An Evaluation of the Indian Child Nutrition and Development Program

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Abstract

The Indian Integrated Child Development Services (ICDS) aims to improve the physical and psychological well-being of children younger than five. However, previous evaluations find that ICDS fails to significantly impact child stunting and that program placement is faulty. My results contradict the lack of a significant treatment effect, but are consistent with problematic program placement. Previous analyses of ICDS used probit to study placement, but the distribution of state-wise ICDS coverage is negatively skewed violating the normality assumption of probit. To address this, I use beta regression to study placement and compare results with probit analysis. In addition, using Propensity Score Matching (PSM) I find evidence of a significant, positive average and quantile treatment effects on stunting. Data are from the most recent Indian Family and Health Survey (NFHS-3).

1 Introduction

"There are people in the world so hungry, that God cannot appear to them except in the form of bread."– Mohandas Karamchand Gandhi

1.1 Motivation

Malnutrition in the first two years of an infant's life leads to lower educational attainment and lifetime earnings (Alderman et al., 2006). Indias integrated child nutrition program aims to reduce chronic malnutrition using endogenous program placement to target the most vulnerable portion of the population. Stunting is a commonly used measure of chronic malnutrition which reflects long-term damage to a child's nutritional status.[1] This paper evaluates the ability of the Indian government's primary child nutrition intervention to target vulnerable populations through program placement and in reducing child stunting in targeted villages. Other measures of child malnutrition, including wasting and underweight status,

[1] A child exhibits stunted growth if his/her height-for-age is two or more standard deviations below the mean for the World Health Organization's International Reference Population. While other reference populations exist, the consensus in the literature is that these reference populations and thus the health outcomes do not vary significantly.

fluctuate more readily in response to recent food intake. Since the program I study in this paper targets the long-term nutritional development of the child, I use stunting rather than wasting or anemia to measure its impact.[2] Further, since the intervention studied in this paper is endogeneously placed, another determinant of its effectiveness is placement design. Although most intervention centers had been in place for at least ten years, the Indian government expands the program each year, making placement an important element of implementation. In this paper, I examine whether program placement effectively targets vulnerable populations and whether there is a positive treatment effect on targeted children. This analysis uses data from the nationally representative Indian National Family and Health Survey of 2005-06 or NFHS-3 (IIPS and ORC Macro, 2007).

Although real Indian GDP per capita has doubled in the last fifteen years (WDI, 2007), child stunting has only decreased by sixteen percent in the same time period: 69 percent of children under five were stunted in NFHS-1 (1992-93), 68 percent in NFHS-2 (1998-99), and 58 percent in NFHS-3. Data from the NFHS-3 also show that 45.9 percent of all Indian children are severely undernourished (Kandpal and McNamara, 2008a).[3] In his 2008 Independence Day address to the nation, the Indian Prime Minister, Manmohan Singh called the high rate of child malnutrition a "curse that [India] must remove" (Indian Embassy, 2008). The Government of India takes a joint approach to reducing child malnutrition by subsidizing food and directly feeding children: a Public Distribution System makes food available at subsidized prices and the Integrated Child Development Services (ICDS) provide nutritional supplements and bundled child and maternal services to targeted populations. ICDS has been in place since 1977 and although it cost approximately $1.5 billion in 2008, previous evaluations using data from 1998-99 or earlier have not yielded conclusive evidence on its effectiveness (Lokshin et al., 2005; World Bank, 2007a). The World Bank has recommended

[2] A child is wasted if his/her weight-for-height is two or more standard deviations below the mean for the World Health Organization's International Reference Population. Wasting measures acute malnutrition while stunting measures chronic malnutrition. A child less than five is anemic if his/her hemoglobin level is below 11 grams/deciliter.

[3] Severe undernourishment refers to a deficit of two or more standard deviations from the WHO international reference population mean.

a $9.5 billion ICDS redesign project to the Indian government. Given the availability of new data and the sixteen percentage point decrease in malnutrition between NFHS-2 and NFHS-3 coinciding with a period of sustained economic growth, ICDS should be reevaluated before undertaking expensive redesign.

1.2 Summary of Main Results

In order to examine ICDS placement design and estimate the average treatment effect, I conduct my analysis in two steps: the first step examines the placement of ICDS in villages as a function of the observables on which the government bases its placement decision, namely, population, average income, and district-level sex ratios. Results suggest that while ICDS effectively targets poor, rural areas with risky water sources, sex-ratios and landholdings do not play a significant role in placement. ICDS also appears to target areas with higher proportions of educated mothers, although villages with high fractions of uneducated people may benefit most from the intervention.

In the second step of this evaluation, I use Propensity Score Matching (PSM) to identify the effect of ICDS on children who are stunted. Results for the sample of children under the age of four show ICDS significantly reduces child stunting. Unmatched comparisons suggest that children from ICDS villages were significantly worse off than children from non-ICDS villages. On the other hand, matched results tell us the average child from an ICDS village is 5.5 percent closer to the mean height-for-age than similar children from non-ICDS villages, significant at the 99 percent level. The average boy from an ICDS village is six percent closer to the mean height-for-age than boys from non-ICDS villages, also significant at the 99 percent level. The impact of the ICDS on girls is smaller and less significant, but is nonetheless positive (four percent) and statistically significant (at the 90 percent level). Results are similar in significance and magnitude for younger (more vulnerable) children.

Not controlling for endogenous program placement– using unmatched results– confirms the consensus in the literature that the ICDS does not significantly reduce child stunting. Estimates that control for endogeneity with matching contradicts previous findings and shows the ICDS decreases child stunting and the effect size is larger than estimates of treatment effects from similar programs in other developing countries. I also find evidence of a learning effect of ICDS: centers take up to ten years to significantly affect stunting. In summary, results suggest that when a village gets ICDS, the program reduces child stunting by approximately five percent. However, program placement does not work perfectly and often fails to target vulnerable populations. To examine whether ICDS-provided daycare significantly increases female labor supply, I also conduct PSM on whether or not a woman participates in the labor force. I find the ICDS increases the percentage of working women by almost seven percent. Given that 45 percent of Indian children under the age of five have stunted growth, a ten percent improvement does not lift the average child out of malnutrition. Therefore, it seems India's efforts to reduce child malnutrition cannot rely solely on the ICDS.

The ICDS reform project is expected to cost \$9.5 billion; errors in redesign could be very expensive. More importantly, if the ICDS is effective to some extent, but the redesign proceeds under the assumption that it is not, then redesigning the program could take away vital assistance from poor households. Indeed, for redesign to be effective, policy-makers should not attempt to fix successful aspects of the ICDS and should instead focus on aspects that are ineffective. Given the hefty price tag of redesign, the potential impact on poor households and the availability of new data, the impact of the ICDS bears closer evaluation and rigorous econometric analysis. Since children from rural and agricultural communities face reduced access to health-care facilities which in turn renders them particularly vulnerable to the long-term effects of malnutrition, the impact of the ICDS on child stunting is relevant for similar program design elsewhere.

