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 child nutrition by providing nutritional supplements and pre- and post-natal services to targeted villages. However, previous evaluations find that ICDS fails to reduce malnutrition, and program placement does not uniformly target vulnerable areas. I use new data to reevaluate ICDS on several dimensions; in contrast to previous studies, I find significant treatment effects particularly for the most malnourished children. However, results suggest targeting does not work uniformly well. While ICDS effectively targets poor areas, it fails to target areas with low levels of average education or those with unbalanced sex ratios.

1.1 Motivation

Malnutrition in the early years of an infant's life often leads to lower educational attainment and lifetime earnings (Alderman et al., 2006). India's Integrated Child Development Services (ICDS) is one of several integrated child development programs in developing countries around the world. These integrated programs target long-term nutrition and development of children. However, much of the literature evaluating these programs tends to find little or no evidence of significant, sizable causal effects on chronic child malnutrition. Such causal effects depend on the details of the program and on their context; reductions to long-term child malnutrition need not occur homogeneously with a direct effect. Further, most such programs are endogenously placed to target areas of most need making effective placement of centers crucial to their success. 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 chronic child malnutrition is relevant not only for Indian policy-makers but also for similar

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program design in other developing countries.

In this paper, I evaluate the ICDS on two main counts: whether the program has a positive treatment effect on the long-term nutrition of targeted children and whether program placement effectively targets vulnerable segments of the population. I use data from the nationally representative Indian National Family and Health Survey of 2005-06 or NFHS-3 (IIPS and ORC Macro, 2007). The best way to evaluate the long-term effectiveness of the ICDS is with an index of chronic malnutrition: stunting. Stunting reflects long-term damage to a child's nutritional status.[1,2]

Although real Indian GDP per capita doubled in the last fifteen years (WDI, 2007), child stunting only decreased by sixteen percent over the same period: 69 percent of children under five were stunted in 1992-93 (NFHS-1), 68 percent in 1998-99 (NFHS-2), and 58 percent in 2005-06 (NFHS-3). Further, data from the NFHS-3 show that 45.9 percent of all Indian children are severely undernourished.[3] The Indian government takes a two-step approach to reducing child malnutrition: a Public Distribution System makes food available at subsidized prices, and the Integrated Child Development Services (ICDS) provides nutritional supplements, bundled child and maternal services, and day-care facilities to targeted households. The ICDS has been in place since 1977 and although it cost approximately $1.5 billion in 2008, previous evaluations using data from 1998-99 and earlier failed to show its effectiveness (Lokshin et al., 2005; World Bank, 2007a). Recently, the World Bank recommended that the Indian government redesign the ICDS for a total price tag of $9.5 billion. Given the hefty price tag of redesign, the potential impact on poor households and the availability of new data, the impact of the ICDS must bear closer evaluation and rigorous analysis.

This evaluation of the ICDS differs from the literature on several counts. First, since ICDS targets children who would otherwise be malnourished, I use Propensity Score Matching to control for endogenous placement. While most evaluations of such program only estimate an Average Treatment Effect for the entire sample, I also estimate treatment effects for children in the two lowest quartiles of stunting to determine whether the program decreased stunting rates in most-at-risk children. Second, since changes in child nutrition often depend on changes in parenting behavior, improving child stunting may require a contextual or learning effect. To find evidence of such an effect, I examine whether ICDS centers exhibit a time trend in effectiveness. Third, past analyses of ICDS have come primarily from summary statistics of nutritional outcomes or simple probit analyses of program placement. This paper conducts econometric analysis of placement, treatment, and time effects of the ICDS. Fourth, most analyses of program placement (for ICDS, see Lokshin et al., 2005) rely on probit regressions to study targeting, but the distribution of state-wise ICDS coverage exhibits negative skewness which violates normality of the probit link function. This paper uses beta regression to control for the negative skewness of coverage to determine whether program placement works as intended. Comparison with probit specifications highlights the importance of accounting for skewness. Finally, this paper uses newly available household survey data as well as supplemental budget data from the Indian government to evaluate the ICDS. These different approaches enable me to find unambiguous evidence that the ICDS significantly reduces child stunting in India.

To estimate the average treatment effect of the ICDS and to examine placement design, I conduct my analysis in two steps: first, Propensity Score Matching (PSM) identifies the effect of ICDS on stunting. Next, probit and beta regression examine 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. PSM shows ICDS reduces average stunting by approximately six percent; this effect size is larger than treatment effects from similar programs in other developing countries. Results also yield evidence of significant treatment effects for the worst-off children, girls in particular. I also find that ICDS centers take up to ten years to significantly affect stunting, consistent with contextual or correlated effects influencing parenting behavior. Treatment effect estimates thus suggest the program significantly reduces chronic child malnutrition.

Placement results suggest that while ICDS effectively targets poor areas with risky water sources, sex-ratios and landholdings do not play a significant role in placement. ICDS targets areas with more educated mothers, which appears regressive because villages with fewer educated people might benefit most from the intervention. I also find that voting patterns influence the allocation of national ICDS funds to states while the states' chronic child malnutrition levels do not. In summary, my results show that while the ICDS significantly reduces child stunting, program placement does not work perfectly; villages of most need are not uniformly targeted and although they should not matter, political alliances play an important role in budget allocation. This paper contributes by being the first in this literature to estimate distributional treatment effects for child stunting. This paper also highlights the importance of accounting for the nature of the data distribution in estimating, say, program placement. Finally, this paper is one of few to consider the contextual effects of programs like the ICDS.

2 ICDS: Background and Monitoring

Evaluations of integrated child development programs in most 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 childhood nutritional supplements have a small impact on child stunting. Behrman and Hoddinott (2005) find that the Mexican PROGRESA caused a three percent decrease in the probability of a child being stunted. Stifel and Alderman (2006) study a Peruvian milk subsidy program, *Vaso de Leche* to find that although the intervention decreased overall malnutrition by 28 percent, it reduced child stunting by only three percent. Thus the lack of evidence of a large and significant effect of ICDS on stunting appears to be the norm rather than an exception. Worldwide, chronic malnutrition as measured by stunting appears to be the hardest measure to improve.

ICDS targets the physical and psychological development of children younger than six in the most vulnerable and economically disadvantaged sections of the population. Village ICDS centers provide food supplements, health services like immunizations and referral services, and information on nutrition and health. Centers also provide early childhood care, daycare and preschool education (Ministry of Women and Child Development). The government uses ICDS to target poor and less-developed areas. Targeting occurs at two levels– national and then state. The national government provides each state with an ICDS budget based on state-level development characteristics. Each state then uses its ICDS budget to place centers in villages based on village-level development characteristics. Community-level surveys and the enumeration of families living below the poverty line provide national and state governments information on development characteristics like poverty rates, infrastructure, and health outcomes. The government also hopes to reduce the incidence of female infanticide and feticide by placing ICDS in areas with significantly fewer girls than boys. ICDS centers provide information on the benefits of having (and educating) a girl child.

Reports evaluating ICDS tend to concur that while the program is well-intended, its effectiveness is limited by implementation issues. The World Bank (2007a) finds that while using all the services provided by local ICDS centers might result in health and nutritional benefits, most families use only nutritional supplements, immunization services, or day care facilities, which yield insignificant benefits. Other studies have identified similar limitations, 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. While full utilization of ICDS services results in a significant improvement in stunting, wasting, and weight-for-age, partial utilization has a much smaller impact.

The multi-agency Indian Coalition for Sustainable Nutrition Security contends that food supplementation appears to be the key service delivered by the ICDS, although such supplementation may not be the optimal nutrition intervention. The Coalition argues that immunization services and parental counseling may have a greater effect on stunting than food supplements. However, researchers have found that ICDS 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 and find that participation in an ICDS center affects neither breastfeeding patterns nor the involvement of the mother in the child's growth monitoring. Gragnolati et al. (2006) observe that ICDS centers provide minimal parental counseling and often lack linkages with the health sector. Although much of an individual's nutritional status is determined in the first three years of life, Lokshin et al.(2005) find that ICDS services are more likely to reach children older than three.

