**Early Childhood Nutrition is Positively Associated with Adolescent Educational Outcomes: Evidence from the Andhra Pradesh Child and Parents Study (APCAPS)**[[1]](#footnote-2)

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**Abbreviations:** APCAPS, Andhra Pradesh Children and Parents Study; CI, Confidence interval; COHORT, Consortium of Health-Orientated Research in Transitioning Societies; INCAP, Institute of Nutrition for Central America and Panama; ICDS, Integrated Child Development Services; LMIC, Low- and middle-income countries; PSM, Propensity score matching

**ABSTRACT**

**Background:** India’s Integrated Child Development Scheme (ICDS), which provides supplementary nutrition and other public health services to more than 91 million women and children under six years of age, is the largest program of its kind in the world.

**Objective:** We estimated the long-term associations of maternal and early childhood nutrition provided under ICDS with educational outcomes when the children became adolescents.

**Methods:** We used longitudinal data from a controlled nutrition trial conducted near the city of Hyderabad. From 1987 to 1990, a balanced protein-energy supplement (corn-soya meal, called ‘upma’) was offered to pregnant women and children under six years of age in 15 intervention villages, while no supplementation was offered in 14 control villages. Both groups had equal access to other public programs such as immunization and anemia control in pregnancy. Children born during the original trial period were re-surveyed (654 intervention and 511 control group children) in 2003–2005. We used propensity score matching methods to correct for estimation bias in our regression models to assess the associations of supplementary nutrition with school enrollment, schooling grades completed, and academic test performance of these adolescents.

**Results:** Children born in intervention villages were 7.8% (95% Confidence Interval [CI]: 0**.**1%, 15**.**4%; *p*<0**.**05) more likely to be enrolled in school and completed 0**.**84 (95% CI: 0**.**28, 1**.**39; *p*<0**.**005) more schooling grades than children born in control villages. We found no association between supplementary nutrition and academic performance, as measured by school test scores.

**Conclusions:** Offering a nutritional supplement to pregnant women and children under six years of age during the Hyderabad Nutrition Trial was associated with improved school enrollment and schooling grades when the children became adolescents.

**Keywords:** India; ICDS; Hyderabad nutrition trial; APCAPS; child development; early childhood nutrition

**INTRODUCTION**

An estimated 200 million children under five years of age—most of whom live in South Asia and sub-Saharan Africa—fail to achieve their potential primarily because of undernutrition (1–3). Undernutrition during the first 1,000 days of life is associated with lower survival, higher incidence of acute and chronic diseases in later life (3,4), reduced cognitive skills, fewer schooling grades (5–7), lower economic productivity (8), and lower birth weight and shorter children in the next generation (9). Consequently, undernutrition among pregnant women and young children has drawn significant attention from researchers and policymakers (8,10,11).

Adequate nutrition in early life may help break the cycle of poor outcomes from childhood to adulthood. Under the “fetal origins” hypothesis, better nutrition *in utero* can significantly improve later-life health, educational and economic outcomes (4,12). However, data supporting the hypothesis are primarily from high-income countries and typically use birthweight to measure nutrition *in utero* rather than through interventions that improve maternal nutrition (4,5,13,14). Also, a number of these studies examine the effects of nutrition *in utero* or during early childhood only on future health outcomes (4).

Long-term educational or economic benefits of improving nutrition *in utero* or in the early years of life are poorly quantified among children in low- and middle-income countries (LMICs). A recent systematic review found that the relationship between early childhood nutrition and adult outcomes had been researched so far in just eight developing countries (14). Seven of these country studies used observational data and found positive associations primarily with health outcomes such as height and weight, disability, and prevalence of heart diseases. Another review study (5) analyzed the COHORT (Consortium of Health-Orientated Research in Transitioning Societies) (15) data from Brazil, Guatemala, India, Philippines and South Africa and found a knowledge gap especially on the association between maternal nutrition and children’s educational outcomes. Little is known about how maternal height (a general measure of mothers’ lifetime nutrition) or supplemental nutrition during pregnancy might affect child outcomes in LMICs (16).