2 ICDS: Background and Monitoring

ICDS is the world's largest early child development program (Prinja et al. 2008) although it covered only about a third of all age-eligible children in 2005-06 (IIPS and ORC Macro, 2006). The ICDS targets the foundations of physical and psychological development of the child in the most vulnerable sections of the population, including children under the age of six, pregnant and nursing women, and the economically disadvantaged. Identification of target areas occurs through community-level surveys and through enumeration of families living below the poverty line. District and village level ICDS centers provide vitamin A supplements, immunizations, health exams, referral services, early childhood care, daycare and preschool education, and information on nutrition and health (Ministry of Women and Child Development). The government also hopes to reduce the incidence of female infanticide and feticide by placing ICDS in areas with significantly fewer girls than boys: centers provide information on the benefits of having a girl child in an attempt to reduce excess female mortality.

Reports evaluating the program tend to find that while ICDS is well-designed, its effectiveness is limited by issues of implementation. The World Bank (2007a) finds using all the services provided by local ICDS centers results in significant health and nutritional benefits; however, most families use only the nutritional supplements, immunization services or the day care facilities. Other studies have identified similar limitations of the ICDS, albeit on a smaller scale: Saiyed and Seshadri (2000) study a sample of 610 children under the age of three receiving full, partial or no services through ICDS over a one-year period. The authors find complete utilization of ICDS services resulted in a significant improvement in stunting, wasting, and weight-for-age, but partial utilization had a much smaller impact. The multi-agency Indian Coalition for Sustainable Nutrition Security finds that food supplementation appears to be the key service delivered by the ICDS, although such supplementation may not be the optimal nutrition intervention.

The ICDS also fails to improve parenting practices and is often unable to provide necessary medical referrals. Prinja et al. (2008) study 60 ICDS centers in the Northwestern state of Haryana to find that participation in an ICDS center did not affect the timing and nature of breastfeeding and the involvement of the mother in the child's growth monitoring. Gragnolati et al. observe that parental counseling and linkages with the health sector are minimal (2006). Although much of an individual's nutritional status is determined in the first three years of life, Lokshin et al. find that the ICDS services are less likely to reach children under the age of three than three to six year olds. Much of an infant's nutritional status is determined before the third year of its life, rendering later interventions substantially less effective. Lokshin et al. also blame poor quality of services, irregular food availability, and a lack of representation of local needs or diets for further decreasing the program's effectiveness.

Evaluations of integrated child development programs in other developing countries have yielded little evidence of an impact on child stunting. Walker et al. (1996) find that early childhood food supplementation does not improve stunting outcomes in Jamaica, while Walsh et al. (2002) report that a nutrition education program in South Africa failed to affect stunting, although it had significant, positive effects on other measures of nutrition. Similarly, Armecin et al. (2006) evaluate a Philippine early child development program to find significant positive effects on short-term nutrition and on cognitive, social, motor and language development, but not on child stunting. In contrast, a few studies find that childhood nutritional supplement have a small impact on child stunting. Stifel and Alderman (2006) study a Peruvian milk subsidy program, *Vaso de Leche* to find that although the intervention decreased overall malnutrition rates by 28 percent, it reduced child stunting by only three percent. Behrman and Hoddinott (2005) find that the Mexican PROGRESA caused a three percent decrease in the probability of a child being stunted. Thus the literature on treatment effects of integrated child nutrition programs suggests that a lack of large and significant effect of ICDS is the norm rather than an exception.

Recently, the World Bank recommended an ICDS reform project to the Indian government

(2007b), motivated by the studies discussed above which find that the ICDS does not have a significant impact on child stunting, wasting, or anemia. However, recent work by Gragnolati et al. and the World Bank (2007a) is based solely on summary statistics. Other major evaluations by NIPCCD (1992) and Lokshin et al. use data from the older waves of the NFHS (1992-93 and 1998-99). In contrast, quantile regression on NFHS-3 data by Kandpal and McNamara (2008b) provides evidence of a small-but-significant, negative correlation between child stunting and ICDS coverage, particularly in the lower quintiles of the distribution of child stunting. The Indian government places ICDS centers in target poor areas, large population points, and districts with sex ratios that indicate the presence of sex-selective abortion and infanticide (Lokshin et al., 2005).[4] Using procedures that fail to control for the endogeneity of program placement yields biased estimates of the treatment effect.

3 Data and Summary Statistics

3.1 Description of the Dataset

Data are from the Indian National Family Health Survey (NFHS) of 2005-2006. The NFHS of 2005-06 is the third in a series of national surveys. The first NFHS survey was conducted in 1992-93 and the second in 1998-99. The third wave of the NFHS interviewed more than 230,000 women between 15 and 49 years old from all 29 states in India using a Demographic and Health Survey questionnaire as its basis. The urban and rural samples within each state were drawn separately and the sample within each state was allocated proportionally to the size of the state's urban and rural populations. The rural sample is selected in two stages: first stage selection of primary sampling units (villages) with probability proportional to population size was followed by the random selection of households within each village in

[4] It is entirely possible that these criteria are not strictly followed in all cases. Politicians may influence the placement of an ICDS center for political gains, while ICDS workers may prefer to place centers in districts that are easy to access. However, evidence for such divergence from the stated policy is difficult to find.

the second stage (IIPS and ORC Macro, 2007). The NFHS-3 interviewed the household head or any adult household member of each selected household for personal and household characteristics. The NFHS follows a standard Demographic and Health Survey (DHS) questionnaire and does not report income figures. The only measure of wealth in DHS is a wealth index which is a summary measure of asset ownership (land, livestock, jewelery, vehicles), housing characteristics (material and quality of roof, walls and floor), and ownership of durables (television, radio). Each asset is assigned a weight and normalized asset scores are assigned to each household.

The NFHS also asked a special module of questions to a randomly-chosen sample within 3842 villages covering 36,850 respondents who had given birth to at least one child in the past five years. This module measured the height, weight, and hemoglobin content of 31,556 of these women, and also collected the same anthropometric measures for 41,306 of their children below the age of five. This portion of the survey sample provides the necessary stunting data. Anthropometric measures are not reported for 1385 women and their children (or slightly over four percent of the sub-sample). Missing observations are of econometric concern because if these 1385 women were systematically unhealthier than the other women, the infants they give birth to would more likely be unhealthy. These children may have benefited disproportionately from ICDS intervention; by not including them in the sample, we may be underestimating the effect of the ICDS. Conversely, if the mother is simply too sick to look after her child or to take her child to the ICDS center, these children may be foregoing any of the ICDS benefits, in spite of living in an ICDS village. If this case is true, results would overestimate the impact of having an ICDS center in the area. While I recognize that these missing anthropometric measurements may introduce a source of bias to my estimates, I am unable to conclusively determine the direction of this bias.