The ICDS redesign project is motivated by 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.(2006) and the World Bank (2007a) is based solely on summary statistics of earlier waves of the data I use in this paper. Other major evaluations by NIPCCD (1992) and Lokshin et al. conduct rigorous econometric analyses but also use older data. In contrast, work done on the most recent Indian household survey provides evidence of a small but significant negative correlation between ICDS and child stunting, particularly in the lower quintiles of child stunting (Kandpal and McNamara, 2008b). The Indian government places ICDS centers in target areas, but most previous studies evaluating ICDS fail to control for such endogeneity, leading to downward biased results. The notable exception to this literature is the evaluation by Lokshin et al. which uses matching techniques to control for targeted program placement. However, Lokshin et al. restrict their analysis of ICDS impact to estimating average treatment effects for the entire distribution. Since the ICDS aims to improve child health in villages of most need, this paper estimates treatment effects for the worst-off children (lowest quartiles of stunting) in addition to average treatment effects for the whole sample.

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 third in a series of national surveys. The first NFHS survey was conducted in 1992-93 and the second in 1998-99. The 2005-06 wave of the NFHS used a Demographic and Health Survey questionnaire to ask a special module of questions of a randomly-chosen sample of 36,850 women who had given birth to at least one child in the past five years.[4] This sub-sample covered 3842 villages in all 29 Indian states.[5] 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 provides the necessary stunting data for my analysis. Anthropometric measures are not reported for 1385 women and their children or slightly over four percent of the sub-sample.[6]

In previous rounds, the NFHS provided district- and village-level data which could be used to compute the probability of a village hosting an ICDS center. These data included 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, village sex ratio, percentage of mothers with primary and secondary education, and whether the village had electricity. The NFHS-3 includes HIV testing data for a small sample of the population, so any geographic identifiers below the state-level were scrambled to protect the identity of tested individuals. As a result, village-level characteristics are no longer available, although it is possible to tell which people live in the same village. Therefore, to determine the likelihood of a village receiving ICDS coverage, I developed village-level aggregates using available data. I was able to 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. While the resulting data are as close as possible to the earlier waves, I do not have information on distance to the nearest town, connection to all-weather roads, presence of other development programs, or history of epidemics. Robustness checks presented at the end of this paper show that results are unlikely to have been contaminated by the lack of these village-level data.

3.2 Summary Statistics

Table 1 tells us the average woman in the sample of mothers was 27 years old and had completed four years of education. Her first (and in most cases only) marriage occurred when she was eighteen and she had an average 3 births ever and 1.6 births in the past five years. Only 29 percent of surveyed respondents were working at the time of the survey. 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. The average child in this sample was two years old, so most children had lived in either an ICDS village or a non-ICDS village their entire lives.

Table 1 also shows the average child in this sample 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, significant at the 95 percent level, which means that on average boys were significantly more stunted. Table 2 illustrates considerable variation in the distribution of child stunting. The lowest 25 percent are on average 2.78 standard deviations below the WHO reference population mean, while the highest 25 percent are 2.89 standard deviations above the mean. A shortfall of three standard deviations from the mean translates to severe stunting, so the children in lowest 25 percent are close to severely stunted. Girls appear to be slightly better off than boys in all four quartiles. Also note that only the healthiest 25 percent of children are above the WHO reference population mean.

If the ICDS effectively targets poor areas, we would expect to see a positive correlation between the percentage of districts in a state covered by the ICDS and the percentage of poor households in that state. Figure 1 presents a quantile map of the state-wise percentage of districts covered by ICDS, while Figure 2 presents a quantile map of the percentage of population in each state that lives in the two lowest quintiles of the wealth index. A darker color in Figure 1 indicates a higher percentage of districts covered by ICDS, while a darker color in Figure 2 indicates a higher poverty rate (as defined by the percent of households living in the two lowest quintiles of the wealth index distribution). These figures show an apparent negative correlation between ICDS prevalence and poverty rates for many central states and some Northeastern states. Even though ICDS coverage should be highest in the poorest states, implementation issues result in faulty placement. Such problems in placement are consistent with previous analysis of NFHS-1 and NFHS-2 data (Lokshin et al., 2006).

Figure 3 presents a quantile map of stunting rates. A darker color represents a lower average level of stunting. A few states have relatively few stunted children, high poverty rates, and high ICDS coverage. In general, however, poor states with high rates of stunting tend to have relatively low rates of ICDS coverage. These maps suggest that ICDS targeting may not be entirely effective because several states with high rates of stunting may not be adequately covered by ICDS. I 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.

Since ICDS centers are endogenously placed to target poor villages, I expect household wealth to be 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.[7] Figure 4 shows kernel densities of stunting for children from ICDS villages and those from non-ICDS ones, without controlling for income. The distribution of stunting for children from ICDS-covered districts has a higher mass below the mean, which suggests these children are more likely to be stunted and that centers are placed in areas of most need but do not appear to significantly improve stunting.

Figure 5 presents a series of five kernel density estimates showing the distribution of stunting rates for children living in ICDS villages and those from non-ICDS villages, by quintiles of the wealth index. In the poorest quintile, ICDS appears to decrease the likelihood of being stunted: children from ICDS villages are more likely to be just above the mean. Similarly, in the next two poorest quintiles, ICDS covered children seem less likely to be below the mean than those from non-ICDS areas. These graphs indicate that among the poorest, children from ICDS villages have better stunting outcomes than those from non-ICDS villages. The picture is perhaps clearest for the fourth quintile.

In the fourth quintile, the ICDS appears to shift out the distribution of child stunting: ICDS covered children are less likely to be below the mean and more likely to be at or above the mean. 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 emphasize the importance of controlling for income: when not 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.

4 Empirical Analysis

4.1 The Impact of the ICDS

Endogenous program placement leads to selection bias because individuals are treated if their observed value of some outcome is significantly different from the value of the same outcome for untreated individuals. To control for bias from 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, on average, 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. Like Lokshin et al., I use PSM to measure the impact of ICDS on child stunting. However, Lokshin et al. only estimate the average treatment effect on the treated (ATT) of the ICDS over the entire survey sample which may have masked a positive impact on target groups. Quantile regression results suggest that the ICDS has a positive impact at the left-tail of the distribution of child stunting (Kandpal and McNamara, 2008), so in this paper I extend Lokshin et al.'s analysis by estimating treatment effects by quartiles of stunting and of propensity scores, in addition to estimating an ATE for the full sample.

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 the balancing condition is satisfied (Cameron and Trivedi, 2007). Rubin (1973) shows that PSM eliminates selection bias if selection bias from endogenous placement is eliminated by controlling for $\mathbf{x}$.[8] Each child in ICDS areas is matched with replacement with one from areas without ICDS based on the propensity score of each child. In this paper, I use kernel matching in which all treated observations are matched with a weighted average of the propensity score for all control observations. Weights are inversely proportional to the distance between the propensity scores of treated and control observations (Becker and Ichino, 2002). 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 (Lokshin et al., 2005), and then test for the significance of differences in average stunting outcomes. For the purposes of this paper, I maintain the unconfoundedness assumption (Imbens and Wooldridge, 2009):

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

where $\perp$ signifies independence. 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.

I conducted PSM in STATA using *psmatch2* provided by Leuven and Sianesi (2003) for the sample of 30,521 ICDS-treated and 9,425 control children for whom the NFHS-3 provides anthropometric measures. Table 3 presents the results of matching analysis for the entire distribution and the two lowest quartiles of the stunting distribution. 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, the ICDS appears to have a significant *negative* impact on child nutrition. In contrast, matched results in the lower panel tell us that ICDS significantly decreases stunting. Children who live in ICDS villages are, on average, ten percent of one standard deviation– or 5.6 percent– closer to non-stunted status than the average child from a non-ICDS village. Over the entire sample, the ICDS has a greater effect on the stunting rates of boys: boys from ICDS villages are over six percent closer to mean height-for-age than boys from non-ICDS villages, while treated girls are only four percent closer to the mean than untreated girls. These results contrast with Lokshin et al.'s insignificant (and sometimes negative) estimates of ATT from matching on NFHS-2. The major difference between this paper and the older one by Lokshin et al. is the sampling wave, which suggests that ICDS has become more effective in the five years between the second and third waves of the NFHS.

