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Estimating the Effects of Social Safety Net Programmes in Bangladesh on Calorie Consumption of Poor Households

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The Social Safety Net (SSN) programmes play a key role in Bangladesh to protect the poor households from food insecurity. This study examines the effect of these programs on calorie consumption of poor households using the 2005 Household Income and Expenditure Survey data. Three treatment effect evaluation designs are applied to compare the estimated effects. Mean difference and matching estimators that do not consider endogeneity of treatment dummy produce significant negative effects when applied to the whole sample. Unconfoundedness and overlap assumptions do not exist and the assumptions are satisfied after dropping some observations using the criteria of propensity score. The effect of the SSN programmes on calorie consumption is estimated in the reduced sample using the same econometric methods, and it is found that there are insignificant positive effects in all cases. However, the treatment dummy has serious endogeneity problem, as selection for treatment is also determined by some unobserved factors such as corruption. In this case, instrumental variables regressions taking regional dummies as instruments that do not have relation with calorie consumption are applied, and produce significant positive average treatment effect.

JEL Classification: C21, C31
Keywords: Food insecurity, Average treatment effect

I. INTRODUCTION

In spite of moderate GDP growth (6 per cent), a large part of population in Bangladesh is still poor (about 40 per cent). Moreover, natural disasters such as flood, cyclone, river erosion that are common in Bangladesh force many people into vulnerable situation. Especially in rural areas, not only poor but also non-poor people who are fully dependent on agriculture are affected by these disasters. Due to entitlement failure, after these shocks these people face severe food insecurity (Sen 1982), which decreases their productivity and then income (Pitt, Rosenzweig and Hossain 1990). Besides, food price hike followed by the

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disasters adds fuel to food insecurity (del Ninno and Dorosh 2001). It barely turns into famine that kills many people (Ravallion 1990). Therefore, the Bangladesh government, the United Nations World Food Programme (WFP) and some other national and international organisations have been helping vulnerable people since the famine in 1974, through giving food, or cash, or both under several programmes that belong to the so-called Social Safety Net (SSN). Day by day, the number of SSN programmes and their benefits and coverages are increasing. Sixty-six SSN programmes are currently being operated.

It is expected that the SSN programmes have positive effect on calorie consumption of their beneficiaries. Literature also reveals that any food transfer to poor households increases their calorie consumption (e.g. Barrett 1999, Quisumbing 2003), and any cash transfer improves calorie consumption too (e.g. Bouis and Haddad 1992), Gibson and Rozelle 2002)). However, there are a few studies examining the effect of the SSN programmes on calorie (or nutrition) consumption of Bangladeshi households. For example, Ahmed and del Ninno (2002) have found that Food for Education increases nutrition of preschoolers of beneficiary households. del Ninno, Dorosh, Smith and Roy (2001) have shown that most households under Cash Transfer program can increase their income, and thereby the quality and quantity of their food intake. Matin and Hulme (2003) have claimed that Vulnerable Group Development (VGD) increases the number of meals of beneficiary households from 2 to 3 in a day. Khanum (2000) has reported that 90 per cent of the Rural Maintenance Program beneficiaries have improved their sustainable consumption.

However, previous studies in examining the SSN programs have produced biased effect for not treating the treatment dummy as an endogenous variable when it is endogenous as many unobserved factors influence it. On the other hand, no study has considered more than one program to examine the effect on calorie consumption. Moreover, sample size is small in all cases where survey data are not national representative. In this study, the author has estimated the average treatment effect of a number of SSN programs on calorie consumption of beneficiary households using Household Income and Expenditure Survey (HIES) 2005, the largest survey in Bangladesh.

The author applied three treatment effect evaluation designs that are classified into two parts. First, the author applied mean difference method and matching estimators that do not consider endogeneity problem in treatment dummy. Second, the author applied instrumental variables regressions method that tackles endogeneity problem. To compare the estimated results, and then to see how much severe the endogeneity problem is, both types of methods were applied.

Using mean difference and matching estimators over the whole sample, negative effects were estimated. Then some observations were dropped to satisfy the unconfoundedness and overlap assumptions suggested by Abadie and Imbens (2002) to get satisfactory results. Same methods are applied in a short sample to estimate the average treatment effect. These resulted in statistically insignificant positive effects. Considering endogeneity in treatment dummy, instrumental variables regressions were run taking region dummies as instruments, which are unrelated to calorie consumption. Although region dummies are weak instrument in some senses, instrumental variables regressions produced statistically significant positive effects on calorie consumption.

The rest of the paper contains the following. Section II holds the discussion of estimation methodologies. Programmes and data are analysed in section III. Estimated results are discussed in section IV, and section V concludes the paper.

