Anemia and School Participation

Gustavo J. Bobonis
Edward Miguel
Charu Puri-Sharma

ABSTRACT

Anemia is among the most widespread health problems for children in developing countries. This paper evaluates the impact of a randomized health intervention delivering iron supplementation and deworming drugs to Indian preschool children. At baseline, 69 percent were anemic and 30 percent had intestinal worm infections. Weight increased among assisted children, and preschool-participation rates rose by 5.8 percentage points, reducing absenteeism by one-fifth. Gains were especially pronounced for those most likely to be anemic at baseline. Results contribute to a growing view that school-based health programs are an effective way of promoting school attendance in less developed countries.

I. Introduction

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). In particular, iron deficiency anemia leads to weakness, poor physical growth, and a compromised immune system—

Gustavo J. Bobonis is an assistant professor of economics at the University of Toronto. Edward Miguel is an associate professor of economics at the University of California, Berkeley and an NBER faculty research fellow. Charu Puri-Sharma is a Director for the Niramaya Health Foundation. *The authors wish to thank Harold Alderman, Simon Brooker, Rick Hanushek, Chang-Tai Hsieh, Matthew Jukes, David Lee, Steven Levitt, Michael Kremer, and an anonymous referee 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. The authors are grateful to ICICI Bank for funding. This material is based upon work supported by the National Science Foundation under Grant No. 0418110, awarded to Edward Miguel. Miguel acknowledges funding from the World Bank and the University of California, Berkeley Center for Health Research, Committee on Research, and Hellman Fund. The authors claim responsibility for all errors. The data used in this article can be obtained beginning March 2007 through February 2010 from Gustavo J. Bobonis <gustavo.bobonis@utoronto.ca>.*
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decreasing the ability to fight infections and increasing morbidity—and also is thought to impair cognitive performance and delay psychomotor development. Recent macroeconomic estimates suggest that the average impact of iron deficiency anemia, through both physical and cognitive channels, could be as large as 4 percent of GDP in less developed countries (Horton and Ross 2003). Through its impact on schooling, anemia also could 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 studies estimating the impact of anemia on education are difficult to interpret due to possible omitted variable bias.[1]

This study takes a step toward addressing this gap by evaluating the impact of a project in the slums of Delhi, India, which delivers iron supplementation and deworming drugs to 2–6 year-old preschoolers. At baseline, 69 percent of sample children were anemic while 30 percent suffered from intestinal helminth (worm) infections. The 200 preschools in the sample were randomly divided into groups and then gradually phased into the program. Exploiting this experimental design, we find substantial gains in child weight—roughly 0.5 kg (1.1 lbs.) on average—in the treatment schools relative to control schools during the first five months of the project. Most important for this study, average preschool-participation rates increased sharply, by 5.8 percentage points, among assisted children, reducing preschool absenteeism by one-fifth. Program treatment effects are most pronounced for subgroups with high baseline anemia rates, in particular, for girls and children in low socioeconomic status areas, suggesting that the program boosts school participation by reducing anemia. The results point to the key role that poor health can play in limiting school participation.

The key econometric identification issue in this study is sample attrition: During the first school year (2001–2002), approximately one-quarter of school attendance observations are missing—largely due to "churning" of preschools, which open and close at relatively high frequency, as well as difficulties tracking respondents who have moved. However, attrition rates are nearly identical for program treatment and control groups in Year 1 (at 26 and 25 percent, respectively), allowing us to place reasonably tight nonparametric bounds on treatment effects.

Although most common in less developed countries, iron deficiency and iron deficiency anemia[2] are not rare in wealthy countries like the United States: Iron deficiency prevalence was estimated at 7 percent for U.S. toddlers (one and two years old) and 5 percent for preschool aged children (three through five years old) in 1999–2000, with higher rates for African-Americans and Latinos (Centers for Disease Control and Prevention 2002), so some results of the current study may generalize to certain U.S. subgroups. Iron deficiency was long a major public health problem in the United

1. Refer to Miguel (2005) for a review of the literature on child health, nutrition, and schooling. Strauss and Thomas (1998) survey the links between health and underdevelopment.

2. Iron deficiency is the most common cause of anemia, and the two conditions are closely related. Iron deficiency is often defined as having abnormal values for biochemical measures of iron status, including serum ferritin, transferring saturation, and free erythrocyte protoporphyrin. People with iron deficiency anemia and low hemoglobin (Hb) values are considered iron deficiency anemic (Centers for Disease Control and Prevention 2002). In settings where venous blood draws are impractical or too costly, as here, Hb alone is used to determine iron deficiency anemia, as discussed below.

States, and as late as the 1970s prevalence among one through four year-olds nationwide was still 17 percent (Ramakrishnan and Yip 2002). Some have argued that the fortification of food, and particularly infant formula, with iron in recent decades has been critical in reducing iron deficiency in the United States. (Sherry, Mey, and Yip 2001). However, in the absence of a large-scale food processing campaign, iron supplementation through schools (as in the program we study) may be the most cost-effective way to tackle widespread iron deficiency among children in less developed countries, where much food that households eat is produced on their own farms and thus cannot easily be fortified.[3]

One plausible channel through which the preschool attendance gains we estimated 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 own children to the preschools. Unfortunately, there is little direct evidence (at least that we are aware of) linking preschool participation to later educational outcomes in less developed countries. One exception is Berlinski, Galiani, and Gertler (2006), who find primary school test score improvements of 8 percent for children who had earlier participated in public preschool programs in Argentina. There also is growing evidence from the U.S. Head Start program that early childhood interventions reduce later grade repetition and increase educational attainment (Currie and Thomas 1995; Garces, Thomas, and Currie 2002). In terms of long-run evidence, Cascio (2004) finds 30 percent reductions in high school grade repetition among African-American and Latino children, and a 20 percent reduction among white children, who had earlier participated in public kindergarten programs in the U.S. South. Magnuson, Ruhm, and Waldfogel (2004) find evidence of medium-term gains from prekindergarten participation on first grade mathematics and reading, especially for children whose parents have low education or low income. Currie (2001) surveys the related U.S. literature and concludes that there is considerable evidence linking early childhood interventions to improvements in later educational attainment and cognitive development. It is possible that preschool attendance impacts could be even more persistent in the context we examine where there are fewer school remedial programs and where households are poorer (consistent with Magnuson, Ruhm, and Waldfogel 2004).

The current study complements existing experimental studies on child nutrition and improves on them in certain dimensions. One limitation of existing studies of child nutrition and education outcomes is their narrow focus on academic and cognitive tests, typically tests of recall and verbal skills. Nokes, van den Bosch, and Bundy (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 until now. A second limitation of existing studies is their small sample size. For instance, the well-known INCAP study (Martorell, Habicht, and Rivera 1995) provided different nutritional supplements to Guatemalan children and later found significant impacts on their cognitive skills during adolescence. However, that study randomly assigned villages to the treatment and

3. Helminth infections remained widespread in the U.S. South into the 1960s (Martin 1972).

control groups 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 an early childhood nutrition program in Jamaica limits the generalizeability of their findings. In contrast, our sample contains 200 preschools each with an average of 12 children. Moreover, we study and demonstrate the effectiveness of a real-world school-based health program.

