Estimating the Average Treatment Effect of Social Safety Net Programmes in Bangladesh

MOHAMMAD MAHBUBUR RAHMAN*
Division of Economics, University of Stirling, Scotland, UK

ABSTRACT *Since the famine in 1974, the Bangladesh government and some national and international agencies have been providing food, or cash, or both to poor households through the Social Safety Net programmes. I seek to estimate how much these programmes affect the well-being of poor households. Most previous studies have estimated impacts of these programmes on calorie consumption, simply computing the raw differential. However, both observed and unobserved characteristics bias this treatment effect. Using fuzzy Regression Discontinuity (RD) design, I control for these selection effects.*

1. Introduction

A wide literature has established that food and cash transfers to poor people have positive effects on their nutritional status (Barrett, 1999; Quisumbing, 2003; Bouis and Haddad, 1992; Gibson and Rozelle, 2002; Pitt, Rosenzweig, and Hassan, 1990; Strauss, 1986). In quantifying these effects, leakages in distribution and the targeting error produce biased estimates (World Bank, 2005; Coady, Grosh, and Hoddinott, 2002). Leakages underestimate the effects, and the targeting error makes the treatment endogenous.

In this study, I examine the average treatment effect of a number of food and cash transfer programmes in Bangladesh, called Social Safety Net (SSN) programmes, on calorie consumption of poor households. In general, it is believed that SSN programmes play a key role in mitigating the food insecurity problem in Bangladesh. However, there is a targeting error in these programmes, as they often cannot reach eligible people. The main reason is corruption among all (observed and unobserved) factors[1]. Corruption also results in some leakages in distribution. Those involved in distribution sometimes distribute less than the

* *Correspondence Address:* Division of Economics, School of Management, Cottrell Building (Room: 3B61), University of Stirling, FK9 4LA, Scotland, UK. E-mail: mohammad.rahman@stir.ac.uk

allocated amount to beneficiaries. These two features (the targeting error and leakages) provide challenges in estimating the true average treatment effect. Unfortunately, it is not possible to correct for the bias resulting from any leakages unless we have the correct information on it. Bias due to the targeting error can be corrected if we have an instrument defining the eligibility of treatment.

RD design is a popular method of analyzing the average treatment effect. It is close to a randomised experiment, what it does is local randomisation assuming that all observed and unobserved characteristics that affect the outcome variable have the same distribution just below and above a cutoff that determines the eligibility of treatment. No other evaluation methods, such as matching estimators, linear regression, general instrumental variable (IV) regression and so forth, have this randomization feature, which ensures an unbiased treatment effect. When observable and/or unobservable characteristics affect treatment other than a treatment rule based on a cutoff, then the appropriate method is fuzzy RD design.

Here, I apply fuzzy RD design to estimate the local average treatment effect of SSN programmes. In fuzzy RD design, a binary instrument can be used after ensuring that it satisfies local randomisation, and it affects the outcome through treatment receipt. I use such a dummy instrument constructed by exploiting an income cutoff determining the eligibility of a household to become a beneficiary of SSN programmes. The Household Income and Expenditure Survey (HIES) 2005, a nationally representative survey in Bangladesh, is used as the data source for this study. Approximately, 12% of households in HIES 2005 are treated by at least any one SSN programme.

Few studies have analysed the effects of SSN programmes on calorie consumption. Most of them (Ahmed and del Ninno, 2002; Roy et al., 2008) have not considered the bias resulting from the targeting error, that is the endogeneity of the treatment, but have used the simple raw differential of outcomes between the treatment and control groups. Ahmed, Quisumbing, and Hoddinott (2007) used a propensity score matching estimator on primary survey data claiming that the targeting error is very low. However, they used programme participants' income distribution only to assess the targeting effectiveness. While income is a key factor in determining the target group, this is subject to measurement errors in a developing country context.

del Ninno et al. (2001) and Rahman (2012) applied an IV technique to correct for possible bias resulting from imperfect compliance. Both of these studies found a significant positive average treatment effect. However, del Ninno et al. (2001) potentially suffers from the violation of the exclusion restriction, and Rahman (2012) from the weak-instrument problem (Staiger and Stock, 1997).

In Bangladesh, different SSN programmes have different target groups. But there is a common criterion to be selected in any SSN programme, which is poverty or poor/poorest status of the eligible people. In 2005, a World Bank report (World Bank, 2005) mentions that the bottom 10% poor people among the poor were the target group for SSN programmes. Therefore, I consider 1st decile of the distribution of poor people's household per capita income as the cutoff point (290). Below that cutoff point, the bottom 10% poor people among the poor, who are eligible for SSN programmes, exist. Because of poor institutional set up, such specific cutoff is not strictly followed. I then select 285 as the final cutoff, which gives the highest discontinuity in the probability of treatment. It increases the strength of dummy instrument.

One may concern about the use of cutoff point endogenously. This does not make any flaw of this study, because this does not violate any assumption of RD design. To apply fuzzy RD design, the first main issue is to have discontinuity in the probability of treatment at a cutoff point on the assignment variable, given that other programme specific factors affect the eligibility of treatment (Lee and Lemeiux, 2010). Then the local randomisation needs to be satisfied. This study uses two established ways (discontinuities in covariates and the discontinuity in the density of the assignment variable) for checking the local randomization.

According to Ravallion (2007), a variable that is associated with outcome, such as income here, cannot be an assignment variable. However, in the RD design, an outcome variable can be an assignment variable when it is determined before any treatment programme is taken. For example, if any scholarship is provided based on any test score, then that test score can be used as an assignment variable, and any future or post treatment/scholarship test score can be an outcome variable. That means in this study the assignment variable (household per capita monthly income) is supposed to be taken when households did not get benefits from SSN programmes. This income should be pre-programme income of households because pre-programme income defines households' eligibility to be selected into SSN programmes. However, HIES 2005 is a single cross section survey, and it is not possible to get pre-programme income. Rather, I estimate the proxy of pre-programme income, by subtracting the monetary value of SSN programmes' benefits from current income[2]. I think that this is a good proxy. For example, one household has been receiving benefits from a SSN programme since 2001, and therefore we should take income of that household before 2001, because based on income before 2001, that household was selected for programme's benefits. As that household is still receiving benefits from a SSN programme, we can believe that its income did not change.

However, the assignment variable (current income minus the monetary value of SSN programme's benefits) may not always give credible information about pre-programme income. The assignment variable may suffer from the measurement error. As this variable has two parts (current income and the monetary value of SSN programme's benefits), both parts may suffer from the measurement error. There are many reasons of the measurement error. For example, data entry error, misreporting of current income and error in recalling current income in the case of informal job are general reasons. Some households may suffer from a shock that temporarily raises (lowers) their current income. Besides, some households do not receive all the benefits they are entitled to because, for example, some benefits are diverted by programme implementers. Some households are unable to collect some of their benefits (for example, a head is sick and is unable to get to a benefit receiving point on the day benefits are distributed), and some households are unable to work their full entitlement (in the case of public works). Because of the measurement error in the assignment variable, some eligible (ineligible) households below (above) cutoff income are seen as untreated (treated). Therefore, treatment is endogenous. Different selection criteria of different programmes (for example, being female headed households, having freedom fighter(s) in a household, disability) and the corruption (for example, political affiliation of a household, bribe) also make treatment dummy endogenous because they also affect treatment. Some other observed and unobserved covariates such as landholding, location, awareness of programmes also affect treatment, and therefore they cause endogeneity in treatment. To remove bias in treatment effect resulted from endogeneity in treatment, I use fuzzy RD design, which is an IV method.

