Cash Versus In-Kind Transfers, The Case of Bangladesh

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13 July 2012

Abstract

This chapter empirically analyses the effect of in-kind versus cash transfers on household consumption behaviour. Redistributive welfare programme motivated by paternalism are often implemented in-kind to promote outcomes that might not be achieved under cash transfers. Using evidences from a natural experiment in Bangladesh, where the same set of households were treated to different types of transfer, food grains and cash, at two different periods in time; we test whether this form of paternalism is necessary. A fixed effect instrumental variable model is used in the estimation and the comparison of household's behaviour under each type of transfer. This estimation fixed the endogeneity in treatment variables as a result of failure to account for household-specific fixed effect and selection into treatment. The estimation results show that though in-kind food grains transfers did caused households to consume more grain than they would have chosen under an equal-valued cash transfers, the impact on calorie consumption and children health status is minimal. Households who received cash were able to reallocate their funds more effectively, and chose to spend their extra income on children's education, purchase of useful items such as clothing and children's non-food consumption, while at the same time spending no more on vices such as cigarettes. While other justifications for providing transfers in-kind may certainly be valid, the evidence supporting the paternalistic one in this context is minimal.

Keywords: In-kind transfers, Paternalism, Bangladesh
JEL classification: H43, D12, O12, I38

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1 Introduction

In virtually all countries, developed and developing, a significant amount of redistribution takes place in-kind (Currie and Gahvari 2008). In-kind transfers are often designed to change consumption behaviour and promote consumption of the good being transferred. Nonetheless, if there were zero transaction costs, there would be no difference between in-kind and cash transfers if the good could be marketed. Also, if the household already consumes a large amount of the good, the transfer may be infra-marginal and, hence, treated like cash. However, where transaction costs are non-negligible and the household originally consumed little of the in-kind good (making the transfer extra-marginal), we might expect greater consumption of the transferred good.

It is well established in economic theory that a cash transfer is weakly superior in terms of the recipient's utility than an equal-value in-kind transfer, as an in-kind transfer may act as a constraint on the consumption choice of the recipient. It is therefore, at first glance, puzzling that many redistribution programmes give out benefits in-kind as opposed to in cash. Trying to understand why governments conduct their welfare programmes in-kind has been a much-researched question in Economics.

Many theoretical explanations have been put forward to explain preference for in-kind over cash transfers. These include one based on imperfect information and the ability of in-kind programme to induce "self-targeting" among rich and poor recipients resulting in a more efficient transfer system (Nichols and Zeckhauser 1982), pecuniary effects (Coate, Johnson and Romano 1996b), where provision of transfers in the form of in-kind goods has a direct effect on local supply inducing a general equilibrium effect, such as a fall in local prices of the in-kind goods and complementary products, which can increase recipients' utility more than a cash transfer. Other explanations include the argument that in-kind programmes help improve the efficiency of the tax system through the provision of in-kind goods complementary to labour that can help reduce distortions in the labour supply caused by the tax system, the "Samaritan's dilemma" (Bruce and Waldman 1991), and political economy considerations (Bearse et al. 2000). A recent paper by Curries and Gahvari (2008) provides a detail account of all these motivations for in-kind transfers stated here.

By far the most common explanation provided for in-kind transfer is "paternalism". Provision of paternalistic in-kind transfers can be justified mainly by two reasons. The first justification is based on externalities, where differences exist between social and individual welfare functions within the society or, in an intra-household setting, when members of households make decisions that are not in the interests of other household members. The second justification for paternalism abandons the assumption of complete rationality among recipients. In this case, where the recipient is rationally bounded, it is argued that an act of "libertarian paternalism" by a central planner that alters individual behaviour could result in a better outcome (Thaler and Sunstein 2003). In either case, social welfare could be improved by selecting paternalistic policies with the goal of influencing the choices of affected parties in a way that makes these parties better off. Many development projects, including the recently popularised conditional cash transfers, often have an element of paternalism in them. All of these justifications for in-kind transfer require that the in-kind transfers actually increase consumption of the transferred good more than cash transfers.

However, while the motivations for in-kind transfers are well established in the theoretical literature, there is very little empirical evidence on how redistribution programmes alter consumption choice. Studies conducted on the United States Food Stamp Programme showed that food vouchers

are infra-marginal for most recipients and thus are treated like cash. In terms of those recipients whose consumption is distorted, Whitmore (2002) found that food stamp recipients have access to a well developed resale market in food stamps and that over-provided stamps that are not sold tend to induce consumption of some non-nutritious foods, such as soft drinks. This seems to suggest the failure of the programme to induce the intended constraint on the recipient's behaviour. The developed country context, however, is very different from the one studied here, where the focus is on transfer programmes in low-income countries.

There is a small, recent body of evidence that directly compares differences in outcomes under cash and in-kind transfers in low-income countries. Cunha (2010) compares the effect on household consumption and health outcomes between households in villages that were randomly assigned either equal-valued cash or in-kind transfers or no transfer at all in the Programa de Apoyo Aimentario (PAL) in rural Mexico. Using the difference-in-difference method to control for baseline differences of households, the author finds no evidence that the in-kind food transfer induced more food to be consumed by beneficiaries than did an equal-valued cash transfer. While the in-kind food transfers, which were made up of more than 10 food items, did indeed alter the types of food consumed in the beneficiary households, the households that received cash were found to have consumed equally nutritious foods and the extra cash was not spent on vices, such as alcohol and tobacco. There were also few differences in the health outcomes between children of households under the two types of transfer. As a result, Cunha found little evidence in support of the necessity of paternalism.

Other studies on the same PAL programme by Jayachandra et al. (2010) provide evidence of the existence of a pecuniary effect. The authors found that the price decline in in-kind villages increases the programme's net transfer by 12 percent for the recipients of food transfers, while the price increase in cash villages dissipates 11 percent of the transfer. Skoufias and Gonzalez-Cossio (2008) also found that, although both in-kind and cash transfers did increase consumption, there were no significant differences between the two and there was no evidence that the programme, irrespective of the type of the transfer, affected overall labour participation.

While these studies on a randomised controlled trial provides strong evidences on the difference - or lack thereof - between in-kind and cash transfers from many perspectives, this paper makes a new contribution to this literature using a different approach. While the data set used in this paper is non-experimental, it utilises a natural experiment in which the same sets of households in the survey were treated with different types of transfers, in-kind and cash, at two different periods in time. This makes it theoretically possible to identify the difference in outcomes within the same households who were given an in-kind transfer in one year and a cash transfer in the others.

In addition, the setting in Bangladesh, which is one of the poorest countries in the world, is different from richer developing countries in Central or Latin America such as Mexico. Households in Bangladesh are likely to have poorer access to markets for resale where they can offload the over-provided food grains. The Bangladesh programme also concentrated the transfer in one basic food item (wheat or rice) rather than offering a bundle of goods, perhaps increasing the likelihood that in-kind transfer was extra-marginal. Given the paucity of existing rigorous studies on this issue, this paper provides valuable empirical evidence on whether the findings from Mexico hold true in other developing country contexts.

The data for this study is a longitudinal data set collected by the International Food Policy Research Institute (IFPRI) in Bangladesh. The panel data set covers the period from 2000 to 2006, during which

Bangladesh implemented two conditional transfer programmes where one was a continuation of the other. The first, the Food for Education programme (FFE), started in 1993, provided an in-kind food grain subsidy to eligible households conditional on their children being enrolled in primary school, and was still active in 2000. By the second round of the survey in 2003, the FFE programme had been replaced by the Primary Education Stipends programme (PES), which was a continuation of the FFE programme with the only difference being that transfers were now given out in cash.

