline 69 percent of children in the sample were anemic, while 30 percent suffered from intestinal helminth (worm) infections. The 200 pre-schools in the sample were randomly divided into groups, and then gradually phased into the program as it expanded over the next two years. Exploiting this experimental design, we find large gains in child weight – roughly 0.5 kg (1.1 lbs.) on average – in the treatment schools relative to comparison schools during the first five months of the project. Most importantly for this study, average pre-school participation rates increased sharply – by 5.8 percentage points – among assisted children, reducing pre-school absenteeism by roughly one-fifth. Program treatment effects are most pronounced for subgroups

[1] Refer to Behrman (1996) and Miguel (2004) for reviews of the literature on child health, nutrition, and schooling. Strauss and Thomas (1998) survey the links between health and underdevelopment.

with high baseline anemia rates, in particular, for girls and children in low socioeconomic status areas. Given the low cost of the intervention – US$1.70 per child per year – these results suggest the health intervention is a highly cost-effective means of improving child pre-school participation in a poor urban setting where iron deficiency anemia and worm infections are widespread.

One plausible channel through which these pre-school attendance gains could have long-run impacts is an improvement in future primary school performance – and in fact, 71 percent of parents in the study area claimed (in a baseline survey) that improved primary school preparedness was an important motivation for sending their children to pre-schools. Unfortunately, there is little rigorous evidence (we are aware of) that links pre-school participation to later educational outcomes in less developed countries. However, there is evidence from the U.S. Head Start program that early childhood interventions reduce later grade repetition and increase educational attainment, at least in certain populations (Currie and Thomas, 1995; Garces et al, 2002).

Randomized evaluations of this kind can provide particularly credible estimates of program impacts, as well as structural parameters. However, two issues that arise in this study – sample attrition, and non-random sorting among treatment groups – highlight limitations of randomized evaluations and should be factored into the design of future studies. The key econometric identification issue in this study is sample attrition: during the first school year (2001-02), approximately one-quarter of school participation observations are missing – largely due to "churning" of pre-schools, which open and close at relatively high frequency, as well as difficulties tracking down respondents who have moved. Fortunately for the analysis, attrition rates are nearly identical for program treatment and comparison groups in year one (at 26 and 25 percent, respectively), allowing us to place reasonably tight non-parametric extreme bounds on treatment effects, following the method in Lee (2002). In year two of the program (2002-03), however, attrition rates rise sharply (again mainly due to school closures). Year two results are also unreliable due to non-random sorting among treatment groups: we find sharply different characteristics across treatment groups among the incoming cohort of students in year two. If this new cohort knew that the health program would only be offered in treatment pre-schools starting in year two, parents with unobservably greater interest in child health and education might have self-selected into treatment schools – contaminating the randomization and complicating causal inference. For these reasons we focus on year one in the main empirical analysis.

The current study complements existing experimental studies on child nutrition, and improves on them in certain dimensions. One limitation of existing studies is their small sample size. For instance, in the well-known INCAP study, Martorell et al (1995) provided different nutritional supplements to Guatemalan children and later find significant impacts on their cognitive skills during adolescence. However, that study randomly assigned children to the treatment and comparison groups at the village level, and thus has an effective sample size of only four villages. Similarly, the small sample size of only 100 children in Grantham-McGregor et al's (1997) study of a randomized early childhood nutrition program in Jamaica limits the generalizeability of their findings. In contrast, our sample contains 200 pre-schools, each with an average of 12 children. A second limitation of existing studies is their rather narrow focus on academic and cognitive tests, typically tests of recall and verbal skills. Nokes et al (1998) survey a number of such experimental studies on the impact of iron supplementation, and many find positive impacts on cognitive and motor development, as well as on educational achievement. The current study, by contrast, focuses on school participation, an outcome largely ignored in existing work.

This study is most closely related to Miguel and Kremer (2004), who find that deworming leads to large school participation gains (7 percentage points) among 6-17 year-old children in rural Kenyan primary schools. Though the results of the current study thus largely confirm the earlier Kenya findings, the demonstration that a similar relationship holds in another geographic setting (urban India), with a younger age group, and a different health intervention provides compelling evidence that there is a robust positive relationship between child health status and school participation in poor countries. More broadly, this study also contributes to a growing literature on the economic impacts of iron deficiency anemia in less developed countries – and in particular, complements recent work on the effects of iron repletion on adults, which finds significant improvements in energetic efficiency, self-reported health status, earnings, and labor supply (Thomas et al 2003).

The remainder of the paper is structured as follows: section 2 discusses medical aspects of anemia and its effects on children. Section 3 presents the program design and baseline population characteristics. Section 4 discusses the identification strategy, and Section 5 presents the main results. Section 6 contains a methodological discussion on the limitations of randomized evaluations in the presence of attrition and sorting among schools. The final section concludes.

2. Iron Deficiency Anemia and Children

Iron is a component in many proteins, including enzymes and hemoglobin, the latter being important for the transport of oxygen to tissues throughout the body (National Academies of Sciences, 2002). Iron deficiency anemia (IDA) – that is low levels of hemoglobin (Hb) in combination with abnormal levels of other iron indicators such as transferrin saturation (e.g., iron stores) – can lead to weakness, poor physical growth, increased morbidity, and impaired cognitive performance and delayed psychomotor development. In particular, iron deficiencies early in life are thought to potentially inhibit the function of neurotransmitters, compromising brain function.[2]

It is hypothesized that iron supplementation could generate school participation gains through two mechanisms: first, improved physical activity (e.g., motor development), and, second, through cognitive development. Yet little work examines the impact of iron supplementation on school participation. One study documents an association between poor school attendance and mild levels of anemia and *A. lumbricoides* (roundworm) infection among primary school children in Jamaica (Hutchinson et al 1997). However, the study does not investigate the association in younger pre-school children, and their study is purely observational, and thus subject to well-known omitted variable bias concerns.

More studies have investigated the association between iron supplementation and cognitive ability among children. Nokes et al (1998) survey a number of such experimental studies on the impact of iron supplementation (e.g., Soemantri et al 1985; Soemantri 1989; Seshadri et al 1982; Seshadri and Gopaldas 1989), and most find positive impacts. Seshadri et al (1982) examine the effect of iron

[2] These findings are based on animal studies; see Horton and Ross (2003) for a discussion.

supplementation on IQ tests in children 5-6 years old, and find that treated children subsequently had large gains in verbal performance and in IQ scores. Stoltzfus et al (2001) conducted a randomized trial of iron supplementation on 614 preschool children aged 6-59 months, and find that iron supplementation significantly improved iron status, and language and motor development among severely anemic children. Soemantri et al (1985) estimate the impact of iron supplementation on educational achievement (i.e. what children learn in school), and find that it improved achievement in among Indonesian school children.

Finally, existing experimental studies examining whether iron deficiency affects child growth have produced somewhat mixed results. Bhatia and Seshadri (1993) examine the effect of a six month supplementation intervention for pre-school children 3-5 years of age in India, and find that the growth performance – as measured by weight and weight-for-height Z-scores – of the anemic children supplemented with iron was superior to that of anemic placebo children. Similarly, Angeles et al (1993) found increases in height and height-for-age Z-scores among anemic Indonesian pre-school children after two months of daily iron supplementation. However, not all studies show growth gains: Dossa et al (2001) find no significant difference in anthropometric measures across experimental groups – even among the sub-samples of stunted and anemic children – after a three month iron supplementation intervention among children 3-5 years old in Benin.

3. Project Design and Data

3.1 The Pratham Delhi Pre-School Health Program

Pratham Delhi, an Indian non-governmental organization (NGO)[3], began establishing a network of pre-schools in poor communities in eastern Delhi during 2000. Children in the pre-schools are 2 to 6 years old, typically come from families of poor migrant laborers, and are at high risk of anemia and other nutritional deficiencies. The pre-schools teach children basic literacy and numeracy skills, and the pre-school teachers are usually young women living in the communities. Classes usually take place in teachers' homes; thus when a teacher moves, marries, or obtains other employment, her pre-school may

[3] Pratham health programs are coordinated with the Niramaya Health Foundation.

close. This leads to considerable turn-over in pre-schools, particularly in communities with high levels of residential mobility – an issue we return to below in the discussion of attrition below.