In contrast to results over the entire sample, matched results for the lowest quartile of child stunting show that girls benefit more from the ICDS compared to boys. Note the magnitude of decrease in stunting due to ICDS is smaller than for the entire sample: treated girls are only two percent closer to not being stunted and treated boys are not significantly better off than the untreated. However, stunting is a non-linear index and the decrease from -3.78 to -3.73 is harder to effect than the decrease from -1.86 to -1.76. Results for the second quartile of stunting (lowest fifty percent of the sample) exhibit similar effects.

These results suggest that the worst-off girls benefit disproportionately from ICDS relative to the worst-off boys. Rose (1999) documents the presence of a "son syndrome" in some poor and rural parts of India. This son syndrome suggests 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, the worst-off girls might 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.

In addition to estimating treatment effects for quartiles of child stunting, I also estimate treatment effects for quartiles of the propensity score. Treatment effects for the distribution of child stunting impose the structure on matching that children in the same quartile of stunting are more similar than children in other quartiles of stunting. Treatment effects for the distribution of propensity scores, on the other hand, impose the structure that children with similar propensity scores are more alike. Since propensity scores are calculated using several observables, not just the stunting outcome, these quantile treatment effects may be more reliable than those obtained for the distribution of stunting. Table 4 presents estimates of quartile treatment effects for the distribution of propensity scores. In this table, higher quartiles appear to the left while lower quartiles appear to the right. This reversal in order of quartiles reflects the fact that children with higher propensity scores, i.e. those more likely to be treated, also have the worst stunting outcomes. Thus highest quartiles of propensity score represent the worst-off children.

Although treatment effects for the two highest quartiles of propensity scores are similar in magnitude to the average treatment effect, most results are not statistically significant. This lack of statistical significance may result from the relatively small control groups, particularly in the higher quartiles of propensity score, which leads to restricted variation and therefore lower statistical significance. Particularly in the quartile most likely to be covered by ICDS, most observations did in fact have ICDS coverage which suggests, to a certain extent, that program placement works. The estimated treatment effect of thirteen percentage points translates to a seven percent reduction in stunting, close to the estimated six point reduction for the overall population. Boys in the third quartile of propensity scores see an even larger reduction in stunting: their treatment effect of eighteen percentage points corresponds to an eight percent reduction in stunting. Although the treatment effects for girls are insignificant, the estimates are positive and their magnitudes (a difference of 0.10 or 0.12 corresponds to about a five percent reduction in stunting) are similar to the four percent reduction estimated for the entire sample of girls. So, ICDS significantly reduces stunting in the children most likely to be treated.

Since nutritional status is largely determined in the first two or three years of an infant's life, I estimate ICDS impact on stunting for children younger than three. Table 5 presents evidence of significant positive treatment effects for children less than three of both sexes. Once again, we observe the importance of controlling for endogeneity via matching since unmatched results show a significant and large negative effect of the ICDS. These results suggest that ICDS significantly improves the nutritional outcomes of children in their vital formative years.

The behavioral changes needed to significantly affect stunting may be difficult to achieve and may not occur immediately after an ICDS center is placed in a village. Contextual effects or correlated effects may cause ICDS centers to have an effect only have they have been in a village for a few years. Contextual effects occur when people's behavior depends on the distribution of group characteristics, while correlated effects are when people from a group exhibit similar behavior because they are alike or live in similar environments (Manski, 2007). Further, since the ICDS is targeted to areas with higher levels of women's education, increases in women's education that have occurred over time may cause similar time delays. A third reason might be a gradual increase in health-seeking behavior as a result of exposure to ICDS (Alderman, 2007). To study the possibility of delay from contextual or correlated effects, I conduct PSM on stunting outcomes by duration of ICDS presence in the village. These results, presented in Table 6, 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 from not-stunted status. In summary, results show ICDS significantly reduces stunting among all children, as well as among the worst-off and the very young; however, these effects can take up to ten years.

4.2 Program Placement

The presence of significant positive treatment effects for the two lowest quartiles of stunting indicate that ICDS provides vital assistance for the most at-risk Indian children. In order to determine whether the program effectively targets these children, I study the placement of ICDS centers in this section. As described above, national and state governments target ICDS centers to reach the most vulnerable households in low-income areas with unbalanced sex ratios, inferior infrastructure, and poor development outcomes. If placement is effective, state- and village- level values of the target criteria should influence the size of funds allocated at each level. In this section, I examine program placement at both levels to see whether the allocation of funds and placement of centers actually follows the stated criteria. To study national-level placement, I examine the determinants of the amount of national ICDS funds allocated to a state. However, since the data do not allow geographic identification of districts, I cannot use budget allocation for state-level placement analysis. Instead, I use two dependent variables on the placement of ICDS centers in a village: the first is whether a village has an ICDS center and the second is the proportion of villages in a state that have ICDS centers.

I estimate state-level coverage as a function of available and constructed village characteristics like population, the share of girls of the population, average wealth of the village, average landholding (in acres), average number of acres irrigated, electrification, average distance to water source, and whether the water source is unimproved.[9] 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 capture unobserved community-level factors which partly determine participation probability, like the presence of other development programs focused in rural areas.

Lokshin et al. study the placement of ICDS centers at the national- and state- levels using probit analysis with and without state dummies. They use the specification without state dummies for national level placement, and the one with state dummies for state-level placement. However, as Figure 6 shows, the state-wise distribution of ICDS centers has negative skewness. Probit analysis assumes normally distributed errors and the probit link function derives from the normal distribution, both of which invalidate the use of probit for evaluating asymmetric distributions like the one in Figure 6. 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. The key assumption underlying beta regression is that the parameters are beta distributed.

To demonstrate the value of accounting for negative skewness in state-level ICDS coverage, I estimate both probit specifications used by Lokshin et al. (with and without state dummies) and the beta regression discussed above. The dependent variable in the beta regression is the state-wise proportion of villages covered by ICDS. This specification presents an alternative to probit in studying state-level placement. I compare results from the beta regression to results from the two probit specifications and highlight the differences in determinants of state-level placement. The first column (probit I) in Table 7 presents probit estimates without state dummies (national-level placement in Lokshin et al.), while the second column (probit II) presents estimates with state dummies (state-level placement in Lokshin et al.). Beta regression estimates are presented in the third column of Table 7. 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 I). These AIC outcomes are not surprising because not using state-level indicator variables omits considerable state-level heterogeneity. So, I treat the probit II specification (with state indicators) as more reliable of the two probits. Nonetheless, given the negative skewness of state-wise ICDS coverage, I contend that beta regression results best describe state-level ICDS placement.

Although government policy says ICDS targets large population points, results from the beta regression show that an increase in average village population decreases national-level ICDS coverage. However, probit II results suggest that the government does target large population centers; this contrasting result emphasizes the importance of controlling for negative skewness. This beta regression result also contrasts with Lokshin et al.'s finding of a significant positive correlation between population and ICDS coverage at the state level. The changes in sign and significance suggest that ICDS now targets smaller population points; perhaps an indicator of overall expansion in ICDS coverage over time. Also in contrast to stated policy, I find that 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 is consistent with Lokshin et al.'s results. In keeping with policy, poorer villages are more likely to receive ICDS coverage at the state level which is also consistent with Lokshin et al.'s findings. However, in contrast to probit specifications and stated target criteria, beta regression results indicate that rural areas are not significantly more likely to receive ICDS coverage. Once again, the contrast between beta and probit specifications highlights the importance of accounting for distributional characteristics.

Villages with larger irrigated landholdings receive more ICDS coverage, which contradicts the policy of targeting areas with poor infrastructure. Here again, results underline the difference between probit and beta specifications as probit analysis suggests that size of irrigated landholdings does not significantly influence placement. All three specifications show that areas with higher fractions of mothers with primary or secondary education receive greater ICDS coverage at both levels of placement. Lokshin et al. report an insignificant positive correlation between the proportion of educated mothers and ICDS coverage; the increase in significance from their analysis to mine suggests that the government increasingly targets areas with more educated people. Electrification significantly increases the probability of participation in the beta regression (and in probit I). The electrification dummy was highly significant and positive in both specifications in Lokshin et al., so the lack of significance in probit II again suggests ICDS is being expanded to cover more disadvantaged areas. Lack of access to an improved water source significantly reduces participation probability according to probit estimates, although beta regression says it actually increases coverage.