II. ESTIMATION METHODS OF THE AVERAGE TREATMENT EFFECT

Estimation methods for estimating the average treatment effect have been divided in two parts. The first part analyses mean difference method and matching estimators that do not consider endogeneity in treatment dummy. The instrumental variables regressions method that considers endogeneity in treatment dummy is analysed in the second part.

2.1 Mean Difference and Matching Estimators

Let, N is the number of households where K households are treated and N–K are not, T_i is a dummy of treatment with 1 if household i is treated by any of SSN programs and 0 otherwise, and Y_i that is the outcome variable is per capita daily calorie consumption of household i. Y_i can be written as follows:

$$Y_i = T_i Y_{1i} + (1 - T_i) Y_{0i} = \begin{cases} Y_{1i} & \text{if } \mathrm{T_i} = 1 \\ Y_{0i} & \text{if } \mathrm{T_i} = 0 \end{cases}$$

where Y_{1i} is the outcome of household i if treated, and Y_{0i} is the outcome of household i if not treated. Now, it is assumed that households are randomly selected for treatment. Therefore, the sample average treatment effect, $\hat{\tau}$, will be (Neyman 1923).

$$\hat{\tau} = \frac{\sum_{i|T_i=1} Y_{1i}}{K} - \frac{\sum_{i|T_i=0} Y_{0i}}{N-K}.$$

The same sample average treatment effect can be easily estimated if one run the following OLS regression:

$$Y_i = \hat{\alpha} + \hat{\tau}T_i + e_i \qquad (1)$$

where $\hat{\tau}$, the coefficient of T_i, is then the sample average treatment effect.

The average treatment effect under mean difference method is unbiased under only randomized experiment which is barely found in the social science field. Rather, treatment might be provided condition to some observed characteristics of household i, X_i, that are assumed not to be related with T_i. Therefore, these characteristics are called pre-treatment variables or exogenous variables. In this case, matching estimators are popular to estimate the average treatment effect based on the following assumptions (Heckman, Ichimura and Todd 1998, Dehejia and Wahba 1999, Abadie and Imbens 2002).

Assumption 1: *Y_{1i} or Y_{0i} is independent on T_i conditional on X_i.*

$$(Y_{1i}, Y_{0i}) \perp T_i \mid X_i$$

This assumption is called unconfoundedness.

Assumption 2: *The propensity score, the probability of treatment given X_i, will remain between 0 and 1. Therefore,*

$$0 < Pr(T_i = 1 \mid X_i) < 1$$

This assumption is called overlapping.

Before estimating the average treatment effect on an outcome variable, it is required to test these assumptions. The unconfoundedness test can be run by estimating the average treatment effect on such a covariate which is free from T_i (Heckman and Hotz 1989). Zero value of the average treatment effect is set as a null hypothesis, and if the null hypothesis is not rejected, then it is likely that the unconfoundedness assumption holds. On the other hand, the overlapping assumption can be tested from histogram plots of propensity score for treatment and control groups. If distributions of propensity score are different in treatment and control groups, then one can conclude that this assumption does not hold. For example, if distribution is right skewed in the treatment group and left skewed in the control group, then it is clear that overlapping is absent. If this assumption is violated, dropping some observations is a good practice to satisfy it. Dropping will be done using propensity score. If propensity score is below 0.1 and above 0.9, one can drop observations (Crump, Hotz, Imbens and Mitnik 2006).

Matching estimators use only the outcomes of the nearest neighbours of the opposite group based on covariates. From these neighbours, researchers have to choose the number of matches given the matching metric.

Following Abadie and Imbens (2002), for Y_i, X_i and T_i, an index $l_m(i)$ for $m = 1,2,\ldots M$ exists that satisfies $T_l \neq T_i$ and $\sum_{j|T_l \neq T_i} 1\{\| X_j - X_i \| \leq \| X_l - X_i \|\} = m$ where 1{.} is the indicator function contains 1 if this inequality within brackets holds and zero otherwise. Thus, a set of indices $L_M(i) = \{l_1(i), l_2(i), \ldots, l_M(i)\}$ exists for M matches in both treatment and control units. So, the estimated (treatment and control) outcomes are the following:

$$\hat{Y}_{1i} = \begin{cases} Y_i & \text{if } T_i = 1 \text{ (From sample)} \\ \dfrac{1}{M} \sum\limits_{j \in L_m((i)} Y_j & \text{if } T_i = 0 \text{ (Imputed from M matches)} \end{cases}$$

$$\hat{Y}_{0i} = \begin{cases} \dfrac{1}{M} \sum\limits_{j \in L_m((i)} Y_j & \text{if } T_i = 1 \text{ (Imputed from M matches)} \\ Y_i & \text{if } T_i = 0 \text{ (From sample)} \end{cases}$$

Thus, under matching, the average treatment effect is,

$$\hat{\tau} = \frac{1}{N} \sum_{i=1}^{N} (\hat{Y}_{1i} - \hat{Y}_{0i}).$$

The above $\hat{\tau}$ contains bias that does not disappear with the increase in sample size. To remove bias from matching method, a combination of regression and matching processes is useful. A number of corrections exist to remove bias. Under parametric setting, the way of bias correction is as follows (Rubin 1973, Quade 1982).