The measurement error in calorie consumption is not a cause of biased estimates in the previous studies. Rather, the lack of proper treatment to tackle the endogeneity of the treatment variable is a reason of biased treatment effects. First of all, it needs to mention that every data is suffered by the measurement error. However, if there is any measurement error in the outcome variable (calorie consumption here), we do not need to be worried if the error is random between the treated and untreated groups. Because then the measurement error in the outcome variable will not cause any bias in the treatment effect estimate. If the error is severe in one group, and less severe in another group, then it will produce biased treatment effects. In general, people assume that the error is random between treated and untreated groups. This is a logical assumption. Besides, it is difficult to find out errors in data.

On the other hand, if there is a measurement error in those variables such as income, landholding and age, that define eligibility of treatment, then we can say that the treatment variable is endogenous because the targeting error exists. It does not matter whether there are random errors or not. To remove the endogeneity problem, we need to find out a suitable instrument. In most of the previous studies, IV regressions were not run. Those ran IV regressions can be criticised, because instruments are questionable as they are weak. For example, del Ninno, C. et al. (2001) used some household characteristics (income, age, sex and so forth) as instruments, which have strong correlation with the outcome variable. Therefore, they are not valid instruments. Ahmed, Quisumbing, and Hoddinott (2007) did not run the IV regression, but they said that income is the key factor in defining the eligibility of treatment. The measurement error in income results the targeting error.

The rest of the study is organised as follows. A review of SSN programmes is provided in section 2. The background and data are discussed in Section 3. The methodology is described in Section 4. Estimated results are analyzed in Section 5. Section 6 provides robustness checks, and Section 7 discusses results and the validity of the RD design used here. Section 8 concludes the study.

2. Social Safety Net Programmes

Since the famine in 1974, the Bangladesh government and some national and international agencies have been supplying food, or cash, or both, free of charge to a number of food insecure households under various SSN programmes. The programmes are one of the key policy tools for fighting against food insecurity. As a result, the number of SSN programmes is increasing, and by 2008 there were 66 SSN programmes (Bangladesh Economic Review, 2008). Expenditure by these programmes is also increasing. For example, the Bangladesh government spent around USD 366 million, 7% of its annual budget and 0.9% of real GDP, on SSN programmes in 1995-96 (World Bank, 2006), increasing to USD 2450 million, 13.32% of its annual budget and 2.14% of real GDP, in 2007-08 (Bangladesh Economic Review, 2008). World Food Programme (WFP) and other agencies, in general, spend twice as much as the government (Ministry of Food and Disaster Management, 2008). The coverage of these programmes is increasing too, though there is no exact figure on this. In fact, the government and all other agencies have the aim to cover all poor households. The rest of this section provides a brief review of some specific SSN programmes whose effects are examined in this study.

Vulnerable Group Feeding (VGF). This is the first SSN programme which was started by WFP during the famine in 1974. Now, the Bangladesh government and WFP are jointly operating the programme. It supplies only rice to the poorest households. It covers all regions of Bangladesh and has the highest number of beneficiary households of all SSN programmes. Community leaders select beneficiary households for this programme and provide them a card called the VGF card to get continuous benefits after selection. Every selected household receives one VGF card which is given to the household head. Until 2009, there were around 10,467,000 card holding households. 10 kilograms (kg) of rice per month are given to each card holders without any conditions. This is mainly a continuous programme where card holders receive rice every month until their eligibility is expired, but it also acts as a transitory programme at times of disaster. When any disaster hits in an area, the programme distributes VGF cards immediately to affected households, eligible for a minimum of three months.

Old Age Pension (OAP). Under this programme, the government provides 250 Taka per month to poor, old aged individuals who are not entitled to a formal pension (given to civil servants only) and who are aged 65 years or over. The local government selects beneficiaries. More than one member of a household can be selected. There are currently 10 million individuals aged 65 or over in Bangladesh. Of them, only 2 million (of whom 50% are women) are treated under this programme.

Vulnerable Group Development (VGD). This programme, which was started by WFP in 1975, is now being jointly operated by the government, WFP and BRAC, a Non-Governmental Organisation (NGO). It is targeted at female headed households with no male members to earn a sufficient living. Initially, eligible women were those who lost male family member(s) during the liberation war in 1971. This programme provides financial loans and training, in addition to food, to the female head of the beneficiary household, without any conditions. Each beneficiary household receives 30 kg wheat per month. Moreover, it provides 18-24 months employment for the eligible women and provides 150 hours of training to them to increase income earning capacity. Until 2010, the number of beneficiary households was 500,000. Beneficiary households are selected by local government officials. In some cases, VGF has been converted to VGD.

Test Relief (TR). This is an irregular programme, which creates temporary employment in rural areas for poor people through repairing roads, bridges and so forth. It starts with a formal advertisement. A beneficiary of this programme receives 3.5 kg rice daily for a maximum of 30 days. Again, the local government selects beneficiaries.

Freedom Fighters Pension (FFP). This programme is designed by the government for households whose members participated in the liberation war in 1971. Of 125,000 households participating in 2008-09, each received 900 Taka (about US $13) per month under this programme.

Cash for Education (CFE). This is a conditional programme operated by the government. Since 2003, it has been providing cash to the poorest households subject to regular attendance of their children at school. A beneficiary household receives 100 taka per month for one pupil and 125 taka per month for more than one pupil.

Gratuitous Relief (GR). This is another irregular programme designed to provide emergency relief to households affected by natural disasters such as flood or, cyclone. It was started by the government during the flood in 1998 when 51,200 metric tons of rice were distributed to flood victims. The average amount of rice received per flood-affected household was around 15 kg.

Integrated Food Security (IFS). Since 2000, this programme has been providing food to vulnerable people in addition to capacity building for them with financial help from the government and other agencies. Until 2005, 250,000 individuals were treated, and entitled to receive 20 kg wheat and 75 taka per month.

Food for Work (FFW). Operated by the government, this programme generates 100 days employment in the lean season for hard core poor through construction and reconstruction of rural infrastructure. Here, both food (rice and wheat) and cash are provided to the beneficiary person. This programme is targeted at poor individuals, not households. Therefore, more than one member from a household can receive benefits. The local government selects beneficiary persons. The programme does not entitle beneficiaries any fixed amount of benefit, as it depends on the level of work a beneficiary does. In 2008-09, about 1,600,000 persons earned, on average, about 3000 Taka (US $50) monthly under this programme.

Rural Maintenance Programme (RMP). This programme that was started in 1983 by CARE, an NGO, providing rural disadvantaged women employment through regular maintenance of rural roads. The local government selects eligible women and makes a contract with them for four years. As per contract, women have to work 6 days a week for 6 hours a day at lower than the local minimum wage. However, banks disburse four-fifths of their wage and retain the rest of it as savings. Until 2005, 181,000 women had participated in this programme.

3. Methodology

3.1. Identifying the Assignment Variable and the Cutoff

We saw in the preceding section that different programmes have different target groups (for example, women, aged persons, freedom fighters and so forth). However, all groups belong to the poorest in the country. In 2005, the year of this study, the Bangladesh government and WFP targeted the 10% poorest households among the poor, the eligible group for treatment, for support through SSN programmes (World Bank, 2005).

Like many other countries, Bangladesh identifies poor households as being those with household per capita monthly income below the poverty line. Households below the first decile of the distribution of per capita monthly household incomes of poor households are the 10% poorest households among the poor. Therefore, per capita monthly income for each household in 2005 is the assignment variable defined by x, and the first decile of the distribution of per capita monthly household incomes of poor households is the cutoff point defined by c. Note that x excludes any amount of benefits received from SSN programmes.

The target group lies below c, and all households under the target group are eligible for treatment. However, some households above the cutoff are treated, and some households below the cutoff are not treated, due to measurement error in x, and observed and/or unobserved characteristics that affect treatment. However, we assume that treated households are proportionately higher in the target/eligible group at least near the cutoff than that in the non-eligible group. We can expect that the conditional probability of treatment is discontinuous at the cutoff. This point, which is the main assumption of fuzzy RD design, enables us to apply fuzzy RD design here.