The study area consists of recent "resettlement colonies", typically 10-20 years old, that have absorbed large population in-flows during Delhi's relentless recent expansion. The study sample contains 200 preschools, with an average of 12 children, yielding a total sample size of 2392 children. The initial sample of 268 pre-schools were assigned to 189 different "clusters" at the start of the study, where each cluster contained one to three pre-schools, usually all located on the same city block. The Pratham Delhi pre-school program was being expanded in 2001 at the start of the data collection, and 68 of these initial 268 pre-schools closed down between August and December 2001, leaving our final sample of exactly 200 pre-schools, in 155 distinct clusters. The clusters were then randomly divided (using a computer random number generator) into three treatment groups, Groups I, II, and III.[4] The randomization was done at the pre-school cluster level for several reasons, including a desire to reduce potential health spillovers from the intervention;[5] to limit transfers of children across nearby pre-schools belonging to different treatment groups (which could contaminate the randomization and bias program estimates); and to avoid discord within neighborhoods – for instance, if some children on the block received an assistance package in their pre-school but others did not.

The pre-school health intervention consisted of iron supplementation (33.3 mg of elemental iron with folic acid)[6] and deworming drugs (400 mg of albendazole).[7] Deworming drugs were included since

[4] Seventeen additional pre-schools closed down after the randomization during year one.

[5] In the presence of positive local health externalities from deworming, the difference between treatment and comparison pre-school children would underestimate actual treatment effects (as in Miguel and Kremer 2003).

[6] This dosage was chosen by the NGO based on pharmaceutical availability and the desire to provide a therapeutic intervention for a 13 kg child at close to 3mg per kg per day, for three months (CDC 1998). The U.S. National Academies of Sciences (2002) claim that up to 40 km of daily iron is a safe dose for children. The WHO's recommended iron supplementation for children between the ages of 24-60 months in populations with significant iron deficiency is 2 mg/kg body weight per day, up to 30 mg for 3 months (WHO 2001).

worm infections such as hookworm and roundworm contribute to anemia (Hall et al 2001 and references therein). The deworming drugs were taken at "health camps" held at the pre-school approximately every three months. Following WHO recommendations, pre-school teachers in treatment schools were instructed to administer daily iron doses for 30 school days following each health camp, to all children attending preschool that day. Children in both treatment and comparison pre-schools also received 200,000 I.U. of Vitamin A (at alternate health camps), and thus treatment effect estimates should be interpreted as the impact of iron and deworming in addition to Vitamin A supplementation, relative to Vitamin A alone; note that in addition to its other potential benefits for the immune system and eyesight, Vitamin A may also promote the absorption of iron (FAO/WHO 1998), so this is a relevant point.

The intervention package in treatment schools – iron, deworming, and Vitamin A together – is relatively inexpensive, with drug purchase and delivery costing approximately US$1.70 per child per year. The estimation of project costs is complicated somewhat in this context by the fact that NGO enumerators carry out a range of intervention activities as well as research, making the precise isolation of intervention costs difficult.

During October and November 2001, the NGO enumerators – called Community Health Volunteers (CHV's)[8] – conducted parent-teacher meetings in each pre-school to inform them about the project, and to obtain consent for their children's participation. The first health camp took place in December 2001, and approximately every three months afterwards (the project timeline in Appendix Table A1 contains the details). The health package was gradually phased in: Group I began receiving assistance in December 2001 and become the "treatment group" for the 2001-02 school year, the focus of this study, while the Group II pre-schools received treatment beginning in November 2002, and by November 2003 all pre-schools had begun receiving treatment.

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[7] Albendazole is a broad-spectrum anti-helminthic effective against intestinal helminths such as roundworm and hookworm; it causes minor transient side effects, such as stomach ache.

[8] Though referred to as volunteers, CHV's receive a stipend from the NGO.

3.2 Data and Measurement

A Household Survey collected information on socioeconomic and demographic characteristics, parent educational attainment, labor market outcomes, access to sanitation, health knowledge (particularly regarding anemia and worms) and recent illness episodes for all household members. The first round of the Household Survey was administered among a representative subset of households during August-September 2001, and a second round in September-November 2002. The 2001 round consisted of a 30 percent representative clustered sample of the child population in each pre-school. In all, 515 of the 715 sampled households were interviewed, where survey completion rates were imperfect due to frequent household mobility, and difficulties locating some households in these dense urban communities. Fortunately, survey completion rates were similar across the three treatment groups (at 77, 69, and 69 percent, respectively), as expected, since the pre-schools had not been divided into the treatment groups at the time of the 2001 Household survey. Similar difficulties affected the 2002 Household survey, in which 650 of 756 sampled households were surveyed.

Hemoglobin (Hb) and parasitological surveys were conducted in both December 2001 and October 2002 among a subsample of the children selected for the Household survey, with Group I pre-school children surveyed in 2001, and Group I and II children in 2002.[9] Due to difficulties in locating children, as well as in securing their cooperation with sample collection, hemoglobin information was obtained for only 187 of the 280 sampled children – and parasitological information for 159 children – in 2001, and there were similar rates in 2002. A number of other studies in less developed countries have found similarly high refusal rates for blood draws (Martorell 1995 is one example). To the extent that children with worse health were more likely to be missing from the sample, these baseline figures under-estimate the health problems in this population – although we cannot rule out the possibility that the

[9] The Hb test consisted of a finger prick blood draw conducted by professional laboratory staff, analyzed using the Cyanometh technique. For parasitological tests, stool samples were collected by CHVs at children's homes, and analyzed using the Kato-Katz technique to determine infection intensity, proxied with worm eggs per gram of stool.

parents of sicker children were more likely to allow their children to be tested in the hope of receiving future treatment; we discuss this issue further in Section 3.3 below.

In addition to providing deworming drugs during each health camp, CHV's also measured the height and weight of each child. Anthropometric measures of child weight-for-height ("wasting", acute undernutrition), weight-for-age ("underweight", chronic and acute malnutrition), and height-for-age ("stunting", chronic undernutrition) Z-scores are used to proxy for child nutritional status, where Z-scores reflect the standardized difference (by age and gender) from the mean of reference healthy U.S. children.

Pre-school enrollment rosters were collected from August to October 2001, and pre-school participation data was then collected during monthly, unannounced visits to all pre-schools from November 2001 through April 2003. Unannounced checks serve as a representative measure of pre-school attendance, and are preferable to teacher records, which are often thought to be of poor quality in less developed countries. At each visit, CHV's determined whether each child from the baseline roster was present, absent, had left school, or had transferred to another school; those children present in school are counted as school "participants", and those who had dropped out or were absent as "non-participants". Children who transferred to other NGO pre-schools were tracked at those schools during subsequent attendance checks. Children who have moved away from the area (for instance, to another neighborhood of Delhi, or back to their home village), or about whom the teacher did not have good information, are counted as attriters and lost from the sample (until they return to an NGO pre-school). As discussed in Section 4.2 below, attrition rates are relatively high.