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 six through 17 year-old children in rural Kenyan primary schools. The medical intervention in that study was narrower, consisting of deworming drugs alone, unlike the current program, which features iron supplementation and Vitamin A in addition to deworming. Though the results of the current study 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 intervention provides further evidence on the robust relationship between child health and school participation in poor countries. This study also contributes to the growing literature on the economic impacts of 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).

II. 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 Hb in combination with abnormal levels of other iron indicators such as transferring saturation (iron stores)—can lead to weakness, poor physical growth, increased morbidity, and delayed psychomotor development. In particular, animal studies suggest that iron deficiency early in life could inhibit the function of neurotransmitters, compromising later brain function.[4]

It is hypothesized that iron supplementation could generate school-participation gains through two mechanisms: first, improved physical activity (motor development and overall decreased morbidity), and, second, through cognitive development. Yet little work examines the impact of iron supplementation on school participation (an exception is Hutchinson et al. 1997).[5]

Other studies have investigated the association between iron supplementation and cognitive ability among children. Nokes, van den Bosch, and Bundy (1998) survey a number of such experimental studies on the impact of iron supplementation and most find positive impacts. Seshadri et al. (1982) find that five and six year-old children treated with iron subsequently showed gains in verbal performance and IQ scores. Stoltzfus et al. (2001) conducted a randomized trial of iron supplementation on 614

4. See Horton and Ross (2003) for a discussion of the biochemistry of iron deficiency.

5. Hutchinson et al. (1997) documents an association between poor school attendance and mild levels of anemia and roundworm infection among primary school children in Jamaica. However, their study is purely observational and thus subject to well-known omitted variable bias concerns.

preschool children aged six through 59 months, and find that language and motor development improved significantly among severely anemic children. Soemantri, Pollit, and Kim (1985) find that iron supplementation among Indonesian school children improved achievement.

Experimental studies examining whether iron deficiency affects child growth have produced mixed results. Bhatia and Seshadri (1993) examine the effect of a six month supplementation intervention for Indian preschool children three through five years of age, and find less wasting—as measured by weight and weight-for-height *Z*-scores—among anemic children supplemented with iron relative to anemic placebo children. Similarly, Angeles et al. (1993) found increases in height and height-for-age *Z*-scores among anemic Indonesian preschool children after two months of daily iron supplementation. However, Dossa et al. (2001) find no impact on anthropometric measures—even for the stunted and anemic subgroups—during a three month iron supplementation intervention among three through five year olds in Benin.

III. Project Design and Data

A. The Pratham Delhi Preschool Health Program

Pratham Delhi, an Indian nongovernmental organization (NGO),[6] began establishing a network of preschools in poor communities in eastern Delhi during 2000. Children in the preschools are two through six years old, typically come from families of migrant laborers, and are at high risk of anemia and other nutritional deficiencies. Pratham's preschools focus on the development of literacy, numeracy, and social skills to prepare children for primary schooling. Grounded in a belief that reading readiness is a key foundation for later learning and educational achievement, daily activities in the preschools are centered around books and alphabet and word games (Banerji 2005). The preschool teachers are usually young women living in the communities, and classes usually take place in teachers' homes; thus when a teacher moves, marries, or obtains other employment, her preschool may close. Combined with programmatic considerations, this leads to turnover in preschools, particularly in communities with high levels of residential mobility, an issue we return to below in the discussion of attrition.

The study area consists of recent "resettlement colonies," typically 10–20 years old, that have absorbed large inflows during Delhi's recent expansion. The study sample contains 200 preschools with an average of 12 children, yielding a total sample size of 2,392 children. The initial sample of 268 preschools were assigned to 189 different "clusters" at the start of the study, where each cluster contained one to three preschools, usually all located on the same city block. The Pratham Delhi preschool program had expanded rapidly in mid-2001 at the start of the data collection, and 68 of these initial 268 preschools closed between August and December 2001, by chance leaving a final sample of exactly 200 preschools in 155 distinct clusters. In early December 2001, the clusters were randomly divided using a computer random

6. Pratham health programs are coordinated with the Niramaya Health Foundation.

number generator into three treatment groups, Groups I, II, and III.[7] The randomization was carried out at the preschool cluster level for several reasons including: a desire to reduce health spillovers between preschools;[8] to limit transfers of children across preschools belonging to different treatment groups (which would contaminate the randomization); and to avoid discord within neighborhoods, for instance, if some children on the block received the full assistance package but others did not. There was a time lag for the research team in finding out about many of the preschool closures. Thus, the randomization was carried out for the initial 189 cluster sample, and the numbers of clusters and preschools in the analysis sample are not perfectly balanced across groups. However, note that preschool communities were not informed of their program treatment status until mid-December, and this timing means treatment assignment should not have been a factor in the closure of these preschools between October and December 2001. Figure 1 contains a graphical description of the evaluation design.

The preschool health intervention consisted of iron supplementation (33.3 mg of elemental iron with folic acid)[9] and deworming drugs (400 mg of albendazole) administered during "health camps" conducted three times a year and in the preschools.[10] Following WHO recommendations, teachers in treatment preschools administered daily iron doses for 3 school days following each health camp, to all children attending school that day. Deworming was included since worm infections can contribute to anemia (see Hall et al. 2001 and references therein). The deworming drugs were taken at "health camps." Treatment and control preschools both also received 200,000 I.U. of Vitamin A at the first health camp during the study period, and thus treatment effects should be interpreted as the impact of iron and deworming in addition to Vitamin A, relative to Vitamin A alone. Note that in addition to its potential benefits for the immune system and eyesight, Vitamin A may promote iron absorption (Food & Agriculture Organization/World Health Organization 1998), so this is a potentially important interaction.[11] 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. Estimating intervention costs is complicated by the fact that enumerators carried out data collection as well as project activities, but the above figure attempts to isolate intervention costs. We do not attempt a program rate of return calculation in this paper given these concerns about disentangling evaluation and project costs, as well as the lack of reliable estimates of the long-run return to additional preschool attendance in India.

7. Seventeen additional preschools closed down after the randomization during Year 1.

8. In the presence of positive local health externalities from deworming, the difference between treatment and control preschool children would underestimate actual treatment effects (as in Miguel and Kremer 2004).

9. 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 3 mg/kg per day, for three months (Centers for Disease Control and Prevention 1998). The U.S. National Academies of Sciences (2002) claim that up to 40 mg 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 two mg/kg body weight per day, up to 30 mg for three months (World Health Organization 2001).

10. Albendazole is a broad-spectrum anthelminthic effective against roundworm and hookworm.

11. Araujo et al. (1987) documented a reduction in the percent of children with deficient or low Hb levels among children receiving iron, deworming, and Vitamin A.