Placement results underline important differences from stated policy: unbalanced sex ratios do not influence state-level coverage, and electrified villages are more likely to receive ICDS coverage. Areas with more educated mothers are targeted rather than areas with worse levels of educational attainment. Population is inversely correlated with state placement, although stated policy is to target large population points. On the other hand, a decrease in average wealth increases state-level ICDS coverage which is consistent with the stated objective of targeting poor areas, and a lack of access to improved water sources increases placement probability. Thus targeting appears to work for wealth and sanitary conditions, but fails in other important aspects like sex ratio, average educational attainment, and infrastructure. Villages without many educated mothers or electrification may also benefit most from ICDS, so the government should improve coverage of these villages.

In order to study national-level placement of ICDS centers, I examine what determines the disbursement of ICDS funds from the national government to the states. Results presented in Table 8 show that given the average wealth of the state, the higher the percent of the state's votes that went to the political alliance that won the national election of 2004, the greater were the ICDS funds that state received from the national government in 2005-06. In contrast, the state's observed level of child malnutrition did not significantly influence the allocation of national-level funds. These results highlight the problems in national allocation of ICDS funds. Political alliances should not affect the funds a state receives from the federal government but they do. Malnutrition levels should affect national allocation of ICDS but they do not. Such errors in placement design almost certainly limit the effectiveness of the ICDS.

5 Sensitivity Analyses

A concern with this analysis may be that stunting may not be the best measure of ICDS treatment effect. I conducted PSM on wasting (a measure of acute wasting) and anemia (a measure of inadequate blood hemoglobin) to find significant average treatment effects for both outcome variables. However, as asserted in the introduction, ICDS aims to improve long-run nutritional outcomes of children, so stunting which is an index of chronic malnutrition is the best measure of ICDS' effectiveness. Other potential problems with this analysis include uncertainty over the quality of matching and bias from unobserved village-level characteristics contaminating the treatment effects. To address these potential problems, I conduct the following robustness checks.

5.1 Quality of the Matching

Since ICDS centers are placed to target worst-off areas, malnutrition levels might also be worse in villages with ICDS. Failing to account for targeted placement results in downward bias that might lead one to find that children from ICDS villages are worse-off than those from non-ICDS villages. In general we expect matching to reduce the bias from targeted placement, but the ability to effectively reduce such bias, of course, depends on the quality of the matching. Several studies show that PSM is an efficient econometric technique for non-experimental estimations of treatment effect. Rubin (1973, 1979) shows that PSM efficiently controls the bias from matching. Dehejia and Wahba (2002) demonstrate the ability of PSM to replicate the experimental benchmark when observed treated samples are very different from control samples. Figure 7 presents kernel density plots of weighted propensity scores for treated and untreated observations used in matching the entire sample. This figure shows the propensity scores for the two samples closely track each other, representing "goodness" of matches. Further, t-tests show matching significantly reduces the bias in unmatched sample means for most variables.[10] Even in cases where matching fails to remove most of the bias between treated and control observations, the magnitude of the difference in means is small. For instance, matching reduces bias from differences in birth order number by 0.1 percent, but the average treated child is 2.65^{th} in birth order, and the average untreated child is 2.73^{th}, after reduction in bias. The economic difference between an average birth order of 2.65 and 2.73 is small and unlikely to meaningfully bias the treatment effects. PSM appears to effectively reduce the bias from targeted program placement.

5.2 Bias from Unobserved Village-level Characteristics

Omitted variable bias from the lack of village-level information on development characteristics, presence of other programs that might indirectly affect child health, and proximity to an administrative headquarters could contaminate PSM estimates of the effect of ICDS. To examine whether such unobserved characteristics cause an (upward) bias in PSM results, I replicated the village-level aggregates I used in my analysis above, this time using data from NFHS-1 (1992-93) and NFHS-2 (1998-99). Lokshin et al. (2005) report insignificant treatment effects of ICDS using NFHS-1 and NFHS-2 data, so significant estimates for these waves using the village-level aggregates I employed in my analysis would suggest that the unobserved village characteristics are indeed contaminating my ICDS treatment effect estimates. However, as Table 9 and Table 10 show, even using the village-level aggregates I developed for NFHS-3, the ICDS treatment effect is insignificant for the two earlier waves of data. This result is consistent with results presented by Lokshin et al. (2005). Although all estimates are insignificant, it is worth noting the similarity in effect sizes: for instance, Lokshin et al. report an ATT of 0.056 in 1992-99 and 0.024 in 1998-99. The corresponding effects presented below are 0.05 and 0.03. For boys in 1992-93, Lokshin et al. find a significant treatment effect of 0.15, while my estimate is 0.14 and close to being significant (t-statistic of 1.79). For boys in 1998-99, Lokshin et al. report a difference of 0.09, while my results indicate a difference of 0.01. These robustness checks show that unobserved village-level characteristics do not seem to introduce positive bias to my estimates of ICDS treatment effect.

6 Conclusion

India's primary child nutrition intervention, the Integrated Child Development Services aims to improve the physical and psychological well-being of children under the age of five using targeted program placement. However, previous literature studying this program finds that the ICDS has little or no effect and that it does not target the right children– the poorest of the poor and the very young. Most of this literature does not control for the targeted placement design of ICDS leading to downward biased estimates of its effectiveness. Nonetheless, based on such evidence, the World Bank recommended an expensive ICDS redesign project to the Indian government. In the light of this redesign project being underway and potential errors being costly, this paper reexamines the ICDS using new data and different econometric techniques.

This paper analyzes the ICDS on several dimensions, including estimating treatment effects for the worst-off children as well as for the entire distribution, and program placement, while controlling for endogenous placement. Results show evidence of effectiveness for some goals. I find that ICDS shifts out the marginal distributions of stunting for the worst-off children. Results also show that although ICDS centers take up to ten years to have a significant effect, nonetheless ICDS decreases average child stunting by about six percent. I improve on previous analyses by controlling for negative skewness in the distribution of state-wise ICDS coverage. I find that placement does not uniformly target less-developed areas and that not controlling for skewness can lead to misleading policy conclusions. Finally, evidence suggests voting patterns influence national-level budget allocation, which might hamper the effectiveness of ICDS.

The UNICEF estimates that child malnutrition costs India up to four percent of its national income. This paper finds ICDS reduces the shortfall to not-stunted (i.e. chronically well-nourished) status by an average of six percent. If we assume stunting is the only form of child malnutrition to decrease Indian GDP, the ICDS intervention increases Indian GDP by .0024 percent (since 0.06*0.04 equals 0.0024). The 2007 estimate of Indian GDP was $2.97 trillion. Now, 0.0024 percent of $2.966 trillion equals $7.118 billion. So, in the absence of the ICDS intervention, the Indian GDP in 2007 would have been $7.118 billion lower at $2.958 trillion. Since ICDS costs $1.5 billion per year, the net increase in Indian GDP from the ICDS was $7.118 billion less $1.5 billion, which equals $5.618 billion. This net benefit of $5.618 billion equals a 3.75 fold return on ICDS expenditure. In other words, every dollar spent on the ICDS in 2007 returned up to $3.75 dollars net of cost. The net benefit of $5.618 billion results in a per capita benefit of $4.89. Indian life expectancy at birth is 69.89 years (WDI, 2007), so each dollar invested in ICDS returns up to $341.45 over the lifetime of each individual. Thus even in its current form, ICDS generates substantial returns for the Indian government, a fact the redesign team should keep in mind.

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 bias would likely be in the downward direction; in this case, the results presented here would be a lower bound. Further, since the NFHS does not include information on utilization of other services provided by the program, I can only study the effect of ICDS treatment on child health. These qualifications notwithstanding, my results suggest ICDS reduces child stunting particularly among the most vulnerable. However, benefits may increase if the program is strengthened to better target the least-developed regions. Current flaws in placement may be one reason the ICDS has thus far failed 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.