A few studies have compared the impact of food and cash transfer programmes in Bangladesh. A study by Del Ninno and Dorosh (2003), using data from various food grain distribution and cash transfer programmes in Bangladesh, finds that the marginal propensity to consume out of wheat transfer in-kind is significantly higher than out of cash transfers. A previous study of the FFE programme using the first year of the data examined in this paper by Ahmed and Del Ninno (2002)[1] finds the FFE programme had a significant effect on increasing calorie consumption, although the study found no effect on the health status of beneficiaries. All of these studies, however, used only non-experimental cross-sectional data, which likely leads to bias due to the endogeneity of treatment associated with unobserved heterogeneity across households.

Ahmed (2005) compared in-kind and cash transfers using the first two rounds of same IFPRI data set but suffer from severe shortcomings. By separately analysing each programme, the author found that beneficiaries of the FFE programme consumed significantly more food than non-beneficiaries, while the same is not true for the beneficiary of the PES programme, therefore concluding that there is a difference between the two programmes. However, Ahmed did not take full advantage of the panel structure of the data set by estimating the impact of both programmes separately. Using only cross-sectional data in the first wave of the FFE programme, the study failed to control for unobserved heterogeneity across households (especially among beneficiaries and non-beneficiaries of the programme), which again results in a biased estimation of programme impact. The study also use only one-third of the sample to estimate the impact of the PES programme by only looking at households who lived in a location with no FFE programme in 2000 but with the PES programme later in the 2003 round. While this might allow for cleaner estimates of the impact of the PES programme using the difference-in-difference estimation technique, Ahmed did not make use of the valuable remaining two-thirds of the samples that would have allowed the testing of the effect of those who switched status. In addition, given the geographical targeting of the earlier FFE programme, a location with no FFE programme in 2000 would have been richer in comparison to a location with the programme. These differences will be problematic when comparing impact of the FFE and PES programmes. By running two separate regressions on two different samples, a formal test of the difference in the impact is not possible.

This paper contributes to the existing literature by providing new empirical evidence on the difference in impact of in-kind versus cash transfers using a credible identification strategy that controls for household fixed effects and exploits an interesting natural experiment in Bangladesh. This paper benefits from the latest public release of the data from the most recent 2006 survey of the same set of households. In this period, the PES programme is still active and the availability of more data enables more precise estimation of programme impact. Only a study by Baulch (2010) on the medium-term impact of the two education transfer programmes has been done using the full three waves of the data set. Baulch found the impact of these education transfer programmes to be very small relative to a

[1]This study only make use of the first wave of the panel data set (year 2000) used in this paper

programme of its size. However, unlike this paper, Baulch's study did not focus on comparing in-kind and cash transfers. This paper, to the best of our knowledge, is one of the first that used the new complete IFPRI data set on Bangladesh to compare the impact of in-kind and cash transfers. It is also the first study to directly compare the difference in outcome on the same set of households that lived through a period where the mode of transfer received was changed from in-kind to cash. The evidence presented in this paper thus should contribute to current debates on in-kind versus cash transfers.

Using the fixed effect instrumental variable (FE-IV) approach in the analysis, which controls for unobserved household-specific fixed effects and the endogeneity of programme participation, the difference in the impact of both programmes on calorie consumption was found not to be statistically significant. However, a closer look at the expenditure share of various food and non-food items finds an indication that households that received an in-kind wheat transfer were constrained to consume wheat as a higher share of their total expenditure than they would have chosen under an equal-valued cash transfer. This is due to the constraints caused by the in-kind wheat transfer, which was both extra-marginal and binding for most households.

The findings in this paper should ease some paternalistic fears, as cash households who were under no constraint and therefore able to switch the composition of their expenditures from food to non-food more easily chose to increase their spending on children's education, and purchase useful items such as clothing and children's non-food consumption, while at the same time spending no more on vices such as cigarettes. Given the lack of significant difference in calorie consumption, it is not surprising that no differences were found in the health status of children between households under the two programmes and, perhaps more importantly, no difference compared to non-beneficiary households either.

The structure of this paper is as follows. Section 2 outlines a simple model of consumer demand under in-kind and cash transfers that underlies the empirical analysis. Section 3 provides a detailed description of the FFE and PES programmes. Section 4 describes the survey design and presents the descriptive statistics from the IFPRI Chronic Poverty and Long-term Impact study in Bangladesh. Section 5 discusses the identification of the parameter of interest. Section 6 explains the econometric specification for estimating the parameters identified in section 5, and for testing the differences between in-kind and cash transfers. Section 7 presents the empirical results. Section 8 offers a discussion of the policy implications of the findings and concluding remarks.

2 Theoretical Model of In-kind vs. Cash Transfer

This section presents the simple theory of consumer demand under cash and in-kind transfers (Moffit 1989, Cunha 2010) that underlies the empirical investigation in later parts of this paper. To keep the explanation as simple as possible, the case of a single in-kind good that is transferred to households will be considered. This will sufficiently illustrate the differences in outcomes under cash and in-kind transfers without over-complicating the analysis.

Assume a household with a well behaved utility function $U(x_w, x_c)$ over two goods, wheat, x_w, and a composite good, x_c. Ignoring the issue of intra-household allocation of resources, we assume that the household acts as a single unit and maximises its utility with respect to a linear budget constraint $p_w x_w + p_c x_c \leq Y$. Where p_w, p_c and Y are the price of wheat, the price of the composite good, and

household income respectively. With a cash transfer of value T, the budget constraint takes the form:

$$p_w x_w + p_c x_c \leq Y + T \tag{1}$$

With an equal-valued in-kind transfer of wheat $\overline{x}_w \, (= \frac{T}{p_w})$, the budget constraint takes the form:

$$p_w x_w + p_c x_c \leq \begin{cases} Y + p_w \overline{x}_w = Y + T & if \ \overline{x}_w \leq x_w^c \\ Y + \overline{p}_w \overline{x}_w & if \ \overline{x}_w > x_w^c \end{cases} \tag{2}$$

The budget constraint in equation 2 is kinked depending on two factors; the amount of the good received in-kind $\bar{x}_w$ relative to the amount of the good consumed under an equal-valued cash transfer x_w^c, and the resale price $\overline{p}_w$. As shown above, where the in-kind transfer is "infra-marginal", that is the fixed amount of in-kind transfer received is less than the quantity the household chooses to consume of that good under an equal-valued cash transfer ($\overline{x}_w \leq x_w^c$), the in-kind transfer is identical to the cash transfer. This is because the household could use the income that is saved from not having to spend on the transferred good to spend freely on other goods.

However, where the transfer is "extra-marginal", that is the fixed amount of in-kind transfer is more than what the household would choose to consume with an equal-valued cash transfer ($\overline{x}_w > x_w^c$), an in-kind transfer will distort the household's consumption. The extent to which this distortion affects the household's budget constraint depends on the resale price $\overline{p}_w$ of the transferred good. Here, we assume that the resale price lies between zero and the market price for the good ($\overline{p}_w \epsilon [0, p_w]$) which reflects the search and transaction cost involved in the reselling process. Unless $\bar{p}_w = p_w$, the budget constraint under the in-kind transfer is kinked at the point where ($\overline{x}_w = x_w^c$) and the slope of the kink becomes shallower the bigger the discount in re-sale price from the market price.