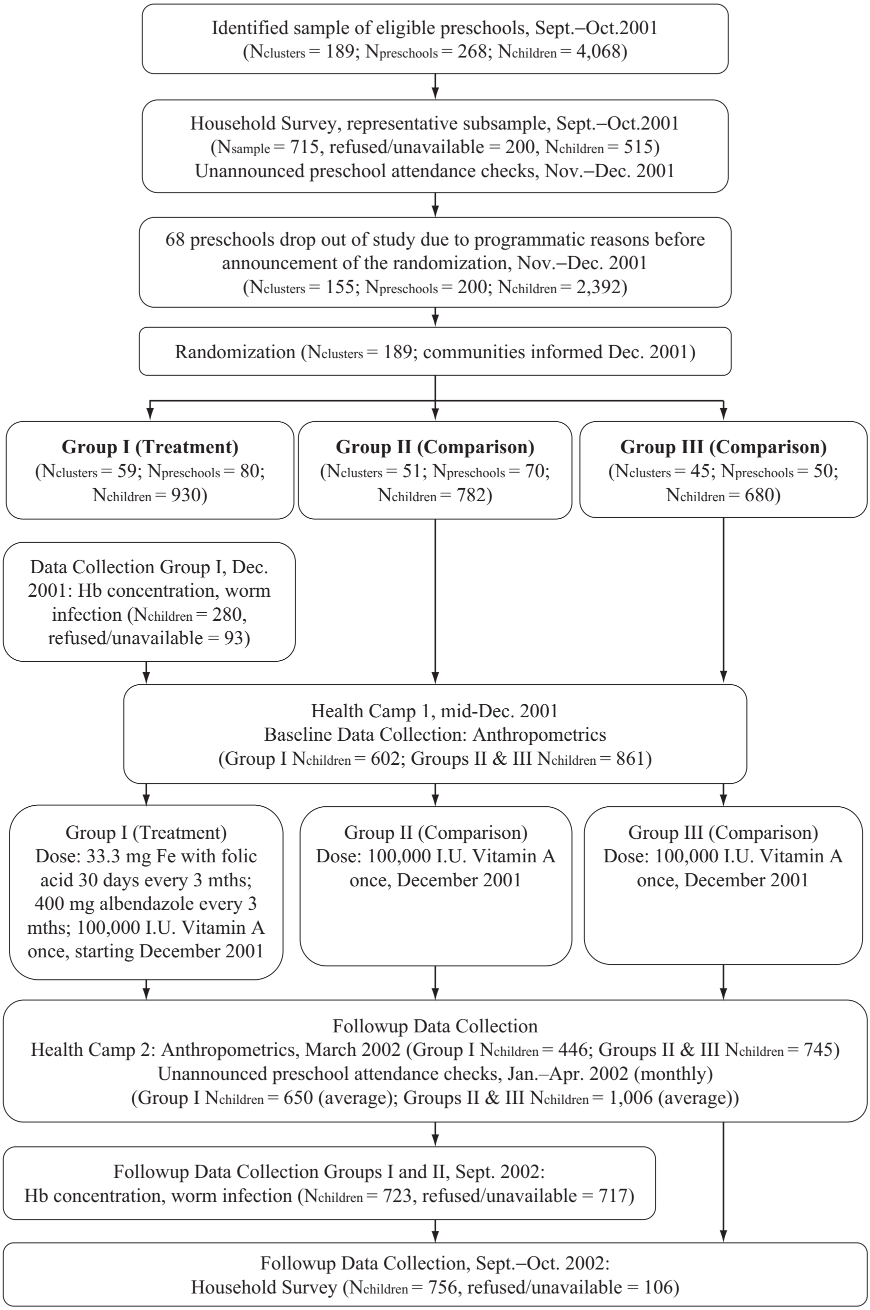

Figure 1
Consolidated Standards of Reporting Trials (CONSORT) Chart of the Pratham Preschool Program Evaluation

During October and November 2001, the NGO enumerators conducted parent meetings at each preschool to inform them about the project, discuss child health issues, and obtain consent for their children's participation. These talks were given at all treatment and control preschools. The first health camp took place in December 2001, and approximately every three months afterward in all schools during the school year. The health package was phased in: Group I began receiving assistance in December 2001 and is the treatment group for the 2001–2002 school year we study, while the Group II preschools received treatment in November 2002 and by November 2003 all preschools were receiving treatment. We do not study Year 2 due to evidence of contamination of the experiment due to nonrandom sorting of incoming pupils across treatment groups (not shown).

B. Data and Measurement

A household survey collected information on socioeconomic and demographic characteristics, parent schooling, labor market outcomes, health knowledge (particularly regarding anemia and worms) and recent illness episodes for all household members. The first household survey round was administered among a representative subset of households during August-September 2001 and the second round in September–November 2002. The 2001 sample consisted of a 30 percent representative clustered sample of the child population from each preschool. In all 515 of the 715 sampled households were interviewed in 2001. Survey completion rates were imperfect due to frequent household mobility, difficulty simply locating some households in these dense urban areas, and occasional refusal. Fortunately, survey completion rates were similar across the three treatment groups (at 77, 69, and 69 percent, respectively), as expected, since the preschools had not yet been divided into treatment groups. Similar difficulties affected the 2002 household survey round in which 650 of the 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 preschool children surveyed in 2001 and Groups I and II children in 2002.[12] 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 the parasitological information for only 159 children in 2001, and there were similar rates in 2002.[13] Baseline Hb data was not taken in 2001 from Groups II and III children since it was thought unethical to collect such information from children who would not be treated immediately.

Enumerators measured child height and weight at the health camps. Anthropometric measures of child weight-for-height ("wasting," acute undernutrition), weight-for-age ("underweight," chronic and acute malnutrition), and height-for-age ("stunting," chronic

12. 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 at children's homes, and analyzed using the Kato-Katz technique to determine infection intensity, proxied by worm eggs per gram of stool.

13. Other studies have found similarly high refusal rates for blood draws (Martorell, Habicht, and Rivera 1995).

undernutrition) Z-scores proxy for nutritional status, where Z-scores reflect the standardized difference, by age and gender, from the mean of reference healthy U.S. children.

Preschool enrollment rosters were collected from August to October 2001, and participation data was then collected during monthly, unannounced visits to all preschools from November 2001 through April 2002, which serve as a representative measure of preschool attendance. At each visit, enumerators determined whether each child from the baseline roster was present, absent, had left school, or had transferred to another school. Those present at school are counted as "participants" and those who had dropped out or were absent as "nonparticipants." Children who transferred to another NGO preschool were tracked at those schools during subsequent attendance checks. Children who moved away from the area (for instance, to another Delhi neighborhood or back to their home village) or about whom the teacher did not have good information, are lost from the sample (until they return to an NGO preschool, if ever).

C. Baseline Characteristics

The baseline surveys identified anemia and helminth infections as important health problems in this population. Anemia in children six through 60 months of age is defined as hemoglobin concentrations below 11 g/dL (World Health Organization 2001) and 69 percent of the sample met this standard (Table 1, Panel A).[14] 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 Delhi households from the 1998–99 Indian National Family Health Survey or NFHS (International Institute for Population Sciences and ORC Macro 2002).

Hemoglobin status varies significantly with child and preschool characteristics. In a probit specification, boy children are considerably less likely to be moderately to severely anemic than girl children (the magnitude is 15 percentage points, statistically significant at 95 percent confidence—regression not shown) conditional on child age and average preschool socioeconomic characteristics. Similarly, children in preschools with high average parent education are significantly less likely to be anemic (a drop of four percentage points per additional year of average father schooling, significant at 95 percent confidence), and children in predominantly Muslim schools are more likely (a change from zero to 100 percent Muslim population is associated with an increase of eight percentage points in moderate to severe anemia although the effect is not significant at traditional confidence levels). As shown below, program treatment effects on weight and school participation are more pronounced for these subgroups with higher baseline anemia prevalence, a finding consistent with existing studies in which iron supplementation only has impacts on the Hb levels of anemic individuals (see Thomas et al. 2003 and references therein).[15]

14. 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 have the Hb test, then the true prevalence would be higher than 69 percent.