Let, for a single match index, $l_m(i)$ is equal to $l(i)$. So, the estimated covariates are the following in the single match case.

$$\text{Treatment covariates, } \hat{X}_{1i} = \begin{cases} X_i & \text{if } T_i = 1 \text{ (From sample)} \\ X_{l(i)} & \text{if } T_i = 0 \text{ (Imputed from M matches)} \end{cases}$$

$$\text{Control covariates, } \hat{X}_{0i} = \begin{cases} X_{l(i)} & \text{if } T_i = 1 \text{ (Imputed from M matches)} \\ X_i & \text{if } T_i = 0 \text{ (From sample)} \end{cases}$$

The matching is exact, if $\hat{X}_{1i} - \hat{X}_{0i} = 0$ for each unit. If this is not the case, bias will exist and then the gap between $\hat{X}_{1i}$ and $\hat{X}_{0i}$ can be used in the following OLS regression to correct bias.

$$\hat{Y}_{1i} - \hat{Y}_{0i} = \hat{\tau} + \hat{\beta}'(\hat{X}_{1i} - \hat{X}_{0i}) + \varepsilon_i \qquad (2)$$

In the above regression, $\hat{\tau}$ is the bias corrected average treatment effect.

To get an efficient estimate from matching estimators, it is required to increase the number of matches with the increase in the number of sample size. On the other hand, to choose optimal matches, Euclidian metric can be used as a distance metric. A weight matrix is also useful with the distance metric to standardise the covariates. Abadie and Imbens (2002) suggest diagonal matrix of the inverse of the covariate variances as a weight matrix.

2.2 Instrumental Variables Regressions

At the presence of endogeneity in T_i, the average treatment effect, if it is estimated by the above methods, will be invalid. It is likely in the case of the SSN programs' effects that T_i is endogenous, as some unobserved covariates (e.g. corruption) influence it. For example, one common corruption in Bangladesh is that political party affiliation of households helps them to be selected by the programs administrators. This information is unobserved in the data set. However, in the case of endogeneity in T_i, instrumental variables (IV) regression is an important method for estimating the average treatment effect (Angrist 1990).

Let, Z_i is a vector of p instruments of household i which are binary variables. Now, the estimated first stage of two stage OLS regressions, where T_i is instrumented by Z_i, will be,

$$\hat{T}_i = \hat{\delta} + \hat{\theta}' Z_i \qquad (3)$$

where $\hat{\theta}$ is a vector of p coefficients. After substituting $\hat{T}_i$ into (1) in place of T_i, the estimated second stage will be,

$$\hat{Y}_i = \hat{\alpha} + \hat{\tau}\hat{T}_i \qquad (4)$$

where $\hat{\tau}$ is the average treatment effect under IV regressions.

Now, to identify the average treatment effect, the following key assumptions are required to satisfy.

Assumption 1: *Each variable in* Z_i *is independent on* Y_i *and* T_i.

$Z_i \perp Y_i, T_i$

This assumption is called independence.

The independence assumption cannot be examined directly. However, it is a combination of the following two assumptions.

Assumption 2: *Each variable in* Z_i *is randomly assigned.*

Assumption 3: $Y_i(z,t) = Y_i(z^{'},t)$, *for all* $z, z^{'}, t$. *It is called exclusion restriction.*

Like any other cases, randomness of each variable in Z_i can be checked whether other observed covariates, X_i, are varying with Z_i. For this, each variable in X_i is required to be regressed on Z_i. Then, if the coefficients of Z_i are insignificant in most of the X_i, it can be inferred that each variable in Z_i is randomly assigned. On the other hand, if they are significant in most cases then there is a high chance of violation of randomization of Z_i. Therefore, it is essential to incorporate X_i into both stages of two stage IV regressions, which will control any variation of X_i with Z_i. Therefore, the estimated first stage and second stage OLS regressions will then be,

$$\hat{T}_i = \hat{\delta} + \hat{\theta}^{'} Z_i + \hat{\pi} X_i \tag{5}$$

$$\hat{Y}_i = \hat{\alpha} + \hat{\tau}\hat{T}_i + \hat{\phi} X_i \tag{6}$$

III. PROGRAMMES AND DATA

Householed Income and Expenditure Survey (HIES) has been being conducted by Bangladesh Bureau of Statistics in every five years since 1991-92 with the financial aid from the World Bank. It is a nationally representative survey that collects information on household food and non-food consumption, income, expenditure, etc. In this study, HIES 2005 has been used, the latest available one, that records richer information about SSN programmes in terms of a number of households treated and the number of programmes than that in previous HIESs.