Figure 1: Cash versus In-kind Transfer

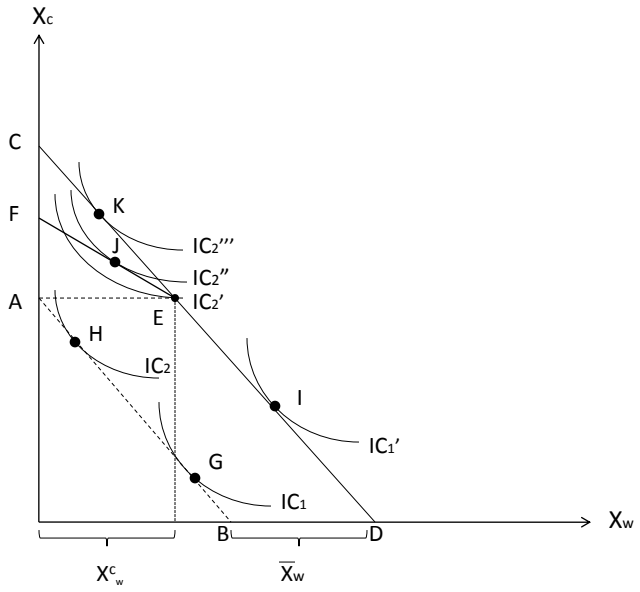


Figure 1 illustrates the above discussion. The original budget line before the household receives any type of transfer is given by $\overline{AB}$. The budget line shifts outward to $\overline{CED}$ when the household

receives the cash transfer of $T = p_w \bar{x}_w$. With an equal-valued in-kind transfer, the budget line also shifts outward but takes the kinked form ($\overline{FED}$). The kink occurs at point E, where the transferred amount of wheat in-kind ($\overline{x}_w$) is equal to the amount the household chooses to consume under the cash transfer (x_w^c), meaning the distance is $\overline{AE} = \overline{BD}$. The slope of the kink above point E depends on $\overline{p}_w$; the closer the resale price is to the market price, the closer the kinked budget line is to the cash budget line. In the extreme case, where no in-kind good can be resold or reselling is forbidden, the budget line is $\overline{AED}$.

Whether these differences will result in distortions in household consumption depends on three conditions. First is whether the in-kind transfer is extra- or infra-marginal, in other words, whether the tangent point of the household indifference curve to the cash budget line ($\overline{CED}$) lies above point E or not. From Figure 1, the preference of a household for whom the in-kind transfer is infra-marginal is represented by the indifference curve $IC_1$and IC_1'. For this household, the outcome under cash and in-kind transfers is identical and is represented by point I on the diagram. For a household whose in-kind transfer is extra-marginal, the indifference curve is given by IC_2. Under the cash transfer, the outcome is point K on the indifference curve IC_2'''. Under equal-valued in-kind transfer, K is unattainable and the household is always on a lower indifference curve in all cases except when the in-kind good can be resold at the market price.

The second condition is the degree to which the in-kind transfer is "binding", which links directly to the resale price. An in-kind transfer is considered "fully binding" if a household consumes exactly the amount of good that was given to it, "partially binding" if the household consumes less than the amount of good that was given but more than what it would have chosen to consume under equal-valued cash transfer, and lastly "non-binding" if it consumes the same amount as it would have chosen under equal-valued cash transfer. Similarly, given no restriction on reselling, a transfer is partially binding for any positive resale price except for when $\bar{p}_w = p_w$, where transfer is non-binding. The in-kind transfer is only fully binding when the resale price is zero.

Point E on indifference curve IC_2' represents an outcome when the in-kind transfer is both extra-marginal and fully binding. A household at E who is either not allowed to resell any of their transfer or is facing a zero resale price is constrained to consume all of the transfer received. When the resale price is strictly positive but less than the market price p_w the transfer is only partially binding, and the distortion from an extra-marginal in-kind transfer is less. At point J on IC_2'', the household consumes less wheat than at point E and is better off, but is still constrained to consume more than it would have under an equal-valued cash transfer (point K, where in-kind transfer is non-binding). It must be noted that this model is time-independent. This leaves re-sale as the only explanation for observed consumption level that is not fully-binding whereas, in practice, households may choose to store or consume received goods in a lumpy manner.

The third condition is the degree of substitutability between the in-kind good and the composite good. The more substitutable the in-kind good is with the composite good the lower the welfare loss due to provision of an extra-marginal in-kind transfer. This is not shown in Figure 1 but could easily be demonstrated to be true by drawing a shallower indifference curve to represent the higher degree of substitutability between wheat and the composite good.

The model can be straightforwardly extended to the case of multiple in-kind transferred goods and multiple non-transferred goods. With multiple transferred goods, the results of the model above holds for each good separately (Neary and Roberts 1980). When more than one non-transferred good is

available, the distortion caused by the extra-marginal in-kind transfer is less, because the household would be induced to consume fewer substitutes and more complements of the transferred goods compared to when these goods are not available. For example, in this case, households could respond to extra-marginal transfers of wheat by lowering rice consumption, thus keeping overall consumption of cereals constant.

In conclusion, the theoretical model shows that household utility with cash transfers is weakly superior to that with equal-valued in-kind transfers, as there are some outcomes under cash transfers that are not attainable with in-kind transfers. The model puts emphasis on the distinction between infra-marginal and extra-marginal in-kind transfers, with the latter resulting in lower utility for households than cash. The welfare loss, however, also depends on the resale price and the degree of substitutability of the in-kind good with other goods. A lower degree of extra-marginality, higher resale price or higher degree of substitutability of in-kind good with other goods will result in a lower welfare loss.

The theory suggests that a cash transfer is always at least as good as an in-kind transfer. A utility-maximising household would, therefore, prefer to receive transfers in cash and freely choose their consumption bundle. However, as mentioned earlier, paternalism is one of the main justifications for the extensive use of distorting in-kind transfers over cash transfers. In considering the goals of paternalism, it is important to distinguish between whether the society cares about recipients' consumption of the specific item being transferred, in this case wheat and rice, or whether it cares about the outcomes that these in-kind transfers are designed to promote. If we believe that it is the latter rather than the former, one appropriate test for the advantage of paternalistic in-kind food transfers is whether the health and nutritional status of recipients improves significantly compared to when they receive equal-valued cash transfer programmes.

The theoretical model has shown that an extra-marginal in-kind transfer can lead to a constraint in the behaviour of the recipient that would not be the case with an equal-valued cash transfer. These constraints were argued to be the motivation behind the use of in-kind transfers as a method of redistribution by a paternalistic government that believes this constraint to the behaviour of the recipient will lead to an improved recipient and, hence, social welfare.

There are two main hypotheses to be tested in this paper. First is whether an in-kind transfer leads to a constraint in behaviour in comparison to an equal-valued cash transfer. This is a test of whether in-kind transfers are extra-marginal and binding. We look at the effect of both types of transfers on *per capita* calorie consumption and will examine changes in the expenditure shares of various food and non-food items to determine the effect of both programmes on the composition of consumption. This can help determine household substitution behaviour and also the difference in household consumption allocation under cash and in-kind programmes.

Second, we test whether the in-kind transfer programme leads to improved health status of household members, especially young children. We look at the effect of both types of transfer on health outcomes among children in each household and test whether the two programmes lead to a significant difference in these outcomes. The next two sections describe in detail the background of the transfer programme and the data set used in the empirical analysis.

3 An Overview of the FFE and PES Programmes in Bangladesh

This section presents the origins and the overview of the two conditional cash transfer programmes in Bangladesh studied in this paper; the FFE and the PES programmes.