15. Soemantri's (1989) study of iron supplementation for eight through 11 year-old Indonesian school children finds increases in Hb levels among, but not nonanemic, children after three months of treatment. Unfortunately, their small sample and the fact that standard errors are not reported complicate inference. See Nokes, van den Bosch, and Bundy (1998) for additional references.

Table 1
Baseline Characteristics

	Group I	Groups II, III	Group I—Groups II, III	Standard error
Panel A: Nutrition Characteristics				
Mean Hb 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	—	—	
Weight-for-height *z*-score (N = 1,412)	–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 = 2,371)				
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)	6.1	5.9	0.1	(0.2)
Number of children	2.4	2.2	0.3	(0.2)
Uttar Pradesh (N = 520)	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)
Hindu (N = 509)	0.75	0.82	–0.07	(0.06)
Muslim	0.25	0.17	0.08	(0.06)
Panel D: Mother characteristics				
Educational level (years) (N = 510)	3.3	3.4	0.0	(0.5)
Housework / homemaker (N = 478)	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)
Panel E: Father characteristics				
Educational level (years) (N = 497)	5.8	6.6	–0.8	(0.6)

Table 1 (*continued*)

	Group I	Groups II, III	Group I—Groups II, III	Standard error
Laborer (N = 475)	0.35	0.30	0.05	(0.06)
Salaried employment	0.27	0.34	–0.07	(0.05)
Self-employment (for example, business)	0.14	0.16	–0.02	(0.04)
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: Robust standard errors (clustered) in parentheses; statistically significant at the (*) 90, (**) 95, and (***) 99 confidence levels. Anthropometric and school-participation rate estimates are weighted to adjust for the probability of attrition by preschool cluster. Panel F presents parent reports.

Thirty percent of the baseline sample has a helminth infection (Table 1, Panel A), mainly roundworm (21 percent prevalence) although some children also were infected with hookworm, *Hymenolipis nana* or 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).

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). Treatment children were somewhat worse off preintervention 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, the randomization was not entirely successful. However, there are no significant differences across treatment groups in the age or gender of children, and most importantly, no difference in preintervention school attendance, the key outcome variable in the empirical analysis (Table 1, Panel B).

Household size is relatively large at 6.0 members (Panel C). The population is mainly composed of individuals whose "original" home lies outside of Delhi (76 percent), many of whom are recent migrants (although some who were born in Delhi continue to identify with their ancestral region), and of which the majority are from Uttar Pradesh (54 percent, Panel C). Only 20 percent of the population has lived in their current home for more than 15 years (not reported in the table), further evidence of the largely transient nature of the population. While the average educational attainment of mothers is nearly identical across treatment groups—3.3 versus 3.4 years (Table 1, Panel D)—average education for fathers is somewhat lower in Group I (Panel E). There are no statistically significant differences in occupational characteristics for mothers or fathers across the treatment groups (Panels D and E).

Few households reported that their child had received iron, deworming, and Vitamin A prior to the NGO program in the 2001 household survey: Only 1 percent had received iron supplementation in the previous year, 6 percent deworming, and less than 1 percent Vitamin A (Table 1, Panel F). These rates are considerably lower than 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 other parts of Delhi.[16]

D. Program Takeup

Teachers were instructed to give each child in the treatment preschools 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 student absenteeism (recall that children received iron at school). Note that iron takeup was only recorded for the 30 preschool days following each health camp, so it is likely children received some additional days of iron between that point and the following health camp, and as a result, these reports are lower bounds on actual iron compliance.[17] Similarly, only 65 percent of eligible children received deworming drugs during the first two health camps, largely due to absenteeism at the health camps, though deworming takeup is nearly universal for children who attend the camps.

IV. Econometric Identification

A. Estimation Strategy

The random assignment of preschool clusters to treatment and control groups allows us to interpret mean differences in outcomes as causal effects of the health program. The panel dimension of the data for the main nutritional and education outcomes allows us to control for any initial differences across groups in a difference-in-differences model:

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

16. However, these figures could understate treatment if parents do not always know the purpose of treatment or if they have forgotten about treatment.

17. Due to these data limitations, we are unable to determine how variation in iron takeup affected subsequent Hb, anthropometric outcomes, and school participation. However, note that Thomas et al. (2003) and Ekström et al. (2002) show that even imperfect compliance can result in complete iron repletion in adults.

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 analysis, this is an indicator that takes on a value of one for Group I preschools after the intervention started in December 2001); G_c is an indicator variable for the treatment group (Group I), to control for any mean baseline differences across groups; X_{ict} is a set of baseline child and preschool characteristics, which control for remaining pretreatment differences and improve statistical precision; P_t is an indicator variable for the period after the intervention started, and ε_{ict} is the disturbance term; these are allowed to be correlated at the preschool cluster level. Here θ is the average program effect. We also use Equation 1 to estimate heterogeneous program effects by restricting the sample to particular subgroups. Baseline students are assigned the treatment group of their original preschool cluster throughout the empirical analysis in an intention to treat (ITT) design, even if they later transfer preschools. To further deal with possible omitted variable concerns due to baseline differences across the treatment groups, in certain specifications we include additional baseline child and preschool characteristics interacted with the time control to capture divergent trends over time.

Because there is no baseline Hb information for Group II or III children, we estimate the cross-sectional difference between treatment and control pupils (in October 2002) conditional on baseline individual and preschool characteristics (X_{ict}). Second, if anemia is the key channel through which the program affects school participation, then treatment effects should be concentrated among those children who were anemic at baseline. Thus, we also examine whether the subgroups with higher anemia prevalence experience larger schooling treatment effects. Although not entirely conclusive—other hypotheses, such as differential parental responses as a function of child nutritional status, remain possible—the finding that subgroups with greater predicted anemia show the largest weight and school-participation gains is consistent with the hypothesis that reducing anemia is the key underlying mechanism driving the results.

In any experimental study, Hawthorne (placebo) effects are relevant and could potentially threaten the interpretation of program impacts as the causal effect of reducing anemia on school participation. Unfortunately, a double-blinded trial was not possible in this context. However, placebo effects appear unlikely for at least two reasons. For one, as shown below, treatment effects are concentrated among the subgroups of children most likely to be anemic, rather than across the board as would be the case with a general placebo effect. Second, placebo effects are not pervasive in development pilot projects: Many other recent health and education interventions have had no measurable impact on schooling outcomes (Kremer 2003).

B. 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 preschools (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.

Attrition due to school closure is not a major problem in 2001–2002 since most closed down prior to the announcement of the randomization into project treatment

Table 2
Attrition Rates in Attendance Checks, Health Camps and Hb Tests

	Group I	Groups I, III	Group I—Groups II, III	Standard error
Panel A: Attendance checks (N = 2,392)				
Proportion of time out-of-sample:				
November 2001–April 2002 (overall)	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)
Panel B: Health camps (N = 2,392)				
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)
Panel C: Hemoglobin (Hb) Tests				
October 2002 (Year 2) Hb test[a]	0.46	0.54	–0.08	(0.06)

Notes: 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.

groups, and we drop these closed schools from the subsequent analysis. To illustrate, from the original preschool 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 groups occurred and was announced. We restrict the analysis to the 2,392 children who had not been dropped from the sample by then, and call them the "baseline sample."