In previous rounds of the NFHS, district- and village-level data were provided, which could then be used to compute the probability of a village hosting an ICDS center, including factors such as distance to the district headquarters, connection to an all-weather road and train station, any history of epidemics in the past two years, average household wealth, the sex ratio, percentage of mothers with primary and secondary education, and whether the village had electricity. The NFHS-3, however, includes HIV testing data for a small sample of the population, and any identifiers below the state-level are scrambled to protect the identity of the tested individuals. While we can identify the state of a village, further village characteristics are no longer available. Therefore, to determine the likelihood of a place receiving coverage by the ICDS, I generated village-level aggregates using the available data. I could generate the average household wealth of the village, sex ratio, percentage of mothers with primary and secondary education, average landholding size, use of irrigation, availability of drainage and electricity. I could not, however, proxy for distance to the nearest town, connection to an all-weather road, presence of any other development programs, or any history of epidemics. These missing variables would have improved the matches found via the propensity score method, but data constraints preclude such improvements.

3.2 Summary Statistics

As table 1 shows, the average woman in the sample of mothers was 27 years old and had 3.9 years of education. The average age at first marriage was 18 and the average number of births in the last five years was 1.6. Only 29 percent of surveyed respondents were working at the time of the survey. The average child in this sample was two years old and was 1.7 standard deviations below the WHO reference mean height-for-age. Boys were 1.73 standard deviations below the mean, while girls were 1.68 standard deviations below the mean. The difference between male and female child stunting was -0.05 and was significant at the 95 percent level. About 74 percent of the respondents lived in areas covered by the ICDS. Slightly over half of all ICDS centers had been present in the village for over a decade: thus, most children in this sample had either lived in an ICDS village or a non-ICDS village their entire lives.

Table 2 shows the considerable variation in the distribution of child stunting. The lowest 25 percent are 2.78 standard deviations below the WHO reference population mean, while the highest 25 percent are almost three standard deviations above the mean. Girls appear to be slightly better off than boys in all four quartiles. Also note that only the highest 25 percent of children are above the WHO reference population mean. Table 3 below shows a negative correlation between maternal education and the incidence of moderate (two or more standard deviations below the WHO reference mean) and severe stunting (three or more standard deviations below WHO mean). Although women with twelve or more years of schooling are less likely to have stunted children than less educated women, the incidence of stunting remains high at 22 percent moderate stunting and seven percent severe stunting among children of women with at least a high school education.

If the ICDS effectively targets poor states (and then further targets at the sub-state level), we would expect to see a negative correlation between the percentage of districts in a state covered by the ICDS and the percentage of households in that state that live in the bottom two quintiles of the wealth distribution. Figure 1 presents a quantile map of the percentage of districts covered by the ICDS in the left panel, and a quantile map of the percentage of population in each state that lives in the two lowest quintiles of the wealth index in the right panel. A darker color in the left panel of Figure 1 indicates a higher percentage of districts covered by the ICDS, while a darker color in the right panel indicates a higher poverty rate (as defined by the percent of households living in the two lowest quintiles of the wealth distribution). The figure shows a surprising negative correlation between ICDS prevalence and poverty rates for many of the central states and some of the North-eastern states. Thus even though by ICDS coverage should be highest in the poorest states, implementation may not always result in such placement. This trend is consistent with the analysis of NFHS-1 and NFHS-2 data (Lokshin et al., 2006). Figure 2 shows the central states that are in the lowest quartile of poverty but in the third quartile of ICDS coverage also have very poor stunting outcomes. In contrast, a few states in the North, South and Northeast have relatively few stunted children, high ICDS coverage and low poverty. These maps suggest that ICDS targeting may not be entirely effective and areas of most need may not be adequately covered. We will return to the issue of program placement in the empirical analysis section to examine which aspects of ICDS placement work and which ones do not.

Bardack (2008) finds in keeping with the program's endogenous placement, household wealth is a significant predictor of a family's utilization of ICDS services. To explore the impact of wealth on the effectiveness of ICDS, I present two sets of kernel density estimates– one controlling for wealth and the other not. Figure 3 below shows the kernel densities of child stunting for children living in ICDS covered districts and those living in areas not covered by the ICDS. We see that the distribution of stunting rates for children living in ICDS covered districts has a higher mass below the mean: it suggests children living in ICDS are more likely to be stunted and that centers are placed in areas of most need.

Figure 4 presents a series of five kernel density estimates that show the distribution of stunting rates for children living in ICDS covered areas and those in places not covered by ICDS, broken down by quintiles of the wealth index. In the poorest quintile, ICDS appears to decrease the likelihood of being stunted: children living in ICDS districts appear to be equally likely to be below the mean compared to children from non-ICDS districts but are more likely to be just above the mean. The tails of the two distributions are similar. In the next poorest quintile, apparently, ICDS covered children are less likely to be below the mean than those from non-ICDS areas.

The story is very similar for the third quintile, but the picture is perhaps clearest for the fourth quintile. In the fourth quintile, the ICDS appears to shift the distribution of child stunting in the positive direction: ICDS covered children are less likely to be below the mean but more likely to be at or above the mean than children from non-ICDS covered districts. In the richest quintile, the two distributions overlap for the most part indicating the lack of a significant difference between ICDS and non-ICDS villages. These kernel density plots suggest without controlling for income, ICDS appears not to have any impact on child stunting, while after controlling for income, ICDS coverage tends to be correlated with better stunting outcomes in all but the richest quintile. Since endogenous placement of ICDS centers targets poor areas, controlling for income eliminates bias resulting from targeted placement. These kernel density plots demonstrate the effect of endogenous program placement and underline the importance of correcting for endogenous program placement in the following empirical analysis.

4 Empirical Analysis

4.1 Program Placement

The presence of an ICDS center in a district/village is the outcome of not only program placement, but also program retention (Lokshin et al. 2006). The lack of community-level characteristics in the data makes it difficult to test the suitability of a village for program placement or the importance of certain characteristics in retaining an ICDS center. For this reason, I model the probability of a village receiving an ICDS center and then assume the center is retained; this assumption is likely reasonable given that over half of all ICDS centers had been in a village for at least ten years. I estimate the probability of ICDS being located in a specific village as a function of available and constructed characteristics which include: the population of the village, the share of girls of the population, the average wealth of the village, the average landholding (in acres), the average number of acres irrigated, electrification, average distance to water source, and whether the water source is uncovered/unprotected. I also include a dummy for rural and semi-rural areas. Although this variable will be correlated with the average number of acres irrigated, I expect it to pick up unobserved community-level factors which partly determine whether a village receives an ICDS center, like the presence of other development programs (which tend to be focused in rural areas). The national government provides each state with an ICDS budget based on state-level values of the stated target criteria. States then allocate this budget based on village-level values of the target criteria.