3.2. Identification Strategies of the Local Average Treatment Effect

In this study, the potential outcome denoted by y is per capita daily calorie consumption for each household, and w denotes a binary variable of treatment with 1 if a household is treated by at least one SSN programme and 0 otherwise. Now, the potential outcome can be written in the following structural form equation (Angrist and Pischke, 2009):

$$y = \mu_0(x) + \tau_c w + e, \tag{1}$$

where τ_c denotes the local average treatment effect on y, which is estimated in the fuzzy RD design by extrapolating the compliance group (Imbens and Angrist, 1994), and

$$y = \begin{cases} y_1 = \mu_0(x) + \tau_c + e & \text{if } \mathrm{w} = 1 \\ y_0 = \mu_0(x) + e & \text{if } \mathrm{w} = 0, \end{cases}$$

where y_0 denotes the potential outcome of a household without treatment that is explained by x in $\mu_0(x)$ and other (observed and unobserved) covariates in the error term denoted by e, and y_1 denotes the potential outcome of a household with treatment, where τ_c is added with y_0.

The administrators set the 10% poorest among the poor as the target group. If it were a sharp RD design, all households in the target group would have been treated, and no household in the control/non-target group would not have been treated. However, both observed and unobserved covariates and the measurement error in the assignment variable also affect the selection for treatment[3]. We may find some households in the target group as untreated and vice versa. We expect that the proportion of treated households in the target group is higher than that in the control group. The conditional probability of treatment $\Pr(w=1|x)$ is expected to be discontinuous at the cutoff, c (the first decile of poor people's income). Thus, it can be written in the following form:

$$\Pr(w=1|x) = \mathrm{E}(w|x) = \begin{cases} g_1(x) & \text{if } \mathrm{x} \le \mathrm{c} \\ g_0(x) & \text{if } \mathrm{x} > \mathrm{c}, \end{cases}$$

where $g_1(c) > g_0(c)$ indicates discontinuity in $\Pr(w=1|x)$ at c. Now, $\mathrm{E}(w|x)$ can be written in the following functional form:

$$\begin{aligned} \mathrm{E}(w|x) &= g_0(x) + [g_1(x) - g_0(x)]z \\ &= g_0(x) + \pi z, \end{aligned}$$

where $g_1(x) - g_0(x) = \pi$, and $z = 1[x \leq c]$, an instrument for w, determines the eligibility of households to be beneficiaries of SSN programmes. Thus, w is

$$w = g_0(x) + \pi z + \xi_1, \tag{2}$$

where ξ_1 denotes an error term that captures observed and unobserved factors plus measurement error in x influencing w. Equation (2) is a reduced form equation, while equation (1) is a structural one. From equation (1), the local average treatment effect, τ_c, is not identified as $\mathrm{E}(w, e) \neq 0$, which indicates that w is an endogenous variable.

τ_c can be identified applying Indirect Least Squares (ILS). Under ILS, we need to substitute equation (2) into equation (1). After doing this, we have the following reduced form equation of y:

$$\begin{aligned} y &= \mu_0(x) + \tau_c\{g_0(x) + \pi z + \xi_1\} + e \\ &= \mu_0(x) + \tau_c g_0(x) + \tau_c \pi z + \tau \xi_1 + e \\ &= k(x) + \tau_c \pi z + \xi_2, \end{aligned} \tag{3}$$

where $\mu_0(x) + \tau_c g_0(x) = k(x)$, $\tau_c \xi_1 + e = \xi_2$. Now, we can estimate the local average treatment effect, τ_c, dividing $\tau_c \pi$, the coefficient at z in equation (3), by π, the coefficient at z in equation (2). Alternatively, we can run the following IV regression:

$$y = \mu_0(x) + \tau_c \mathrm{E}(w \mid x) + e, \tag{4}$$

where the coefficient at $\mathrm{E}(w \mid x)$, τ_c, is the local average treatment effect of compliers, and $\mathrm{E}(w \mid x)$ comes from equation (2), which can be treated as the first stage regression.

To identify τ_c, the identification assumption – the local randomisation (the continuity of $\mathrm{E}(y_0 \mid x)$ at c) – needs to be satisfied. Otherwise, the local average treatment effect estimation will be biased. The local randomisation assumption states that observed and unobserved covariates that affect the outcome variable (calorie consumption here) are continuous at the cutoff point. We can test the local randomisation to some extent by checking whether observed covariates are continuous at the cutoff point (Imbens and Lemieux, 2008). Alternatively, we can check discontinuity in the density of the assignment variable at the cutoff point (McCrary, 2008). Any discontinuity in the density of the assignment variable will also violate the local randomisation assumption. This second test is considered when we assume that there are cases of misreporting of income, and they change households' position from right of the cutoff point to the left of it. In general, misreporting of income is unlikely in this study because the cutoff income is not explicitly known to households. However, I consider also the second test of local randomisation as it indirectly checks whether both observed and unobserved covariates that affect calorie consumption are continuous.

It should be noted that observed and unobserved covariates that affect only treatment (but not calorie consumption) are not considered for the local randomisation test. In other words, their discontinuities at the cutoff point will not violate the local randomisation assumption. They only cause the endogeneity problem, which is dealt with fuzzy RD design. For example, some selection criteria such as being freedom fighters, disability and political affiliation affect only treatment. They do not affect calorie consumption. Calorie demand literatures do not use them in calorie demand function (Bouis and Haddad, 1992). Their discontinuities (observed differences on either side of the cutoff income) will not violate the local randomisation assumption. On the other hand, being female headed, landholding, location, household size and so forth affect calorie consumption (they also affect treatment), and they are considered for the local randomisation test. I consider those variables for the local randomisation test, which are regularly used in calorie demand function.

4. Background and the Data

The Bangladesh Bureau of Statistics has been conducting the HIES, a cross section survey, on Bangladeshi households in every five years since 1991 with financial help from the World

Bank (WB). HIES collects data about households' and their members' characteristics, households' income, expenditure, food and non-food consumption, agricultural production, and many other household related information from all regions of Bangladesh. This study uses HIES 2005, the latest available, which has a sample size of 10,070 households. It contains sufficient information about SSN programmes compared to the previous HIESs.

There is a section about SSN programmes in the HIES 2005 questionnaire. Questions are designed to collect information on whether households are receiving treatment under any SSN programmes, the duration of treatment, the reasons why they qualify for treatment, the amount of cash or food they receive from the programme, whether they have paid any bribe to be selected for treatment, and from whom they collect cash and food. Therefore, I estimate the treatment dummy, w, from the first question, which reveals that 1226 households (12% of total households) are treated by at least one SSN programme. Most of the treated households are beneficiaries of just one SSN programme, and only 48 households receive treatment from more than one programme.

Households' food consumption and expenditure are collected using the recall method where households provide information on their consumption and expenditure during the past two weeks on an itemised basis. The unit of measurement is different for different items, such as kg for rice, vegetables and so forth, litre for oil, milk and so forth, number for egg, banana and so forth. To estimate the calorie consumption of households, all food items are converted into a single unit of measurement, grams. The calorie content for each food item is available from the Food and Nutrition Department, University of Dhaka, Bangladesh. The following procedure is followed to estimate y of a household:

$$y = \frac{\sum_k (C_k * c_k)}{H},$$

where C_k is the amount of daily consumption of food item k (from all sources such as own production, purchases, gifts, SSN programmes) measured in grams, c_k is the per gram calorie content in kilo calorie (kcal) of food item k, and H is the household size.