There was some additional 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 and returned later in the school year. Attrition rates for the baseline sample in the unannounced attendance checks are relatively high for each treatment group, in terms of the proportion of time that the child was out of the sample, at approximately 25 percent of all attendance checks (Table 2, Panel A).[18] However, fortunately for estimation, attrition rates are similar across 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 this difference is 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 the treatment and control groups (Panels B and C).

18. A subset of the attriters enrolled in primary school during the 2001–2002 academic year, although our best estimate is that this was the case for fewer than 9 percent of the sample.

Table 3
Attrition Rates in Attendance Checks, by Reason and Period

Dependent variables: Reason for attrition indicator	Group I	Groups II, III	Group I—Groups II, III	Standard error
Panel A: Out-of-town/moved-out				
November–December 2001	0.080	0.099	–0.019	(0.019)
January–February 2002	0.141	0.137	0.004	(0.022)
March–April 2002	0.209	0.198	0.010	(0.025)
Panel B: Temporarily out-of-town				
November–December 2001	0.062	0.065	–0.003	(0.017)
January–February 2002	0.116	0.086	0.030	(0.018)
March–April 2002	0.159	0.117	0.042*	(0.022)
Panel C: Moved within city/borough				
November–December 2001	0.001	0.003	–0.003*	(0.001)
January–February 2002	0.002	0.002	0.000	(0.002)
March–April 2002	0.005	0.003	0.002	(0.004)
Panel D: Moved outside of city/borough				
November–December 2001	0.018	0.031	–0.013*	(0.008)
January–February 2002	0.024	0.050	–0.026**	(0.012)
March–April 2002	0.045	0.079	–0.034**	(0.016)
Panel E: Preschool closed				
November–December 2001	0.003	0.000	0.003	(0.003)
January–February 2002	0.009	0.013	–0.003	(0.008)
March–April 2002	0.062	0.051	0.011	(0.031)
Panel F: Teacher absent				
November–December 2001	0.001	0.002	–0.001	(0.001)
January–February 2002	0.003	0.003	0.000	(0.002)
March–April 2002	0.005	0.003	0.002	(0.003)

Notes: Group differences are estimated from OLS regressions of attrition indicators on Group I indicator. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within cluster, but not across clusters; statistically significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence level. Subgroup attrition rates do not add up to total attrition rates because of nonresponse in reason for being out-of-sample.

We next examine causes of attrition as informed by the preschool teacher. The main cause is children being temporarily out of town (Table 3, Panel B): By March–April 2002 13.3 percent of children are reported to be out of town. Many families apparently return to their ancestral villages during religious holidays or as seasonal agricultural workers. A smaller proportion of children are reported to have migrated permanently from the neighborhood, either within the borough (0.4 percent) or outside of it (6.5 percent). Teachers in Group I preschools are significantly more likely to report that children are out of town temporarily and less likely to report that they have moved permanently compared to Group II and III preschools (Panels B, C, and D). Although it is possible that mobility patterns did change as a result of the intervention, it seems more plausible to us that observed differences are due to recall errors, or perhaps desirability bias, in teacher reporting. Other reported reasons for sample attrition during December 2001–April 2002 include the closing of preschools (5.6 percent of the children by March–April 2002, Panel E), and teacher absences (0.4 percent during March–April 2002, Panel F), and these are balanced across groups.

Children who leave the sample ("attriters") are broadly similar to those who stayed along observable characteristics (Table 4). We present estimates from OLS regressions that use the attrition indicators (the overall attrition indicator, and attrition by reason) as dependent variables, and include a series of baseline child and household characteristics, the Group I indicator, and the interactions between these characteristics and the Group I indicator. Neither overall attrition nor attrition for particular reasons is strongly correlated with individual and household observable characteristics. For overall attrition, coefficient estimates on 24 of the 27 observable characteristics are not statistically significant at 95 percent confidence (Column 1), and similarly for 22 of 27 observables for attrition due to residential mobility (Column 2).

Our main focus here is the interaction term, namely, the difference across treatment groups in attrition as a function of child characteristics. It is important that there is no evidence for differential attrition: There is no statistically significant difference (at 95 percent confidence) across treatment groups for 24 of 25 observable characteristics, and the hypothesis that these 25 interaction terms jointly have no effect cannot be rejected (F-test p-value = 0.153, Table 4, Column 1). Differential attrition from residential mobility is statistically significant (Column 2, F-test p-value = 0.045), but differential attrition from closings of preschools is not (Column 3, F-test p-value = 0.907). Overall, these tests support the claim that attrition is symmetric across treatment groups, at least in terms of observables, alleviating some attrition bias concerns.

Various methods are used in an attempt to further address nonrandom sample attrition. First, individual, household, and preschool characteristics that could be determinants of attrition are included as explanatory variables (Alderman et al. 2001). We also weight each observation by the inverse of the probability of staying in the sample (by cluster) in some specifications, and carry out weighted least squares estimation in order to maintain sample balance along observable dimensions.[19] However, this method of course does not eliminate attrition bias if unobserved characteristics are correlated with both sample attrition and child outcomes.

We thus also employ the nonparametric method in Lee (2005) to place extreme bounds on program effects in the presence of differential attrition across the treatment and control groups. As shown below, given the similar attrition patterns across groups, this method yields reasonably tight bounds. The Lee (2005) approach relies on a monotonicity assumption in attrition: the model assumes that every treatment group child who

19. We do not adjust standard errors for the fact that weights are estimated. Constructing standard errors that take into account the intra-preschool cluster correlation and the estimated nature of the attrition weights is a very complicated task; thus we report robust standard errors which only take into account of intracluster correlations.

Table 4
Relationship between attrition and characteristics of children at baseline

	Dependent variables							
	Out-of-Sample		Out-of-town / Moved		Preschool closed		Teacher absent	
	Main effect	Interaction with Group I indicator	Main effect	Interaction with Group I indicator	Main effect	Interaction with Group I indicator	Main effect	Interaction with Group I indicator
	(1) OLS (Same Regression)		(2) OLS (Same Regression)		(3) OLS (Same Regression)		(4) OLS (Same Regression)	
Group I	–0.051		0.039		–0.007		0.003	
	(0.162)		(0.153)		(0.045)		(0.004)	
Child's age	0.041*	0.004	0.051**	–0.008	–0.001	0.002	0.000	0.000
	(0.024)	(0.036)	(0.022)	(0.033)	(0.004)	(0.008)	(0.001)	(0.001)
Boy	0.008	–0.034	–0.022	–0.022	0.000	0.002	0.003	–0.003
	(0.027)	(0.045)	(0.025)	(0.044)	(0.011)	(0.014)	(0.003)	(0.003)
Mother no schooling	0.028	0.012	0.046*	–0.037	–0.015	0.036**	–0.008	0.008
	(0.038)	(0.058)	(0.026)	(0.049)	(0.011)	(0.017)	(0.007)	(0.007)
Mother high school	–0.066	0.124	–0.010	0.071	–0.004	–0.045	–0.006	0.006
	(0.072)	(0.112)	(0.062)	(0.093)	(0.012)	(0.039)	(0.005)	(0.005)
Father no schooling	–0.012	0.096	–0.020	0.078	–0.009	0.028	0.008	–0.008
	(0.040)	(0.062)	(0.031)	(0.051)	(0.009)	(0.022)	(0.008)	(0.008)
Father high school	0.074	–0.082	0.047	–0.090	0.001	0.033	0.002	–0.002
	(0.048)	(0.084)	(0.039)	(0.070)	(0.016)	(0.038)	(0.002)	(0.002)