Notes

[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.

[2]Other measures of child malnutrition including wasting and anemia fluctuate more readily in response to recent food intake. 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.

[4]Demographic and Health Surveys (DHS) do 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, jewelry, 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.

[5]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 was selected in two stages: first stage selection of primary sampling units (villages) with probability proportional to population size, followed by the random selection of households within each village (IIPS and ORC Macro, 2007).

[6]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, despite living in an ICDS village. If this case is true, results would overestimate the impact of having an ICDS center in the area. While missing measurements may introduce a source of bias, I am unable to conclusively determine the direction of this bias.

[7]As a robustness test, I also conducted PSM using metrics other than kernel. These metrics included Mahalanobis, nearest neighbor, and caliper. Results were similar in size and significance to those presented below.

[8]In the current data, every community-level characteristic which should determine participation in ICDS is not available. Thus, it is possible that the observed $\mathbf{x}$ variables do not entirely eliminate selection bias.

[9]The WHO and UNICEF consider the following to be "improved water sources": household connections, boreholes, protected dug wells, protected springs, and rainwater collection. I define a village to be lacking access to improved water sources if at least half of the village's population does not use an improved water source.

[10] I do not present the table of t-tests due to space constraints, however the table is available upon request.

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8 Figures and Tables

8.1 Figures

Figure 1: Quartiles of Percentage of Villages Covered by the ICDS: Indian NFHS-3 Data

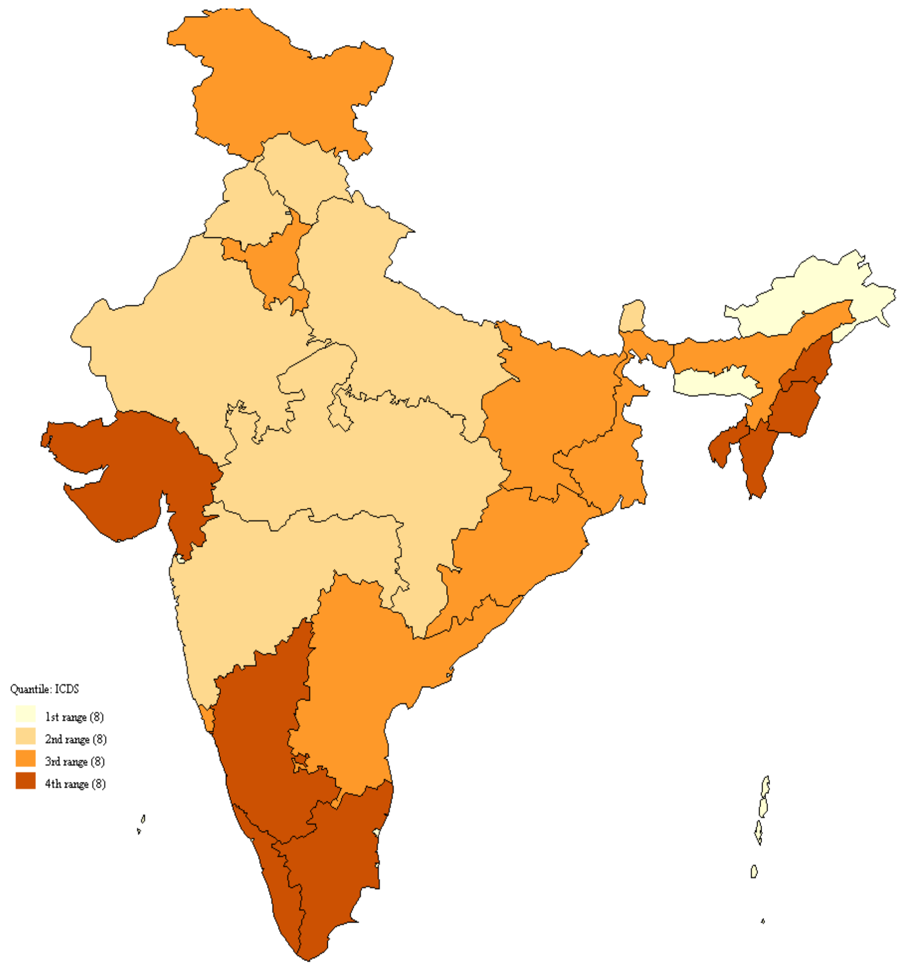


Figure 2: Quartiles of Percentage of People in Two Lowest Quintiles of Wealth Index: Indian NFHS-3 Data