Lokshin et al. also study the placement of ICDS centers at the national- and state- levels using probit analysis with and without state dummies. As Figure 5 shows, the state-wise distribution of ICDS centers has negative skewness. However, probit analysis assumes an underlying normal distribution which is invalid for evaluating asymmetric distributions like that exhibited by program placement at the state-level. Beta distributions are useful in modeling proportions (variables continuously distributed on the (0,1) interval) such as state level ICDS coverage because the distribution can assume a variety of shapes, depending on the governing shape parameters α and β. Ferrari and Crebari-Neto (2004) present a beta regression which assumes the dependent variable is beta distributed on the interval (0,1) with shape parameters determined by the mean and dispersion of the empirical density function.

To include both levels of the ICDS placement decisions, I estimated two probit specifications on whether or not a village has an ICDS center– one with indicator variables for states and one without. Given the negative skewness of state-wise ICDS coverage, a conditional beta regression was also estimated. The first column (Probit I) in Table 4 presents probit estimates without state dummies, while the second column (Probit II) presents estimates with state dummies. Beta regression estimates are presented in the third column of Table 4. According to the Akaike Information Criterion (AIC), the probit specification with state dummies (Probit II) performs better than the probit specification without state dummy variables (Probit II). The AIC also indicates that the Beta regression is the best specification of the three presented in Table 4. These AIC outcomes are not surprising because not using state-level indicator variables results in omitted state-level heterogeneity. So, I treat the Probit II specification (with state indicators) as a more reliable estimate of state-level placement and the Beta regression estimates as measures of national-level placement.

An increase in population size decreases national-level allocation of ICDS funds. At the state-level, population is not a significant predictor of ICDS coverage. A change in the share of girls in total population does not significantly affect the probability of participation in either probit or beta specification, which contradicts the government's stated goal of targeting areas with skewed sex ratios. Poorer villages are more likely to receive ICDS coverage at both state- and national-levels. Rural areas are significantly more likely to receive ICDS coverage at the state level but not at the national level. At the national level, states with larger irrigated landholdings receive more ICDS coverage, which contradicts the policy of targeting poor areas. Areas with higher fractions of mothers with primary education or secondary receive more ICDS coverage at both levels of ICDS allocation. Electrification significantly increases the probability of participation at the national level (and in Probit I), although it is an insignificant determinant at the state-level. A lack of access to an improved water source significantly reduces participation probability at the state-level, although it increases coverage at the national-level.[5] Villages without many educated mothers, safe sources of drinking water or electrification may also be the ones to most benefit from participation in a government development project such as the ICDS, so not extending coverage to these villages indicates regressive policy.

These estimates highlight important differences in targeting at the state and national levels. While poorer villages are more likely to have ICDS centers, states with larger irrigated landholdings are more likely to receive national-level funds. Electrification does not significantly affect state-level placement, but increases national-level placement. Population is inversely correlated with national coverage, but not with state placement. At the state level, access to a risky water source reduces the likelihood of receiving an ICDS center, but increases national-level coverage. Targeting appears to work on some counts, including wealth and rural location, but fails in other important aspects like population, sex ratio and average educational attainment. Overall placement of ICDS exhibits some progressive traits, but

[5]The WHO and UNICEF consider the following to be "improved water sources": household connections, boreholes, protected dug wells, protected springs, and rainwater collection.

also a few regressive ones.

4.2 The Impact of the ICDS

This paper uses Propensity Score Matching (PSM) to measure the impact of the ICDS on child stunting. The notion of propensity scores is useful in the context of non-random treatment assignment. The propensity score is a conditional probability measure of treatment participation, given observable characteristics, $\mathbf{x}$, and is expressed as follows

$$P_i(\mathbf{x}) = P[D_i = 1|\mathbf{X} = \mathbf{x}], \tag{1}$$

given that we can satisfy the balancing condition (Cameron and Trivedi, 2007). Rubin (1973) shows that PSM eliminates selection bias, if selection bias is eliminated by controlling for $\mathbf{x}$. In this paper I use nearest-neighbor matching which matches treated observations to the control observation with the closest propensity score. In the current data all the community-level characteristics which determine participation in ICDS are not available. Thus, it is possible that the observed $\mathbf{x}$ variables do not entirely eliminate selection bias. Each child in the ICDS areas with the ICDS program is paired with one in the areas without the program based on the propensity score of each child. I conduct this matching based on observed factors that likely affect both ICDS participation and child stunting: age, birth order, and sex of the child, the mother's age, education, caste, and religion, household wealth, village population and other community-level development indicators, and then test for the significance of differences in the stunting variable. For the purposes of this paper, I maintain the unconfoundedness assumption (Imbens and Wooldridge, 2009):

$$D_i \perp (Y_i(1), Y_i(0))|P_i(\mathbf{x}) \tag{2}$$

Here, the unconfoundedness assumption means that treatment assignment, D_i is independent of stunting outcomes, Y_i after controlling for propensity scores, or that there are no unobservables that affect stunting and probability of treatment.

To control for endogenous program placement, Lokshin et al. (2005) use Propensity Score Matching (PSM) on the first two rounds of the NFHS (1992-93 and 1998-99), and find that the ICDS fails to reduce child stunting. PSM controls for endogenous program placement by matching treated individuals to untreated individuals on a conditional probability measure of treatment participation (Cameron and Trivedi, 2007). PSM allows the comparison of treated individuals to an untreated (control) group, using observables such as demographic and economic characteristics to construct the control group. However, Lokshin et al. look at the average treatment effect (ATE) of the ICDS over the entire survey sample, which may mask a positive impact on target groups. Kandpal and McNamara's quantile regression results suggest that the ICDS has a positive impact at the left-tail of the distribution, so in this paper I extend Lokshin et al.'s analysis by conducting quartile-wise PSM in addition to estimating the ATE for the full sample.

Quantile treatment effects are relatively new to economic literature and denote differences between quantiles of two marginal potential outcome distributions (Imbens and Wooldridge, 2009). The $q-th$ quantile treatment effect (QTE) is denoted as follows:

$$\tau_q = F^{-1}_{Y(1)}(q) - F^{-1}_{Y(0)}(q) \tag{3}$$

The estimates of the quantile treatment effect tell us the effect of ICDS on the distribution of a quantile, based on the outcome variable. For example, a question the QTE allows us to answer is whether the ICDS shifts to the right (toward less malnutrition) the marginal distribution of the lowest quartile of treated individuals.

Propensity Score Matching was conducted for the sample of 30,521 ICDS-treated and 9,425 control children for whom the NFHS-3 provides anthropometric measures. Then, the matching analysis was done separately for each quartile of the distribution of child stunting. Table 5 presents the results of matching analysis for the entire distribution and the two lowest quartiles of the stunting distribution. The unmatched observations over the entire distribution in the upper panel of the table suggest that children in ICDS villages are shorter for their age than children from non-ICDS villages. In other words, that the ICDS appears to have a significant, *negative* impact on child nutrition. In contrast, the matched results in the lower panel tell us that the ICDS increases child height-for-age. Children who live in villages with an ICDS center have, on average, a height-for-age that is ten percent of one standard deviation– or 5.5 percent– closer to the international reference population mean than the average height-for-age of children from other villages. Over the entire sample, the ICDS has a greater effect on the stunting rates of boys: boys from ICDS villages are six percent taller for their age than boys from non-ICDS villages, while treated girls are only four percent closer to the mean than untreated girls. The results for the lowest quartile yield interesting distribution information. In this quartile, even unmatched results show a significant positive impact of the ICDS.