The assignment variable, x, for a household (excluding any benefits received) is estimated by dividing monthly income from all sources by household size. This variable may contain a measurement error if households do not provide correct information to surveyors. There are also many other reasons of measurement error in income (for example, data entry error, error in recalling income in the case of informal job, income shock, leakages of programmes' benefits and so forth). However, the measurement error in income is one source of fuzzy RD design. Because of it, some eligible households do not receive treatment, and vice versa.

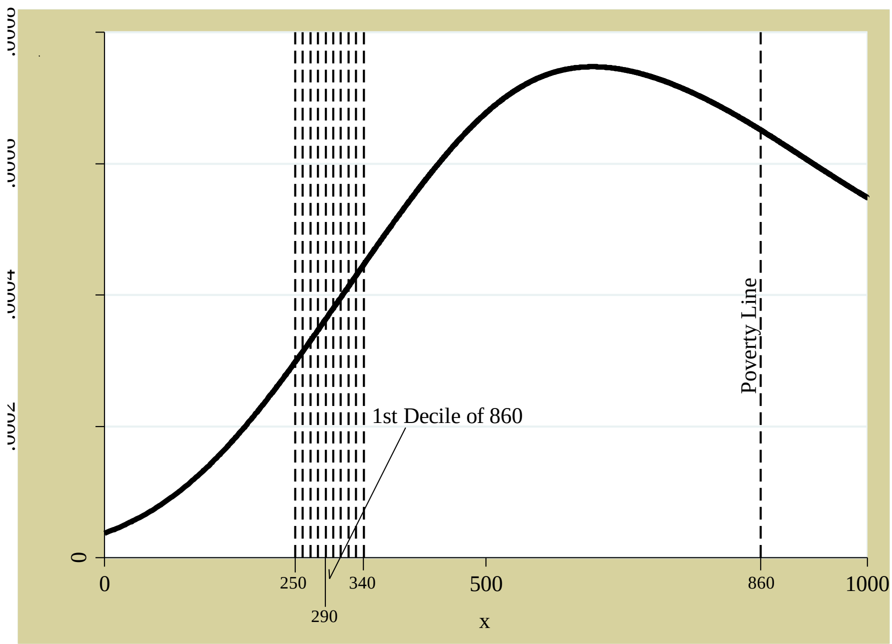

Figure 1: Truncated kernel density of $x \leq 1000$, HIES 2005

In 2005, the national poverty line income in Bangladesh was 860 taka (Bangladeshi currency)[4]. 290 taka that is the first decile of the distribution of the per capita monthly household income (x) of poor households below the national poverty line is named as the official cutoff[5]. Figure 1 presents them with dotted vertical lines through truncated kernel density of x where values above 1000 are ignored to concentrate near the cutoff. In addition, other points near 290 are examined to identify the highest discontinuity in $\mathrm{E}(w|x)$ with statistical significance to increase the strength of z as an instrument. Some such points around 290 are also shown in Figure 1 by dotted vertical lines.

The size of the discontinuity in $\mathrm{E}(w|x)$ can be higher at any other points than at the official cutoff, largely due to the measurement error in x, but also corruption. Therefore, if the local average treatment effect is estimated at an unknown point with the highest size of discontinuity in $\mathrm{E}(w|x)$ with statistical significance, the number of compliers will increase, and thus the robustness of the local average treatment effect will increase.

Table 1: Descriptives (Mean)

Variable	Whole Sample		Regression sample	
	Treated	Untreated	Treated	Untreated
y	2193.27	2261.78	2133.98	2128.76
w	1	0	1	0
x	808.79	1459.35	543.04	636.51
$z = 1(x \le x_0 = 290)$	0.12	0.03	0.16	0.06
Rice price per kg (Taka)	21.87	21.86	21.79	21.49
Head's education (years of schooling)	1.65	4.34	1.32	2.31
Sex of head (1 if male, 0 otherwise)	0.81	0.91	0.82	0.92
Household size	4.59	4.90	4.67	5.02
Number of adult (18-60 aged)	2.10	2.55	2.06	2.37
Location (1 if rural, 0 otherwise)	0.79	0.62	0.81	0.70
Landholding (decimal)	0.46	1.27	0.34	0.56
Total observations	1226	8844	959	4172

Descriptive statistics for all variables (key variables x, y, w, z and observed covariates) used in this study are presented in Table 1. Comparing the means of the variables of the treated and untreated households, it is clear that all observed covariates differ between the two groups both in the whole sample and the regression sample ($x \le 1000$)[6]. These differences indicate that randomisation (that observed and unobserved covariates are similar between treated and untreated groups) does not exist. However, to identify τ_c, RD design considers local randomisation around the cutoff point, which may exist. Local randomisation is checked in the next section by two identification tests suggested by Imbens and Lemieux (2008) and McCrary (2008).

5. Estimated Results

Using the regression sample, OLS regressions of equations (2) and (3), and an IV regression of equation (4) with the quartic form of x are run considering 250, 260, 270, 280, 285, 290 and 300 as cutoff points on x. Table 2 presents discontinuity parameters ($\hat{\pi}$ and $\hat{\tau}_c\hat{\pi}$) and treatment effect parameter ($\hat{\tau}_c$) at these cutoff points. The value of $\hat{\pi}$ is the highest at 285, which indicates that $z = 1[x \leq 285]$ will be a stronger instrument at this cutoff than any other points. The t-ratio (ratio of estimate and standard error) is also the highest at this point not only for $\hat{\pi}$ but also for the local average treatment effect $\hat{\tau}_c$. Therefore, to estimate the local average treatment effect, 285 can be considered as the cutoff point rather than the official cutoff, 290. Before fixing it as the final cutoff point let us consider local regression estimates.

Table 2: Estimates from quartic regressions

Cutoff	OLS $\hat{\pi}$	OLS $\hat{\tau}_c\hat{\pi}$	IV $\hat{\tau}_c$
250	0.032(0.042)	63.54(53.36)	1,965.80(3030.00)
260	0.067(0.041)	95.15(51.50)	1,425.70(1152.70)
270	0.104(0.039)	105.00(49.58)	1,011.30(605.50)
280	0.095(0.038)	117.00(48.11)	1,226.10(695.00)
285	0.149(0.038)	104.00(47.44)	702.00(361.80)
290	0.115(0.037)	92.81(46.71)	808.10(478.10)
300	0.082(0.036)	63.12(45.84)	774.20(653.10)

Note : Robust standard errors are in parentheses.

Table 3 shows the same parameters, but now local linear regressions are run in all cases with bandwidths of 30, 60 and 120, and considering 270, 280, 285 and 290 as cutoffs. Other cutoffs are ignored due to poor estimates as shown in Table 2. At 285, estimates show consistent behavior even in narrower bandwidths. As bandwidth decreases, $\hat{\pi}$ increases up to bandwidth of 60, $\hat{\tau}_c\hat{\pi}$ decreases but remains positive, and $\hat{\tau}_c$ also decreases but remains positive. $\hat{\pi}$ is significant in all bandwidth cases. On the other hand, at other cutoff points, $\hat{\tau}_c$ becomes negative in narrower bandwidths because either $\hat{\pi}$ becomes negative or $\hat{\tau}_c\hat{\pi}$

becomes negative. Thus, 285 is chosen as the final cutoff for further analyses.