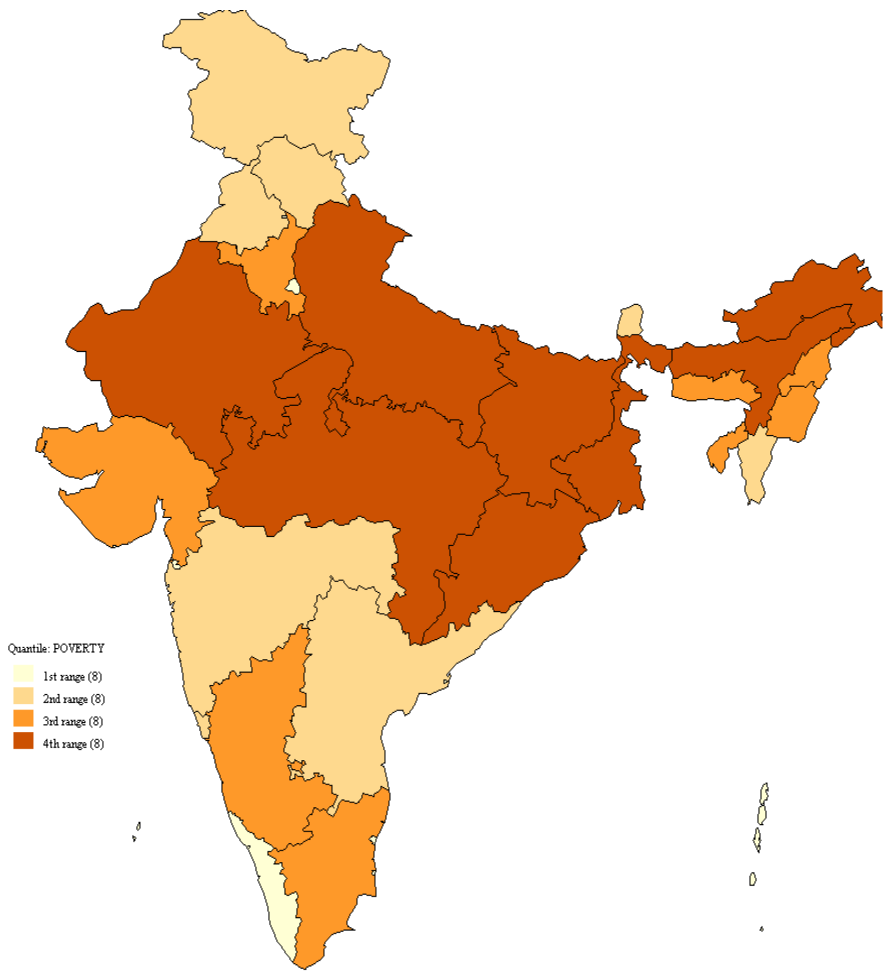


Figure 3: Quartiles of Stunting Outcome by State: Indian NFHS-3 Data

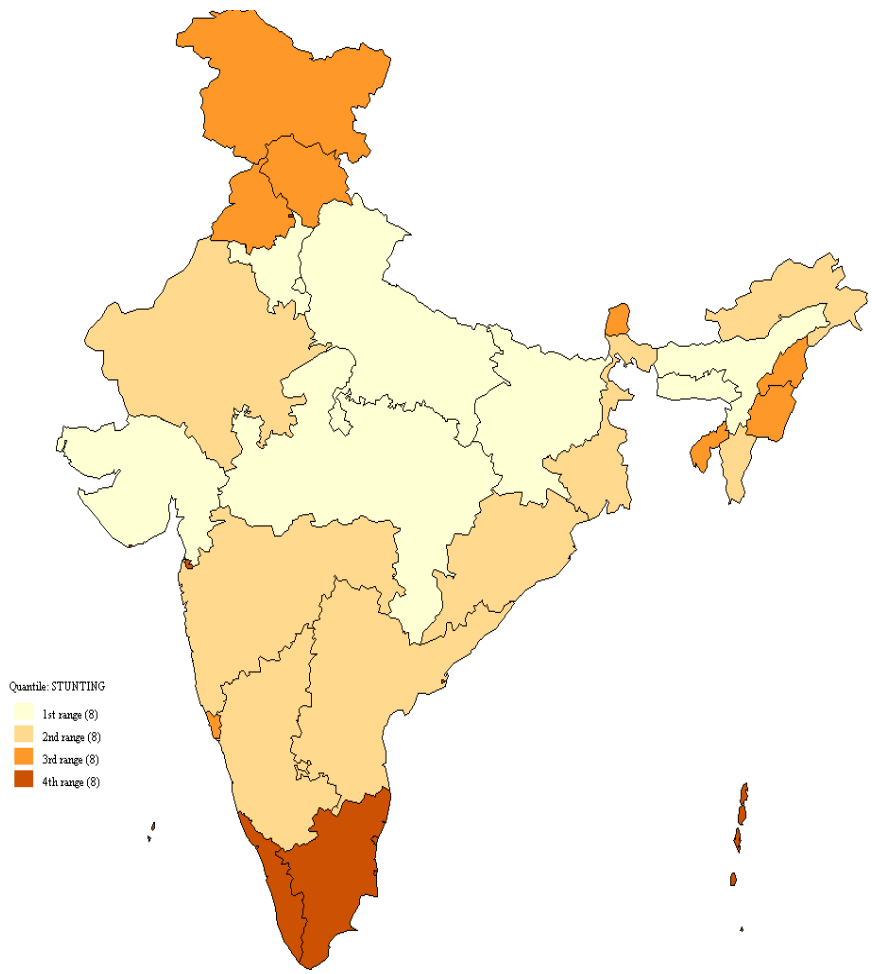


Figure 4: Kernel Density Estimates of Stunting Prevalence by ICDS Coverage: Indian NFHS-3 Data

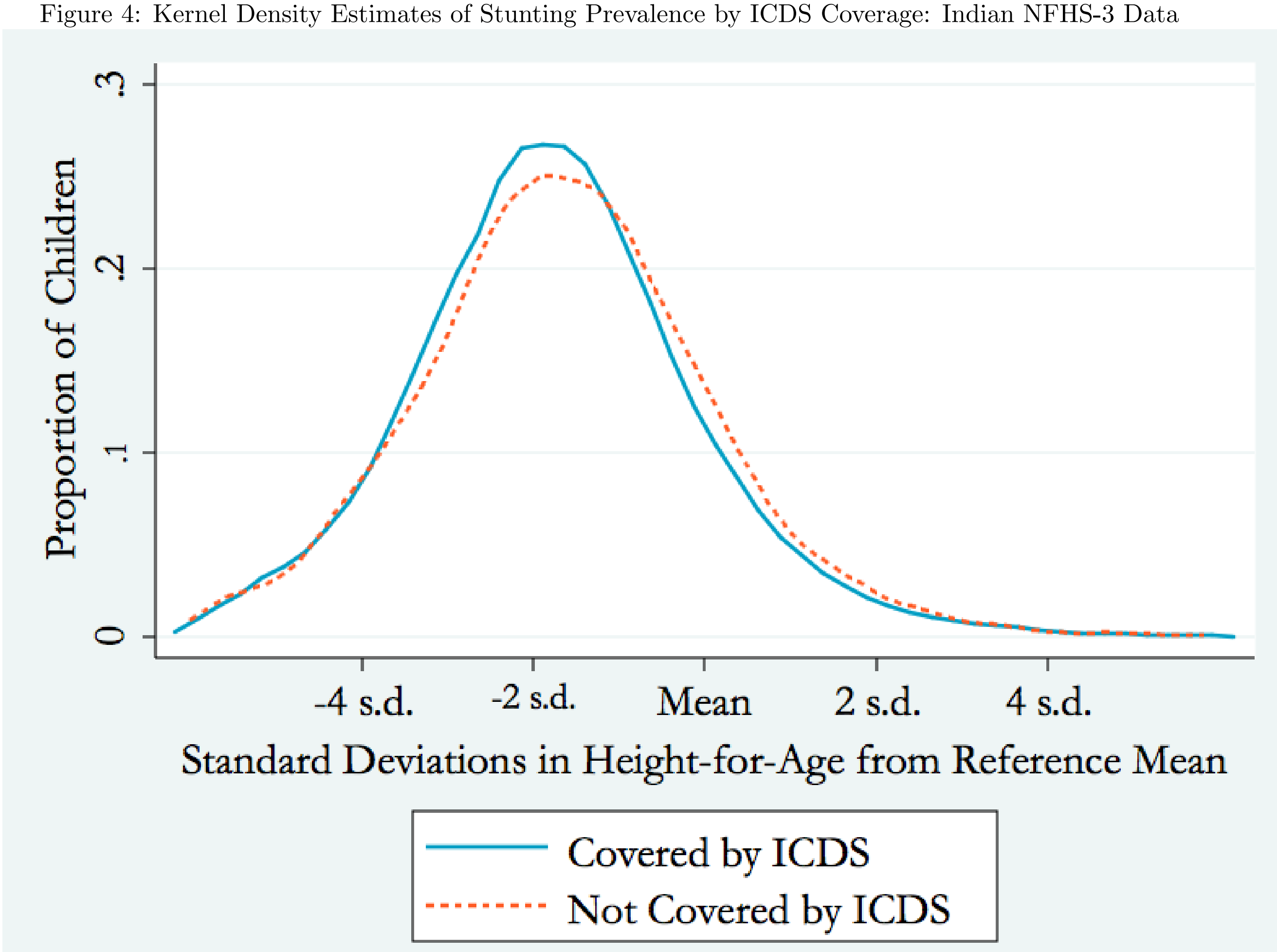


Figure 5: Kernel Density Estimates Stunting Prevalence by ICDS Coverage and Wealth Index Quintiles: Indian NFHS-3 Data