Our understanding of the long-term consequences of early childhood nutrition comes from a few experimental studies with longitudinal follow-up. The Institute of Nutrition for Central America and Panama (INCAP) study that followed approximately 1,500 children in four villages of Guatemala between the years 1969 and 2004 found that protein-rich nutritional supplementation before 24 months of age was associated with 1.2 higher schooling grades completed for women, increased adult cognitive skills for both men and women by about a quarter of a standard deviation, and increased average adult male wages by 46% (17–19). A study in which 390 adolescent Nepalese children were randomized to maternal vitamin A supplementation and a placebo control found that the intervention was associated with an 11.3 percentage point reduction in schooling repetition rates but no association with test scores (20). Another study evaluated exposure to iodine supplementation *in utero* among 1,400 adolescent Tanzanian children. Using a quasi-experimental method involving variations in district level coverage rates of an iodine program, the authors found that children exposed to the supplementation completed 0.4-0.6 more schooling grades, with larger effect sizes for girls (21). In a cohort study involving 3,493 participants in Brazil, at least 12 months of breastfeeding in childhood was associated with a 3.76 point increase in intelligence quotient, 0.9 increase in grades completed, and 341 Brazilian Reals (approximately US$168) increase in income at age 30 years (22). Finally, Muslim Indonesian children exposed to *in utero* daylight fasting during Ramadan do more child labor, score 7.4% and 8.4% lower on cognitive and math tests, and as adults, work 4.7% fewer hours and are more likely to be self-employed (23).

**MATERIALS AND METHODS**

**Data: ICDS and the Hyderabad Nutrition Trial**

The ICDS program of India, launched in 1975 and universalized in 2008-2009, is the world’s largest maternal and child health program (24). It is designed to address nutritional and developmental needs of children below six years of age, pregnant women, and nursing mothers. ICDS has a network of more than 1.3 million mother-child health centers, known as *Anganwadi*, that provide supplemental nutrition, preschool education, immunization, health check-ups and referral services, and nutrition and health education (24–26). In 2011, the program provided nutrition to 91.9 million women and children, as well as preschool education to 35.5 million children (27).

The 1987-1990 Hyderabad Nutrition Trial, conducted by the National Institute of Nutrition, evaluated effects of food supplementation during pregnancy under ICDS on newborn birth weights. The study enrolled pregnant women and young children from 29 villages in administrative areas near the city of Hyderabad in the state of Andhra Pradesh (now part of the new state of Telangana). First, two adjacent sub-districts (group of villages, called a block) were selected for the study – one for the intervention group and the other as control. Then, a prominent village at the center of the block was randomly located on the map, and all villages within a 10km radius were selected for the study – resulting in 15 intervention and 14 control villages. Expectant mothers and children up to age six years in the intervention villages received daily food supplements under ICDS—a corn-soya meal (‘upma’)made using locally-available ingredients—as part of the study. The meal provided on average about 2**.**09 MJ (500 kcal) of energy and 20–25 grams of protein to pregnant or lactating women, and about 1**.**25 MJ (300 kcal) of energy and 8–10 grams of protein to children (28,29). The supplement was offered in intervention villages during 1987-1990, and the program was expanded to include the control villages three years after the end of the study. Intervention and control group villages had similar levels of access to other public health programs such as immunization and anemia control in pregnancy. A short abstract describing the study was published, but no further study documents or reports are available (28).

In 2003–2005, households with at least one living child born during the original trial period were located and invited to take part in a follow-up study (along with further follow-ups, it is now known as the Andhra Pradesh Child and Parents Study, or APCAPS) (30). Complete details of the trial and follow-up study were published in Kinra et al. (2008) (28) and are not repeated here. A brief summary is presented below.

The follow-up survey first identified women (from 1,815 families) who participated in the original trial. These women reported a total of 2,601 children born during 1987-1990, who were still alive in 2003. These children were then matched with baseline data using date of birth and gender information. The study team set up a health clinic in each village and invited children who were successfully matched with baseline for the follow-up survey at the clinic. A total of 1,165 such children – 654 in intervention villages and 511 in control villages – attended the clinics (see Figure 1). The survey collected various data – including height, adiposity, blood pressure, and insulin resistance – for the then 13-18 year-olds. Self-reported information on current school enrollment status, highest schooling grade completed, and performance at the last major test at school for each child was also collected (30). Furthermore, household and parental socioeconomic, demographic, and educational data were gathered. Summary statistics of the follow up study sample are presented in **Table 1**.

**Ethics**

Ethical approval for the study was obtained from the Ethics Committee of the National Institute of Nutrition of India, Hyderabad. Approval was also obtained from the local administrative authorities in all villages. Informed consent was obtained from the participants and their parents or guardians.

**Estimation Procedures**

We used the 2003-2005 follow-up data to estimate the differences in educational outcomes between children born in intervention and control areas of the trial. Three educational outcomes were examined: i) enrollment (whether children were currently enrolled in school), ii) schooling grade, 0–12, completed by children, and iii) academic performance (most recent test scores at school). Test scores were measured as: fail = 1, third class = 2, second class = 3, first class = 4, and distinction = 5. We used discrete choice econometric models for analyzing these educational outcomes: probit for current school enrollment and ordered probit models for schooling grade completed and academic performance. We included both currently- and previously-enrolled children in our analysis. Therefore, the schooling grade completed by a child captured a combined effect of age of school entry, schooling grade repetition, and possible drop-out. Data were analyzed using STATA version 12.