Table 3: Discontinuities from local linear regressions with different bandwidths

	Cutoff=270			Cutoff=280		
Bandwidth	$\hat{\pi}$	$\hat{\tau}_c\hat{\pi}$	$\hat{\tau}_c$	$\hat{\pi}$	$\hat{\tau}_c\hat{\pi}$	$\hat{\tau}_c$
Cutoff	0.06(0.04)	116(46)	2053(1685)	0.05(0.04)	134(44)	2494(1980)
120	0.06(0.07)	140(71)	2277(2713)	0.05(0.06)	141(67)	2655(3389)
60	0.13(0.09)	156(101)	1232(1207)	0.15(0.09)	129(97)	888(866)
30	0.09(.013)	-72(152)	-841(2172)	-0.11(0.13)	77(130)	-674(1287)
	Cutoff=285			Cutoff=290		
Bandwidth	$\hat{\pi}$	$\hat{\tau}_c\hat{\pi}$	$\hat{\tau}_c$	$\hat{\pi}$	$\hat{\tau}_c\hat{\pi}$	$\hat{\tau}_c$
Cutoff	0.11(0.04)	114(43)	1082(565)	0.09(0.04)	87(42)	942(590)
120	0.16(0.06)	124(66)	766(500)	0.08(0.06)	76(65)	959(1091)
60	0.37(0.09)	39(97)	107(266)	0.19(0.09)	-23(95)	-119(499)
30	0.22(0.12)	40(133)	177(607)	0.08(0.12)	-30(134)	-394(1810)

Note: Robust standard errors are in parentheses. $\hat{\tau}_c$ is estimated from IV regression. Cutoff under bandwidth means that the size of bandwidth is equal to the size of a cutoff.

Table 4: Discontinuities from local regressions at the cutoff, 285

Model Type	$\hat{\pi}$	$\hat{\tau}_c\hat{\pi}$	$\hat{\tau}_c\hat{\pi}/\hat{\pi}$	$\hat{\tau}_c$
Local Linear	0.11	106	964	967
	[0.04]	[43]		[509]
Local Linear with Covariates	0.11	123	1118	1111
	[0.04]	[41]		[522]
Local Quadratic	0.17	84	494	482
	[0.04]	[48]		[300]
Local Quadratic with Covariates	0.16	107	669	665
	[0.04]	[47]		[336]
Local Cubic	0.11	126	1145	1104
	[0.05]	[57]		[689]
Local Cubic with Covariates	0.11	155	1409	1463
	[0.05]	[55]		[845]
Local Polynomial [a]	0.21	123	586	623
	[0.07]	[62]		[422]
Local Polynomial with Covariates	0.20	153	765	843
	[0.07]	[60]		[475]

Note: Robust standard errors are in parentheses. All models are run within IK optimal bandwidth, 285. Household characteristics mentioned in Table 2 are used in models with covariates. $\hat{\tau}_c$ is estimated from IV regression. IV regressions with covariates uses covariates as instruments as well as exogenous variables.

[a] Using Bin test (Lee & Lemieux, 2010), optimal polynomial terms are chosen as 8 in w regression and 4 in y regression.

Imbens and Kalyanaraman (2009) (IK) optimal bandwidth for local linear regressions at the cutoff, 285, is also 285. Using this bandwidth, I run some local polynomial regressions along with local linear regressions. Table 4 reports the key parameters ($\hat{\pi}$, $\hat{\tau}_c\hat{\pi}$ and $\hat{\tau}_c$) from these regressions. In addition, I run regressions incorporating the other covariates listed in Table 1. This will control other covariates' effects on the outcome variable near the cutoff, and therefore will produce a valid treatment effect in case the identification condition (the continuity of $E(y_0 \mid x)$ at the cutoff, see Imbens and Lemieux, 2008) is not satisfied. I also choose optimal polynomial orders for w and y regressions using the Bin test (Lee and Lemieux, 2010). The optimal polynomial order in w regression is 8, and that in y regression is 4, and thus I have named them local polynomial (regressions). The key parameters from these regressions are also reported in Table 4. In the case of local polynomial regressions (with the optimal polynomial orders), $\hat{\pi}$ contains the highest value that indicates the strength of the instrument, z. The local average treatment effect, $\hat{\tau}_c$, is here 623 kcal, which increases to 843 kcal if observed covariates are incorporated in the regressions with the optimal polynomial orders. I choose 843 kcal as my final estimate, which is significant at the 10% level.

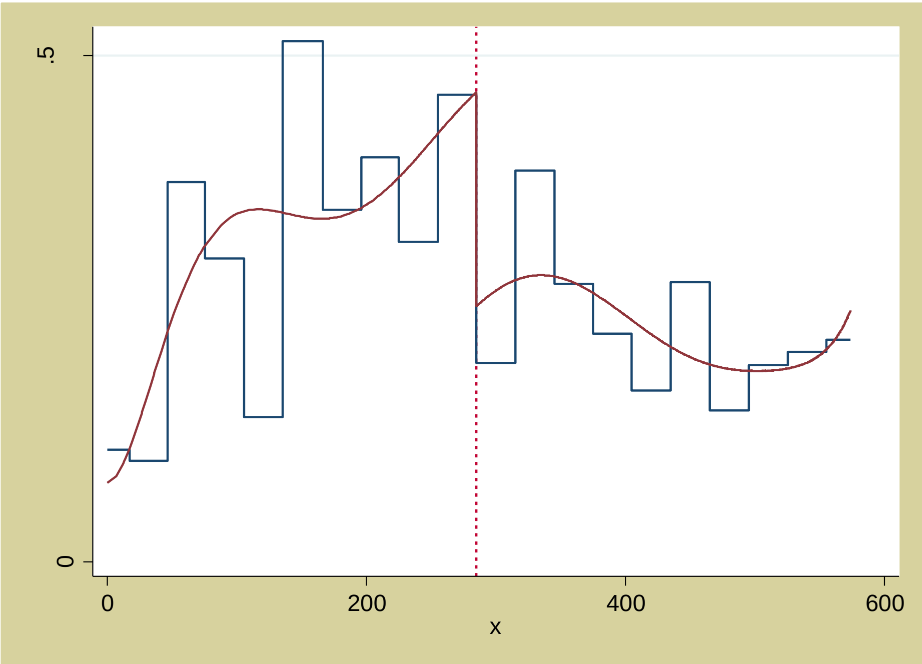

Figure 2: Estimating $E(w \mid x)$ from polynomial regression mentioned in Table 4 with mean values of w under bins of 30 bin size ($c = 285$)

Figures 2 and 3 show predicted lines from these polynomial regressions with the optimal polynomial orders along with bin means under 30 bin size. We see that both figures show discontinuities in $\mathrm{E}(w \mid x)$ and $\mathrm{E}(y \mid x)$ respectively at the cutoff point 285. To note that we have noisy data around the cutoff, and therefore figures with raw data cannot give clear idea about any discontinuity. Specially, in the case of the probability of treatment, figures with raw data are not meaningful. In these cases, Lee and Lemeiux (2010) have suggested to show predicted values (from regressions) in figures with mean values under bins on the assignment variable.

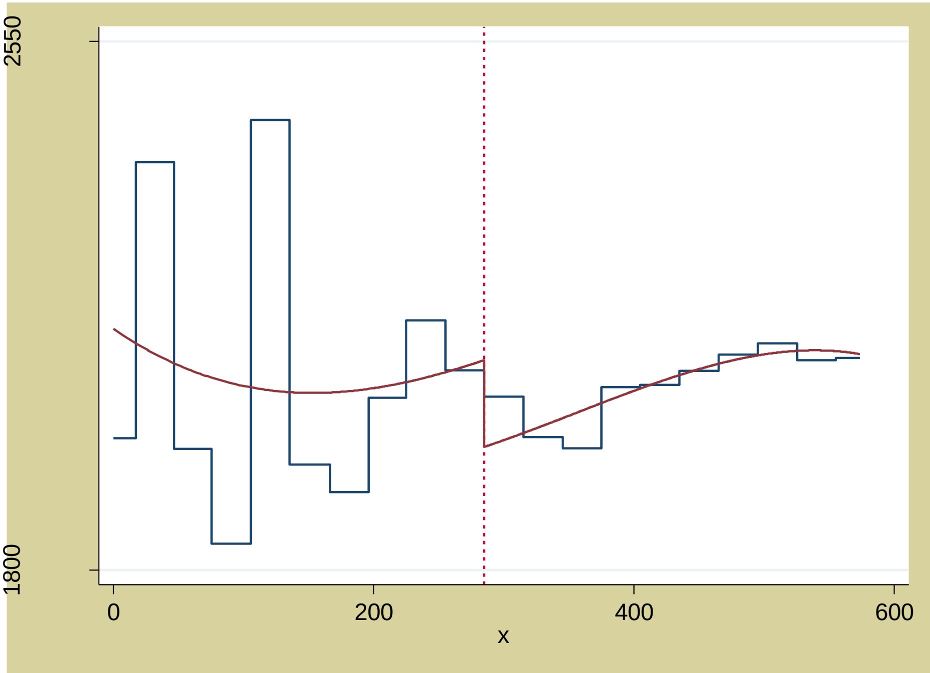

Figure 3: Estimating $\mathrm{E}(y \mid x)$ from polynomial regression mentioned in Table 4 with mean values of y under bins of 30 bin size ($c = 285$)