In contrast to results for the entire sample, matched results for the lowest quartile show that girls benefit more from the ICDS than do boys. However, the amount of decrease in stunting due to ICDS is smaller for the lowest quartile: treated girls are only two percent closer to not being stunted and treated boys are not significantly better off than the untreated. The results for the second quartile (lowest fifty percent of the sample) show similar trends. An interesting result for these two quartiles is that girls appear to benefit more than boys from the ICDS. Rose (1999) documents the presence of a "son syndrome" in some poor, rural parts of India. This son syndrome would suggest that boys are better off than girls, not the other way around. Perhaps the ICDS works to change parental practices– ever so slightly– and leads to a more equitable distribution of household allocation. Alternatively, since ICDS services are free, worst-off girls benefit disproportionately from the medical and nutritional services which they would not have received in the absence of an ICDS center. In either case, the ICDS appears to somewhat mitigate the effects of the son syndrome.

Since nutritional status is largely determined in the first two or three years of an infant's life, I present estimates of ICDS impact on stunting for children below two and three. Table 6 presents estimates of the average treatment effect on children less than two years old. Results show a positive treatment for both sexes together and for boys. Although the difference between treated and control girls is positive, it is not significant– probably due to a lack in variation in data. The effect sizes are similar to those reported above for the entire distribution. Once again, we observe the importance of controlling for endogeneity via matching because the unmatched results show a significant and large, negative effect of the ICDS. Table 7 presents treatment effects for children less than three. Here, the differences are significant and positive for both sexes together and separately. These results suggest that ICDS significantly improves the nutritional outcomes of the most vulnerable groups of children.

The behavioral changes needed to significantly affect stunting may take time; learning effects may not occur immediately after an ICDS center is placed in a village. To study whether ICDS indeed requires time to start having an impact, I conduct PSM analysis on stunting outcomes by duration of ICDS presence in the village. These results, presented in table 8 show that it takes an ICDS center up to ten years to significantly affect child stunting. After one year and up to five years, unmatched results show a large, negative effect of ICDS while matched results show a positive albeit insignificant treatment effect. After ten years of exposure, ICDS effects a four percent decrease in the deficit to nourished status.

ICDS centers have daycare facilities that are designed to increase the educational attainment of children through preschool education but also facilitate the mother's return to the labor force. If these daycare centers effectively incentivize or facilitate female labor force participation, we would expect to see a positive treatment effect of ICDS on the number of women who work. I conduct PSM on the number of women who work in villages with ICDS compared to villages without ICDS. Treated and control samples are matched on the woman's age, years of schooling, husband's educational attainment, religion, female health indicators (body-mass-index and anemia variables), caste, household wealth and village development variables include average wealth, average landholdings, sex ratio, share of mothers with primary and secondary education, and access to improved water source and sanitation. Results show that 32 percent of mothers in ICDS villages worked at the time of the survey while only 30 percent from non-ICDS villages worked. ICDS thus increases female labor force participation by two percentage points which translates to an increase of 6.67 percent. These estimates are intent-to-treat only because we do not know whether these women actually used the ICDS daycare facility. As a result, this treatment effect is a lower bound of the actual treatment effect. Further, increased female labor force participation is not one of the stated objectives of the ICDS, but is simply a positive externality of coverage by ICDS.

5 Sensitivity Analyses

The lack of village-level information on development characteristics, the presence of other programs that might indirectly affect child health, and proximity to an administrative headquarters could possibly confound PSM estimates of the effect of ICDS. To examine whether such unobserved characteristics are leading to an upward bias in PSM results, I constructed the village-level aggregates used in the PSM analysis presented above for NFHS-2 (1998-99) data. Lokshin et al. report insignificant treatment effects of ICDS using NFHS-2 data, so significant estimates would suggest that unobserved village characteristics are indeed contaminating the ICDS treatment effect. However, as Table 10 shows, the ICDS treatment effect is insignificant for 1998-99 which is consistent with results presented by Lokshin et al. (2005). Although all estimates are insignificant, it is worth noting the similarity in effect sizes: Lokshin et al. report a matched difference of 0.024 for the entire sample, while the estimate presented below is 0.03. For boys, Lokshin et al. report a difference of 0.09, while my results indicate a difference of 0.01. However, Lokshin et al. find an insignificant difference of -0.06 for girls, while my results yield a difference of 0.05. Thus these results show that unobserved village-level characteristics are probably not resulting in positively-biased estimates of ICDS treatment effect.

6 Conclusion

India's primary child nutrition intervention scheme, the Integrated Child Development Services, aims to improve the physical and psychological well-being of children under the age of five. However, previous literature studying the impact of this program finds that the ICDS does not target the right children– the poorest of the poor and the very young. Studies also find that the ICDS not only fails to bring about any quantifiable improvement in health outcomes, but is also unable to improve parenting patterns. However, most of this literature does not control for the targeted placement design of ICDS, leading to downward biased estimates of the effectiveness of ICDS. Nonetheless, based on the evidence in the literature, the World Bank recently recommended a $9.5 billion ICDS redesign project to the Indian government. In the light of such an expensive redesign project being underway, the impact of the ICDS bears rigorous analysis. Indeed, results presented in this paper suggest that although targeting does not work perfectly, ICDS causes a moderate reduction in child stunting, particularly at the bottom of the distribution.

As Lokshin et al. (2005) point out, panel data which track villages and individuals are the appropriate way to analyze the effectiveness of the ICDS. Cross-sectional data may introduce selection bias, if placement and the effectiveness of the treatment are based on unobservables. However, such a bias would be in the downward direction, meaning that the results presented here may be a lower bound. Further, I can only study the effect of ICDS treatment on child health (and indirect effects on female labor supply) because the NFHS does not include information on utilization of other services provided by the program. Ideally, we would want to consider the hours of female labor supply in determining the effect of ICDS, but NFHS-3 data do not include such information which restricted this analysis to using an indicator variable for female labor force participation.

Restrictions notwithstanding, the ICDS appears to be finally having an impact but this effect may increase if the program is targeted at the right age groups (0-3 years) and at the poorest end of the distribution. Such flaws in placement may be one reason the ICDS fails to have a larger impact. India's economic growth has been spectacular, but for the socio-political stability of the country the Indian Government cannot neglect its poor and its young.