Our regression models controlled for children’s age, gender, birth order, and father’s and mother’s education (literate, completed primary, and completed secondary or above). Also included were household socioeconomic characteristics, including caste (scheduled caste or scheduled tribe, other backward class), religion (Hindu or other), and household assets (28,31,32). Standard errors were clustered at the village level using the *cluster( )* option in STATA regression commands.

Among children invited to participate in the 2003-2005 follow-up survey, 82% in the intervention villages and 74% in the control villages complied. The participants represented 49% and 41%, respectively, of all children born in the study villages during 1987-1990 who were alive in 2003 (see Figure 1). To mitigate effects of possible systematic differences between intervention and control children due to selective attrition or other reasons, we conducted additional analyses using the quasi-experimental method of propensity score matching (PSM) (33–37). PSM compares each individual affected by the intervention with a control individual(s) who was not affected by the intervention but had basically the same probability of being affected by the intervention on the basis of observed characteristics. This comparison attributes the differences in educational outcomes solely to the intervention if any unobservable factors that affect intervention are distributed randomly between intervention and control groups.

In the PSM analysis, we regressed intervention assignments on children’s background characteristics. Then, based on predicted probabilities (propensity scores) from the estimated model, we matched intervention-group children with similar control-group children. We the used one-to-one nearest-neighbor matching method (with replacement) within a probability radius of 0**.**01. We considered only observations from the two groups that are in “common support”—that is, with overlapping propensity scores. Standard errors of our analysis were adjusted using a bias correction method (38). Further methodological details of the PSM analysis are presented in online supporting material.

After matching, the differences in average educational outcomes between the intervention and matched control children yield the estimated nutritional supplement associations (termed as the “average treatment effect on the treated”). However, the estimated associations can vary depending upon the choice of matching method. We therefore examined the robustness of our findings by considering additional propensity score estimation techniques and matching algorithms. We tested several alternative strategies – a Kernel (Epanechnikov) method for matching intervention and control children instead of a nearest-neighbor match, matching each intervention child with five nearest-neighbors in the control group instead of a single neighbor, and using a logit model of propensity score estimation (nearest-neighbor matching with replacement within 0**.**01 radius) instead of a probit model. Finally, we also used a covariate matching technique that matched an intervention observation with its nearest control group counterpart on the basis of the joint distribution of all covariates, instead of reducing the dimension to a single propensity score (39). Results from our regression and PSM analyses are presented as percentage change in the mean current school enrollment rate, and changes in the mean values of schooling grade completed and test scores at school. Estimates are considered statistically significant at the 5% level.

It is also important to ensure that the covariates were balanced and systematic differences between intervention and control groups were reduced after PSM. We evaluated this in three different ways. First, we calculated the percentage bias (difference) in mean values of covariates between the intervention and unmatched control groups. We then examined whether the percentage bias declined significantly if the intervention group was instead compared with the matched control group. Second, we tested matching quality using the pseudo-*R2*statistic from re-estimation of propensity scores from the matched sample, following Sianesi (2004) (40). If matching quality is good, pseudo-*R2*should decrease substantially from the unmatched to the matched data. Third, a likelihood ratio test of joint significance of the covariates was conducted separately in the unmatched and matched data. A failure to reject the null hypothesis of joint nonsignificance in the matched data indicates good matching quality.

**RESULTS**

Probit results are reported in **Table 2**. The first row shows estimated associations of the intervention with educational outcomes. Controlling for child, parental, and household characteristics, the intervention was associated with a 6**.**6% (95% confidence interval [CI]: 0**.**8%, 12**.**4%; *p*<0**.**05) rise in current school enrollment. We did not find any significant association between the intervention and schooling grade completed or test scores.

Figure 2 presents differences among intervention, unmatched control, and matched control observations. We found that intervention-group children were on average 6**.**9% (95% CI: 2**.**1%, 11**.**6%; *p*<0**.**01) more likely to be currently enrolled in school (Figure 2, Panel A) and completed 0**.**5 (95% CI: 0**.**3, 0**.**8; *p*<0**.**001) additional schooling grades (Figure 2, Panel B) compared with children in the unmatched control data. There was no statistically significant difference in their test scores (Figure 2, Panel C).

The PSM estimates indicate that the nutritional supplement was associated with a greater likelihood of being currently enrolled in school by 7.8% (95% CI: 0**.**1%, 15**.**4%; *p*<0**.**05) and completing 0**.**8 (95% CI: 0**.**3, 1.4; *p*<0**.**005) more schooling grades. As with the probit regression models, the results from matching did not show any association between the intervention and test scores.