To test the identification condition – the local randomisation (the continuity of $\mathrm{E}(y_0 \mid x)$ at c), observed covariates that affect calorie consumption are checked to see whether they are discontinuous at the cutoff (see Table 5). From both local linear and quartic regressions under the IK optimal bandwidth, it is seen that none of these covariates is significantly discontinuous at 285, which indicates that the identification condition is satisfied (Imbens and Lemieux, 2008). On the other hand, the density of the assignment variable is seen as continuous in the local quartic regression (see Figure 4). A histogram plot also suggests that the density of x is not discontinuous at 285. Only a local linear regression suggests discontinuity, but it has very bad fit. Thus, it can be concluded that the identification condition is satisfied at 285 (see McCrary, 2008).

Table 5: Discontinuities of Covariates from Local Linear Regressions at the Cutoff, 285

	Local Linear		Local Quartic	
Variable	Discontinuity	Robust S.E.	Discontinuity	Robust S.E.
Rice price per kg (Taka)	-0.23	0.32	-0.21	0.47
Heads Education (years of schooling)	-0.42	0.27	-0.64	0.40
Sex of head (1 if male, 0 otherwise)	-0.03	0.03	-0.02	0.04
Household size	0.14	0.17	0.40	0.25
Number of adult (18-60 aged)	0.05	0.09	0.03	0.13
Location (1 if rural, 0 otherwise)	-0.06	0.04	-0.03	0.05
Landholding (decimal)	-0.04	0.09	-0.02	0.13

Note: Local linear regression of each variable is run on X and z at 285 cutoff, and local polynomial regression of each variable is run on X, x^2, x^3, x^4 and z at the same cutoff. In every case, IK optimal bandwidth, 285, is used. For each variable, the coefficients of z are reported with robust standard errors.

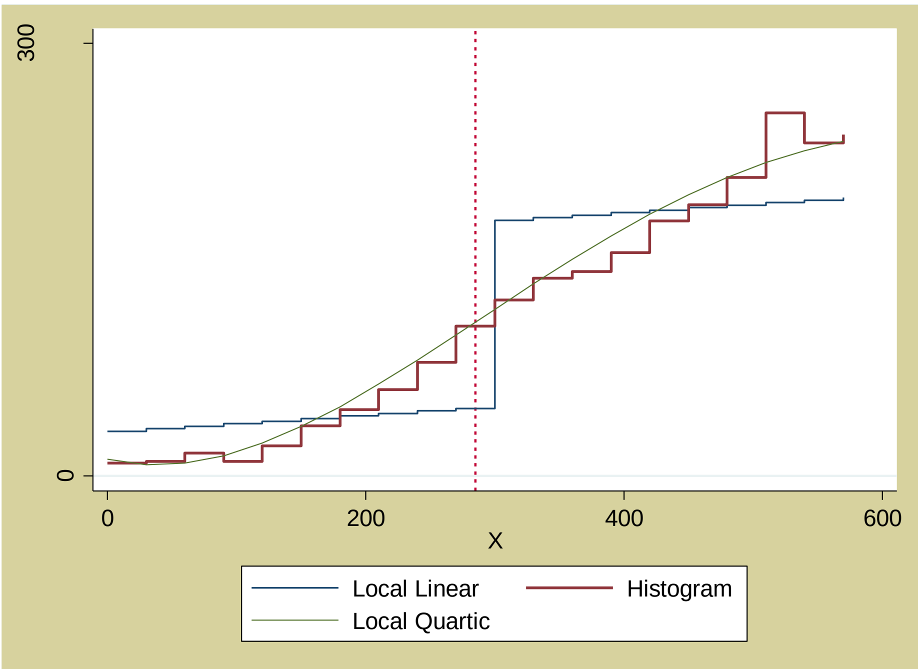

Figure 4: Estimating density of x from local (linear and quartic) regressions suggested by McCrary (2008) ($c =$ 285)

6. Robust Analysis

The issue is that a programme might have more than one criterion/cutoff to determine the eligible people for treatment. To apply fuzzy RD design, we can select anyone. But the condition is that the probability of treatment must be discontinuous at the cutoff point. If there is a treatment effect, result will be same or equivalent if we consider another criterion or cutoff. Therefore, this section considers the OAP with another criterion (age of the oldest person), to check whether it also gives similar results. If we see that results are similar, our results are robust. If I did the same exercise for other criteria or cutoffs, the robust analysis would have been stronger.

Table 6: The number of programme wise treated households near the cutoff on x

Programme	$x \leq 570$	$165 \leq x \leq 405$	$225 \leq x \geq 345$	$255 \leq x \leq 315$
VGF	180	89	53	26
OAP	79	43	20	8
VGD	106	64	31	13
TR	87	33	22	8
FFP	54	19	13	8
CFE	20	8	4	2
GR	4	3	2	1
IFS	1	1	1	1
FFW	1	0	0	0
RMP	0	0	0	0
Others	0	0	0	0

As several SSN programmes are considered here to estimate the local average treatment effect, it might be interesting to know which programmes have greater influence on the estimate. Table 6 shows that if we focus closer to the cutoff on x, VGF, OAP, VGD, TR and FFP are the programmes whose average treatment effects are mainly estimated, as the last column in the table (for 30 bandwidth around the cutoff 285) says that the number of treated households under these programmes is high.

Different programmes have the different target groups such as women, old people and so forth, although they all belong to the common target group, the poorest group. The average treatment effects of different programmes might be different. Therefore, it might be attractive to know the average treatment effect of each of the programmes separately. However, it is difficult to set proper instrument(s) for the treatment dummy for the programmes separately to address the endogeneity problem. If we had instrument(s) for each of the programmes, we could have used IV regression method separately for them. Programme specific treatment effect analysis using the same fuzzy RD design with an income cutoff is not appropriate as there are only a few treated households around the cutoff (see Table 6). I therefore consider a programme, OAP, which exploits an age cutoff of household heads to determine the eligibility for treatment. Of course, eligible household heads should also be in the poorest group. To estimate the local average treatment effect of the OAP programme, I apply another fuzzy RD design using the age of the household head as an assignment variable.