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Figure 1: Percentage of Villages Covered by the ICDS (Panel a); Percentage of People in Two Lowest Quintiles of Wealth Index (Panel b)

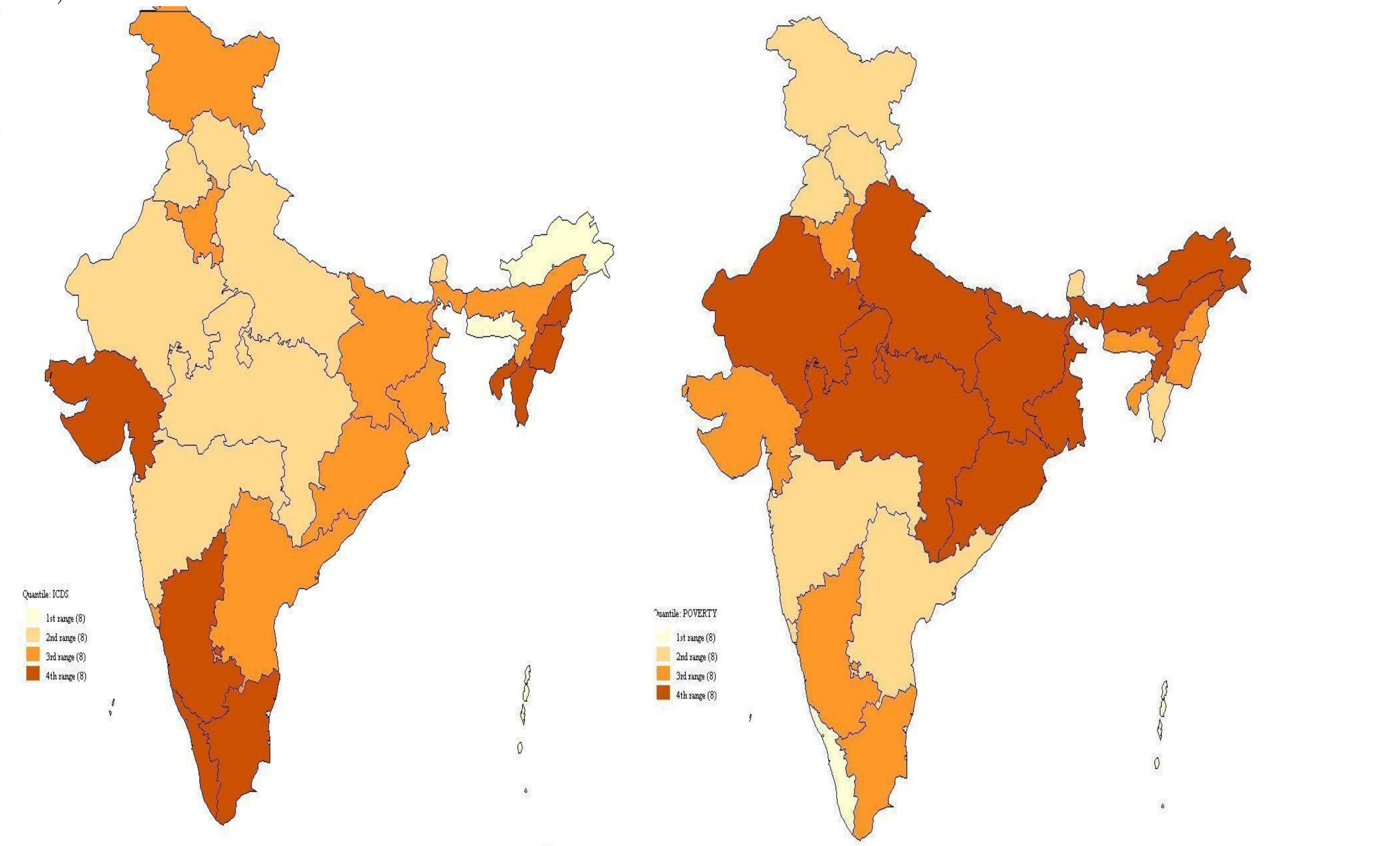

Figure 2: Quartiles of Stunting Outcome by State

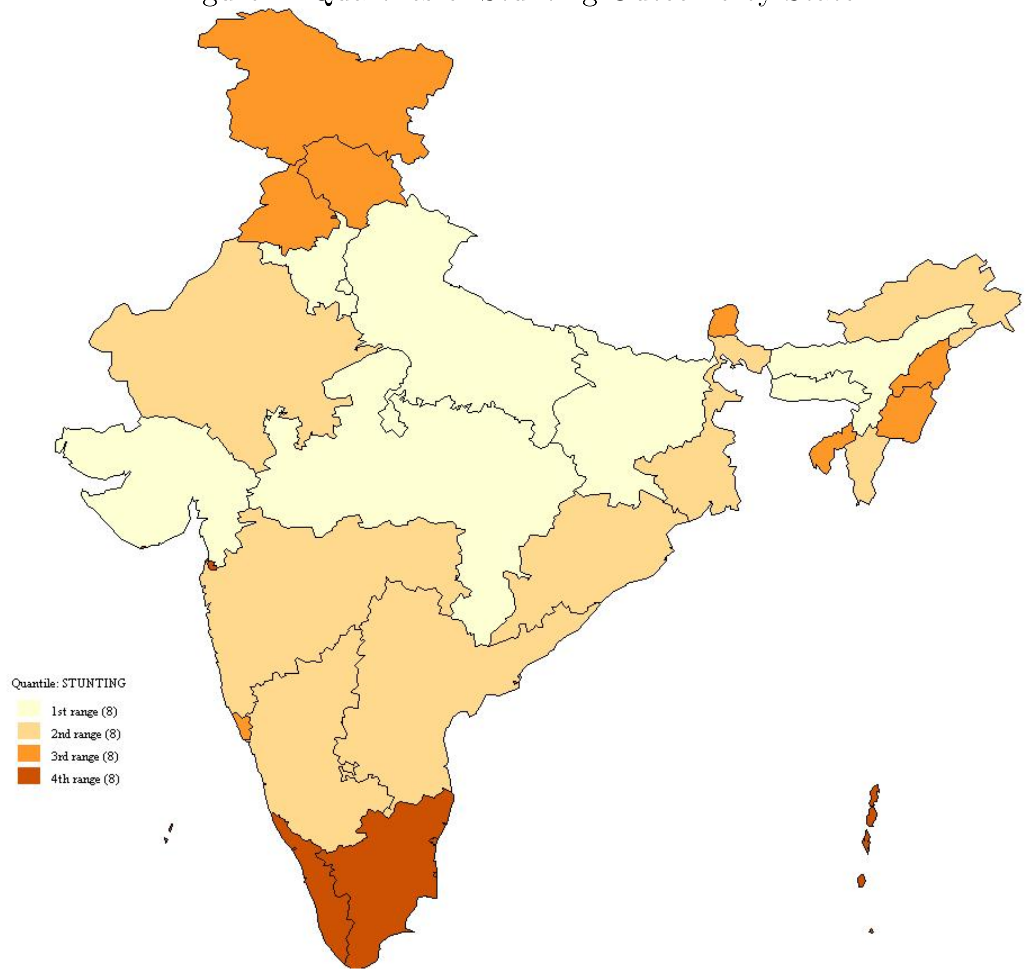

Figure 3: Kernel Density Estimates of Stunting Prevalence by ICS Coverage

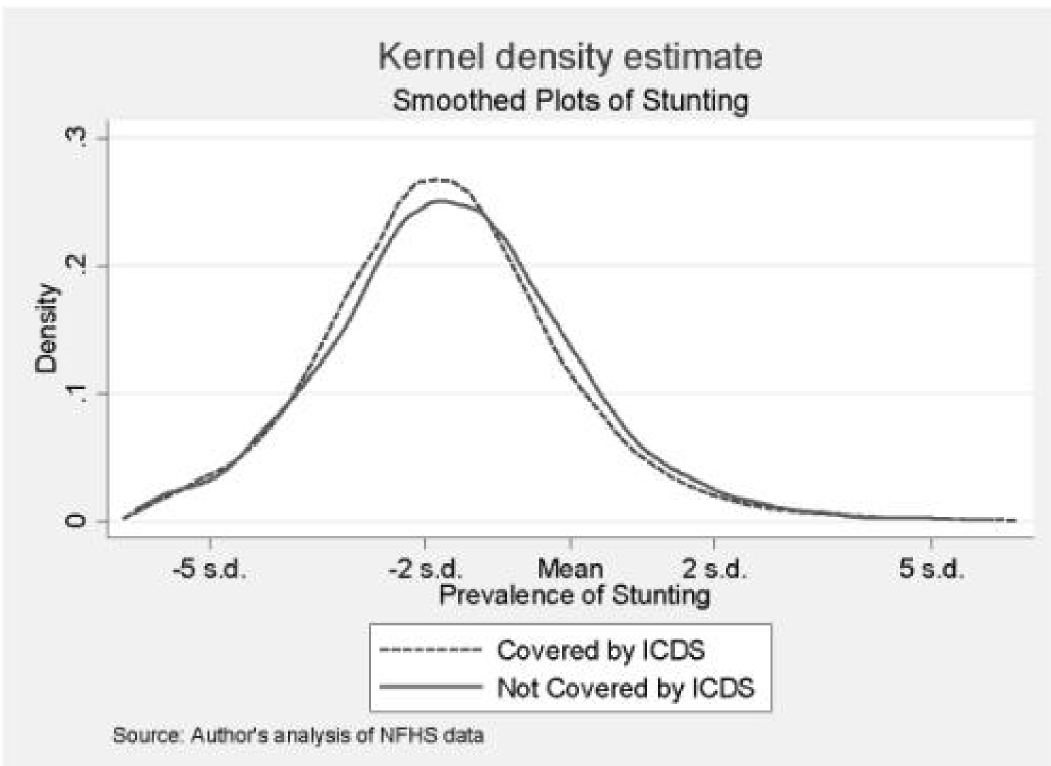

Figure 4: Kernel Density Estimates Stunting Prevalence by ICDS Coverage and Wealth Index Quintiles