3.3 Baseline Characteristics

Baseline hemoglobin and parasitological surveys identified anemia and helminth infections as important health problems in this population. The World Health Organization defines anemia in children 6-60 months of age at hemoglobin concentrations below 11 g/dL (WHO 2001), and 69 percent of children in the baseline population met this standard (Table 1, Panel A).[10] Moreover, 7 percent of sample children were severely anemic (< 7 g/dL) and 41 percent moderately anemic (7-10 g/dL). Anemia rates in the sample are similar to those from the 1998-99 Indian National Family Health Survey (NFHS, part of the DHS series)[11]: in the NFHS urban Delhi sample, 69 percent of children aged 6-35 months old were anemic, of whom 4 percent were severely anemic and 43 percent had moderate anemia, and thus our population of pre-school children appears quite representative for Delhi in terms of anemia.[12]

Hemoglobin status varies considerably with child and pre-school characteristics. In a probit specification, boy children are considerably less likely to be moderately-severely anemic than girl children (the magnitude is 15 percent points, statistically significant at 95 percent confidence – regression not shown), conditional on child age and average socioeconomic characteristics in the pre-school. Similarly, children in pre-schools with high average father education are significantly less likely to be anemic (a drop of 4 percentage points per additional year of average father schooling, significant at 95 percent confidence), and children in predominantly Muslim schools are more likely to be anemic (a change from zero to 100 percent Muslim population is associated with an increase of 8 percentage points in the likelihood of moderate-severe anemia – although the effect is not significant at traditional confidence levels). As shown in Section 5, program treatment effects on weight and on school participation are more pronounced for girls and for children in low socioeconomic status areas, a finding consistent with the existing literature, in which iron supplementation only has impacts on hemoglobin among anemic children (Soemantri 1989).[13]

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[10] Even under an extreme assumption on attrition – namely, that none of the children with missing Hb data were anemic – a lower bound on the rate of anemia in this population is 34 percent, still a reasonably high rate. If, on the other hand, anemic children are less likely to be tested, then the true rate would be higher than 69 percent.

[11] Source: International Institute for Population Studies. *National Family Health Survey, 1998-99*.

[12] In year two, severely anemic children were removed from the sample and received iron supplementation.

[13] Soemantri's (1989) study of iron supplementation for 8-11 year old school children in Indonesia finds increases in Hb levels among anemic and non-anemic children of 3.6 g/dl and 0.3 g/dl, respectively, after 3 months of treatment.

Thirty percent of the baseline sample has a helminth infection (Table 1, Panel A), mainly roundworm (21 percent prevalence), although some children were also infected with hookworm, *Hymenolipis nana*, and threadworm. The NGO followed the WHO recommendation of mass deworming treatment in child populations where anemia is widespread and helminth infection prevalence at least 20 percent (Montresor et al. 1998). Unfortunately, the NFHS does not contain information on helminth infection rates, so we cannot determine how representative the sample is for Delhi along this dimension.

There was marked undernutrition at baseline: 21 percent of children were wasted (weight-for-height Z-score < –2), 30 percent underweight (weight-for-age Z-score < –2), and 24 percent stunted (height-for-age Z-score < –2), and note that the NFHS again found similar rates for a representative sample of Delhi children (not shown). Treatment group children were somewhat worse off pre-intervention in terms of nutrition: Group I children have lower baseline weight-for-age and height-for-age Z-scores than Group II and III children on average (Table 1, Panel A). Thus although the randomized assignment to treatment groups should have eliminated most differences between groups (by the Law of Large Numbers), the randomization was not entirely successful in this case.

There are no significant differences across treatment groups in the age, gender, and pre-intervention school attendance patterns of children (Table 1, Panel B). Household size is relatively large, at 6.0 members (Panel C), compared to the mean size of Delhi households from the NFHS (5.3 members). The population is mainly composed of individuals whose "original" home lies outside of Delhi (76 percent), many of whom are recent migrants (although some of these people were born in Delhi, but continue to identify with their ancestral area), and of which the majority are from Uttar Pradesh (54 percent, Panel C). Only 20 percent of the population has lived in their current household for over fifteen years (not reported in the table), further evidence of the largely transient nature of the population in these

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Unfortunately, the small sample size (130 children) and the fact that standard errors are not provided for these estimates complicates inference. Thomas et al (2003) find similar results for adults in Indonesia. See Nokes et al (1998) for additional references.

poor “resettlement colonies”. A significant share of the population is Muslim (21 percent, Panel C) – higher than in the NFHS, which reports that only 8 percent of urban Delhi household heads are Muslim – and Group I pre-schools have a somewhat higher proportion of Muslims relative to Group II and III.

While the average educational attainment of pre-school children’s mothers is nearly identical across treatment groups – at 3.4 years (Table 1, Panel D) – average education for fathers is somewhat lower in Group I (Panel E). The marked difference in educational outcomes by religious affiliation – Muslims have consistently lower attainment – is also of interest: 69 percent of Muslim mothers never attended school – versus 51 percent for Hindu mothers – and similarly, 42 percent of Muslim fathers (23 percent of Hindu fathers) had never attended school (not reported). Most fathers work as laborers (31 percent), formal sector wage-earners (31 percent), or are self-employed (15 percent), the latter often running small businesses in the slums. Most mothers, on the other hand, work at home (78 percent), while others either work outside the home as laborers (6 percent) or formal sector wage-earners (5 percent). There are no statistically significant differences in occupational characteristics for mothers or fathers across the treatment groups.

Few households in the sample reported that their children had received iron, deworming, and Vitamin A prior to the NGO program: only 1 percent had received iron supplementation in the previous year according to 2001 Household survey respondents, 6 percent with deworming medicine, and less than 1 percent with Vitamin A (Table 1, Panel F). These rates are considerably below treatment rates reported in the Delhi NFHS, possibly because the poor communities where the current study took place have less access to government services than many other parts of Delhi.[14] Access to sanitation is also inadequate in this area: only 5 percent of households have a flush toilet, while 48 percent lack access to a private toilet or latrine (not reported in the table).

3.4 Program Take-up

[14] However, our figures could understate the true extent of treatment if parents do not always know the purpose of a medicine that has been given to their children, or if they have forgotten about such treatment.

Teachers were instructed to give each child in the treatment pre-schools 30 days of iron after each health camp. Only 17.5 and 18.5 days of iron had been administered on average in treatment (Group I schools) in the 30 school days after the first and second health camps (in December 2001 and March 2002, respectively) though, and this was mainly due to high pre-school absenteeism rates (recall that children receive iron supplementation at the pre-schools). Note that iron take-up was only recorded for the 30 pre-school days following each health camp, so it is likely many children received some additional iron between that point and the following health camp, and as a result, these reports are lower bounds on actual iron compliance.[15] Similarly, only 65 percent of eligible children received deworming drugs during the first two health camps – largely due to absenteeism at the health camps – although deworming take-up is nearly universal for children that actually attended the camps.

4. Econometric Identification

4.1 Estimation Strategy

The random assignment of pre-school clusters to treatment and comparison groups allows us to interpret mean differences in outcomes as causal effects of the health program. The panel dimension of the dataset – including longitudinal data on the main educational and nutritional outcomes – allows us to control for initial differences across treatment groups in a difference-in-differences model:

(1) $$Y_{ict} = \alpha + \theta \cdot TREATMENT_{ct} + G_c' \gamma + X_{ict}' \beta + P_t' \pi + \varepsilon_{ict}$$

Y_{ict} is an outcome of interest (for example, a school participation observation) of individual *i* in cluster *c* at time *t*; $TREATMENT_{ct}$ is an indicator variable for assignment to treatment (for the main year one analysis, this is an indicator that takes on a value of one for Group I pre-schools after the intervention was

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[15] Due to these data limitations, we are unable to determine how variation in iron take-up affected subsequent hemoglobin (Hb), anthropometric outcomes, and school participation. However, note that Thomas et al (2003) and Ekstrom et al (2002) show that even imperfect iron compliance can result in complete iron repletion in adults and pregnant women, respectively.

launched in December 2001); G_c is a vector of indicator variables for the treatment groups, to control for any mean baseline differences across groups; X_{ict} is a set of child and pre-school characteristics, which control for remaining pre-treatment differences and improve statistical precision; P_t is a vector of time controls; and ε_{ict} is the disturbance term, and these are allowed to be correlated at the pre-school cluster level. Here θ is the average program effect. Equation (1) is also extended to estimate heterogeneous program effects as a function of individual and pre-school characteristics in some specifications, by interacting the treatment indicator with those characteristics. Baseline students are assigned the treatment group of their original pre-school throughout the empirical analysis – in an intention to treat (ITT) design – even if they later transfer among sample pre-schools.