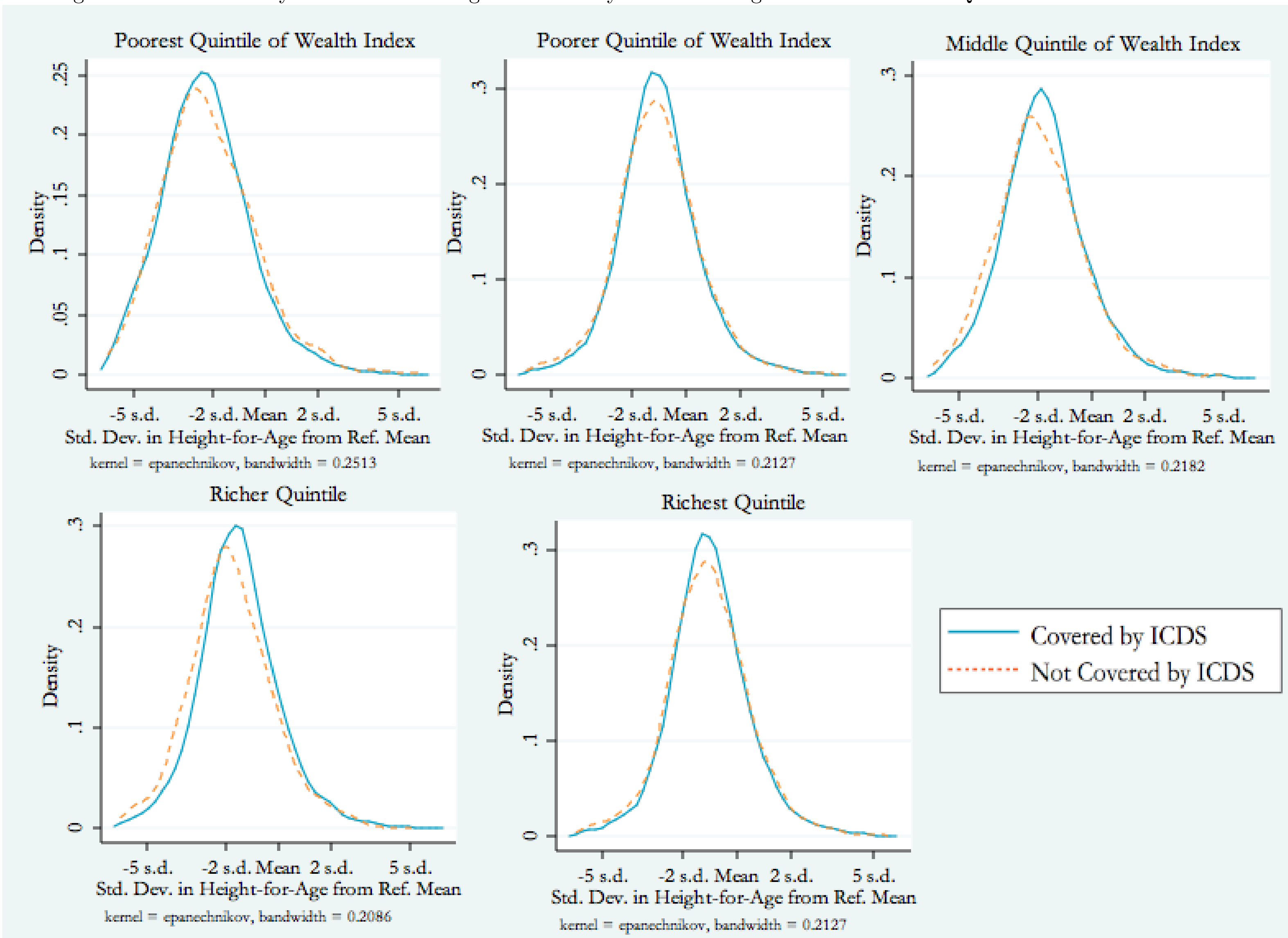


Figure 6: Distribution of State-wise ICDS Coverage: Indian NFHS-3 Data

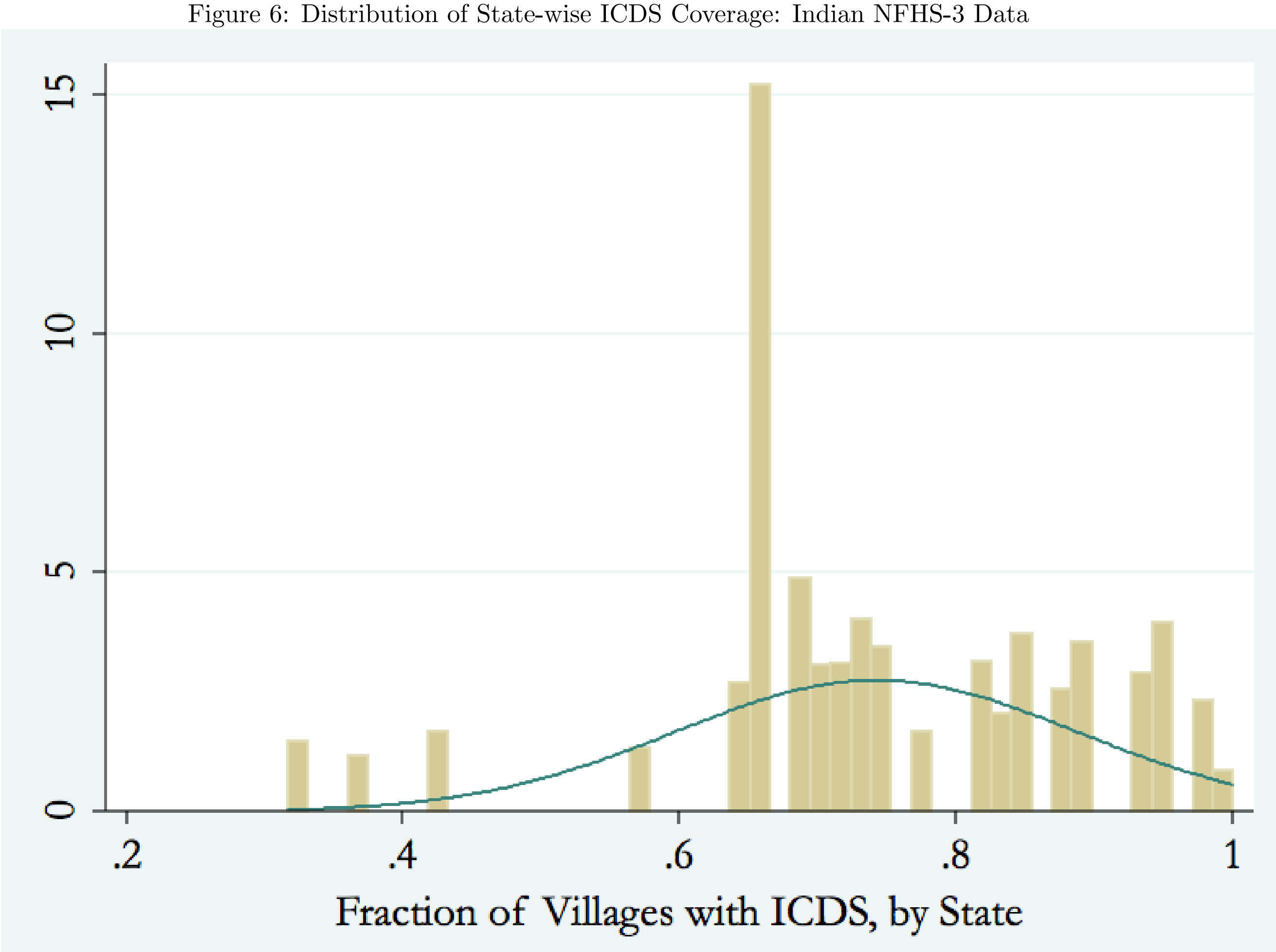
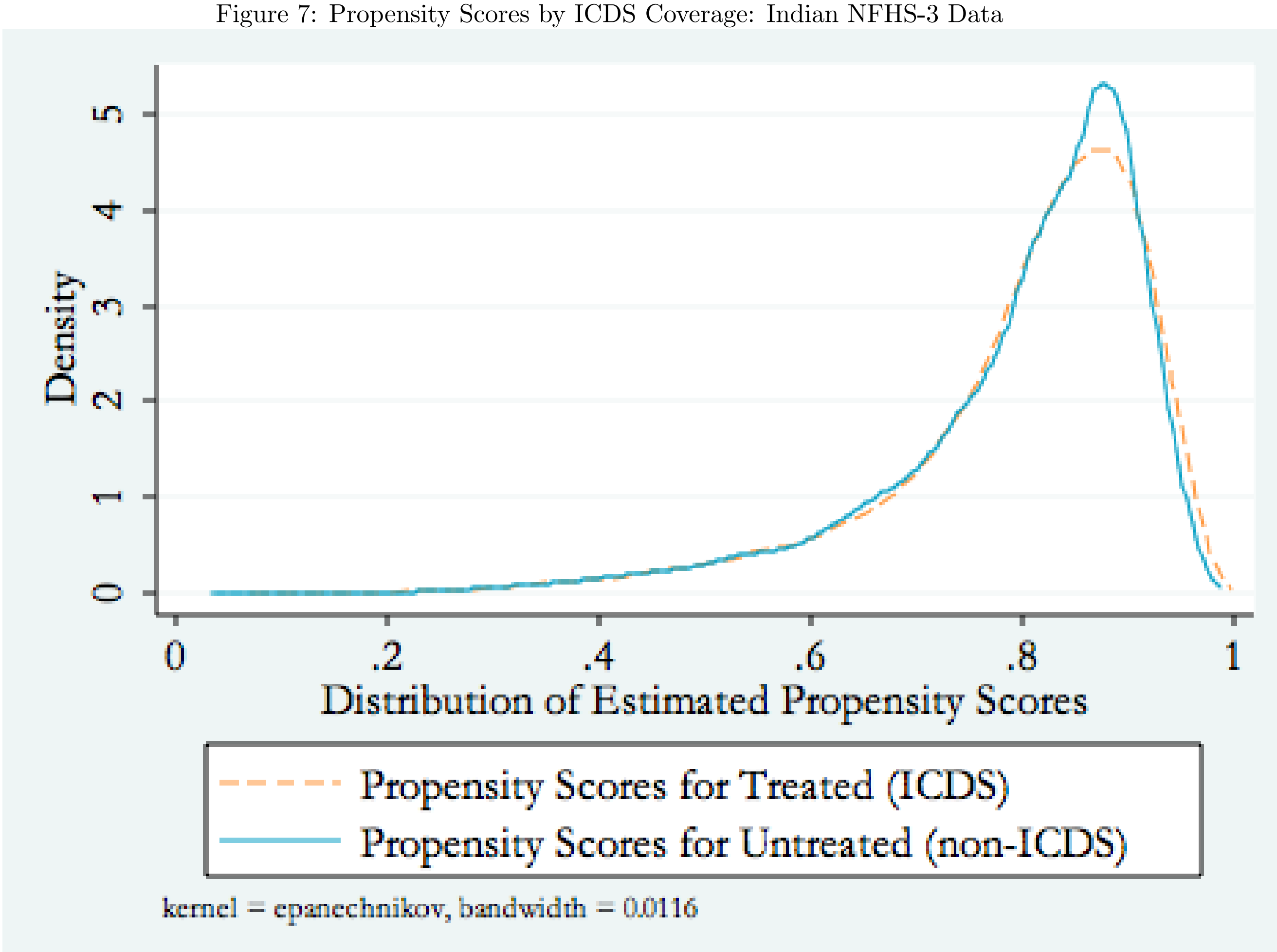