Mother salaried employment	0.031	–0.065	0.113	–0.173	–0.016	0.073	–0.007	0.007
	(0.106)	(0.136)	(0.089)	(0.105)	(0.020)	(0.059)	(0.007)	(0.007)
Father government employment	0.024	–0.105	0.015	–0.049	0.030	–0.036	–0.002	0.002
	(0.061)	(0.081)	(0.054)	(0.078)	(0.019)	(0.024)	(0.002)	(0.002)
Muslim	–0.026	0.048	0.005	0.029	–0.001	–0.004	–0.004	0.004
	(0.044)	(0.064)	(0.032)	(0.055)	(0.010)	(0.026)	(0.003)	(0.003)
Other religion	0.247**	—	0.284***	—	–0.027	—	–0.005	—
	(0.103)		(0.083)		(0.041)		(0.008)	
Family size	0.015	–0.025	0.009	–0.019	0.004	0.000	0.000	0.000
	(0.012)	(0.018)	(0.010)	(0.017)	(0.002)	(0.004)	(0.001)	(0.001)
Number of adults	–0.041**	0.067**	–0.028**	0.061**	–0.006	–0.001	0.001	–0.001
	(0.016)	(0.027)	(0.014)	(0.025)	(0.004)	(0.007)	(0.001)	(0.001)
Time in residence	0.000	–0.003	0.001	–0.003	0.000	–0.001	0.000	0.000
	(0.001)	(0.003)	(0.001)	(0.003)	(0.000)	(0.001)	(0.000)	(0.000)
F-test: Interaction terms = 0	1.33		1.61		0.63		0.15	
[*P*-value]	[0.153]		[0.045]		[0.907]		[1.00]	
Mean of dependent variable	0.205		0.119		0.016		0.001	
Observations	2,370		2,370		2,370		2,370	
R^2	0.04		0.07		0.06		0.05	

Sources: Attendance Checks, November 2001–April 2002; 2001 Household Survey.

Notes: Coefficients from OLS regressions are reported. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within clusters, but not across clusters; significantly different from zero at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. *F*-statistic (25,131) of joint significance of interaction terms and *p*-value of test (in brackets) are reported. Regressors also include region of origin indicator variables for Uttar Pradesh, Bihar, Punjab, Rajasthan, Madhya Pradesh, Bengal, and other states, an indicator variable for owning the residence, and indicators for mother and father occupational groups (self-employment, laborer, and other employment indicators for mother; salaried employment, self-employment, and other employment indicators for fathers), and interaction of these with Group I indicator. We also take these into account in our *F*-tests of joint significance of all interaction terms.

reported an outcome would have reported an outcome if she had been assigned to the control group (or vice versa). This assumption is restrictive enough to generate a testable restriction. Essentially, the monotonicity restriction is inconsistent with nonresponse being positively (negatively) correlated with Group I children's characteristics and negatively (positively) correlated with Group II and III children's characteristics.[20] As shown in Table 4, the associations of observables with attrition are neither significant nor of opposite signs across the treatment groups, and thus this evidence is consistent with the assumption. We believe the robustness of our results to these varied methods of dealing with attrition provides confidence that they are not mainly driven by selective attrition bias.

V. Program Impacts

A. Overall Health and Nutrition Impacts

The program led to 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 5, Panel A, 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 (Regression 4) or height-for-age (Regression 5), but this pattern is not surprising: 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. The Lee (2005) 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 estimate the treatment effects using a specification with individual fixed effects and find largely similar results, with an estimated average weight-for-height *Z*-score gain of 0.36 (standard error 0.12, regression not shown).[21]

Recall that treatment group children have somewhat lower baseline weight than control group children. To assess the extent to which regression adjustment may be obscuring possible mean reversion, we also report basic difference-in-difference program impacts that are not regression adjusted (Table 5, Panel B).[22] Estimated impacts on weight and BMI are robust to the exclusion of the baseline covariates (Regressions 1–4), while height gains are again near zero. In addition, the simple posttreatment difference between treatment and control children is statistically significant for the

20. Formally, the monotonicity assumption is inconsistent with the existence of children i and j such that $\Pr[A = 0 \mid G = 1, X = x_i] < \Pr[A = 0 \mid G = 0, X = x_i]$ and $\Pr[A = 0 \mid G = 1, X = x_j] > \Pr[A = 0 \mid G = 0, X = x_j]$, where A is the attrition indicator, G is the Group I indicator, and X are the child's observable characteristics (see Lee 2005).

21. Unlike Miguel and Kremer (2004), cross-school treatment externalities are small and not statistically significant for both child nutritional status and school participation—regressions not shown. This is not surprising since the prevalence and intensity of worm infections in our setting is only moderate and children treated for worms in NGO preschools are a small fraction of infected individuals in these dense urban areas.

22. We thank a referee for this suggestion.

Table 5
Program Impacts on Anthropometrics and Hemoglobin Levels

	Dependent variables						
	Weight-for-height z-score	Weight-for-age z-score	Weight	BMI	Height	Height-for-age z-score	Hemoglobin (g/dL, 2002)
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) OLS
Panel A: Controls and attrition correction							
Treatment indicator	0.52***	0.31*	0.50	0.70**	–0.75	–0.19	0.08
	(0.18)	(0.17)	(0.30)	(0.27)	(0.84)	(0.21)	(0.22)
Group I	–0.18	–0.29**	–0.46**	–0.14	–1.03	–0.28*	—
	(0.12)	(0.12)	(0.21)	(0.18)	(0.66)	(0.16)	
Time control	Yes	Yes	Yes	Yes	Yes	Yes	No
Observations	2,383	2,383	2,383	2,383	2,383	2,383	688
R^2	0.11	0.09	0.13	0.14	0.23	0.16	0.16
Panel B: No controls/no attrition correction							
Treatment indicator	0.47**	0.37**	0.63**	0.61**	0.07	0.00	0.22
	(0.19)	(0.17)	(0.29)	(0.29)	(0.85)	(0.22)	(0.27)
Group I	–0.09	–0.28**	–0.34	–0.01	–1.05	–0.39**	—
	(0.13)	(0.11)	(0.20)	(0.21)	(0.79)	(0.18)	
Time control	Yes	Yes	Yes	Yes	Yes	Yes	No

Table 5 (*continued*)

	Dependent variables						
	Weight-for-height z-score	Weight-for-age z-score	Weight	BMI	Height	Height-for-age z-score	Hemoglobin (g/dL, 2002)
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) OLS
F-Test: Treatment + Group I = 0	5.50	0.36	1.17	5.35	1.09	3.12	—
[*P*-value]	[0.02]	[0.55]	[0.28]	[0.02]	[0.30]	[0.08]	
Mean of dependent variable	−1.05	−1.18	13.4	14.2	96.9	−0.46	10.5
Observations	2,383	2,383	2,383	2,383	2,383	2,383	688
R^2	0.01	0.01	0.01	0.01	0.01	0.01	0.004