The findings are robust to alternative matching specifications. With a Kernel (Epanechnikov) matching method, the intervention was associated with a 7.9% (95% CI: 2.9%, 12.9%; *p<*0.005) rise in current school enrollment and 0.6 (95% CI: 0**.**3, 0.9; *p*<0**.**001) more schooling grades. Alternatively, after matching each intervention child with five nearest neighbors in the control group, the intervention was associated with a 9.3% (95% CI: 3**.**7%, 14**.**8%; *p*<0**.**005) current school enrollment rise and a 0**.**6 (95% CI: 0**.**3, 1**.**0; *p*<0**.**005) increase in schooling grades. With a logit model in our propensity score estimation, the intervention was associated with an 8.8% (95% CI: 1.6%–16.1%, *p*<0**.**05) increase in current school enrollment and 0**.**7 (95% CI: 0**.**2, 1**.**3; *p*<0**.**01) rise in schooling grades. Finally, using the covariate matching technique, the intervention was associated with a 7.2% (95% CI: 1.6%, 12**.**9%; *p*<0**.**05) rise in current school enrollment and 0.4 (95% CI: 0**.**1, 0.8; *p*<0**.**05) increase in schooling grade. No significant association with test scores was seen in any of the above estimates.

The basic propensity score estimation model is presented in **Table 3**, and matching quality tests are presented in **Table 4**. We found that percentage biases in covariates decreased from an average of 8% in the unmatched data to 3**.**7%–5% with PSM. The number of covariates that had statistically significant biases decreased greatly from the unmatched to matched data, while the pseudo-*R2*statistic went down by 50% or more after matching. Further, the likelihood ratio test of joint significance of the covariates was highly statistically significant (*p* = 0**.**003) in unmatched data but nonsignificant after matching. These tests confirmed high matching quality and the validity of our methodology.

In additional analyses, we examined whether nutritional associations were significantly different for girls as compared with boys. In the probit regression models of educational outcomes, we included an interaction term between the intervention status and gender of each child (whether female). The estimated coefficient of this interaction term was statistically nonsignificant for the current enrollment and test score regression models, indicating that there was no gender gap in the association between the intervention and these outcomes. However, we found that the intervention was associated with a 0.3 (95% CI: 0.1, 0.6; *p*<0.05) greater increase in schooling grade completed by girls as compared with boys. In our propensity score matching analysis, we computed the difference-in-difference average treatment effect on the treated for girls as compared with boys (more details are presented in the online supporting material). The results showed that the supplement was associated with a 16.1% (95% CI: 0.9%, 31.2%; *p*<0.05) greater rise in current school enrollment of girls. No differential association by gender was seen for the two other educational outcomes.

**DISCUSSION**

Over half—almost 60 million—of the world’s underweight children under five years old are Indian (41). The proportion of Indian children who are stunted or underweight declined from 48% and 43%, respectively, in 2005–2006 (31) to 39% and 30% in 2013–2014 (42). However, the prevalences vary considerably across states and districts. In the poorer states of Bihar, Jharkhand, Madhya Pradesh, Orissa, Rajasthan, and Uttar Pradesh, 58**.**8% and 43**.**3% of children were still stunted and underweight in 2011 (43).

ICDS has the potential to reduce India’s undernutrition burden, but little is known about its effectiveness. Existing studies of ICDS use different methodologies and data from various time periods and reveal positive associations with children’s nutritional status (28,41,44–48). However, most of these studies use observational data that may produce biased estimates of causal effects because of systematic differences between beneficiaries and non-beneficiaries. In addition, there is no evidence on associations of ICDS participation with adolescent educational outcomes.

In the broader context of all LMICs, evidence of associations of early childhood nutrition with later-life education is very limited. The most noteworthy studies are from INCAP in Guatemala, showing that early-life nutritional supplementation was associated with improved schooling grades, reading comprehension, nonverbal skills, wages, and birth weights and heights of the next generation (11,17,19,49). But it is very unclear that these results from Guatemala in the 1970s generalize to other contexts. Here, we estimated long-term associations of exposure to nutrition *in-utero* and during early life with educational outcomes of Indian adolescents. Using regression and propensity score matching methods, we found that the supplement was associated with consistently higher levels of school enrollment, and possibly also with higher schooling grades, though not test scores.

In recent years, India has seen net primary-school enrollment rates over 90% (50). However, 80 million, or 42% of primary-school children, drop-out before completing grade 8 (51). Among girls, the dropout rate by grade 10 is as high as 63.5% (52). As our results for 13- to 18-year-olds show, ICDS participation may potentially help reduce these high dropout rates. However, we did not find associations of the intervention with children’s academic performance. Test scores in our study were self-reported and might have suffered from reporting bias. Although unlikely, if the rates of misreporting were different between intervention and control groups, they could distort our findings. Test outcomes are also likely to depend on school quality (53,54). Sending children for longer time to inferior quality schools could result in negligible intervention associations with test scores (55). The lack of schooling characteristics data makes such analysis beyond the scope of our study.