Figure 7: Propensity Scores by ICDS Coverage: Indian NFHS-3 Data



8.2 Tables

Table 1: Summary Statistics of Indian NFHS-3 Data

Mother's Characteristics	Mean	Standard Deviation
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	
Currently Working (percent)	29.01	
Children's Characteristics	**Mean**	**Standard Deviation**
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: Indian NFHS-3 Data for Children Younger than Five

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: Average Treatment Effects for All Children and Quantile Treatment Effects for Lowest Quartiles of Stunting: Indian NFHS-3 Data

	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)	(0.01)***
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	-0.01	0.05
	(0.03)***	(0.04)***	(0.04)**	(0.02)***	(0.03)	(0.03)**	(0.01)	(0.01)	(0.01)***
Observations	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Treated	28,929	15,024	13,906	7497	3853	3700	7396	3828	3547
Untreated	8853	4608	4232	2157	1145	1018	2138	1117	1026

Standard errors in parentheses.

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

Table 4: Quantile Treatment Effects for Quartiles of Propensity Scores: Indian NFHS-3 Data

	Highest Quartile			Higher Quartile			Lower Quartile			Lowest Quartile		
Unmatched	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Treated	-1.77	-1.78	-1.75	-1.91	-1.94	-1.88	-1.78	-1.79	-1.77	-1.45	-1.44	-1.46
Controls	-1.85	-1.80	-1.87	-2.02	-2.11	-1.92	-1.86	-1.91	-1.81	-1.43	-1.46	-1.39
Difference	0.08	0.02	0.12	0.11	0.17	0.04	0.08	0.11	0.04	-0.03	0.03	-0.06
	(0.05)	(0.07)	(0.07)*	(0.05)**	(0.07)**	(0.07)	(0.04)*	(0.06)*	(0.06)	(0.03)	(0.05)	(0.05)
Matched	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Treated	-1.77	-1.78	-1.75	-1.91	-1.95	-1.87	-1.78	-1.79	-1.77	-1.46	-1.46	-1.44
Controls	-1.86	-1.82	-1.87	-2.04	-2.12	-1.98	-1.85	-1.89	-1.81	-1.47	-1.49	-1.43
Difference	0.09	0.04	0.12	0.13	0.18	0.10	0.07	0.09	0.04	0.01	0.05	-0.03
	(0.06)	(0.08)	(0.08)	(0.05)***	(0.07)**	(0.07)	(0.05)*	(0.06)	(0.07)	(0.04)	(0.05)	(0.06)
Observations	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Treated	8567	4463	4110	8170	4894	3925	7648	3985	3653	4514	2318	2200
Untreated	1099	555	543	1290	675	615	1840	949	886	4623	2429	2201

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Table 5: Average Treatment Effects from PSM on Children Younger than Three: Indian NFHS-3 Data

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)*
Observations	**All**	**Boys**	**Girls**
Treated	23,095	11,956	1137
Untreated	7014	3634	3380

Standard errors in parentheses

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

Table 6: PSM Results by Years of Exposure to ICDS: Indian NFHS-3 Data

	Less than One Year	Up to Five Years	Ten Years
Unmatched	All	All	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)***
Observations	**All**	**All**	**All**
Treated	759	5161	12,260
Untreated	5042	8855	8855

Standard errors in parentheses.

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

Table 7: Village Participation in ICDS: Results from State- and District-level Regressions

	Probit with State Dummies (I) Village has ICDS?	Probit without State Dummies (II) Village has ICDS?	Beta Regression Statewise ICDS Coverage
LN(Village Population)	-0.022	0.122^{*}	-0.228^{***}
	(-0.46)	(2.26)	(-8.27)
Sex Ratio	0.092	0.034	0.103
	(0.55)	(0.19)	(1.12)
Average Wealth	-0.381^{***}	-0.292^{***}	-0.264^{***}
	(-6.82)	(-4.64)	(-7.92)
Average Landholding	0.0940	-0.0265	0.0276
	(0.95)	(-0.23)	(0.48)
Average Irrigated Landholding	-0.0694	-0.0517	0.096^{*}
	(-0.85)	(-0.51)	(2.11)
Mothers with Primary Education	0.406^{*}	0.532^{*}	0.249^{*}
	(2.24)	(2.57)	(2.26)
Mothers with Secondary Education	0.792^{***}	0.475^{***}	0.620^{***}
	(6.66)	(3.59)	(9.57)
Electrification	0.369^{**}	0.224	0.347^{***}
	(3.06)	(1.50)	(5.68)
Rural	1.066^{***}	1.263^{***}	-0.00780
	(15.35)	(15.38)	(-0.19)
Risky Water Source	-0.118	-0.276^{**}	0.198^{***}
	(-1.40)	(-2.86)	(4.31)
Akaike Information Criterion	2997.72	2680.62	1543.26
Observations	3288	3239	3240

t statistics in parentheses

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

Table 8: Allocation of State ICDS Administrative Budget: Results from a State-level Regression

	LN(ICDS Admin Budget
Wealth	-1.070
	(-1.62)
Vote	-0.01^{*}
	(-1.93)
Wealth*Vote	0.029^{*}
	(2.13)
LN(Population)	0.736^{***}
	(9.90)
LN(Stunting)	0.361
	(0.28)
LN(Moms with secondary ed)	0.855^{**}
	(2.97)
LN(Moms with primary ed)	1.366^{**}
	(4.26)
LN(Rural)	0.456^{*}
	(2.38)
LN(Sex Ratio)	-2.015
	(-1.17)
Constant	16.81
	(1.39)
Observations	29

t statistics in parentheses

* $p < 0.10$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Average Treatment Effect from PSM: Indian NFHS-1 Data

Unmatched	All	Boys	Girls
Treated	-2.44	-2.23	-2.43
Controls	-2.68	-2.59	-2.48
Difference	0.23	0.36	0.04
	(0.05)***	(0.06)***	(0.06)
Matched	All	Boys	Girls
Treated	-2.45	-2.45	-2.43
Controls	-2.49	-2.38	-2.34
Difference	0.03	0.14	0.09
	(0.06)	(0.08)	(0.08)

Standard errors in parentheses.

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

Table 10: Average Treatment Effects from PSM: Indian 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)

Standard errors in parentheses.

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