In the HIES 2005, 1,226 of 10,070 surveyed households are treated ($T_i = 1$) by mainly ten key SSN programmes where their primary goal is to provide free food, or cash, or both to the poorest households to protect them from any food insecurity. Every treated household receives treatment from any one of these programs except 48 households who are treated by more than one programme. Local government selects eligible households based on household income, landholding, sex of head etc. In general, head of a treated household receives benefits of a programme (food, or cash) from the programme administrators on behalf of that household.

TABLE I
EXPECTED CALORIE CONSUMPTION FROM SSN PROGRAMMES

SSN Programmes	HIES Participation (%)	Total coverage	Monthly Entitlement	
			Rice (kg)	Cash (Taka)
Vulnerable Group Feeding	32.66	1,04,67,000	10	
Old Age Pension	16.85	2,00,000		250
Vulnerable Group Development	15.59	5,00,000	30	
Test Relief	13.12	Transitory	105	
Freedom Fighters Pension	12.45	1,25,000		900
Food for Education	7.46		16	
Gratuitous Relief	0.52	Transitory	15	
Integrated Food Security	0.3	2,50,000	20	75
Money for Work	0.22	16,00,000		3,000
Rural Maintenance Programme	0.07	1,81,000		1,500
Others	0.75			
Total	100			

Source: HIES 2005 and WFP.

Table I presents SSN programmes listed in HIES 2005, with the participation rate of heads of treated households in this data, total coverage in the whole country and monthly entitlements for each treated household. To estimate participation rate in any programme, the number of household heads treated in that program is divided by the total number of treated household heads. In this case, if one household head is treated by two programmes, then he/she is counted twice like two heads. Total coverage includes the total number of treated (HIES and non-HIES) households in the whole country. It is seen that Vulnerable Group Feeding (VGF) has the highest coverage with the highest participation rate from HIES 2005.

Fourteen days' food consumption data of around 200 food items are available in this survey which are collected by recall method. Bought, own production, transfer and wage in kind are mentioned as sources of these foods, which do not indicate that how many foods are consumed from those benefits received from the SSN programmes. There would not have been required any econometric analysis had it been known. Food items consumed by households are in several measures, such as kilo gram, liter, number. These have been transformed into a single measure, gram. Daily household consumption of all of these items is then estimated taking an average of 14 days consumption. Per capita daily household consumption of all items is then estimated dividing daily household consumption by household size. Using kilo calorie measure of per gram food consumption set by Food and Nutrition Department, Dhaka University, Bangladesh, household food consumption per capita per day is converted from these items to household calorie consumption per capita per day. Then, adding over items, total per capita daily calorie consumption for each household, Y_i, is estimated.

Other variables or household characteristics, X_i, used in this study are also estimated from the data set. For example, there are mainly two types of income, monthly and yearly, from different sources, such as salary from job, transfer payment, remittances, earnings from crops or goods selling. Converting yearly incomes into monthly, all incomes are added to get total household monthly income. Dividing total household monthly income by household size, total per capita monthly income, denoted as Income, is estimated. Education of head represents total academic years of education achieved by a household head in his or her life. Male Head is a dummy variable which refers to sex of household head, 1 if male and 0 otherwise. Household size, the number of total members in a household, is also categorised by age and sex, such as Male 1-5, Male 6-11, Male 12-17, Male 18-60 and Male 60+, which refer to the total number of males in a household who are in 1 to 5, 6 to 11, 12 to 17, 18 to 60 and 60 plus age groups respectively. Female members in a household are also categorised in a similar fashion such as Female 1-5, Female 6-11, Female 12-17, Female 18-60 and Female 60+. Rural location is the dummy variable which carries 1 if the household is in a rural area and 0 otherwise. Landholding is the variable that implies the size of land in acre that a household owns. It includes all types of land including cultivable, non-cultivable and homestead land.

Five zonal dummies are also used in this study. Bangladesh was divided by six divisions in 2005 (currently 7), which are Dhaka, Chittagong, Rajshahi, Khulna, Barisal and Sylhet. Zonal dummies are named by these divisions. Each zonal dummy contains 1 if a household lives in that division and 0 otherwise. Dhaka is taken as the base category. These zonal dummies will be used as instruments, Z_i, in IV regressions.