There are some limitations of our analysis. First, attrition between the original trial and the 2003-2005 follow-up was non-trivial, albeit lower than that in other relevant studies (28). However, attrition rates in intervention and control groups were not widely different and therefore may not have affected our results. Furthermore, our findings are robust to several econometric model specifications.

Unobserved factors might affect our results in additional ways. After the Hyderabad Nutrition Trial ended, supplementary nutrition was also provided in control villages starting in 1992-1993 (28). Children born in these villages could have received the extra nutrition when they were 3-6 years old, and this may attenuate associations between the intervention and educational outcomes. However, nutritional interventions are thought to have the strongest effects before age 2-3 years (1,3).

Access to other public health programs, e.g., immunization and health education, was similar across intervention and control villages. By introducing the nutritional supplement, ICDS integrated the delivery of such services under a single program. The integration could have increased the uptake of these other components in intervention villages (28), possibly confounding the association between the nutritional supplement and education in our study. Due to lack of data, such effects cannot be separately identified. However, evidence from LMICs with high rates of undernutrition suggests that among early-childhood interventions in undernourished populations such as in India, nutrition may be the most important factor to affect future educational outcomes (5,14,17,19). Moreover, the other components of ICDS such as immunization and improved hygiene could reduce infections, and therefore increase the effectiveness of the nutritional supplement.

Finally, in the absence of reliable data on actual supplement intakes by participants, we could not conduct dose-response type analysis whereby associations between duration of exposure to the nutritional supplement and educational outcomes can be tested.

While we find long-term educational associations with ICDS program participation from a small trial, the population-level program effectiveness depends on its quality and coverage, which vary across children’s age and location (41,46). ICDS currently provides two types of nutritional supplements. Children aged 3–6 years are given cooked meals or ready-to-eat food for feeding at the *Anganwadi* centers. Younger children are often given uncooked take-home rations ranging from fortified baby mixtures to ready-to-eat foods (46).

The nutrition provided through cooked meals at *Anganwadi* centers may be truly supplementary if children or pregnant women continued to receive all or most of their usual food intake at home as found in Guatemala and the Philippines (56,57). In comparison, take-home rations may be more extensively reallocated among all family members, thereby increasing energy intakes of women or children only marginally or perhaps not at all.

In 2005–2006, the most recent years for which national data are available (31), coverage of *Anganwadi* centers ranged from 100% in Tripura to 27% in Meghalaya (national average of 72%). However, only 26% of under-6 children who had neighborhood *Anganwadi* centers reported receiving nutrition from the centers. Another 2013 report by the Comptroller and Auditor General of India found that over 33% of eligible beneficiaries were not receiving supplementary nutrition from ICDS in many states (58).

In 2013, a multi-year restructuring plan for improving and expanding the ICDS was adopted (24). It included an improved package of services for children under 3 years of age and women, enhanced growth tracking, stronger linkages with the public healthcare system, improvements in infrastructure and staffing at *Anganwadi* centers, introduction of public-private partnerships, and information campaign activities for greater community participation. Considering the significant educational benefits of the program found in our analysis, along with the health benefits shown by previous studies, the ICDS probably should be restructured and fully funded.

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SK collected the data. AN, AA, and RL conducted the analysis. AN, AA, SK, JRB, and RL interpreted the findings and wrote the manuscript. RL and JRB were responsible for the final contents. All authors have read and approved the final manuscript.

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Table 1: Demographic and socioeconomic characteristics of children in the 2003-2005 follow-up of the Hyderabad nutrition trial[[2]](#footnote-3)