Notes: Robust standard errors in parentheses; disturbance terms are allowed to be correlated within clusters; * = significant at 90 percent; ** = significant at 95 percent; *** = significant at 99 percent confidence level. Panel A reports coefficient estimates from OLS regressions adjusting for attrition by preschool cluster and includes baseline controls. Controls include child age and child gender indicators, neighborhood indicators; teacher age, teacher years of schooling, and preschool language; proportion Muslim in the preschool cluster, mean mothers' and fathers' education in the preschool cluster. The time control is an indicator for the posttreatment period. Panel B reports coefficients from OLS regressions with no attrition adjustment weighting and no controls. *F*-statistics of sum of treatment indicator and Group I indicator coefficients represent tests of simple posttreatment differences in outcomes.

weight-for-height and BMI variables, although not for weight and weight-for-age. (This is captured in the *F*-test of the hypothesis that the sum of the Treatment indicator and Group I coefficient estimates equal zero.)

Estimated Hb impacts are weaker than the anthropometric findings: treatment (Group I) pupils show somewhat higher hemoglobin than control (Group II) pupils in the September 2002 survey—an average gain of 0.08-0.22 g/dL—but the difference is not statistically significant (Table 5, Column 7). Unlike for the other nutrition measures, there is no pretreatment Hb data for control children, and as a result any baseline differences across the treatment groups are of particular concern. This may make the estimated program impacts on Hb less reliable than other results. We explored other specifications—including indicator variables for anemia thresholds as dependent variables, and quantile regression—but in no case is there a statistically significant improvement in Hb (regressions not shown). One possible explanation for these weak results, at least in part, 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 the 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.[23]

Since iron status is multifaceted, it is also possible that there were in fact iron gains but in the form of serum iron or ferritin (stored iron)—which we do not have information on—with only weak short-run improvements in Hb concentration, as found previously by Palupi et al. (1997), Stoltzfus et al. (2001), and Allen et al. (2000). Allen et al. (2000) also suggest that limited Hb improvement among preschool children following iron supplementation could be the result of other concurrent micronutrient deficiencies, such as vitamin B-12 deficiency.

There is a small but not statistically significant reduction in helminth infections rates by the September 2002 follow-up survey (point estimate –0.018, standard error 0.039, regression not shown). Time lags may also in part account for this result since deworming had taken place in March 2002 and rapid reinfection with some helminths is likely.

B. Overall School-Participation Impacts

The program led to a large improvement in preschool participation during Year 1, an average gain of 5.8 percentage points (Table 6, Regression 1). Given average school-participation rates of approximately 70 percent, this constitutes a reduction in preschool absenteeism of nearly one-fifth. The Lee (2005) extreme bounds range from 1.9 to 7.2 percentage points in this specification (not reported in the table). The posttreatment simple difference for Group I versus Groups II and III (captured in the hypothesis that the sum of the Treatment indicator and Group I coefficient estimates equal zero) yields an *F*-test *p*-value of 0.082, statistically significant at over 90 percent confidence.

23. Kashyap and Gopaldas' (1987) study of eight through 15 year-old Indian school girls finds short-run hemoglobin gains but no difference in Hb levels for treated children four months after the withdrawal of iron supplementation.

Table 6
Program Impacts on School Participation

Specification (dependent variable is the indicator for preschool participation):	Coefficient estimate on Treatment Indicator	Standard error
1) Weighted regression, probit	0.058*	(0.034)
2) Unweighted regression, probit	0.059*	(0.036)
3) Unweighted regression, no controls, probit	0.049	(0.037)
4) Additional baseline covariates and covariates interacted with time control, probit	0.075**	(0.033)
5) Child fixed effects and additional baseline covariates interacted with time control, linear regression	0.059	(0.041)
6) No time control, probit	0.056**	(0.029)

Notes: Each coefficient estimate is from a separate regression. Marginal probit estimates (evaluated at mean covariate values) and coefficient estimates from OLS regressions are presented. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within clusters; * = significant at 90 percent; ** = significant at 95 percent; *** = significant at 99 percent confidence levels. All specifications presented in Table 6 contain an indicator variable for Group I schools (coefficient estimate not shown). All specifications in Table 6 except for Row 6 also include the time control, an indicator for the posttreatment indicator. Row 1 controls include child age and child gender indicators, neighborhood indicators; teacher age, teacher years of schooling, and preschool language; proportion Muslim in the preschool cluster, mean mothers' and fathers' education in the preschool cluster. Row 1 also includes the time control, an indicator for the post-treatment period. Row 2 drops the attrition weights (by preschool cluster). Row 3 drops the attrition weights (by preschool cluster) and all baseline controls. Row 4 is like Row 1 but also includes four additional controls—child weight-for-age, child height-for-age, Uttar Pradesh state origin, and Delhi origin—as well as their interactions with the time control. Row 5 includes child fixed effects in addition to all of the baseline covariates interacted with the time control, in a linear specification. Row 6 is like Row 1 but drops the time control. The sample size in all specifications is 9,275 school participation observations.

We next explore the robustness of the main school-participation result to alternative specifications. First, we find that the result is robust to a specification without the inverse attrition probability weights (point estimate 0.059, standard error 0.036, statistically significant at 90 percent confidence—Table 6, Row 2). Excluding the baseline controls weakens the main school-participation impact slightly (point estimate 0.049, Row 3). We next include the preschool average of all four baseline characteristics found to be significantly different across treatment groups in Table 1—child weight-for-age, child height-for-age, Uttar Pradesh state origin, and Delhi origin—as well as their interactions with the time control and find that the program impact becomes larger (estimate 0.075, standard error 0.033, statistically significant at 95 percent confidence, Row 4). This finding at least partially alleviates omitted variable bias concerns related to any baseline differences across treatment groups. Finally, the statistical significance of the result is weakened somewhat by the inclusion of child fixed effects together with the baseline covariates interacted with the time control though the point estimate remains similar (0.059, standard error 0.041—Row 5), but the result is robust to dropping the time control (0.056, standard error 0.029, significant at 95 percent confidence—Row 6).

It is theoretically possible that the school-participation gains are caused by children attending school to get daily doses of iron rather than due to the effect of child health and nutrition gains per se. However, this is unlikely for several reasons. First, 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. The subgroup analysis below provides further evidence that the underlying causal mechanism is treating anemia.

C. Program Impacts, by Subgroup

Weight gains and school-participation improvements are largest for subgroups with higher rates of baseline moderate to severe anemia (Hb < 10g/dL). For instance, girls have much higher baseline anemia than boys (Table 7, Column 1, Rows 2–3) and there are somewhat larger weight gains for girls than boys and also larger school-participation gains. Similarly, baseline anemia rates, program weight gains and school-participation effects are all somewhat larger in preschools with lower levels of mother education (Rows 4–5, and there are similar patterns for father education—results not shown), suggesting that children living in relatively low socioeconomic status communities gained more. A similar pattern holds for older children aged four through six years versus two through three year-olds in terms of anemia and school-participation gains, though not for weight gains (Rows 6–7).