In the case of the OAP programme, if Head Age is at least 65 years, a household can be considered for treatment if it belongs among the poorest. Therefore, 65 is the cutoff point. Table 7 presents results of the fuzzy RD design for the two samples of the poorest households (Bandwidth=10 (55 $\leq$ Head Age $\leq$ 75), $x \leq 570$, and Bandwidth=5 (60 $\leq$ Head Age $\leq$ 70), $x \leq 570$). Households with $x \leq 570$ are considered to be amongst the poorest. Results are reported for both with and without observed covariates. A quadratic term of the assignment variable is used. It is seen that the probabilities of treatment (the coefficients of the instrument dummy, $1[\text{Head Age} \geq 65]$, in w regressions) are negatively discontinuous at the cutoff age. They are supposed to be positive, as the treatment rule says that those households will be selected whose head's age is at least 65. It might be the case that a high level of corruption produces the negative discontinuity[7]. However, the coefficients of $1[\text{Head Age} \geq 65]$ in y regressions show negative signs, which are consistent. This is justified in IV regressions, where the coefficients of $\mathrm{E}(w \mid \text{Head Age})$ show the sizes of the local average treatment effects as positive under the OAP programme, and they remain around the local average treatment effect, 843 Kcal, found in the RD design for all programmes used here.

Table 7: Results from fuzzy RD design applied on the OAP programme

	Excluding Observed Covariates			Including Observed Covariates		
Bandwidth=10 ($55 \leq$ Head Age ≤ 75), $x \leq 570$						
	w (OLS)	y (OLS)	y (IV)	w (OLS)	y (OLS)	y (IV)
$1[\text{Head Age} \geq 65]$	-0.167	-167		-0.139	-162	
	[0.079]	[110]		[0.073]	[109]	
$E(w \mid \text{Head Age})$			999			1132
			[793]			[364]
Head Age	0.083	68.64	-14.18	0.046	80.58	2.58
	[0.074]	[115]	[142]	[0.069]	[113]	[129]
Head Age ^{2}	-0.0005	-0.413	0.094	-0.0003	-0.511	-0.049
	[0.0006]	[0.909]	[1.105]	[0.0005]	[0.895]	[1.017]
Constant	-2.98	-442	2538	-1.90	-711	2459
	[2.34]	[3619]	[4508]	[2.21]	[3531]	[4080]
Observations	323	323	323	323	323	323
Adjusted R^2	0.018	0.0005	.	0.086	0.037	.
Bandwidth=5 ($60 \leq$ Head Age ≤ 70), $x \leq 570$						
	Excluding Observed Covariates			Including Observed Covariates		
	w (OLS)	y (OLS)	y (IV)	w (OLS)	y (OLS)	y (IV)
$1[\text{Head Age} \geq 65]$	-0.288	-168		-0.241	-163	
	[0.129]	[163]		[0.125]	[167]	
$E(w \mid \text{Head Age})$			584			696
			[615]			[350]
Head Age	0.562	1366	1039	0.456	1323	955
	[0.425]	[507]	[511]	[0.425]	[509]	[505]
Head Age ^{2}	-0.004	-10.51	-8.158	-0.00327	-10.18	-7.51
	[0.003]	[3.85]	[3.95]	[0.003]	[3.85]	[3.90]
Constant	-19.07	-42019	-30891	-15.74	-40456	-27813
	[13.88]	[16617]	[16446]	[13.92]	[16798]	[16318]
Observations	171	171	171	171	171	171
Adjusted R^2	0.0179	0.0271	.	0.1119	0.0585	.

Note: Robust standard errors are in parentheses. Here, the assignment variable is Head Age. Cutoff age is 65.

7. Discussion

In this section, I try to answer two questions – "are the eligible/target people (10% poorest among the poor) the people that policy makers had the intention to treat?", and, "why should we or policy makers care about the calorie consumption of the eligible people?". The answers to these questions will further validate the RD design used here. Moreover, this section discusses the results of this study by comparing them with the results of previous studies.

We have already seen that SSN programmes have a high impact on calorie consumption of the target group (the compliance group according to literature). Now, if we can show that the target group does not use the benfits from SSN programmes to support non-food consumption significantly, we can say that these are food insecure people who have the priority of food/calorie consumption over non-food consumption. Policy makers had intention to treat them. Table 8 shows that, in each regression model, household monthly per capita non-food expenditure is not significantly discontinuous at the cutoff point, 285[8]. These results imply that the target people are food insecure people who are "intent-to-treat" people. In the literature, this type of test is also called falsification test.

One may argue that prices of non-food items are different in different regions, and therefore, insignificant discontinuity in non-food expenditure (which is equal to non-food consumption goods times their prices) cannot tell us whether non-food consumption is insignificantly discontinuous or not. Models with covariates control for differences in prices by using a variable for rice price. In Bangladesh, rice price is the key indicator for understanding regional differences in both food and non-food prices.

Table 8: Discontinuity in household per capita non-food expenditure at the cutoff point 285

Model Type	Disconituity	Robust S.E.
Linear	-0.09	27.08
Linear with covariates	3.28	26.25
Quadratic	-40.84	21.69
Quadratic with covariates	-28.05	18.81
Cubic	-40.72	32.33
Cubic with covariates	-29.89	32.75
Quartic	0.86	25.48
Quartic with covariates	12.65	23.22

Note: Household per capita non-food expenditure is regressed on the dummy instrument, z, and different polynomial orders (linear, quadratic, cubic and quartic) of the assignment variable, x. Models with covariates also use variables shown in Table 5 as regressors. From each model, the discontinuity parameter (the coefficient of z) is reported with robust standard error. The same IK optimal bandwith, 285, is used in every model.

Insignificant results in Table 8 contradict the results of previous studies. For example, Khanum (2000) showed that about 90% of RMP beneficiaries have improved their houses and sustainable (food and non-food) consumption. Begum and Majumdar (2001) found that about 19% of OAP beneficiaries invested their allowances to buy goat, cow, poultry and so forth. These studies estimated their results by considering all treated people/beneficiaries where these beneficiaries included some people with high incomes who were technically ineligible to be on the programme. Any increase in non-food consumption that these studies identified might therefore be due to the inclusion of non-eligible individuals in their sample.

In contrast, this RD study estimates the local average treatment effect of the eligible people (compliance group). On the other hand, RMP, in particular, aims to improve the livelihood of beneficiaries. The RD study ultimately estimates the effect of VGF, OAP and VGD (as in Table 6), which aim to reduce food insecurity of the target group.

Table 9: Discontinuities in probabilities of having some diseases in household members

Diarrhoea	Fever	Dysentery	Pain	Injury	Blood Pressure	Heart Disease
0.006	0.018	-0.003	-0.008	0.001	-0.006	-0.004
[0.007]	[0.017]	[0.005]	[0.008]	[0.003]	[0.004]	[0.002]
Breathing Trouble	**Weakness**	**Dizyness**	**Pneumonia**	**Typhoid**	**Tuberculosis**	**Malaria**
-0.009	-0.014	-0.003	-0.001	0.001	0.000	-0.002
[0.004]	[0.004]	[0.002]	[0.004]	[0.002]	[0.000]	[0.002]
Jaundice	**Female Disease**	**Cancer**	**Leprosy**	**Paralysis**	**Histeria**	**Others**
0.001	-0.003	0.000	0.000	0.002	0.001	0.005
[0.003]	[0.002]	[0.000]	[0.000]	[0.002]	[0.001]	[0.007]

Note: Using OLS method, dummy of each disease (1 yes, 0 no) is regressed on the dummy instrument, z, quartic order of the assignment variable, x and variables shown in Table 5. From each model, the discontinuity parameter (the coefficient of z) is reported with robust standard error in brackets. The same IK optimal bandwith, 285, is used in every disease.

It has been established that calorie consumption in a poor setting improves health and productivity (Aromolaran, 2004; Stiglitz, 1976; Strauss, 1986). Following this, I check discontinuities in the probabilities of the presence of various diseases among household members. To do this, I merge individual level data (with disease information) with household level data[9]. A dummy variable for each disease (1 if a member of the household suffers from a disease, 0 if not) is regressed separately on the dummy instrument, z, quartic form of the asignment variable, x, and covariates used in Table 5. Table 9 reports the discontinuity results only (the coefficient at z). We see that the probabilities of having breathing trouble and weakness only are significantly discontinuous at the cutoff point, 285[10]. These results are interesting because these two health problems can be direct causes of calorie deficiency.