In cases where there is no available data on pre-intervention child outcomes – e.g., for hemoglobin (Hb) concentration, where there is no baseline information for Group II or III children, since it was thought unethical to collect such information from children who were not to be treated immediately – we estimate the cross-sectional difference between treatment and comparison pupils conditional on baseline individual and pre-school characteristics (X_{ict}), but without either the vector of group indicator variables or the time controls.

4.2 Sample Size and Attrition

One key remaining econometric identification issue is sample attrition, which can lead to bias if attrition is asymmetric across treatment groups. While there are many potential sources of attrition, the two most important in this context are the closure of pre-schools (recall that the NGO program was still new in 2001, and many schools closed down in the first months of the study), and household residential mobility.

The first type of attrition, due to pre-schools closing down, is not a major problem in the first year of the study since most closed down prior to the randomization into project treatment groups, and we drop these schools from the subsequent analysis. To illustrate, from the original pre-school population of 4,068 children documented during the initial September 2001 attendance round, 1,676 were lost by the start of the intervention in December 2001, when the randomization into treatment and comparison groups occurred and was announced in these communities.[16] We restrict the analysis to the 2,392 children who had not dropped out of the sample by then, and from now on, they are called the "baseline sample".

There was some attrition during December 2001 to April 2002, the five month period that is the focus of this paper. Some children left the sample permanently, but others left for a limited period – often for a temporary stay in their home village – and returned later in the school year. An examination of attrition rates by treatment group (for the baseline sample) indicates that attrition rates in the unannounced attendance checks are relatively high for each group, in terms of the proportion of time that the child has been out of the sample, at approximately 25 percent of all attendance checks (Table 2, Panel A).[17] However, fortunately for estimation, there is no differential attrition across the various treatment groups: during December 2001 to April 2002, average attrition for Group I children was 26 percent, while for Groups II and III it was 25 percent, and these differences are not statistically significant. The same is true regarding attrition at health camps and in the 2002 Hb survey: there are relatively high attrition rates, but no systematic differences between treatment and comparison groups (Panels B and C).

Children that leave the sample ("attriters") are broadly similar to those who remained, along observable characteristics (Table 3, Panels A-D). For instance, there are no statistically significant differences in the key dimensions of mother education and father occupation across out-of-sample and in-sample children. Moreover, there is also no evidence for differential attrition across groups along observables: there is no statistically significant difference (at 95 percent confidence) across groups for 15 of the 16 observable characteristics (refer to the F-statistics in Table 3).

There was extensive attrition between the 2001-02 and 2002-03 school years, and by the end of the 2002-03 school year up to three-quarters of the December 2001 sample was lost. Even though we

[16] The NGO runs "enrollment drives" during August-September, after which the NGO closes down pre-schools with very low enrollment; many such new pre-schools were closed during August-December 2001.

[17] Note that a subset of the attriters enrolled in primary school during the 2001-02 academic year, although our best estimate is that somewhat fewer than 9 percent of the baseline sample children entered primary school.

find no significant differences in attrition across treatment groups in year two, inference based on a sample with attrition rates this high may not be reliable due to the possibility of selection into attrition, and for this reason, the main analysis focuses on year one.

Various methods are used in an attempt to address non-random sample attrition. First, individual, household, and pre-school characteristics that could be determinants of attrition are included as explanatory variables, as in Alderman et al (2001). We also re-weight each observation by the inverse of the probability of staying in the sample (by cluster), and carry out weighted least squares estimation following Fitzgerald et al (1998), in order to maintain sample balance along observable dimensions.

This method does not eliminate attrition bias if unobserved characteristics are correlated with both sample attrition and child outcomes. We thus also employ a non-parametric method developed by Lee (2002) to place extreme bounds on program effects in the presence of differential attrition across the treatment and comparison groups. The Lee method derives bounds on treatment effects under the two extreme assumptions that "missing" individuals in the group with higher attrition are either among (i) the best performing, or (ii) the worst performing members of their group (say, in terms of school performance). In practice, this requires trimming the upper (lower) tail of the observed outcome distribution in the program group with less sample attrition by the amount of differential attrition across the program groups, in order to generate the lower (upper) treatment effect bound. As shown below, given the similar attrition rates across program groups, the Lee method yields reasonably tight bounds.

5. Program Impacts

5.1 Health and Nutrition Impacts

The program led to large gains in child nutritional status in the first five months of the intervention. There are improvements in child weight-for-height and weight-for-age Z-scores of 0.52 and 0.31, respectively (Table 4, regressions 1 and 2), and both effects are statistically significant. These increases are equivalent to an average weight gain of 0.5 kg, or 1.1 lbs (regression 3), and to a substantial increase in the body mass index (regression 4). There are no average gains in child height-for-age (regression 5), but this pattern makes sense from a clinical standpoint: iron supplementation is thought to reduce acute malnutrition in the short-run in part by improving the absorption of micronutrients and increasing appetite, but improvements in chronic malnutrition are not expected over short periods. Deworming may also lead to anthropometric gains among the subset of children with helminth infections. The treatment effect bounds on the weight-for-height Z-score are 0.36 and 0.53 (not reported in the table), thus the results appear robust to potential attrition bias. To further check robustness, we also estimated the treatment effects using a specification with individual fixed effects and found largely similar results, with an estimated average weight-for-height Z-score gain of 0.36 (standard error 0.12, not shown). These results are consistent with findings on weight gains in Bhatia and Seshadri (1993) among a similar population of Indian pre-school children.

Unlike Miguel and Kremer (2004), treatment externality effects across pre-schools are small and not statistically significant within 4 km of treatment schools (school locations were obtained using GPS technology) for both child nutritional status and school participation. This lack of cross school externalities is not unexpected since the intensity and magnitude of worm infections in this setting is only moderate (30 percent prevalence, compared to 92 percent in the Kenyan sample), the children treated for worms through the Pratham pre-schools are only a small fraction of all individuals infected with worms in these communities (and thus are unlikely to have a major impact on environmental contamination with worm eggs), and also since randomization was carried out at the pre-school "cluster" level, which means that nearby schools are typically assigned to the same treatment group. There is no obvious epidemiological channel through which iron supplementation would generate substantial spillovers.

Nutritional improvements appear largest for groups with high rates of baseline anemia. For instance, there are significantly larger gains for girls than boys (Table 5, regression 3), although no different effect by child age (regression 2). There is no significant differential effect of the program in pre-schools with a higher proportion of Muslims, or with higher levels of parent education (regressions 4-6); however, there are negative coefficient point estimates on the interactions between treatment and each of these three characteristics (with a marginally significant estimate for the father education interaction), suggesting that children living in relatively low-SES communities gain somewhat more. These baseline characteristics – child age, gender, and average school religious composition and parent education – were then used to compute a predicted likelihood of moderate-severe anemia, and we find that weight gains are indeed concentrated among those with a greater predicted likelihood of anemia (regression 7).

The hemoglobin results are weaker than the anthropometric findings: although treatment (Group I) pupils show somewhat higher hemoglobin levels than comparison pupils in the October 2002 survey – a gain of roughly 0.1 g/dL – the difference is not statistically significant (Table 4, column 6). We explored a variety of other specifications with the Hb data – including indicator variables for particular anemia thresholds as dependent variables, as well as quantile regressions – but in no case is there evidence of a statistically significant increase in Hb concentration among Group I children after one year of the program. One likely explanation for these weak results is the fact that the Hb survey was conducted in October 2002, a full six months after the previous round of iron supplementation, which took place in Group I schools in March and April 2002. Existing research indicates that while Hb gains are large in the months immediately following supplementation, they may not persist more than four months after supplementation (Kashyap and Gopaldas 1987)[18]. Since iron status is multi-faceted, it is also possible that there were in fact iron gains, but in the form of serum iron or ferritin (e.g., stored iron) – which we do not have information on – with only weak short-run improvements in Hb concentration, as found previously by both Stoltzfus et al (2001) and Allen et al (2000). Allen et al (2000) also suggest that small Hb improvements among pre-school aged children following iron supplementation could be the result of other micronutrient deficiencies.