Child and school baseline characteristics—child age, gender, and average school religious composition and parent education—were then used to compute an overall predicted likelihood of moderate to severe anemia (regression not shown). As expected, baseline anemia prevalence was much higher among the high predicted anemia group (59.8 percent, Table 7 Column 1, Row 8) than the low anemia group (38.9 percent, Row 9), a 21 percentage point gap. Weight gains are much greater among those with high likelihood of anemia than those with low likelihood (0.62 versus 0.32, Column 2, Rows 8–9), and similarly for school-participation gains (0.114 gain versus –0.011, Column 3, Rows 8–9). This difference in school-participation treatment effects by predicted anemia subgroup is statistically significant at over 95 percent confidence (in a model with the full set of controls and an interaction term between the treatment indicator and high predicted anemia indicator, regression not shown). This effect is robust to including an additional interaction term between the treatment indicator and low baseline anthropometric outcomes in the preschool (regression not shown).

Figure 2 presents the time pattern of preschool-participation rates for Group I versus Groups II and III students through time, comparing those with above median predicted baseline moderate to severe anemia prevalence to those with below median prevalence. School-participation rates are slightly lower for the "Group I, above median predicted anemia" group at baseline (before the first health camp in December 2001) but increase sharply after treatment, and remain greater than school-participation rates in the control schools—and in the "Group I, below median predicted anemia" group—through the end of Year 1. Overall, these findings indicate that program treatment effects are largest among

Table 7

Program Impacts on Weight for Height Z-Scores Levels and School Participation, by Subgroup

	Proportion with moderate/ severe anemia at baseline (in Group I) (1)	Coefficient estimate on Treatment Indicator Dependent variables: Weight-for-height z-score OLS (2)	Coefficient estimate on Treatment Indicator Dependent variables: School-participation Probit (3)
1) Overall	0.516	0.51*** (0.18) [2,383]	0.058* (0.034) [9,275]
Gender			
2) Girls	0.602	0.67*** (0.23) [1,337]	0.077** (0.037) [5,101]
3) Boys	0.424	0.29* (0.17) [1,046]	0.035 (0.044) [4,174]
Mean preschool mothers' schooling			
4) < Median (< 3 years)	0.559	0.63** (0.30) [1,235]	0.096** (0.047) [4,892]
5) ≥ Median (≥ 3 years):	0.465	0.37* (0.19) [1,148]	0.006 (0.048) [4,383]
Age			
6) 4–6 years	0.549	0.46** (0.19) [1,253]	0.095** (0.038) [4,982]
7) 2–3 years	0.480	0.57*** (0.21) [1,130]	0.010 (0.044) [4,293]
Predicted probability moderate/ severe anemia at baseline			
8) ≥ Median (Pr[Hb < 10g/dL] ≥ 0.45)	0.598	0.62** (0.24) [1,186]	0.114** (0.045) [4,529]
9) < Median (Pr[Hb < 10g/dL] < 0.45)	0.389	0.32* (0.18) [1,197]	–0.011 (0.043) [4,746]

Notes: Each coefficient estimate is from a separate regression. OLS coefficients and 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 90 percent; ** = significant at 95 percent; *** = significant at 99 percent confidence. Sample sizes for each regression in brackets. Estimates from weighted regressions adjusting for attrition by preschool cluster. Controls include child age, child gender, and neighborhood indicators; teacher age, teacher years of schooling, and preschool language; proportion Muslim, mean mothers' and fathers' education in the preschool cluster; and a posttreatment period indicator. Stratification is based on baseline characteristics collected in the 2001 surveys. Predicted moderate to severe anemia at baseline is a function of child age, gender, the proportion of Muslims in the preschool, and average mother and father education in the preschool (not shown).

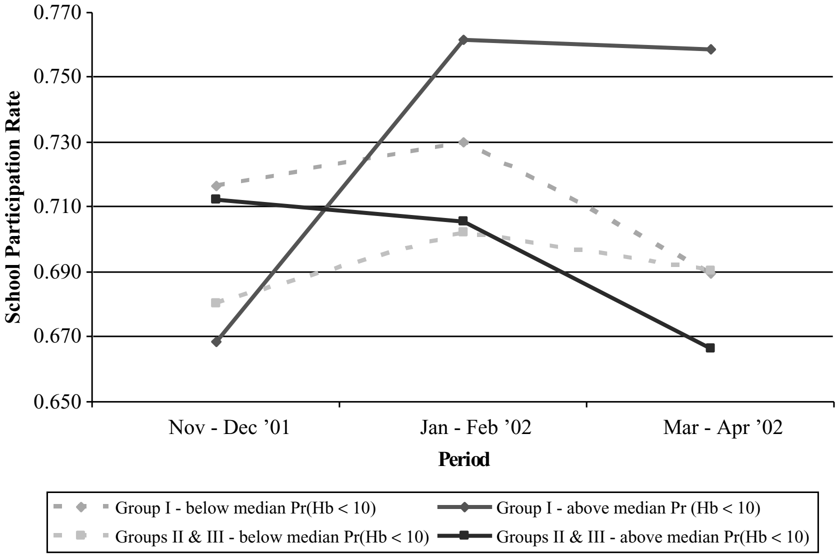

Figure 2
Preschool-participation Rates through Time

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

those subgroups of children with higher baseline moderate to severe anemia prevalence, consistent with reduced anemia being the key cause of preschool-participation gains.

The same child and school baseline characteristics were next used to compute a predicted likelihood of baseline worm infection (regression not shown), allowing us to divide the sample into those with above median predicted worm infection likelihood and below median likelihood. In sharp contrast with the anemia results above, estimated school-participation gains are nearly identical for the high and low predicted worm infection groups, at 0.069 and 0.044, respectively (not reported in the table). In contrast with Miguel and Kremer (2004), who show evidence of large school-participation gains as a result of reduced worm infection rates among Kenyan primary school children, this analysis suggests that changes in worm infections were not the main factor driving the preschool-participation impacts we estimate. Rather our results are consistent with the hypothesis that reductions in anemia were the key causal mechanism.

VI. Conclusion

A preschool health intervention in Delhi, India providing iron supplementation and deworming medication led to substantial gains in child weight and preschool participation, reducing absenteeism by one-fifth. The results contribute to a growing consensus that child nutritional deficiencies are key impediments to human capital accumulation among poor children in less developed countries, and that school-based health programs are likely to be cost-effective ways to promote schooling in these settings (for other recent evidence, see Glewwe, Jacoby, and King 2001 and Miguel and Kremer 2004). Yet despite progress in understanding the impacts of child nutrition and health on education, the long-term effects of child health on adult income and life chances remain poorly understood, and this question also 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 expanding this preschool health model into additional cities. The results of this paper suggest more broadly that other organizations aiming to boost school participation in child populations where anemia is common should consider iron supplementation and deworming.

However, randomized evaluations are clearly not a panacea. We encountered two important methodological issues in the implementation of Year 2 (2002–2003) of this evaluation, namely, high levels of sample attrition (which prevented us from credibly estimating program impacts among the baseline sample), and contamination of the experiment due to nonrandom sorting into treatment groups in Year 2. These concerns must be taken into account in the design of randomized evaluations, just as they are in nonexperimental studies.

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