TABLE II
SUMMARY STATISTICS OF VARIABLES

Variables	Treatment Households		Control Households		Mean Difference
	Mean	S.D.	Mean	S.D.	
Y_i	2193.27	541.32	2261.78	562.24	-68.51
T_i	1	0	0	0	1
Income	843.54	810.38	1459.35	1315.42	-615.81
Education of head	1.65	3.19	4.34	5.1	-2.69
Male Head	0.81	0.39	0.91	0.29	-0.1
Household size	4.59	2.01	4.9	2.08	-0.31
Male 1-5	0.31	0.54	0.34	0.57	-0.03
Male 6 -11	0.42	0.65	0.38	0.61	0.04
Male 12-17	0.35	0.59	0.36	0.6	-0.01
Male 18-60	0.96	0.66	1.25	0.8	-0.29
Male 60+	0.14	0.34	0.15	0.36	-0.01
Female 1-5	0.33	0.6	0.33	0.57	0
Female 6-11	0.41	0.63	0.35	0.59	0.06
Female 6-17	0.35	0.59	0.32	0.57	0.03
Female 18-60	1.14	0.55	1.29	0.67	-0.15
Female 60+	0.19	0.39	0.12	0.33	0.07
Rural Location	0.79	0.41	0.62	0.49	0.17
Landholding	0.46	1.42	1.27	3.29	-0.81
Dhaka	0.35	0.48	0.28	0.45	0.07
Barisal	0.08	0.27	0.08	0.27	0
Chittagong	0.15	0.36	0.18	0.39	-0.03
Khulna	0.09	0.28	0.15	0.36	-0.06
Rajshahi	0.23	0.42	0.25	0.43	-0.02
Sylhet	0.09	0.29	0.05	0.21	0.04
Observation	1,226		8,844		

Source: Author's calculation based on HIES 2005.

Summary statistics of all variables mentioned are reported in Table II which contains means and standard deviations of variables for treatment and control households. The data set gives information on 10,070 households. Of these, 1,226 households are in the treatment group and 8,844 households are in the control group. In the Table, the mean value of the outcome variable, Y_i, is higher in the control group than in the treatment group, which implies that SSN programmes have a negative impact on calorie consumption of households. Raw differential, or mean difference is -68.51, which is the first estimate of treatment effect before doing any sophisticated econometric analysis. The negetive treatment effect is not a surprising result as Income, Education of head, Landholding, which are key variables to affect Y_i, are substantially higher in the control households than in their counterparts. Although not substantial, most of other covariates are also higher in the control households than in the treatment households. These differences imply that unconfoundedness assumption can be violated. On the other hand, randomisation does not hold.

IV. ESTIMATED RESULTS

4.1 Mean Difference and Matching Estimators

Table III presents average treatment effects on Y_i and Education of head that are estimated by mean difference and matching estimators using the full sample where all methods produce negative effects on both variables. For checking the unconfoundedness assumption, Education of head has been selected as an exogenous variable that is free from any effect of the SSN programs because household heads have taken education prior to treatment. Significant negative effects on it imply that the unconfoundedness assumption is violated. The overlapping assumption is also violated in the full sample. In Figure 1, histogram plots of propensity score estimated from a probit model given in Table A1 of appendix A say that distribution of it is different between the treatment group and the control group. As both assumptions are violated, the average treatment effect on Y_i is not valid in the full sample case. Therefore, it is required to cut some observations on the basis of propensity score.

TABLE III

AVERAGE TREATMENT EFFECTS ON Y_i AND EDUCATION OF HEAD USING FULL SAMPLE

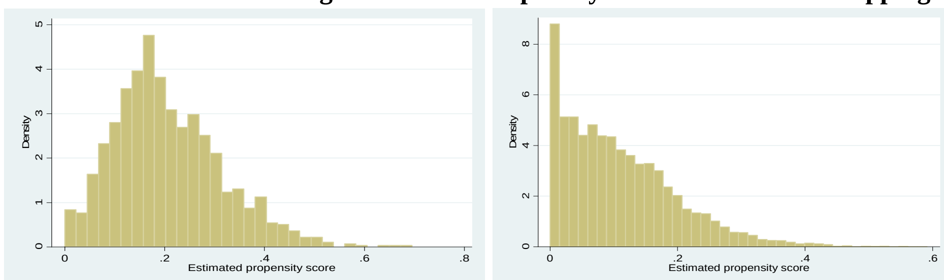
	Dep. Var: Y_i		Dep. Var: Education of head	
	Estimate	Std. Error	Estimate	Std. Error
Mean Difference	-68.51	(16.57)	-2.69	(0.11)
Matching	-30.11	(19.17)	-2.03	(0.14)
Matching With Bias Adjustment	-40.89	(19.17)	-1.48	(0.14)

Source: Author's calculation based on HIES 2005.

Note: Robust standard errors are in parentheses. Sample size is 10,070. In matching estimators, the number of matches is one.

In Table IV, the average treatment effect of the SSN programmes on education of head is presented using reduced samples. First, those observations have been dropped for which propensity score is less than 0.10. After dropping observations, the number of treated and control households has become 1,038 and 4,124 respectively. A large number of control households that created a significant difference in Education of head between two groups have been dropped. However, mean difference is still producing a significantly negative effect while matching estimators is not.

FIGURE 1: **Histogram Plots of Propensity Score to Check Overlapping**



Treatment Group (K=1,226) Control Group (N-K=8,844)

Source: Author's calculation based on HIES 2005.