Time lags may also in part account for the lack of statistically significant reductions in helminth infections rates by October 2002 (deworming had taken place in March 2002 – results not shown), since some helminth re-infection over time is likely.

[18] Kashyap and Gopaldas' (1987) study of poor 8-15 year-old school girls in Baroda, India, finds no difference in hemoglobin levels for treated versus comparison children four months after the withdrawal of iron supplementation.

5.2 School Participation Impacts

The program is associated with a large and statistically significant improvement in pre-school participation during year one, at 5.8 percentage points (Table 6, regression 1). Given average school participation rates of approximately 70 percent, this constitutes a reduction in pre-school absenteeism of roughly one-fifth. Figure 1 presents the time pattern of pre-school participation rates for Group I versus Groups II and III students through time: Group I participation rates are slightly lower than Group II and III rates before the first health camp in December 2001, but increase sharply after treatment, and remain greater than comparison school rates through the end of year one. The Lee (2002) extreme bounds range from 2 to 7 percentage points (not reported in the table).

The average program impact on school participation – among the baseline sample of children – for years 1 and 2 taken together is 9.2 percentage points (standard error 2.8 percentage points, not shown). However, very high attrition rates over 70 percent during year two could lead to substantial bias in this estimate, and thus we do not emphasize the year two results.

There are significant differential effects on school participation by child age in year one (Table 6, regression 2), with children aged 4-6 years old showing large gains (9.6 percentage points, statistically significant at 95 percent confidence), while there is essentially no effect for younger children (0.2 percentage points). This may be due to the fact that parents are more interested in having 4-6 year old children attend pre-school in order to prepare them for primary school, and indeed average school participation is higher for older children.

There are somewhat larger school participation gains for girls than boys (Table 6, regression 3, although not statistically significant), which is consistent with both the patterns of baseline anemia and the anthropometric gains. There is no statistically significant differential program effect in pre-school clusters with a higher proportions of Muslims or with higher levels of parental education (regressions 4-6), but there are once again negative point estimates on all three interaction terms with the treatment indicator (and a marginally significant estimate for the mother education interaction), suggesting again that children living in relatively low-SES communities probably gain more from the program. School participation gains are concentrated among those with a greater predicted likelihood of anemia based on initial characteristics (regression 7), as was also the case for weight gains (in Table 5).

It is theoretically possible that the school participation gains are due to children coming to school in order to take their daily dose of iron, rather than due to the effect of child health and nutrition gains per se. However, school participation gains are almost identical in the month of February 2002 – the estimated difference between these months and other months is just 0.003, standard error 0.028, regression not shown – a month when iron was not distributed, suggesting that the participation gains are not solely a result of the desire to take the iron pills.

We next estimate the causal impact of child nutrition on school participation rates in an instrumental variables framework, imposing the condition that participation gains work solely through improved nutrition (specifically, weight gains in this case). This structural estimation may facilitate comparison with future studies of child nutrition and schooling; unfortunately, no existing study (that we could find) directly examines impacts of child weight on school participation or attendance. We find that a one Z-score increase in child weight-for-height translates into a large 19 percentage point gain in school participation in the IV-2SLS specification, and this effect is statistically significant at 90 percent confidence (regression not shown). Although the analogous IV-2SLS result for child weight is imprecisely estimated, it suggests that, conditional on child age and gender, an increase in child weight of one kilogram (2.2 pounds) leads to a massive 15 percentage point gain in pre-school participation (not shown). First-differenced IV-2SLS estimates are broadly similar, although less precisely estimated.[19] Of course, these IV-2SLS estimates may overstate the impact of weight gains on school participation to the extent that the exclusion restriction is not satisfied – in other words, if the health program affected outcomes through channels other than weight – and for this reason we do not emphasize these results.

[19] The analogous first-differenced IV-2SLS point estimates for weight-for-height and weight are 0.20 (standard error 0.17) and 0.31 (standard error 0.28), respectively. In these specifications, the sample is restricted to children with data from both health camps that were conducted during the 2001-2 school year.

5.3 Intra-household Effects

There is no short-term evidence from the October 2002 Household survey that the pre-school health program led to changes in the economic behavior of other household members: there is no robust difference across treatment groups ten months after the start of the health intervention in either total parent labor supply, or in the school enrollment of older siblings (Table 7 presents the main coefficient estimates on the treatment school indicator). One suggestive result is a moderate gain in mothers' labor supply of 3.9 hours per week (standard error 2.1), however, the effect is only marginally statistically significant at 90 percent confidence. The imprecision of the other main coefficient estimates does not permit us to make strong claims about intra-household effects. Note, however, that impacts could have been dampened somewhat since the 2002 household survey was conducted over five months after the previous round of iron supplementation.

5.4 Program Cost-Effectiveness

Pre-school participation is a form of human capital investment, and in this subsection we conduct a cost-effectiveness assessment of the program with this perspective. This analysis is speculative for a number of reasons. First, although the existing empirical literature estimates the effect of years of completed schooling (i.e., attainment) on wages, in this study we are forced to assume that the impact of an increase in school participation on wages is equivalent to the impact of increases in school attainment. Second, since there are no existing estimates of the returns to pre-school education in India (to our knowledge), we assume the returns to pre-school are equivalent to those to primary school.[20] Kingdon (1998) estimates that the returns to an additional year of education in India are 5 percent and 9 percent for girls and boys,

[20] Currie (2003) surveys the related literature from the U.S., and finds evidence linking early childhood interventions to improvements in later educational attainment and cognitive development.

respectively.[21] Finally, we assume that these estimates are real returns to human capital investment, and not simply a signal of unobserved ability.

The pre-school health program increased average school participation by 7.7 and 3.2 percentage points for girls and boys, respectively, as discussed in Section 5.2. Output per worker in India is $1037 (World Bank, 2001). To calculate the effect of the program on the net present value of discounted wages, we make the following assumptions: (i) 60 percent of output per worker in India goes to wages; (ii) wage gains from higher school participation are earned over 40 years in the workforce and discounted at 5 percent per year; (iii) there is no wage growth over time (this assumption is likely to lead us to understate the impact of the program given current Indian growth trends); (iv) current labor force participation rates of 49 percent and 94 percent for women and men, respectively (World Bank 2001), will remain constant over the period; (v) there are no returns to education in household production (another conservative assumption); and, (vi) the opportunity cost of schooling for 2-6 year old children is zero.

Under these assumptions, the Delhi pre-school health program will increase the net present value of lifetime wages by US$29 per child in the treatment pre-schools, while costing only US$1.70 per child. Returns would be even greater if pre-school participation gains led to greater subsequent primary school attainment. However, increases in school participation could lead to negative externalities due to classroom congestion, so we also must account for the additional teachers needed to offset class size increases. To maintain a ratio of twenty students per teacher, and given the NGO's pre-school teacher compensation of US$135 per year in Delhi, the program requires an additional 0.0027 teachers per child per year, amounting to $0.36 per pupil in treatment pre-schools. So, the pre-school health program would generate US$29 in future wage gains at a cost of $1.70 + $0.36 = $2.06, yielding a large benefit-cost ratio of over 14. It is worth noting that, even if the returns to pre-school participation were only, say, one-fifth as large as returns to primary school, the benefit-cost ratio would remain greater than two.

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[21] Kingdon's (1998) estimates are obtained from a standard Mincerian earnings function, correcting for participation in the labor market using the Heckman two-step procedure.