3.1 The Food for Education Programme

The FFE programme was launched in 1993 after the discontinuation of the Government of Bangladesh's targeted food subsidy programme known as Rural Rationing (Palli). Palli provided unconditional food rations to poor households. However, the high level of leakage and the high cost of the subsidy led the government to end the programme. Concerned about the food security of the 6.1 million households that were formerly entitled to subsidised rural ration, the government started the conditional transfer FFE programme (Ahmed and Khondkar 2010). It was the aim of the paternalistic FFE programme to effectively distribute food to poor families while increasing children's educational attainment.

The FFE programme provided a free monthly ration of food grain to households conditional on the household's children being enrolled in school. Poor children enrolled in primary school grade 1 to 5 were eligible. The FFE programme was implemented selectively, where unions that were economically disadvantaged and had a low literacy rate were targeted. At its start, the programme only covered 460 unions, one union in each of the rural *upazilas* in Bangladesh[2]. Before the programme was terminated it had expanded to 1,255 unions in 2002, covering 27 percent of all primary schools and enrolling about one-third of all primary-school students in Bangladesh. The FFE programme accounted for a significant share of Bangladesh's expenditure on primary education, increasing from 4.7 percent in 1993/94 to 19.9 percent in 1997/98 (Ahmed and Del Ninno, 2002).

Within the unions where FFE programme were available, households with primary-school-age children became eligible for FFE benefits if they met at least one of the following four eligibility criteria:

1. Children from a landless or near-landless household (owns less than half an acre of land);
2. Children of day labourers;
3. Children from female-headed households; or
4. Children from households that earn their living from low-income occupations.

Beneficiary households were chosen by local groups, who, based on the targeting criteria, prepared a list of FFE beneficiary households in every eligible union at the beginning of each year. Households within an FFE union who satisfied these selection criteria should have been eligible for participation, but two important conditions could prevent them from receiving the benefits. The first was that only families with students who enrolled in FFE schools could receive the food subsidy. Some students from eligible household may choose to enrol in non-FFE schools, making them unable to receive the food subsidy.

Second, due to resource constraints, only a maximum of 40 percent of students in each FFE school could receive the FFE rations. If there were more than 40 percent of students who were eligible, decisions on who should receive the subsidy were made by teachers. Teachers were expected to select the poorest households when faced with this problem (Meng and Ryan 2010). However, due to the

[2]The administrative structure of Bangladesh consists of divisions, districts, upazilas, and unions in decreasing order by size. There are 6 divisions, 64 districts, 489 upazilas (29 are in four city corporations), and 4,463 unions (all rural).

lack of clear-cut guidelines or an empirical method for identifying the poorest students, deliberate or unintended bias and distortions in student selection could occur (Tietjen 2003).

Beneficiary households were entitled to receive a free ration of up to **15 kg of wheat** or **12 kg of rice** per month for sending one child of the appropriate age to primary school. The monthly ration increased to 20 kg of wheat or 15 kg of rice if households send sent more than one child to school[3]. Children needed to attend 85 percent of the total classes in each month to maintain their eligibility status.

3.2 The Primary Education Stipends Programme

The PES programme replaced the FFE programme in July 2002. Instead of subsidised food grain, the PES programme provides cash assistance to poor families conditional on sending their children to primary school. Households who meet the PES programme eligibility criteria are entitled to receive **BDT100** per month for having one child enrolled in primary school and BDT125 per month for having more than one child enrol in primary school[4].

The PES programme uses the same four targeting criteria that FFE used with one additional criterion; children belonging to a household that derives its income from sharecropping are also eligible for the PES benefits. Unlike its FFE predecessor, the PES programme does not use any geographic targeting mechanism and covers all rural areas. Based on the five targeting criteria, the School Management Committee with the assistance of head teachers prepares a list of potential PES beneficiary households in every union at the beginning of the year. Like with the FFE, the number of beneficiaries may not exceed an upper limit of 40 percent of students in a school. To maintain eligibility, students must attend 85 percent of classes each month and attain a minimum of 50 percent marks on the annual examination (Ahmed 2005).

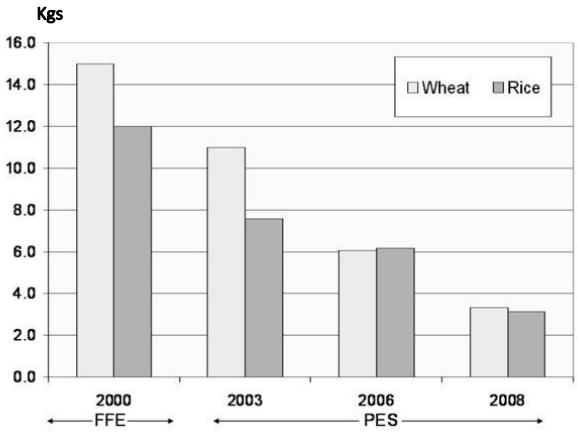
During the first phase (2002/03 to 2006/07), the Ministry of Primary and Mass Education estimated that 5.5 million children received the stipend at a budgetary cost of BDT2.82 billion (approximately $45 million). The PES programme is now extended for a second phase for a further five years with a budget allocation of BDT2.44 billion ($35 million) and 4.8 million children are expected to benefit from the programme (Baulch 2010). A detailed account of the historical development of both the FFE and PES programme can be found in the paper by Ahmed and Khondkar (2010).

Finally, in addition to the differences in the mode of transfer and targeting criteria of the two programmes, it is important to note the gradual decline in the value of the transfer. The nominal value of the PES transfer has been fixed since the programme's inception in 2003. Given inflation, this means that the real value of the transfer has been falling over time. Figure 2 displays the decline in the real value of the transfer by showing the amount of atta (wheat flour) and rice that the transfer would have been able to purchase compared to the 15 kilograms of wheat and 12 kilograms of rice provided by the earlier FFE programme. Table 1 gives a brief summary of the similarities and differences between the two programmes.

[3]Of the total quantity of FFE food grain distributed from 1997/98 to 1999/00, wheat accounted for about 64 percent and rice, about 36 percent (Ahmed, 2005).

[4]BDT stands for Bangladesh Taka, the currency of Bangladesh. The official exchange rate was BDT69.89 to USD1.00 in 2007 on average.

Figure 2: The Declining Value of the Transfer



Source: "Medium-term impact of the Primary Education Stipend in Bangladesh"; Baulch (2010), Figure 1
Note: Kgs= Kilograms; FFE= Food for Education; PES= Primary Education Stipend

Table 1: Programme Descriptions

Programme	Year of Operation	Programme Location	Eligibility Criteria	Transfer
Food for Education (FFE)	1994-2002 (2000 round of survey)	Available in selected disadvantaged union. (Two third of the unions in the survey were FFE unions in the year 2000)	1. Day labour 2. Female Headed household 3. Low Income occupation 4. Own less than 0.5 acre of land	Food grain: 15kg of wheat or 12kg of rice
Primary Education Stipend (PES)	2002-current (2003 and 2006 round of survey)	Available in all rural union. (All unions are PES union since 2003)	1. Day labour 2. Female Headed household 3. Low Income occupation 4. Own less than 0.5 acre of land 5. Sharecropper	Cash: 100 BDT

4 Survey Design, Data and Descriptive Statistic

This section describes the Bangladesh panel data set collected by IFPRI and used in this paper.

4.1 The Surveys

The initial survey, carried out in September to October of 2000, was a cross section of households during the period of the Bangladesh FFE programme. In 2003, after the replacement of the FFE programme by the PES programme, IFPRI resurveyed the same households. In late 2006/early2007, the same households that had been included in the 2003 wave were resurveyed again as part of a larger project to study poverty in rural Bangladesh. All three rounds of surveys were conducted in the same months to control for seasonality in factors such as income and agricultural production, which may affect other outcomes of interest such as expenditures. Details on each round of the survey are presented below.