Further observations have been dropped for propensity score lower than 0.15 and 0.20, and then the average treatment effect on Education of head becomes insignificant in all methods. It can be concluded that the unconfoundedness assumption holds if observations are dropped on the criteria of propensity score. The overlapping assumption also holds after dropping observations in this way. From histogram plots of propensity scores in Figure 2, it is seen that distributions of propensity scores become closer between treatment and control groups if increased number of observations is dropped.

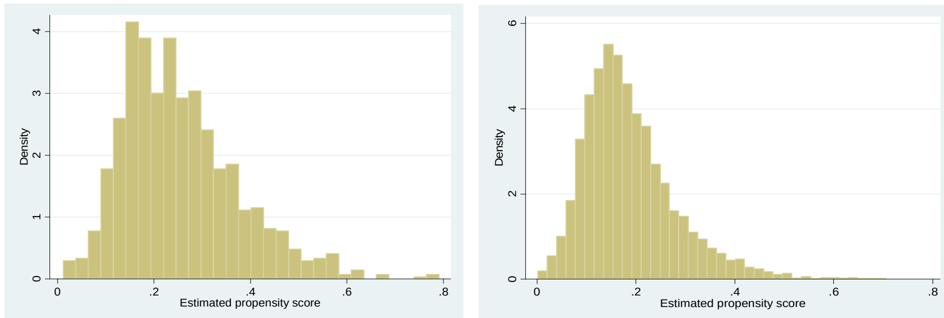
TABLE IV
AVERAGE TREATMENT EFFECT ON EDUCATION OF HEAD USING REDUCED SAMPLES

	Propensity Score $\geq$ 0.10 (K =1,038, $N-K$ =4,124)		Propensity Score $\geq$ 0.15 (K =824, $N-K$ =2,550)		Propensity Score $\geq$ 0.20 (K =563, $N-K$ =1,319)	
	Estimate	Std. Error	Estimate	Std. Error	Estimate	Std. Error
Mean Difference	-0.25	(0.09)	-0.03	(0.08)	-0.05	(0.08)
Matching	-0.16	(0.10)	0.03	(0.09)	-0.02	(0.09)
Matching With Bias Adjustment	-0.08	(0.10)	0.07	(0.09)	0.03	(0.09)

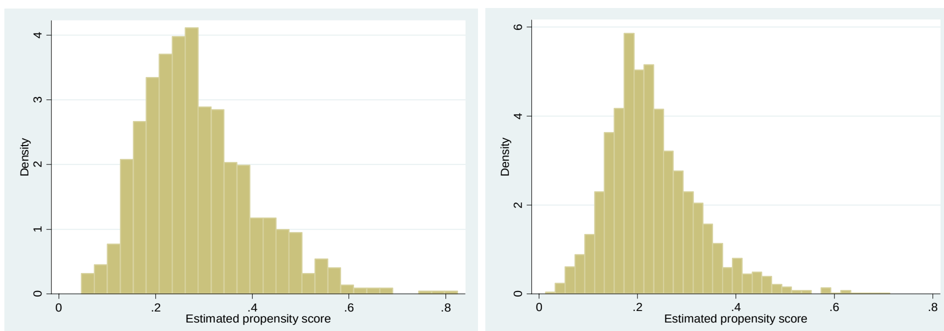
Source: Author's calculation based on HIES 2005.
Note: Robust standard errors are in parentheses. In matching estimators, the number of matches is one.

After dropping observations, the average treatment effect of the SSN programmes on the outcome variable, Y_i, has been estimated in Table V. The average treatment effect is statistically insignificant in all methods, but it turns to positive using any method with the smallest sample. It is therefore tempting to drop observations so that the unconfoundedness and overlapping assumptions are satisfied, and the process results in positive (though insignificant) treatment effect.

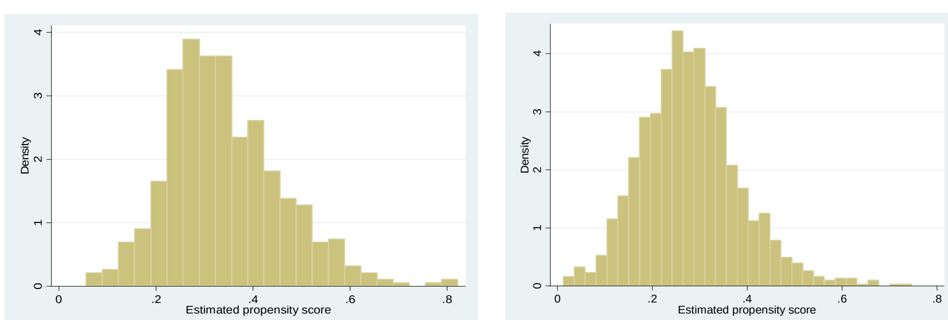
FIGURE 2: **Histogram Plots of New Propensity Scores After Dropping Some Observations to Check Overlapping**



Treatment Group (K = 1,038) Control Group (N-K = 4,124)



Treatment Group (K = 824) Control Group (N-K = 2,550)



Treatment Group (K = 563) Control Group (N-K = 1,319)

Source: Author's calculation based on HIES 2005.