6. Methodological Issues in Program Evaluation: Non-Random Sorting into Treatment Groups

Random assignment to experimental groups addresses a fundamental problem in causal inference, namely, omitted variable bias due to non-random treatment decisions. However, if individuals are able to self-select into the pre-school of their choice after randomization has taken place, the experiment can be partially contaminated (LaLonde 1986, Heckman and Smith 1995, Hotz, Mullin, and Sanders 1997). In year one (2001-02), there is no evidence of large-scale transfers into Group I pre-schools, and this is likely to be the result of the fact that the assignment of pre-schools to treatment groups took place several months after the school year had already started (and thus after students had already enrolled in particular schools). The health program was also new and unproven in year one, and thus less likely to draw additional students into Group I pre-schools.

Unfortunately, there is suggestive evidence of non-random sorting among the cohort of students that first entered pre-school in August 2002 (the "new cohort"). Given that this was not a 'double-blind' evaluation, new cohort parents with greater interest in child health and education could have self-selected into the treatment pre-schools to the extent they knew health treatment would be offered in Group I and II pre-schools starting in November 2002 but not in Group III until the following year – complicating causal inference if children in such households have different school participation levels and time patterns. This appears to have occurred: new cohort children enrolled in Group I and II schools had substantially (although not statistically significantly) higher initial school participation before the first health camp of the 2002-03 school year, at 6.6 and 7.3 percentage points higher participation than Group III pre-school children, respectively (Table 8, regression 1). Group I children also had substantially greater height-for-age Z-scores than Group III on average (regression 3).[22] Year two program impact estimates among the new cohort are weaker than estimates for the baseline sample, and extensive self-selection among the

[22] We do not have as rich individual, household, and pre-school data characteristics data for the new cohort of children. In Table 8, observations are weighted as a function of the probability of attrition based on child gender, group assignment and neighborhood indicators, following Fitzgerald et al (1998).

treatment groups could in part explain why: program impact estimates on child weight-for-height Z-scores for the new cohort of children remain positive but are substantially smaller, at 0.17 standard deviations (standard error 0.14), and similarly, the estimated average program impact on school participation is near zero (not reported).

7. Conclusion

An inexpensive pre-school health intervention in Delhi, India providing iron supplementation and deworming medication led to substantial gains in child weight and pre-school participation, reducing pre-school absenteeism by roughly one-fifth. The results contribute to a growing view that child nutritional deficiencies – and in particular iron-deficiency anemia – are key impediments to human capital accumulation among poor children in less developed countries, and that school-based health programs are likely to be among the most cost-effective ways to promote schooling in these settings (for other recent evidence, see Glewwe et al 2001, and Miguel and Kremer 2004). Yet despite this recent progress in understanding the impact of child nutrition and health on education, the longer-term effects of child health on adult income and life chances in less developed countries remain poorly understood, and this question urgently demands further research attention.

Randomized evaluations like this study provide particularly transparent evidence to policymakers on program impacts, and have the potential to exert considerable influence on actual policy choices, as argued recently by Kremer (2003). For instance, given the results presented in this paper, the Indian NGO that conducted the project is currently expanding this pre-school health model into additional cities.

However, randomized evaluations are not a panacea. We also encountered two important methodological problems in the implementation of this evaluation, namely, high levels of sample attrition (which prevented us from credibly estimating year two program impacts among the baseline sample), and contamination of the experiment due to non-random selection into treatment groups in year two. These concerns must be taken into account in the design of randomized evaluations, just as they are in non-experimental studies.[23] These two issues are likely to be salient in many urban study sites, wherever there are dense populations of highly mobile households, and when study units – in our case, pre-schools – assigned to different treatment groups are located near each other. The recent experience of the Indonesia Family Life Survey (IFLS) indicates that sample attrition can be minimized in a less developed country setting if adequate resources and ingenuity are brought to the task – although it is worth noting that few other studies have had the tracking success of the IFLS. To limit non-random sorting across treatment groups, future evaluations could also randomize treatment across larger units of aggregation – say entire neighborhoods or villages – to the extent this is financially and logistically feasible.

8. References

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[23] See Thomas, Frankenberg and Smith (2002) for a discussion of attrition in longitudinal studies in poor countries.

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9. Tables and Figures

Table 1: Baseline Characteristics

	Group I	Groups II, III	Group I – Groups II, III
Panel A: Nutrition Characteristics			
Mean hemoglobin concentration (g/dl) *(N=180)*	9.95	-	-
Children with anemia (Hb < 11 g/dl)	0.69	-	-
Hb 10-11 g/dl	0.21	-	-
Hb 7-10 g/dl	0.41	-	-
Hb < 7 g/dl	0.07	-	-
Any helminth infection *(N = 159)*	0.30	-	-
Baseline Anthropometrics (N = 1412)			
Weight-for-height z-score	-1.12	-1.02	-0.10
			(0.12)
Weight-for-age z-score	-1.41	-1.15	-0.26**
			(0.12)
Height-for-age z-score	-0.79	-0.45	-0.34*
			(0.18)
Panel B: Child Characteristics (N = 2371)			
Age	3.66	3.65	0.01
			(0.06)
Boys	0.45	0.46	-0.01
			(0.02)
Mean School Participation, Nov-Dec 2001	0.71	0.70	0.02
			(0.03)
Panel C: Household Characteristics			
Family size (N=520)			
Family size	6.1	5.9	0.1
			(0.2)
Number of children	2.4	2.2	0.3
			(0.2)
State of origin (N = 520)			
Uttar Pradesh	0.59	0.50	0.10*
			(0.06)
Delhi	0.20	0.30	-0.10*
			(0.06)
Bihar	0.11	0.12	-0.01
			(0.03)
Religion (N = 509)			
Hindu	0.75	0.82	-0.07
			(0.06)
Muslim	0.25	0.17	0.08
			(0.06)

Table 1: Baseline Characteristics (cont.)

	Group I	Groups II, III	Group I – Groups II, III
Panel D: Mother Characteristics			
Educational level (years) (N = 510)	3.3	3.4	0.0
			(0.5)
Occupation (N = 478)			
Housework / Homemaker	0.78	0.78	0.00
			(0.05)
Laborer	0.04	0.08	-0.04
			(0.03)
Salaried Employment	0.06	0.04	0.02
			(0.02)
Self-employment (e.g., business)	0.03	0.03	0.00
			(0.02)
Panel E: Father Characteristics			
Educational level (years) (N = 497)	5.8	6.6	-0.8
			(0.6)
Occupation (N = 475)			
Laborer	0.35	0.30	0.05
			(0.06)
Salaried Employment	0.27	0.34	-0.07
			(0.05)
Self-employment (e.g., business)	0.14	0.16	-0.02
			(0.04)
Government	0.06	0.08	-0.02
			(0.03)
Panel F: Access to Health Treatments (N = 520)			
Received iron supplementation (in last year)	0.01	0.01	0.00
			(0.01)
Received deworming medicine (in last year)	0.06	0.06	-0.01
			(0.03)
Received vitamin A (in last year)	0.01	0.00	0.01
			(0.01)

Notes for Table 1: Robust standard errors (clustered) in parentheses; statistically significant at the (*) 90, (**) 95, and (***) 99 confidence levels. Anthropometric and attendance rate estimates are weighted to adjust for the probability of attrition based on gender and neighborhood variables. Proportions presented in Panel F are parent reports. Sources: 2001 Household Survey, Health Camp and Attendance Checks, 2001-2002.