4.1.1 The 2000 Survey Round

The 2000 round consists of 600 households in 60 villages in 30 unions in 10 *upazilas*. The sampling process begins by the random selection of 10 *upazilas* using probability proportional to size sampling (PPS) based on the *upazilas*-level population data from the 1991 census. Two FFE unions and one non-FFE union were selected randomly in each *upazilas*. Within each union, two villages were randomly selected using PPS sampling based on village-level population data from the 1991 census. Finally, 10 households that had at least one primary-school age child were randomly selected in each village from the census list of households.

The sample included both beneficiaries and non-beneficiaries of the FFE programme which were in place in 2000. A household that is not a beneficiary of the programme does not receive the transfer for one or more of four main reasons. First, they are not eligible because they did not meet any of the targeting criteria set by the programme. Second, they are eligible for the FFE benefits but are not living in unions where the FFE programme exists. Third, they are eligible to receive the benefits and live in an FFE-union but choose not to enrol their child in primary school or enrolled their child in a non-FFE school. Finally, the eligible child attended a school where more than 40 percent of children were eligible and weren't selected to participate in the programme by the School Management Committee.

4.1.2 The 2003 Survey Round

The 2003 follow-up survey was conducted in September to October 2003. Because of budgetary constraints, only eight of the original 10 *upazilas* were resurveyed. One of the *upazilas* was excluded from the 2003 sample frame as FFE food grain distribution was suspended in this *upazilas* because none of the FFE schools were able meet the criteria for maintaining their eligibility for the programme. One additional *upazilas* was randomly dropped for the financial reason stated above.

As a result, the 2003 round consists of 473 households in 48 villages in 24 unions in eight *upazilas*. Even though the household sample was reduced from 600 to 473, only 7 of these lost households should strictly be regarded as being lost as a result of attrition. It is also important to note that, although the

same proportion of former FFE and non-FFE union were maintained in 2003, the new PES programme was available in all rural unions.

4.1.3 The 2006/07 Survey Round

The 2006/07 round followed the same households in the same eight *upazilas* as the 2003 round. It consists of 511 households in 48 villages in 24 unions in eight *upazilas*. The 511 households are broken down into 455 core households that can be traced back to the original 2000 survey plus 56 households that had split from the core household between 2003 and 2006. Household splits were not followed between the 2000 and 2003 surveys, and in 2006 only those split households who did not migrate outside their home districts were tracked.

Focusing on the core households, the attrition rate over three waves is approximately 5.2 percent, a low rate of attrition in comparison to similar surveys in other low-income countries. However, it is important to test whether attrition occurs randomly or not, and this is dealt with in Section 6.5. In addition, anthropometric data were collected for all household members in this survey round, which enables a study of changes in the health status as a result of the programmes[5].

4.1.4 The Three-Wave Panel

The Bangladesh three-wave panel data set from 2000 to 2006 used in this paper consists of 455 non-attrited households in 48 villages in 24 unions in eight *upazilas*. Household splits are dropped from the analysis as they were only tracked in the 2006 round.

4.2 Sample Construction and Descriptive Statistics

4.2.1 Sample Construction

This section discusses the construction of important dependent and independent variables from the raw data. As with most household surveys in low-income countries, there were problems with incomplete sections and missing data when using the IFPRI Bangladesh data set. It is important to deal with these problems correctly and effectively as mistakes in variable construction could lead to misleading and ultimately biased analysis.

Demographic: Household size was calculated by counting those individuals who reported themselves to be current members of the household. This also includes those who were temporary members, as their presence would have had impacts on other variables of interest such as expenditure and calorie consumption. Not including them could lead to an overestimation of expenditure and calorie consumption *per capita*.

Expenditure: Total household expenditure was calculated from food consumption and the non-food consumption section of the survey. The period of recall for all expenditure on food items was either the past seven or three days. The period of recall for non-food expenditure varied including one week, one month, three months, and one year. There were nearly 300 food items and 70 non-food items included in the survey questionnaire. Households were asked to specify the quantity consumed and the

[5]Anthropometric measures were taken in the original 2000 survey but was not taken in the 2003 survey

unit price at which they purchased the goods. These are converted in to monthly expenditures. When prices were missing for a household, village median prices are used instead[6]. Some food items collected in the first round were not recollected in later rounds. These items were dropped from the expenditure calculation to ensure consistency across all three rounds. All the expenditure-related variables are then converted in to 2006 real expenditures using the overall Bangladesh yearly food and non-food CPI[7].

Calories: Household daily calorie consumption was calculated by converting the quantity of food consumed into kilocalories using the conversion scale from Helen-Keller International provided by IFPRI. Again, food items in year 2000 that were not recollected in later rounds were eliminated to ensure consistency across all three years.

Health Outcomes: Health outcomes were calculated from anthropometric measurement taken in the 2000 and 2006 rounds. No anthropometric survey data were collected in the 2003 round and only the anthropometrics of young children and pregnant mothers were taken in 2000. Due to data limitations, we examined two health outcomes: the proportion of stunted and underweight children aged 0 to 12 years old in a household. Stunted children are those with height-for-age z-score two standard deviations below the World Health Organization (WHO) new 2005 reference group. Underweight children are those with a Body Mass Index z-score two standard deviations below the same WHO 2005 reference group.

Beneficiary Status: Determining the actual beneficiaries of the two programmes was difficult, as there were no questions in the surveys that directly asked households about their beneficiary status. As a result, we have chosen to report a household as a beneficiary of the programme if they reported receiving any amount of the in-kind food or cash benefits in the past month. Those who received the food grain or cash were given a value of one and classified as a beneficiary while those who did not report getting any transfer were given a value of zero and classified as a non-beneficiary in the beneficiary status indicator variable.

Eligibility Status: Households' eligibility statuses were calculated following the strict eligibility rules of the programme described in Section 3. Due to the slight difference in the eligibility criteria between the two programmes, two eligibility indicator variables were created, one for each programme. Eligibility for each programme was calculated for all households in all years regardless of whether the programme was available at those times and locations or not. This yields variables that in fact measured *hypothetical eligibility*. These hypothetical eligibility variables are then interacted with indicator variables for programme existence in the union (one for each programme) in order to generate *actual eligibility*, i.e. the instrumental variables for beneficiary status of each household used later in the empirical analysis. Eligibility variables take a value of one if a household satisfies any one out of the four and five eligibility criteria for FFE and PES, respectively, and, in addition, has at least one primary-school-aged child as a member of the household. Households that do not satisfy these conditions are classified as ineligible and are given a value of zero.

[6]When village median prices were missing, upazilas median prices were used. When upazilas median prices were missing, union median prices were used. This go up until the biggest unit which are the median prices over all sampled household.

[7]Inflation source: Economic Trend, Bangladesh Bank and Monthly Statistical Bulletin, Bangladesh Bureau of Statistics

4.2.2 Descriptive Statistics

This subsection presents descriptive statistics calculated from the IFPRI's Chronic Poverty and Long-term Impact study in Bangladesh data set. Here and throughout the rest of this section we concentrate on households who were present in all three years of the survey, which consists of 455 households in total in each year.