|  |  |  |
| --- | --- | --- |
|  | Intervention group | Control group |
| Child characteristics:  |  |  |
|  Born in 1987, proportion | 0.291 0.455 (184) | 0.289 0.454 (138) |
|  Born in 1988, proportion | 0.443 0.497 (280) | 0.397 0.49 (190) |
|  Born in 1989, proportion | 0.179 0.383 (113) | 0.236 0.425 (113) |
|  Born in 1990, proportion | 0.087 0.282 (55) | 0.077 0.268 (37) |
|  Birth order | 2.922 1.502 (632) | 3.086 1.689 (478) |
|  Male child, proportion | 0.531 0.499 (336) | 0.538 0.499 (257) |
| Social characteristics of household: |  |  |
|  Scheduled caste/ scheduled tribe, proportion | 0.384 0.487 (243) | 0.291 0.455 (139) |
|  Other backward caste, proportion | 0.486 0.5 (307) | 0.569 0.496 (272) |
|  Religion other than Hindu, proportion | 0.033 0.179 (20) | 0.059 0.235 (28) |
| Standard of living: |  |   |
|  Wealth quintile 1, proportion | 0.228 0.42 (144) | 0.262 0.44 (125) |
|  Wealth quintile 2, proportion | 0.162 0.368 (102) | 0.155 0.362 (74) |
|  Wealth quintile 3, proportion | 0.231 0.422 (146) | 0.231 0.422 (110) |
|  Wealth quintile 4, proportion | 0.182 0.386 (115) | 0.195 0.397 (93) |
|  Wealth quintile 5, proportion | 0.197 0.398 (124) | 0.157 0.364 (75) |
| Father’s education: |  |   |
|  Literate, proportion | 0.159 0.366 (100) | 0.139 0.346 (66) |
|  Primary, proportion | 0.113 0.316 (71) | 0.151 0.359 (72) |
|  Secondary and above, proportion | 0.098 0.298 (62) | 0.126 0.332 (60) |
| Mother’s education: |  |  |
|  Literate, proportion | 0.043 0.203 (27) | 0.038 0.191 (18) |
|  Primary and above, proportion | 0.05 0.217 (31) | 0.076 0.265 (36) |

Table 2: Probit and ordered probit regressions of educational outcomes of children in the 2003-2005 follow-up of the Hyderabad nutrition trial[[3]](#footnote-4)

|  |  |  |  |
| --- | --- | --- | --- |
|  | School enrollment | Schooling grade completed | Academic performance |
|  | Coeff. (95% CI) | Coeff. (95% CI) | Coeff. (95% CI) |
| Born in an intervention village | 0.066 (0.008, 0.124) | 0.128 (-0.122, 0.379) | 0.055 (-0.077, 0.188) |
| Age | -0.083 (-0.111, -0.056) | 0.335 (0.256, 0.415) | -0.013 (-0.096, 0.069) |
| Birth order | -0.001 (-0.015, 0.012) | -0.022 (-0.067, 0.024) | 0.009 (-0.035, 0.053) |
| Male child | 0.217 (0.165, 0.268) | 0.378 (0.231, 0.526) | 0.344 (0.148, 0.54) |
| Scheduled caste/ scheduled tribe | -0.036 (-0.128, 0.056) | -0.449 (-0.799, -0.099) | -0.267 (-0.522, -0.012) |
| Other backward caste | -0.093 (-0.174, -0.011) | -0.326 (-0.617, -0.035) | -0.252 (-0.508, 0.003) |
| Non-Hindu household | -0.400 (-0.656, -0.144) | -0.205 (-0.675, 0.265) | -0.367 (-0.759, 0.025) |
| Wealth quintile 2 | 0.056 (0.009, 0.102) | 0.180 (0.033, 0.326) | -0.125 (-0.33, 0.08) |
| Wealth quintile 3 | 0.062 (0.029, 0.095) | 0.099 (-0.049, 0.247) | -0.137 (-0.346, 0.071) |
| Wealth quintile 4 | 0.066 (0.018, 0.113) | 0.400 (0.148, 0.652) | -0.29 (-0.496, -0.085) |
| Wealth quintile 5 | 0.091 (0.037, 0.145) | 0.242 (0.036, 0.449) | 0.295 (0.03, 0.559) |
| Father literate | 0.107 (0.06, 0.153) | 0.194 (0.032, 0.356) | 0.136 (-0.062, 0.334) |
| Father’s education: primary | 0.047 (-0.001, 0.096) | 0.210 (0.046, 0.374) | 0.282 (-0.008, 0.573) |
| Father’s education: secondary and above | 0.140 (0.101, 0.178) | 0.529 (0.23, 0.828) | 0.186 (-0.077, 0.449) |
| Mother literate | 0.130 (0.082, 0.178) | 0.369 (0.032, 0.706) | 0.034 (-0.262, 0.33) |
| Mother’s education: primary and above[[4]](#footnote-5) | 0.116 (0.04, 0.192) | 0.627 (0.303, 0.951) | 0.367 (0.041, 0.693) |
| p-value for  | 0**.**000 | 0.000 | 0.000 |
| Sample size | 1,096 | 1,059 | 904 |