<u>Table 2:</u> Attrition Rates in Attendance Checks, Health Camps, and Hb Tests

	Group I	Groups II, III	Group I – Groups II, III
<u>Panel A:</u> Attendance Checks (N = 2392)			
Proportion of time out-of-sample (November 2001 - April 2002)	0.26	0.25	0.00
			(0.03)
November - December 2001	0.14	0.15	-0.01
			(0.03)
January - February 2002	0.25	0.25	0.00
			(0.04)
March - April 2002	0.36	0.35	0.01
			(0.04)
<u>Panel B:</u> Health Camps (N = 2392)			
Health Camp 1 (December 2001)	0.35	0.39	-0.04
			(0.04)
Health Camp 2 (March 2002)	0.52	0.48	0.04
			(0.04)
<u>Panel C: Hemoglobin Tests</u>			
October 2002 (year two) Hb test[a]	0.46	0.54	-0.08
			(0.06)

<u>Notes for Table 2:</u> Robust standard errors in parentheses; statistically significant at (*) 90 percent, (**) 95 percent, and (***) 99 percent confidence levels. *a* = Data available only for Groups I and II.

Table 3: Characteristics of In-Sample vs. Out-of-sample (on Attendance Checks) Children across Groups

	Proportion Out-of-Sample		F-stat on
	Group I (b_1)	Groups II & III (b_2)	H_0: $b_1 = b_2$
Panel A: Child characteristics			
Child's age	0.0	0.2**	1.32
	(0.14)	(0.12)	
Boy	0.01	-0.06	1.01
	(0.05)	(0.05)	
Panel B: Household characteristics			
Family Size	0.4	-0.9*	2.47
	(0.7)	(0.6)	
Number of children	-1.3**	-1.4***	0.04
	(0.4)	(0.3)	
Uttar Pradesh	-0.12	0.09	1.24
	(0.14)	(0.13)	
Hindu	-0.07	-0.01	0.14
	(0.13)	(0.12)	
Panel C: Mother Characteristics			
Educational level (years)	-1.5	0.0	0.70
	(1.4)	(1.2)	
Housemaker	-0.10	0.05	0.55
	(0.15)	(0.13)	
Salaried employment	-0.08	0.04	1.94
	(0.07)	(0.06)	
Self-employment	-0.06	0.00	1.04
	(0.04)	(0.04)	
Laborer	0.12	0.00	0.78
	(0.10)	(0.10)	
Panel D: Father Characteristics			
Educational level (years)	-3.1**	0.8	4.25***
	(1.4)	(1.3)	
Government	-0.11**	0.01	2.06
	(0.05)	(0.07)	
Salaried employment	0.11	0.11	0.00
	(0.18)	(0.12)	
Self-employment	-0.06	-0.06	0.00
	(0.11)	(0.10)	
Laborer	0.10	-0.14	1.12
	(0.19)	(0.12)	

Notes for Table 3: The coefficient estimates in each row are from a separate regression (not all terms are shown). Robust standard errors in parentheses. Statistically significant at (*) 90, (**) 95, and (***) 99 confidence levels. Sources: Attendance Checks, Nov 2001-April 2002; 2001 Household Survey.

Table 4: Program Impacts on Anthropometrics and Hemoglobin Levels

	Dependent variables:					
	Weight-for-height z-score	Weight-for-age z-score	Weight	BMI	Height-for-age z-score	Hemoglobin (g/dL, 2002)
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Treatment indicator	0.52***	0.31*	0.50	0.70**	-0.19	0.1
	(0.18)	(0.17)	(0.30)	(0.27)	(0.21)	(0.2)
Group I	-0.18	-0.29**	-0.46**	-0.14	-0.28*	
	(0.12)	(0.12)	(0.21)	(0.18)	(0.16)	
Child and pre-school controls	Yes	Yes	Yes	Yes	Yes	Yes
Time trends	Yes	Yes	Yes	Yes	Yes	No
Mean of dependent variable	-1.05	-1.18	13.4	14.2	-0.46	10.5
Observations	2383	2383	2383	2383	2383	688
R-squared	0.11	0.09	0.13	0.14	0.16	0.16

Notes for Table 4: Robust standard errors in parentheses; disturbance terms are allowed to be correlated within clusters; * = significant at 90%; ** = significant at 95%; *** = significant at 99% confidence level. Coefficient estimates from WLS regressions adjusting for attrition by pre-school cluster. Controls include child age and child gender indicators, neighborhood indicators; teacher age, teacher years of schooling, and pre-school language; proportion Muslim in the pre-school, mean mothers' and fathers' education in the pre-school. BMI stands for body mass index, and is defined as Weight (in kg) / $\{\text{Height (in cm)}\}^2$.

Table 5: Program Impacts on Weight-for-Height Z-Scores Levels

	Dependent variable: Weight-for-height Z-score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Treatment indicator	0.52***	0.47**	0.68***	0.65**	0.63*	1.07***	-0.66
	(0.18)	(0.19)	(0.23)	(0.25)	(0.35)	(0.39)	(0.47)
Treatment * Age 2-3 years		0.12					
		(0.16)					
Treatment * Boy			-0.37**				
			(0.18)				
Treatment * Proportion Muslim				-0.46			
				(0.49)			
Treatment * Mean Mothers' Education					-0.03		
					(0.07)		
Treatment * Mean Fathers' Education						-0.10*	
						(0.05)	
Treatment * Predicted moderate-severe anemia at baseline (Hb < 10 g/dl)							2.5**
							(1.1)
Group I	-0.18	-0.12	-0.20*	-0.12	-0.22	-0.51*	0.48
	(0.12)	(0.14)	(0.12)	(0.15)	(0.19)	(0.26)	(0.38)
Child and pre-school controls, time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	-1.05	-1.05	-1.05	-1.05	-1.05	-1.05	-1.05
Observations	2383	2383	2383	2383	2383	2383	2383
R-squared	0.11	0.11	0.11	0.12	0.11	0.11	0.11

Notes for Table 5: Robust standard errors in parentheses; disturbance terms are allowed to be correlated within clusters; * = significant at 90%; ** = significant at 95%; *** = significant at 99% confidence level. Coefficient estimates from WLS regressions adjusting for attrition by pre-school cluster. Controls include child age and child gender indicators; neighborhood indicators, teacher age, teacher years of schooling, and pre-school language; proportion Muslim in the pre-school, and mean mothers' and fathers' education in the pre-school, and, in regression 7, predicted moderate-severe anemia. In the regressions with interaction terms (2-7), we include the relevant explanatory variable, as well as its interaction with the time controls and the treatment group indicators, but do not report these in the table. Predicted moderate-severe anemia at baseline is a function of child age, gender, the proportion of Muslims in the pre-school, and average mother and father education in the pre-school.

Table 6: Program Impacts on School Participation, Year One Results

	Dependent Variable: Indicator for Pre-School Participation						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Probit	Probit	Probit	Probit	Probit	Probit	Probit
Treatment indicator	0.058*	0.096**	0.077**	0.027	0.127***	0.157**	-0.23
	(0.034)	(0.038)	(0.037)	(0.038)	(0.049)	(0.072)	(0.16)
Treatment * Age 2-3 years		-0.094*					
		(0.054)					
Treatment * Boy			-0.045				
			(0.046)				
Treatment * Proportion Muslim				0.128			
				(0.127)			
Treatment * Mean Mothers' Education					-0.022*		
					(0.011)		
Treatment * Mean Fathers' Education						-0.019	
						(0.012)	
Treatment * Predicted moderate-severe anemia at baseline (Hb < 10 g/dl)							0.57*
							(0.32)
Group I	-0.027	-0.056	-0.036	0.025	-0.084*	-0.126	0.21
	(0.034)	(0.041)	(0.038)	(0.037)	(0.051)	(0.088)	(0.12)
Child and pre-school controls, time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	0.703	0.703	0.703	0.703	0.703	0.703	0.703
Observations	9275	9275	9275	9275	9275	9275	9275

Notes for Table 6: Marginal probit estimates (evaluated at mean covariate values) are presented. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within clusters; * = significant at 10%; ** = significant at 5%; *** = significant at 1% confidence levels. Estimates from weighted regressions adjusting for attrition by pre-school cluster. Controls include child age and child gender indicators; neighborhood indicators, teacher age, teacher years of schooling, and pre-school language; proportion Muslim in the pre-school, and mean mothers' and fathers' education in the pre-school, and, in regression 7, predicted moderate-severe anemia. In the regressions with interaction terms (2-7), we include the relevant explanatory variable, as well as its interaction with the time controls and the treatment group indicators, but do not report these in the table. Predicted moderate-severe anemia at baseline is a function of child age, gender, the proportion of Muslims in the pre-school, and average mother and father education in the pre-school.