Table 2 classifies households according to their eligibility and beneficiary status in all three rounds of survey. The first four rows in both sub-tables are exhaustive categories and add up to 100 percent. In the sample, 46.59% and 56.49% of households change their eligibility and beneficiary status at least once. . Out of the total sample, 34.73% were eligible for both the FFE and PES programmes, and 24.83% were the beneficiary of both FFE and PES programmes. These households will allow us to identify the effect of the two programmes on the same households. Due to the problem of endogeneity of the beneficiary status, we chose to identify the impact of both programmes through exogenous eligibility status. This will be discussed in more detail in the next section.

Table 2: Change in status over years

Eligibility Status	Households (%)	Beneficiary Status	Households (%)
Eligible in all 3 years	22.64	Beneficiary in all 3 years	6.37
Eligible for 2 years	26.59	Beneficiary for 2 years	24.40
Eligible for 1 year	20	Beneficiary for 1 year	32.09
Ineligible in all 3 years	30.77	Non-Beneficiary in all 3 years	37.14
Eligible for both FFE&PES	34.73	Beneficiary of both FFE&PES	24.83

Table 3 presents descriptive statistics on the main characteristics of households in the 2000 round when only the FFE programme was in place. Overall, FFE beneficiary households are poorer than non-beneficiary households with smaller total expenditures and expenditures per capita. Parents of the beneficiary households also have fewer years of education and literacy rate. It should be explained that literacy was collected based on a code in the survey, with 2 representing those who "can sign only", and 3 and 4 representing those who "can read" and "can read and write" respectively. Therefore, on average, the rural population in Bangladesh are poorly educated. The number of young children is similar, with beneficiary households having a slightly higher number.

The last five characteristics included in Table 3 are the five eligibility criteria stated in Section 3. The criterion sharecropper was not one of the eligibility criteria for the FFE programme in the year 2000 but we include it here to allow for comparison with the PES programme later. A higher proportion of beneficiary households have less than 0.5 acres of land, are day labourers and are households led by a female who is either divorced or widowed. Given that the FFE is a poverty-targeted programme, the systematic differences are not surprising. It is important to take these considerations into account once we proceed to the empirical analysis. Finally, the differences between eligible and ineligible households are similar to those observed between beneficiary and non-beneficiary households with a slight difference in that the gap between the two is wider. As the eligible population was constructed following a strict eligibility rule, the smaller gap between beneficiary and non-beneficiary households could point to a targeting problem of the FFE programme. Eligibility in this case is the actual and not the hypothetical eligibility discussed earlier, which is true for the other tables in this section unless stated otherwise.

Table 4 presents the same sets of main household characteristics as before for households in the

2003 and 2006 round when the PES programme had replaced the FFE programme. The statistics present a similar story to the FFE programme, where beneficiary and eligible households are poorer and less educated than non-beneficiary and ineligible households. Comparing across years, apart from expenditure, the household characteristics of the eligible and beneficiary households hardly change. This reflects the fact that the PES programme was a continuation of the FFE intended to solve the same problem, and target the same group of people, with the only major change being the method by which the transfers were given out.

One important point to note in this study is that because the original 2000 sample was designed to include households with at least one primary-school-age child, the percentage of primary-school-age children declines as these children become older in later survey waves. This is shown in Table 5. This means that the number and percentage of beneficiaries also falls, as to be eligible for transfer the household must enrol at least one child in primary school. Thus, the gradual decline in the percentage of beneficiary households does not reflect a change in the targeting criteria but rather the aging of the panel. This helps provide a natural source of variation for changes in eligibility status. In Table 5, we use the hypothetical eligibility to illustrate our point regarding the targeting criteria between the two programmes. There are minimal differences in the percentage of households eligible for both programmes in all three years of the survey.

In addition, as the survey excluded those households with no or older children, these statistics are not representative of the overall rural population in the area but, as the main focus of this study is to compare the effect of in-kind and cash transfers to households, as long as households who received or were eligible for these two programmes had similar characteristics, which seems to be the case from evidence in Table 3 and Table 4, we can proceed with confidence.

Table 3: FFE Summary Statistics (2000)

	Non-Ben	Beneficiary	p	Ineligible	Eligible	p	Total
Household size	5.65	5.52	0.520	5.87	5.26	0.001***	5.60
Household Expenditure	5160.62	3986.91	0.006***	5554.04	3678.96	0.000***	4729.83
Per capita Expenditure	919.30	736.32	0.003***	954.07	722.17	0.000***	852.14
Father Years of Education	2.69	2.14	0.125	3.17	1.62	0.000***	2.49
Mother Years of Education	1.90	1.44	0.095*	2.18	1.15	0.000***	1.73
Father Literacy	2.34	2.16	0.201	2.51	1.97	0.000***	2.27
Mother Literacy	2.31	2.20	0.384	2.41	2.09	0.005***	2.27
Children age 0 to 12	2.36	2.58	0.063*	2.37	2.52	0.191	2.44
Near Landless	0.52	0.70	0.000***	0.34	0.90	0.000***	0.59
Daylabour	0.18	0.29	0.007***	0.12	0.35	0.000***	0.22
Female Headed Household	0.10	0.13	0.258	0.08	0.15	0.015**	0.11
Low Income Occupation	0.02	0.02	0.657	0.02	0.03	0.302	0.02
Sharecropper	0.08	0.07	0.758	0.06	0.10	0.102	0.08
Observations	167	288		200	255		455

Main statistics are the mean.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Source: Author's Calculation.

Table 4: PES Summary Statistics (2003 & 2006)

	Non-Ben	Beneficiary	p	Ineligible	Eligible	p	Total
Household size	5.11	5.77	0.000***	5.17	5.48	0.018**	5.32
Household Expenditure	6088.01	5279.50	0.001***	6421.70	5207.88	0.000***	5832.13
Per capita Expenditure	1254.83	933.98	0.000***	1319.70	977.08	0.000***	1153.28
Father Years of Education	2.68	2.08	0.023**	3.04	1.91	0.000***	2.49
Mother Years of Education	1.84	1.49	0.080*	1.93	1.52	0.030**	1.73
Father Literacy	2.32	2.17	0.143	2.47	2.06	0.000***	2.27
Mother Literacy	2.29	2.22	0.434	2.28	2.25	0.679	2.27
Children age 0 to 12	1.15	2.08	0.000***	0.89	2.05	0.000***	1.38
Near Landless	0.54	0.72	0.000***	0.33	0.87	0.000***	0.59
Daylabour	0.17	0.30	0.000***	0.11	0.32	0.000***	0.21
Female Headed Household	0.11	0.13	0.58	0.09	0.14	0.010***	0.12
Low Income Occupation	0.02	0.02	0.877	0.01	0.03	0.043**	0.02
Sharecropper	0.09	0.09	1.000	0.06	0.12	0.001***	0.09
Observations	288	622		425	485		910

Main statistics are the mean.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Source: Author's calculation

Table 5: Beneficiary/Eligibility Status by Year

	2000	2003	2006
% Primary School Aged Kid	0.98	0.78	0.60
% FFE Beneficiary	0.37	.	.
% PES Beneficiary	.	0.38	0.25
% Eligible for FFE	0.64	0.53	0.40
% Eligible for PES	0.65	0.55	0.42
Observations	455	455	455

Source: Author's Calculation

Table 6 and Table 7 present the breakdown of food expenditure for households in the 2000 FFE round and the combined 2003 and 2006 PES rounds, respectively. The main interest lies in the two in-kind food grain subsidies in the FFE programme; wheat and rice. Wheat expenditure is the main focus as most of the in-kind subsidy provided took the form of wheat. The difference between wheat consumption of beneficiary and non-beneficiary households, and more importantly between the two programmes across years, is apparent. FFE beneficiary and eligible households consumed more wheat than non-beneficiary and ineligible households. Moreover, they consumed more wheat than PES beneficiary and eligible households in later years. Given the fact that the pattern of food consumption between the non-beneficiary and ineligible households hardly changes across the years, this observation on expenditure on wheat gives the first sign of evidence that in-kind transfers may create a constraint in the households consumer demand problem.