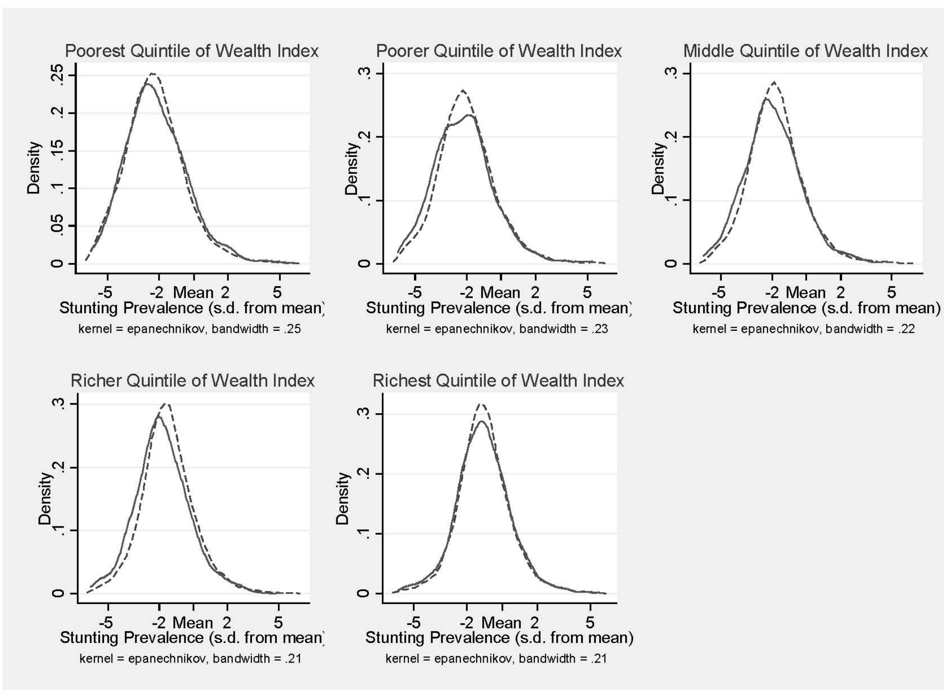

Figure 5: Distribution of State-wise ICDS Coverage

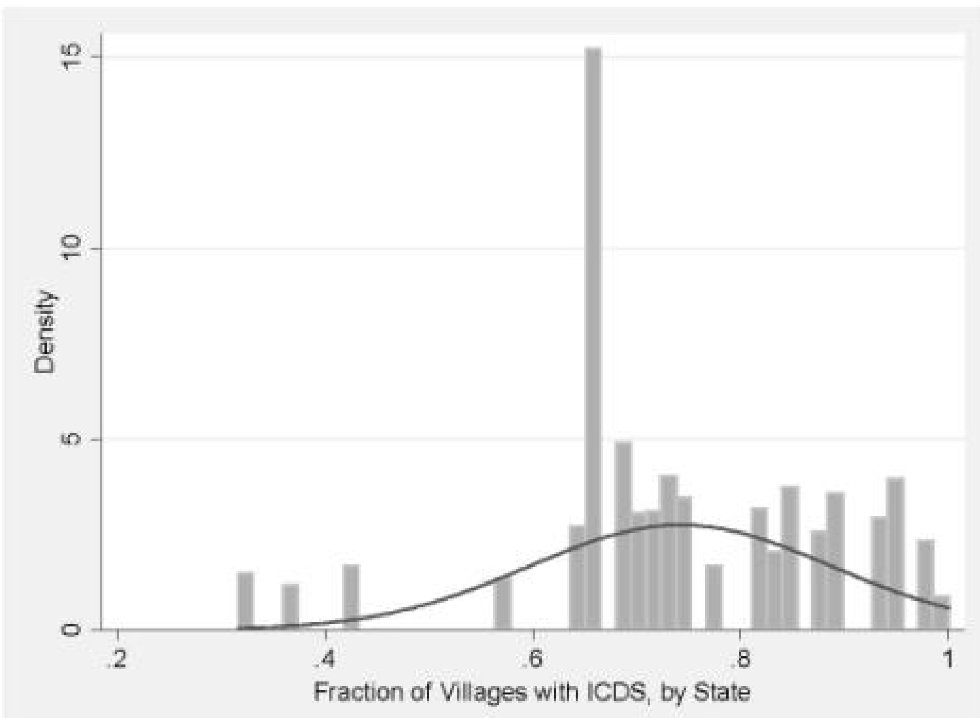

Table 1: Quartiles of Child Stunting

Mother's Characteristics	Mean	Standard Error
Age	26.8	5.37
Years of Schooling	3.90	1.6
Age at First Marriage	18.05	3.75
Births in Last Five Years	1.62	0.67
Total Births	2.92	1.83
Primary Education (percent)	15	
Secondary Education (percent)	38	
Respondent Currently Working (percent)	29.01	
Children's Characteristics	**Mean**	**Standard Error**
Age (years)	2.05	1.39
Stunting(standard deviations)	-1.71	0.66

Table 2: Quartiles of Child Stunting, in Standard Deviations from WHO Reference Mean

Quartile	Entire Sample	%Boys	%Girls
Lowest (25%)	-2.78	-2.81	-2.75
Middle (50%)	-1.76	-1.78	-1.74
Third (75%)	-0.72	-0.74	-0.70
Highest (100%)	2.90	2.80	3.02

Table 3: Maternal Education and Stunting Rates

Maternal Education	%Moderately Stunted	%Severely Stunted
0 years	57.2	31.6
$<$ 5 years	50.4	24.1
5-7 years	45.6	20.3
8-9 years	40.7	15.6
10-11 years	33.0	10.9
12 or more years	21.9	7.0

Table 4: Village Participation in ICDS

	Probit I Village has ICDS?	Probit II Village has ICDS?	Beta Regression Statewise ICDS Coverage
Ln(Population)	-0.075	0.088	-0.204^{***}
	(-1.16)	(1.23)	(-7.52)
Sex Ratio	-0.033	-0.142	0.159
	(-0.16)	(-0.67)	(1.72)
Average Wealth	-0.315^{***}	-0.196^{**}	-0.204^{***}
	(-5.34)	(-3.00)	(-7.31)
Average Land Holding	0.057	-0.002	-0.073
	(0.50)	(-0.02)	(-1.31)
Average Irrigated Landholding	-0.083	-0.079	0.156^{***}
	(-0.87)	(-0.63)	(3.43)
Mothers with Primary Education	0.391	0.517^{*}	0.277^{**}
	(1.78)	(2.07)	(2.67)
Mothers with Secondary Education	0.974^{***}	0.523^{***}	0.726^{***}
	(7.03)	(3.40)	(11.62)
Electrification?	0.125^{*}	0.077	0.097^{***}
	(2.33)	(1.41)	(4.30)
Rural	1.124^{***}	1.403^{***}	-0.017
	(14.15)	(14.61)	(-0.43)
Risky Water Source	-0.109	-0.294^{**}	0.216^{***}
	(-1.14)	(-2.63)	(4.89)
State Dummies	No	Yes	No
Constant	-0.227	-0.340	1.043^{***}
	(-1.00)	(-1.04)	(9.98)

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Unmatched and Propensity Score Matched Results of the Impact of ICDS on Stunting

	Entire Distribution			Lowest Quartile			Second Quartile		
Unmatched	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Treated	-1.77	-1.78	-1.74	-3.73	-3.76	-3.68	-2.25	-2.28	-2.22
Controls	-1.66	-1.69	-1.62	-3.77	-3.80	-3.73	-2.26	-2.27	-2.24