TABLE V

AVERAGE TREATMENT EFFECT ON Y_i USING REDUCED SAMPLES

	Propensity Score $\geq$ 0.10 (K =1,038, $N-K$ =4,124)		Propensity Score $\geq$ 0.15 (K =824, $N-K$ =2,550)		Propensity Score $\geq$ 0.20 (K =563, $N-K$ =1,319)	
	Estimate	Std. Error	Estimate	Std. Error	Estimate	Std. Error
Mean Difference	-23.12	(18.97)	-20.31	(21.26)	21.49	(27.15)
Matching	-14.51	(20.16)	0.14	(23.69)	46.16	(30.04)
Matching With Bias Adjustment	-24.40	(20.16)	-12.14	(23.69)	24.03	(30.04)

Source: Author's calculation based on HIES 2005.
Note: Robust standard errors are in parentheses. In matching estimators, the number of matches is one.

4.2 Instrumental Variables Regressions

Regional dummies, Barisal, Dhaka, Chittagong, Khulna, Rajshahi and Sylhet, are taken as instrumental variables, and among them, Dhaka is dropped to avoid dummy trap. The Bangladesh government has regional bias to select households for treatment. However, these dummies are expected to be independent on Y_i and T_i. First, in general, food consumption habit of households is not different in different regions of Bangladesh. For example, 70 per cent of households' calorie consumption come from rice in all around the Bangladesh. Second, it is highly unlikely that households will change their region to be selected for treatment. However, the assumption of independence is checked to some extent through checking the assumption of random assignment of instruments. In Table A2 in appendix A, four key variables that influence both Y_i and T_i are regressed on instruments separately.

In Table VI, the coefficients of instruments are statistically significant in both first stage OLS regressions run following equations (3) and (5) (model without X_i and model with X_i). Moreover, the sizes of those coefficients do not change much between two models. However, in both second stage IV regressions of equations (5) and (6), the estimated average treatment effect, the coefficient of $\hat{T}_i$, is 447.4 and 415.5 kcal respectively, which are statistically significant. The incorporation of X_i into the second stage model reduces the estimated average treatment effect, but the reduction is not substantial. On the other hand, it is substantially high compared to the previous models.

TABLE VI

ESTIMATES OF THE AVERAGE TREATMENT EFFECT ON Y_i UNDER IV REGRESSIONS

	OLS (Dep Var: T_i)				IV (Dep Var: Y_i)			
	(1)		(2)		(1)		(2)	
	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Constant	0.148	[0.007]	0.271	[0.017]	2199.0	[22.550]	2128.0	[46.750]
$\hat{T}_i$					447.4	[177.785]	415.5	[147.570]
Barisal	-0.0265	[0.013]	-0.0237	[0.013]				
Chitagong	-0.0439	[0.010]	-0.0470	[0.010]				
Khulna	-0.0751	[0.009]	-0.0770	[0.009]				
Rajshahi	-0.0340	[0.009]	-0.0467	[0.009]				
Sylhet	0.0595	[0.019]	0.0649	[0.018]				
Education of Head			-0.00617	[0.001]			5.568	[1.556]
Male Head			-0.0587	[0.014]			-11.66	[26.872]
Male 1-5			-0.0170	[0.006]			-165.6	[10.561]
Male 6-11			-0.000532	[0.005]			-71.97	[8.218]
Male 12-17			-0.00305	[0.005]			11.45	[8.055]
Male 18-60			-0.0264	[0.004]			50.65	[9.146]
Male 60+			-0.0153	[0.009]			44.41	[15.014]
Female 1-5			-0.00580	[0.006]			-196.1	[8.944]
Female 6-11			0.00721	[0.006]			-96.50	[8.347]
Female 12-17			0.0148	[0.006]			-35.80	[8.681]
Female 18-60			-0.00441	[0.005]			-24.53	[9.875]
Female 60+			0.0551	[0.011]			-41.36	[17.559]
Rural Location			0.0482	[0.007]			139.6	[13.083]
Landholding			-0.00343	[0.001]			12.87	[2.227]
Income			-0.0000251	[0.000]			0.0846	[0.006]
Observations	10070		10070		10070		10070	
Adjusted R^2	0.009		0.071		.		0.106	

Source: Author's calculation based on HIES 2005.

Note: Robust standard errors are in parentheses.