Table 3: Estimation of propensity score: probit regression of the likelihood of children belonging to the intervention group in the 2003-2005 follow-up of the Hyderabad nutrition trial[[5]](#footnote-6)

|  |  |  |  |
| --- | --- | --- | --- |
|  | School enrollment | Schooling grade completed | Academic performance |
|  | Coeff. (95% CI) | Coeff. (95% CI) | Coeff. (95% CI) |
| Age | 0.020 (-0.064, 0.104) | 0.022 (-0.064, 0.107) | 0.065 (-0.024, 0.162) |
| Birth order | -0.038 (-0.086, 0.011) | -0.039 (-0.089, 0.011) | -0.045 (-0.102, 0.009) |
| Male child | -0.005 (-0.162, 0.151) | 0.048 (-0.111, 0.207) | -0.157 (-0.342, 0.021) |
| Scheduled caste/ scheduled tribe | 0.088 (-0.195, 0.371) | 0.043 (-0.248, 0.333) | 0.180 (-0.122, 0.495) |
| Other backward caste | -0.203 (-0.47, 0.064) | -0.264 (-0.538, 0.010) | -0.207 (-0.492, 0.081) |
| Non-Hindu household | -0.341 (-0.755, 0.073) | -0.390 (-0.807, 0.028) | -0.576 (-1.074, -0.075) |
| Wealth quintile 2 | 0.168 (-0.079, 0.416) | 0.196 (-0.059, 0.451) | 0.117 (-0.163, 0.39) |
| Wealth quintile 3 | 0.160 (-0.061, 0.381) | 0.134 (-0.093, 0.361) | 0.076 (-0.174, 0.326) |
| Wealth quintile 4 | 0.175 (-0.066, 0.417) | 0.164 (-0.082, 0.409) | 0.086 (-0.172, 0.373) |
| Wealth quintile 5 | 0.458 (0.196, 0.721) | 0.451 (0.184, 0.718) | 0.429 (0.14, 0.724) |
| Father literate | 0.002 (-0.218, 0.223) | -0.034 (-0.256, 0.188) | -0.059 (-0.297, 0.173) |
| Father’s education: primary | -0.261 (-0.507, -0.015) | -0.285 (-0.532, -0.038) | -0.314 (-0.592, -0.046) |
| Father’s education: secondary and above | -0.259 (-0.538, 0.02) | -0.284 (-0.566, -0.003) | -0.345 (-0.647, -0.051) |
| Mother literate | 0.067 (-0.333, 0.468) | 0.039 (-0.362, 0.44) | 0.085 (-0.329, 0.495) |
| Mother’s education: primary and above[[6]](#footnote-7) | -0.238 (-0.596, 0.121) | -0.255 (-0.615, 0.104) | -0.293 (-0.676, 0.089) |
| Constant | -0.025 (-1.338, 1.287) | 0.022 (-1.312, 1.356) | -0.434 (-1.922, 0.957) |
| p-value for  | 0.003 | 0.001 | 0.000 |
| Sample size | 1,096 | 1,059 | 904 |

Table 4: Percentage of bias in covariates between intervention and control groups in unmatched and matched samples of children in the 2003-2005 follow-up of the Hyderabad nutrition trial[[7]](#footnote-8)

|  |  |  |  |
| --- | --- | --- | --- |
|  | Bias in unmatched data | p-value of bias | Bias after matching |
| SchoolEnrollment | Schooling grade completed | AcademicPerformance |
| Age | 4.5 | 0.46 | -2.5 | -2.6 | 6.7 |
| Birth order | -10.2 | 0.09 | 7.4 | 2.6 | -0.4 |
| Male child | -1.2 | 0.84 | 1.9 | 3 | -0.8 |
| Scheduled caste/ scheduled tribe | 19.9 | 0.001 | -9.6 | -9.2 | 0.4 |
| Other backward caste | -16.7 | 0.006 | 5.5 | 1.7 | -3.4 |
| Non-Hindu household | -12.1 | 0.042 | -4.7 | -0.8 | 2.9 |
| Wealth quintile 2 | 1.8 | 0.77 | -1.8 | 0.5 | 2.5 |
| Wealth quintile 3 | 0.2 | 0.98 | -2.7 | -8.6 | -2.7 |
| Wealth quintile 4 | -3.2 | 0.59 | 8.2 | 7.1 | -6.6 |
| Wealth quintile 5 | 10.3 | 0.09 | 3.8 | 8.2 | 6.7 |
| Father literate[[8]](#footnote-9) | 5.6 | 0.36 | 13.1 | 2.3 | 4.4 |
| Father’s education: primary | -11.4 | 0.06 | -1.9 | 0 | 4.4 |
| Father’s education: secondary and above | -8.8 | 0.15 | 2.5 | 6.7 | 2.2 |
| Mother literate | 2.7 | 0.66 | 6.5 | 7.4 | 2.6 |
| Mother’s education: primary and above[[9]](#footnote-10) | -10.8 | 0.07 | -2.00 | 5.4 | 8.6 |
| Average bias | 8.0 |  | 5.0 | 4.4 | 3.7 |
| Pseudo R2 | 0.023 |  | 0.010 | 0.007 | 0.006 |
| p-value for  | 0.003 |  | 0.36 | 0.68 | 0.90 |

Figure 1: Number of children in the intervention and control groups of the 2003-2005 follow-up of the Hyderabad Nutrition Trial. Adapted from Kinra et al. (2008) (28) with permission.