Difference	-0.11	-0.09	-0.13	0.05	0.04	0.05	0.01	-0.01	0.03
	(0.02)***	(0.03)***	(0.03)***	(0.02)***	(0.03)*	(0.03)*	(0.01)	(0.01)	(0.03)***
Matched	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Treated	-1.76	-1.78	-1.74	-3.73	-3.76	-3.68	-2.25	-2.28	-2.22
Controls	-1.86	-1.89	-1.82	-3.78	-3.8	-3.74	-2.27	-2.26	-2.26
Difference	0.10	0.11	0.08	0.05	0.04	0.07	0.02	-0.02	0.05
	(0.02)***	(0.04)***	(0.04)*	(0.02)***	(0.03)	(0.03)***	(0.001)**	(-0.01)	(0.01)***

Bootstrapped standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Average Treatment Effect for Children Younger than Two

	Entire Distribution		
Unmatched	All	Boys	Girls
Treated	-1.61	-1.66	-1.55
Controls	-1.49	-1.56	-1.40
Difference	-0.12	-0.10	-0.15
	(0.03)***	(0.04)***	(0.04)***
Matched	All	Boys	Girls
Treated	-1.61	-1.66	-1.55
Controls	-1.67	-1.76	-1.58
Difference	0.06	0.09	0.04
	(0.04)*	(0.05)*	(0.05)

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Average Treatment Effect for Children Younger than Three

	Entire Distribution		
Unmatched	All	Boys	Girls
Treated	-1.72	-1.76	-1.67
Controls	-1.61	-1.67	-1.55
Difference	-0.11	-0.09	-0.12
	(0.02)***	(0.03)***	(0.03)***
Matched	All	Boys	Girls
Treated	-1.72	-1.76	-1.67
Controls	-1.78	-1.85	-1.75
Difference	0.08	0.09	0.07
	(0.03)***	(0.04)***	(0.04)*

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: ICDS Impact by Years of Exposure

Unmatched	Less than One Year All	Up to Five Years All	Ten Years All
Treated	-1.66	-1.91	-1.85
Controls	-1.49	-1.66	-1.656
Difference	-.17	-0.25	-0.19
	(0.07)***	(0.03)***	(0.02)***
Matched	**All**	**All**	**All**
Treated	-1.66	-1.91	-1.85
Controls	-1.74	-1.93	-1.92
Difference	.08	0.03	0.07
	(0.08)	(0.04)	(0.03)***

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: ICDS Impact on Female Labor Supply (Intent-to-treat)

	Unmatched	Matched
Treated	0.32	0.32
Controls	0.24	0.30
Difference	0.08	0.02
	(0.01)***	(0.01)***

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: Unmatched and Propensity Score Matched Results of the Impact of ICDS on Stunting: NFHS-2 Data

Unmatched	All	Boys	Girls
Treated	-1.72	-1.73	-1.71
Controls	-1.77	-1.73	-1.80
Difference	-0.05	0.00	-0.09
	(0.02)*	(0.03)	(0.04)***
Matched	**All**	**Boys**	**Girls**
Treated	-1.72	-1.73	-1.72
Controls	-1.75	-1.73	-1.77
Difference	0.03	0.01	0.05
	(0.03)	(0.04)	(0.04)

Bootstrapped standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$