Table 7: Intra-Household Program Impacts (2002 Household Survey data)

Dependent variable:	Coefficient estimate on the Treatment School Indicator, Group I (s.e.)	Mean of dependent variable
Mothers' total labor supply (hours per week)	3.9^{*} (2.1)	42.2
Fathers' total labor supply (hours per week)	-1.9 (1.8)	50.7
Primary school enrollment, older sisters	0.059 (0.048)	0.78
Primary school enrollment, older brothers	-0.029 (0.047)	0.80

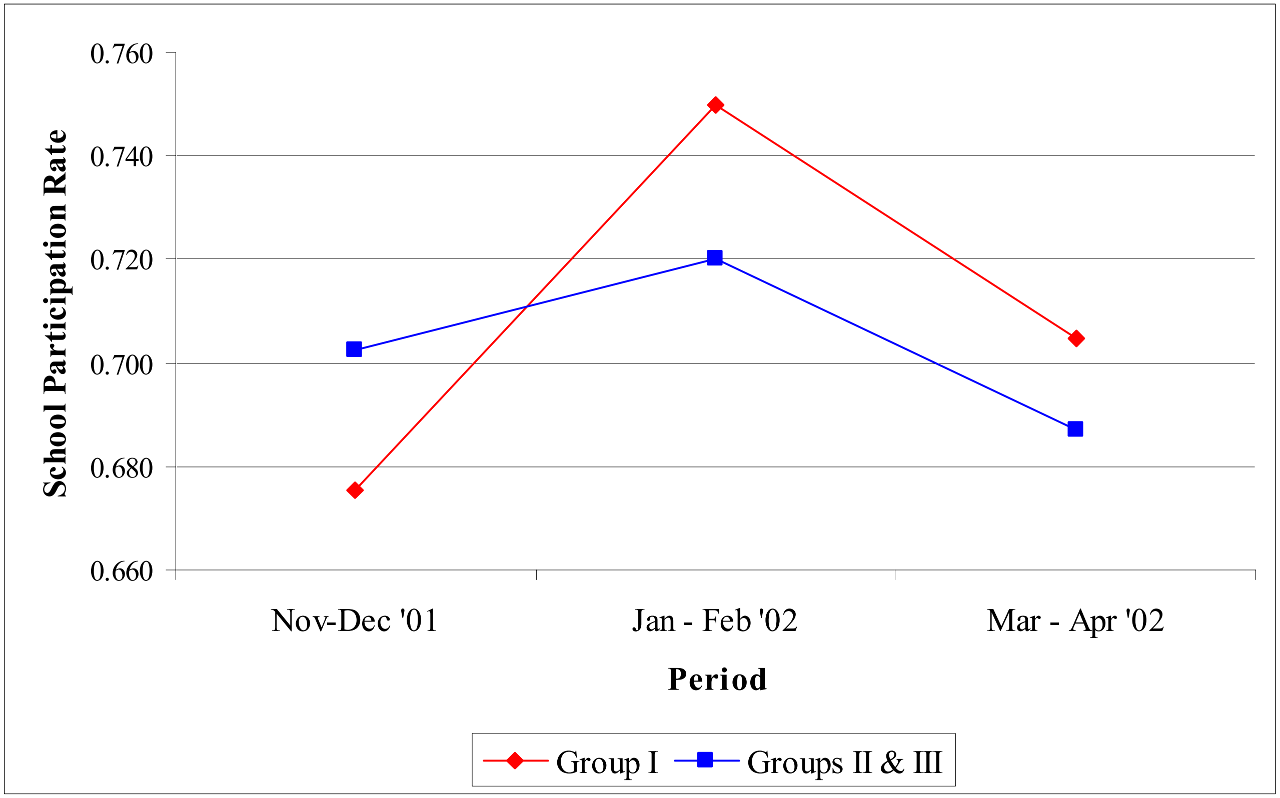
Notes for Table 7: Estimates presented for mothers' total labor supply and fathers' total labor supply effects are OLS coefficient estimates. Marginal probit estimates (evaluated at mean covariate values) are reported for siblings' school enrollment effects. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within clusters; *=significant at 90%; **=significant at 95%; ***=significant at 99% confidence level. Estimates from weighted regressions adjusting for attrition by pre-school cluster. Controls include child age and gender indicators, neighborhood indicators, teacher age, teacher years of schooling, and pre-school language; proportion Muslim in the pre-school, mean mothers' and fathers' education in pre-school. The sample sizes for mothers' and fathers' regressions are 436 and 430, respectively and 386 and 324 for the sisters' and brothers' school enrollment regressions (the different sample sizes across regressions are due to both the fact that not all sample children have a mother, father, or siblings in the household, as well as to missing data).

Table 8: Program Impacts on Anthropometrics and School Participation, New Cohort of Children

	Dependent Variable:		
	School Participation	Weight-for-height z-score	Height-for-age z-score
	(1) Probit	(2) WLS	(3) WLS
Group I	0.066	-0.05	0.22
	(0.081)	(0.18)	(0.16)
Group II	0.073	-0.02	-0.01
	(0.078)	(0.14)	(0.19)
Post-intervention	-0.059	-0.05	-0.05
	(0.059)	(0.10)	(0.13)
Treatment Indicator, Group I	0.034	0.20	-0.13
(Post-intervention * Group I)	(0.090)	(0.19)	(0.16)
Treatment Indicator, Group II	-0.041	0.13	-0.22
(Post-intervention * Group II)	(0.096)	(0.15)	(0.18)
Child and pre-school controls, time trends	Yes	Yes	Yes
Mean of dependent variable	0.561	-1.43	-1.08
Observations	11510	2842	2842

Notes for Table 8: Coefficient estimates and Marginal probit estimates (evaluated at mean covariate values) are presented for anthropometrics and school participation results, respectively. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within clusters; * = significant at 90%; ** = significant at 95%; *** = significant at 99% confidence level. Estimates from weighted regressions adjusting for attrition based on gender, group assignment and neighborhood indicators. Controls include child age, child gender, teacher age, teacher years of schooling, and pre-school language.

Figure 1: Pre-school Participation Rates through Time



Notes for Figure 1: Diamonds denote average pre-school participation for Group I, and squares are Groups II and III. The baseline (pre-treatment) period is November-December 2001. Group I was the treatment group during January-April 2002.

Appendix

Table A1: Project Timeline

Period	Activity
Panel A: Academic Year 1	
August – September 2001	Baseline 2001 Health-Socioeconomic Household Survey
October – November 2001	Parent/teacher meetings to discuss the intervention
November 2001	Attendance checks (Round 1)
November – December 2001	Hemoglobin and parasitological tests (Round 1)
December 2001	Attendance checks (Round 2) Phase-in of Group 1 Pre-school children Health camp (Round 1)
January 2002 and February 2002	Attendance checks (Rounds 3, 4)
March 2002	Health camp (Round 2) Attendance checks (Round 5)
April 2002	Attendace checks (Round 6) End of 2001-02 academic school year
Panel B: Academic Year 2	
August – September 2002	Attendance checks (Round 7)
August – October 2002	2002 Health-Socioeconomic Household Survey
October 2002	Hemoglobin and parasitological tests (Round 2)
November 2002	Phase-in of Group 2 Pre-school children Health camp (Round 3) Attendance checks (Round 8)
December 2002 and January 2003	Attendance checks (Rounds 9, 10)
February 2003	Health camp (Round 4) Attendance checks (Round 11)
March 2003	Attendance checks (Round 12)
April 2003	Health camp (Round 5) End of 2002-03 academic school year
November 2003	Phase-in of Group 3 Pre-school children