Looking at the food share, FFE beneficiary and eligible households spend more on food than non-beneficiaries and ineligibles, while the same is not true for households in the PES programme in 2003 and 2006, in which case the food shares between the two groups are very close to each other. In addition, FFE beneficiary households also spend a higher fraction of their expenditure on food than PES beneficiary households. As total expenditure is divided between food and non-food expenditure by construction, this also suggest that PES households spend a greater share of expenditures on non-food items than FFE beneficiary households do.

These differences may be caused by many factors other than the difference in the nature of the transfer employed by the two programmes. It will be interesting to investigate further whether the difference in the pattern of consumption observed here is a direct result of the change from in-kind

food to cash transfer. If these differences were truly a result of the constraint imposed by the in-kind programme does it lead to a difference in short or long-term outcomes that change household welfare?

Table 6: FFE Food Expenditure Patterns (2000)

Expenditure Shares (percent)	Non-Ben	Beneficiary	p	Ineligible	Eligible	p	Total
Food groups							
Cereals	32.44	35.63	0.009***	31.32	36.52	0.000***	33.61
Wheat	**0.53**	**4.34**	**0.000*****	**0.68**	**3.51**	**0.000*****	**1.93**
Rice	29.91	29.78	0.916	28.62	31.46	0.016**	29.87
Pulses	1.75	1.93	0.378	1.79	1.84	0.805	1.81
Spices	3.53	3.65	0.482	3.50	3.68	0.253	3.58
Oil	1.99	2.04	0.665	2.01	2.01	0.982	2.01
Fruit & Vegetables	13.52	14.35	0.207	13.57	14.16	0.353	13.83
Meat & Fish	16.99	15.87	0.222	17.63	15.24	0.007***	16.58
Out of Home	1.45	1.14	0.188	1.46	1.18	0.236	1.34
Drink	0.76	0.57	0.101	0.74	0.62	0.277	0.69
Other	3.08	2.77	0.244	3.16	2.72	0.090*	2.97
Total Food	**75.52**	**77.93**	**0.072***	**75.17**	**77.97**	**0.032****	**76.40**
Observations	167	288		200	255		455

Main statistics are the mean.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Source: Author's Calculation

Table 7: PES Food Expenditure Patterns (2003 & 2006)

Expenditure Share (percent)	Non-Ben	Beneficiary	p	Ineligible	Eligible	p	Total
Food groups							
Cereals	27.81	31.09	0.000***	27.38	30.40	0.000***	28.85
Wheat	**0.30**	**0.32**	**0.836**	**0.23**	**0.39**	**0.100**	**0.31**
Rice	25.31	28.88	0.000***	24.85	28.12	0.000***	26.44
Pulses	1.73	1.89	0.327	1.69	1.88	0.202	1.78
Spices	3.49	4.16	0.003***	3.64	3.77	0.524	3.71
Oil	2.48	2.57	0.351	2.56	2.45	0.213	2.51
Fruit & Vegetables	14.95	14.54	0.344	15.09	14.54	0.170	14.82
Meat & Fish	15.76	13.86	0.009***	15.81	14.46	0.048**	15.16
Out of Home	4.26	3.18	0.029**	3.80	4.04	0.605	3.92
Drink	0.86	0.64	0.041**	0.93	0.65	0.007***	0.79
Other	2.12	1.65	0.010**	2.18	1.75	0.011**	1.97
Total Food	**73.46**	**73.58**	**0.846**	**73.08**	**73.94**	**0.118**	**73.50**
Observations	288	622		425	485		910

Main statistics are the mean.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Source: Author's Calculation

5 Identification

This section discusses the identification issue in this paper. The non-experimental nature of the data poses various problems that must be dealt with in order to correctly obtained the objects we want to estimate. Instrumental variable and fixed effect approach is proposed to deal with these problems. Here we argued that these techniques and assumptions are sufficient and credible for the identification of the parameters of interest.

5.1 Parameter of Interest

The main object of interest in this paper is the difference in the effect of cash versus in-kind transfer on various household outcomes such as calorie consumption and health status. The approach taken

is to identify the impact of in-kind transfer by comparing the effects of the programme on outcomes of in-kind beneficiary households to non-beneficiary households. Similarly, the impact of the cash transfer is determined by comparing the outcomes of cash beneficiary households to non-beneficiary households. The in-kind and cash impacts are then compared to each other to investigate whether any significant difference exists. This allows us to test whether the change in the medium of the transfer has any impact on household behaviour. To simplify the discussion without the loss of generality in this case, we will focus on just calorie consumption as the outcome variable.

One of the main quantities that we want to estimate are the causal effect of being a beneficiary of the in-kind transfer programme on household calorie consumption. Allowing for heterogeneous programme effect, we imagine two potential outcomes variables

$$Potential\,outcome\,of\,in-kind\,transfer\; = \begin{cases} Y_{i,t}^{K}(1) & if\; B_{i,t}^{K}=1 \\ Y_{i,t}^{K}(0) & if\; B_{i,t}^{K}=0 \end{cases}$$

$B_{i,t}^{K}$ is an indicator of household i beneficiary status under the ***in-kind*** transfer programme (superscript K) at time t, taking the value 1 when household is a beneficiary and 0 otherwise. $Y_{i,t}^{K}(1)$ is household i's calorie consumption when it is a beneficiary of the in-kind transfer programme, while $Y_{i,t}^{K}(0)$ is household i's calorie consumption if it is not a beneficiary at time t. The difference between $Y_{i,t}^{K}(1)$ and $Y_{i,t}^{K}(0)$ is the causal effect of being a beneficiary of the in-kind programme on calorie consumption of household i at time t and is the object that we are interested in. The observed outcome $Y_{i,t}^{K}$ can be written in terms of potential outcomes as

$$Y_{i,t}^{K} \;=\; Y_{i,t}^{K}(1)B_{i,t}^{K} \;+\; Y_{i,t}^{K}(0)(1 \;-\; B_{i,t}^{K})$$

We get to observe one of $Y_{i,t}^{K}(1)$ or $Y_{i,t}^{K}(0)$, but never both. We therefore, hope to measure the average of $Y_{i,t}^{K}(1) \;-\; Y_{i,t}^{K}(0)$. The naive comparison of the outcomes of those who were and were not beneficiaries of the programme are likely to be a poor measure of the causal effect of the programmes given their non-experimental nature.

The conditional naive comparison of beneficiary and non-beneficiary households can be decomposed as

$$\begin{aligned} \underbrace{E[Y_{i,t}^{K}|B_{i,t}^{K}=1,X_{i,t}=x]-E[Y_{i,t}^{K}|B_{i,t}^{K}=0,X_{i,t}=x]}_{Naive\,comparison} &= \underbrace{\{E[Y_{i,t}^{K}(1)|B_{i,t}^{K}=1,X_{i,t}=x]-E[Y_{i,t}^{K}(0)|B_{i,t}^{K}=1,X_{i,t}=x]\}}_{ATT^{K}} \\ &+\underbrace{\{E[Y_{i,t}^{K}(0)|B_{i,t}^{K}=1,X_{i,t}=x]-E[Y_{i,t}^{K}(0)|B_{i,t}^{K}=0,X_{i,t}=x]\}}_{Selection\,bias} \end{aligned} \tag{3}$$

The first term in the curly bracket on the right hand side of equation 3 is the conditional average treatment effect on the treated for the in-kind transfer programme. ATT^{K} measures the effect of the in-kind programme on those who are the beneficiary of the programme. In this case we are particularly interested in ATT^{K} conditional on observables $X_{i,t}$ as there are various observable differences such as income and household structure between beneficiary and non-beneficiary households, due to the non-random assignment of treatment, that also have direct impact on the outcome of interest. As it is the direct impact of the programme on beneficiary household that we want to know, it is necessary to control for these observable differences. Unconditional ATT^{K} is then obtain by taking the expectation

over all conditional ATT^K[8]. ATT^K is a parameter of interest for policymakers implementing the programmes, and is the object we look to estimate in this paper.