V. CONCLUSION

This study attempts to examine the effect of a number of SSN programs in Bangladesh on calorie consumption of poor households using HIES 2005. Three treatment effect evaluation designs were applied that are classified into two parts. First, mean difference method and matching estimators were applied that do not consider endogeneity problem in treatment dummy. Second, instrumental variables regressions method were applied that tackles endogeneity problem. To compare the estimated results, and then to see how much severe the endogeneity problem is, both types of methods were applied.

Mean difference and matching estimators produced negative average treatment effects on calorie consumption in the case of the full sample. To test unconfoundedness assumption, Education of head, which is free from treatment assignment, was used. It was found that this assumption was violated in the full sample. To test overlap assumption, histograms of propensity score for treated and non-treated households were plotted separately. A difference in propensity score distribution between two groups indicated that the overlapping assumption was also violated in the full sample.

To hold these two assumptions, some households were dropped on the criteria of propensity score. It was seen that after dropping observations both assumptions were satisfied. Then the average treatment effect on calorie consumption was estimated using reduced samples, and then the average treatment effect turned from negative values to positive values in each estimation method. However, the positive estimates were not statistically significant.

IV regression method was then applied to estimate the average treatment effect on calorie consumption using region dummies as instruments, which are independent on calorie consumption and assignment for treatment. The Bangladesh government has some regional bias to select households for treatment. Therefore, these instruments significantly influenced treatment dummy in both cases of the first stage regressions—with and without other covariates. In the second stage regressions, predicted values of the treatment dummy, estimated from the first stage regressions, produced significant coefficients in both cases of the second stage regressions—with and without other covariates.

The average treatment effect from the IV regression method suggests that there is a severe endogeneity problem in the treatment dummy. On the other hand, it can be said that many unobserved factors also influence the selection for treatment. Therefore, the IV estimates of the average treatment effect, 447.4 Kcal and 415.5 Kcal, seem high. However, the average treatment effect would have been higher than these IV estimates, if instruments were strong. If we can impute calorie from the food and cash benefits of the SSN programmes received by the treated households, it will be seen that that will be higher than these two figures of treatment effect under IV regressions.

On the other hand, it is likely that treated households are vulnerable group, and therefore they will use all of the benefits of the SSN programmes in food consumption, not in non-food consumption. Previous studies also support this. For example, Matin and Hulme (2003) have found that Vulnerable Group Feeding program has increased food consumption of treated households from two meals in a day to three meals in a day. Calorie equivalence of one meal taken by poor households in Bangladesh is usually more than about 500 Kcal.

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Appendix A

TABLE A1

REGRESSION RESULTS FROM PROBIT MODEL ON T_i USING FULL SAMPLE

	Coefficient	Std. Error
Constant	-0.371	(0.092)
Education of Head	-0.040	(0.005)
Male Head	-0.195	(0.061)
Male 1-5	-0.106	(0.032)
Male 6 -11	-0.025	(0.028)
Male 12-17	0.010	(0.029)
Male 18-60	-0.164	(0.032)
Male 60+	-0.096	(0.057)
Female 1-5	-0.046	(0.031)
Female 6-11	0.021	(0.029)
Female 6-17	0.102	(0.030)
Female 18-60	-0.020	(0.034)
Female 60+	0.280	(0.052)
Rural Location	0.314	(0.042)
Landholding	-0.062	(0.022)
Income	0.000	(0.000)
Barisal	-0.150	(0.066)
Chittagong	-0.258	(0.053)
Khulna	-0.475	(0.062)
Rajshahi	-0.275	(0.046)
Sylhet	0.256	(0.073)
Pseudo R^2	0.126	
Observation	10,070	

Source: Author's calculation based on HIES 2005.

Note: Robust standard errors are in parentheses.

TABLE A2

REGRESSION RESULTS OF SOME KEY HOUSEHOLD CHARACTERISTICS RUN ON INSTRUMENTS FOR RANDOMIZATION CHECK

	Education of Head	Male Head	Landholding	Income
Constant	4.163	0.887	1.166	1480.5
	[0.095]	[0.006]	[0.056]	[25.812]
Barisal	0.538	0.0570	0.813	-186.8
	[0.196]	[0.010]	[0.179]	[45.950]
Chitagong	-0.117	-0.0394	-0.347	-12.04
	[0.152]	[0.010]	[0.072]	[40.580]
Khulna	-0.124	0.0338	0.00897	-155.2
	[0.159]	[0.009]	[0.106]	[41.107]
Rajshahi	-0.596	0.0259	-0.0929	-269.0
	[0.135]	[0.008]	[0.076]	[33.436]
Sylhet	-0.171	-0.00677	0.420	207.2
	[0.237]	[0.015]	[0.206]	[74.766]
Observations	10,070	10,070	10,070	10,070
Adjusted R^2	0.003	0.008	0.008	0.010

Source: Author's calculation based on HIES 2005.

Note: Robust standard errors are in brackets.