We can say that members in eligible households improve their health by improving their calorie consumption. SSN programmes play a key role in improving calorie consumption of the eligible people, and therefore calorie consumption is an important indicator for understanding the improvement in well-being of the targeted people of SSN programmes. Moreover, as improvement in health improves productivity and then income, it is likely that, in the long run, the eligible people can move out of poverty. Therefore, policy makers should concentrate on calorie consumption of the eligible or food insecure people. This study cannot say anything about ineligible treated people. For them, perhaps such improvement in the well being cannot be seen, as they might already have sufficient calorie consumption and good health.

Consistent results in Tables 8 and 9 tell us that the RD design used here is valid. However, one may raise a concern about the use of an endogenous cutoff point, 285. From the econometric point of view, there is no problem in doing this, as long as we have sufficient discontinuity in the probability of treatment at the cutoff point, 285. The main concern of applying RD design is based on whether identification conditions such as local randomisation are satisfied. Because without holding identification conditions, any RD design is invalid. The truth is that no empirical study can claim strongly that these conditions are satisfied, as there is no clear way to test them. As a regular practice, Table 5 (that checks discontinuities in covariates) and Figure 4 (that checks discontinuity in the density of the assignment variable) imply that identification conditions are satisfied to some extent. Now, the question is – "is there any extra risk from using the endogenous cutoff point that violates identification conditions?”. The answer is we don’t know.

The results of this RD study can only be directly compared with the study of Rahman (2012), that used the same survey data (HIES 2005). Using six regional dummy instruments, this previous study estimated about 400 Kcal as an effect of SSN programmes on household per capita daily calorie consumption (the same outcome variable used in this current study). In Bangladesh, food consumption habit does not vary by regions, as rice is the main consumption item in every region. But it is seen in HIES 2005 that the government is biased to some regions (specially Dhaka region) in selecting households for SSN programmes. This regional bias is used to select instruments. In that study, these regional dummies were weak in some senses. That study also used matching techniques and raw differentials. Without controlling for overlapping and unconfoundedness (which are the key assumptions in econometric techniques based on selection on observables such as matching estimators, propensity score matching, and raw differentials), results were negative. After controlling for them (by dropping some data based on Abadie and Imbens, 2002; Crump et al., 2006), results became positive but insignificant. The IV method used in Rahman (2012), has a large estimate of a treatment effect, as it estimates the local average treatment effect. The current RD study’s local average treatment effect is two times higher than that in the previous study. Although the strength of the instrument has no systematic relation with the size of the estimate, if the strength increases then the reliability of a treatment effect increases. The current RD study’s instrument is stronger than instruments in the previous study.

Other previous studies are not directly comparable with this current study, because they used different datasets to examine the effects of different programmes. However, following Rahman (2012), it can be said that any other methods such as matching (except the IV method) will produce a low estimate of treatment effect, because those methods considered both eligible and ineligible groups in estimating the average treatment effect. For example, if there is a high treatment effect on the eligible group and low or no treatment effect on the ineligible group, then combining two groups will give a low average treatment effect. Moreover, overlapping and unconfoundedness assumptions are often violated in these methods. This is another reason for estimating a low treatment effect. Almost all other previous studies except Rahman (2012) did not use proper IV techniques, and therefore, their estimated results are biased downwards.

8. Conclusion

In this study, using HIES 2005 I estmate the local average treatment effect of a number of SSN programmes in Bangladesh on the calorie consumption of food insecure households using a fuzzy RD design. On average, households' per capita daily calorie consumption increases by 843 kcal (37 percent) after being treated under these programmes. This finding is substantially higher than the average treatment effects estimated by previous studies. The statistically insignificant and/or low sizes of the average treatment effects estimated in the existing literature are largely the result of an endogeneity problem with the treatment variable. Even the HIES 2005 yields a negative raw differential of the outcome variable (Rahman, 2012). The fuzzy RD design removes the endogeneity problem, and produces a substantial and significant result.

The technicalities of the study establish the estimated treatment effect as robust. I checked for different cutoffs rather than considering the official one only, in order to achieve the highest discontinuity in the probability of treatment given the assignment variable. This is justified as the assignment variable is subject to measurement error and other covariates also influence the treatment dummy. I used local polynomial regression including observed covariates to control for any discontinuity in the expected value of the outcome variable given the observed covariates. This ensures that the average treatment effect estimated here is identified. Optimal polynomial orders for reduced form regressions are estimated using Bin test. Results from different models are compared to check for consistency. A different fuzzy RD design is applied to the OAP programme, which also shows a significant and substantially positive average treatment effects that provide further evidence in support of the local average treatment effect, 843 kcal. One falsification test (insignificant discontinuity in non-food consumption) and improvement of health of the eligible people imply that the RD design used here is valid.

Finally, the significant treatment effect of SSN programmes in Bangladesh estimated here has its own policy implication. The SSN programmes, although limited in terms of the size of benefits, account for a significant part in the government budget. The positive effect of the programmes thereby provides strong grounds to increase, or at least maintain, the current allocation. It also justifies the continued use of the SSN programmes as effective policy tools for mitigating the food insecurity problem.

Endnotes

[1] HIES 2005 states that some people bribe programme administrators to be selected into SSN programmes. Moreover, in one of my current studies motivated by a Daily Star (renowned newspaper in Bangladesh) article, I have found that many ineligible households are receiving benefits from SSN programmes because they have political affiliation with the ruling party. These are the factors of corruption.

[2] Many papers on programme evaluations in developing countries, including Bangladesh, have used single cross section in the absence of reliable panel data (del Ninno et al., 2001). Therefore, pre-programme household characteristics (for example, income, landholding) that affect treatment are not available. Current income and landholding are used as a proxy of pre-programme income and landholding.

[3] I consider different SSN programs with different selection criteria as if they were one program. Although there are different selection criteria, in every program the common criterion for getting selection for treatment is being the poorest among the poor. Therefore, we expect that the probability of treatment will be discontinuous at the cutoff income. Besides, the main goal of every program is to reduce food insecurity.

[4] There are also 16 poverty lines for 16 strata in Bangladesh. I also tried to get 1st decile of poor peoples' household per capita monthly income by selecting poor people who exist below their stratum' poverty lines. However, that 1st decile did not show any significant discontinuity in the probability of treatment. Therefore, I ignored such measure, because it is mandatory to get the discontinuity in the probability of treatment for applying fuzzy RD design.

[5] First, I define poor households whose household per capita monthly income is below the national poverty line income (860). Then I estimate 1st decile of the household per capita monthly income of poor households, which is 290. Below 290, the 10% poorest households among the poor exist.

[6] Those households are in the regression sample whose household per capita monthly income is below or equal to 1000 taka. As we are interested about the discontinuity at the cutoff point, we have to focus data behaviour near the cutoff point.

[7] We know that many poor people have less education, and therefore, they cannot make a proper application for the potential benefit they can receive from the OAP program. However, they know the cutoff age (65) to be selected into OAP. Even though they are totally illiterate, they know their ages. It is more likely that they exaggerated their ages (higher ages than actual ages) to the program administrator in order to be selected into the OAP program, but they have given their real ages to the HIES surveyors. This explains why most of the treated households exist below the cutoff age, 65. Ultimately, this is a corruption (note that there was no birth registration system in Bangladesh during the period when the old people were born).

[8] Non-food expenditures include non-durable items. Besides, poorest people have low durable consumption.

[9] In HIES 2005, household members were asked whether they suffer from any disease. The response was yes, or no. If it was yes, then they were asked their names. Thus, 21 diseases as in Table 9 are recorded.

[10] For any other specifications of regression models, results remain similar.

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