Figure 2: Average educational outcomes of children in the 2003-2005 follow-up of the Hyderabad Nutrition Trial – intervention, unmatched control, and matched control groups. The matched control observations were obtained from propensity score matching, using one-to-one nearest neighbor match with replacement within a radius of 0.01. Current enrollment (A) is binary, while schooling grade completed (B) and test score at school (C) are measured on scales of 0-12 and 1-5, respectively.

**Propensity Score Matching Methodology**

Propensity score matching (PSM) is a widely used quasi-experimental econometric method for reducing systematic differences in the characteristics of intervention and control group observations in a study (33,35,36). The methodology in the context of our study is briefly described below:

 Let and denote an educational outcome of child *i* under the intervention or control, respectively. Let *I* be the binary intervention indicator, where *I*=1 for the intervention villages. Then, the effect of the intervention for a child is given by . The estimated average treatment effect on the treated (ATT) is defined as:

For a given child, it is not possible to observe both the intervention and control outcomes and at the same time. An alternative is to obtain a simple difference in the mean outcomes between the intervention and control groups. However, it will likely be imprecise due to systematic differences in background characteristics of children.

PSM can reduce the differences in characteristics between the intervention and control group children. Let us consider a covariate vector *X* which includes children’s age, gender, birth order, parental education, caste, religion, and household assets. Following the *unconfoundedness* assumption (33), the educational outcome would be orthogonal to intervention assignment, conditional upon the set *X,* i.e.

Now, let be the probability of being in the intervention group conditional upon the covariates. is known as the propensity score which reduces the dimension of comparison between intervention and control children from many characteristics to a single metric. Then, following the *unconfoundedness* assumption and *overlap* assumption ( of propensity scores (33), the educational outcome of a child would be independent of intervention status conditional upon the estimated propensity score, i.e. .

Therefore,. Following this, a matching estimator of ATT can be formulated as:

 The first term on the right hand side of above equation is the average educational outcome of children in the intervention group while the second term denotes the average outcome from a matched comparison group. The propensity score is the predicted probability of being in the intervention group, obtained from a probit regression on covariate *X.* Then, each intervention child is matched with the nearest neighboring child in the control group based on the propensity score (with replacement and within a radius of 0.01). The average difference in the educational outcome across all such matched intervention-control pair of children yields the ATT in our analysis.

In addition to evaluating the association between the intervention and educational outcomes in the overall sample, we also use PSM to examine if the association is stronger for girls as compared with boys. This is done by constructing a difference-in-difference estimator. First, we use the PSM methodology as described above to estimate the ATT only among girls, denoted by . This is the difference between the average outcomes of intervention group girls and matched control group girls. Similarly, another ATT estimator is calculated separately for boys, denoted by . The difference-in-difference estimator thus measures the differential effect of the intervention for girls as compared with boys. The standard errors of all our PSM ATT estimators are adjusted using a bias-correction method (38).

1. Supplemental methods are available from the “Online Supporting Material” link in the online

posting of the article and from the same link in the online table of contents at jn.nutrition.org [↑](#footnote-ref-2)
2. Values are *mean*$\pm $*SD* (*n*). All variables except birth order are binary. Intervention group includes children born during 1987-1990 in villages where the nutritional supplement was provided. Control group includes children born during 1987-1990 in the control villages. [↑](#footnote-ref-3)
3. Values are regression coefficients with 95% confidence intervals, unless stated otherwise. The number of observations differs across regression models because of some missing values of the outcome variables. Coeff., regression coefficient; CI, confidence interval. [↑](#footnote-ref-4)
4. Because very few mothers have a secondary or higher level of education, we combine them with the category “primary and above”. [↑](#footnote-ref-5)
5. Values are estimated regression coefficients with 95% confidence intervals, unless stated otherwise. The number of observations differs across regression models because of some missing values of the outcome variables. Coeff., regression coefficient; CI, confidence interval. [↑](#footnote-ref-6)
6. Because very few mothers have a secondary or higher level of education, we combine them with the category “primary and above”. [↑](#footnote-ref-7)
7. Values are %, unless stated otherwise. Bias is the percentage difference in the average value of the variable in intervention sample from that of the control sample. Matching was on propensity scores, using a one-to-one nearest neighbor match with replacement within a radius of 0.01. [↑](#footnote-ref-8)
8. “Father literate” had a statistically significant bias even after matching (13.1% bias, *p*<0.05) in the analysis of enrollment. [↑](#footnote-ref-9)
9. “Mother’s education: primary and above” had a statistically significant bias even after matching (8.6% bias, *p*<0.1) in the analysis of academic performance. [↑](#footnote-ref-10)