Another object of interest, as stated before, is the impact of cash programme on household's calories consumption. Similar to before, we make use of the potential outcome framework

$$Potential\,outcome\,of\,cash\,transfer\; = \begin{cases} Y^C_{i,t}(1) & if\ B^C_{i,t} = 1 \\ Y^C_{i,t}(0) & if\ B^C_{i,t} = 0 \end{cases}$$

Here, $B^C_{i,t}$ is an indicator of household i beneficiary status under the ***cash*** transfer programme (superscript C) at time t, taking the value 1 when household is a beneficiary and 0 otherwise. $Y^C_{i,t}(1)$ is household i's calorie consumption when it is a beneficiary of the cash programme, and $Y^C_{i,t}(0)$ is household i's calorie consumption if it is not a beneficiary of the cash programme at time t. The difference between $Y^C_{i,t}(1)$ and $Y^C_{i,t}(0)$ is the causal effect of being a beneficiary of the cash programme on calorie consumption of household i at time t and is the object that we are interested in. Similar to before, the naive comparison of the outcome of those who were and were not beneficiaries of the cash programme are

$$\underbrace{E[Y^C_{i,t}|B^C_{i,t}=1, X_{i,t}=x] - E[Y^C_{i,t}|B^C_{i,t}=0, X_{i,t}=x]}_{Naive\,comparison} = \underbrace{\{E[Y^C_{i,t}(1)|B^C_{i,t}=1, X_{i,t}=x] - E[Y^C_{i,t}(0)|B^C_{i,t}=1, X_{i,t}=x]\}}_{ATT^C}$$
$$+ \underbrace{\{E[Y^C_{i,t}(0)|B^C_{i,t}=1, X_{i,t}=x] - E[Y^C_{i,t}(0)|B^C_{i,t}=0, X_{i,t}=x]\}}_{Selection\,bias} \tag{4}$$

Again, making use of the law of iterated expectation we can obtain the unconditional ATT^C by taking expectation over the conditional ATT^C. This unconditional ATT^C is another parameter of interest in addition to ATT^K mentioned earlier, and it is the comparison between ATT^K and ATT^C that is the ultimate quantity of interest.

However, the major obstacle to the correct identification of both ATT^K and ATT^C from the naive estimator in equation 3 and 4 is the second term on the right hand side of both equations. These two terms are the *selection bias*, the differences in the non-treatment outcome among those who are the beneficiaries and those who are not. Given the poverty-targeting nature of both the FFE and PES programme, selection bias will be a major problem. For example, households who are beneficiaries of the programme will be poorer on average and it is likely that they would have been able to consumed less calories in the first place ($\{E[Y^C_{i,t}(0)|B^C_{i,t}=1, X_{i,t}=x] - E[Y^C_{i,t}(0)|B^C_{i,t}=0, X_{i,t}=x]\} < 0$). The selection bias in this case is expected to be negative and the naive comparison would likely underestimate the true impact of the programme. However, selection based on household observables characteristics, such as income as above, could be accounted for by conditioning on such observables. If it is only selection on observables that is driving the bias, conditional independence assumption (CIA) would ensured that the conditional selection bias terms in both equations are zero and the conditional naive comparisons would be consistent estimates of the conditional ATT^K and ATT^C[9].

Nonetheless, CIA alone is not credible in indetifying the parameters of interest as there are also other unobserved factors that is correlated with treatment status and could affect selection bias in one direction or the other. In an ideal randomised experiment these unobserved factors will tend to

[8]Making use of the law of iterated expectation $E_X\{E[Y^K_{i,t}(1)|B^K_{i,t}=1, X_{i,t}=x] - E[Y^K_{i,t}(0)|B^K_{i,t}=1, X_{i,t}=x]\} = E[Y^K_{i,t}(1)|B^K_{i,t}=1] - E[Y^K_{i,t}(0)|B^K_{i,t}=1]$

[9]The conditional independence assumptions are $\{Y^K_{i,t}(0),\, Y^K_{i,t}(1),\} \perp\!\!\!\perp B^K_{i,t}|X_{i,t}$ and $\{Y^C_{i,t}(0),\, Y^C_{i,t}(1),\} \perp\!\!\!\perp B^C_{i,t}|X_{i,t}$ for both programmes

balanced out between treatment and control group. Given that this is not the case with the FFE and PES programme, the conditional selection bias term will be non-zero causing the conditional naive estimators to be inconsistent. Selection on unobservables in this case often present itself as the problem of partial compliance, where not all those who are eligible will choose to take up the benefits. Compliance is a choice made by households which are likely to be correlated with household's unobserved characteristics. If these characteristics affect the outcome variable, partial compliance will affect the selection bias. Partial compliance almost always lead to overestimation of the true programmes impact as those who are likely to benefit most from the programmes are those who take it up while those who are unlikely to benefit will not.

There are two types of unobservable characteristics, those that are fixed across time and those that vary across time. One example of fixed unobservable characteristics are household preference for food consumption. In a partial compliance scenario, it is possible that household who choose to become beneficiary of the in-kind programme are those who have high preference for the consumption of food and would have benefited more from the in-kind transfer than non-beneficiary household. It is possible that these households would have consumed more food than non-beneficiary household in the non-treatment state. Non time-varying unobservables that causes selection bias can be dealt with by the fixed effect approach utilised later.

Time-varying unobservables such as transitory shock to individual household could also affect selection bias. For example, household may be hit with a negative shock in one period that increase the opportunity cost of sending children to school which ultimately affect its beneficiary status and also its calorie consumption. Not observing the negative shock may cause us to think that this is one of the better off non-beneficiary household and could result in the incorrect comparison between beneficiary and non-beneficiary household. Here again, partial compliance where those who is unlikely to benefit select themselves out of the programme will caused overestimation of programme impact.

In addition, as discussed earlier, the data set could also suffer from a measurement error problem. This resulted from the lack of clarity and inconsistencies in the questionnaire in determining the actual beneficiary status of households. Measurement error doesn't affect selection bias but, assuming classical measurement error, will caused attenuation bias in the naive estimator. We later used the instrumental variable approach to deal with selection based on unobservables and measurement error problem.

5.2 Dealing with Selection Bias

In this subsection we presents the methods and assumptions made to deal with the problems specified in subsection 5.1 above. The problems boil down to finding a credible estimate of the unobserved counterfactuals $E[Y^K_{i,t}(0)|B^K_{i,t} = 1, X_{i,t} = x]$ and $E[Y^C_{i,t}(0)|B^C_{i,t} = 1, X_{i,t} = x]$, and getting rid of the selection bias term in order to obtain a consistent estimates of the conditional ATT of both in-kind and cash transfer programmes.

Given the poverty-targeting nature of both programmes, the assignment of household into treatment and control group are non-random and is based on the eligibility rule stated in section 3. We propose to identify the counterfactual using *actual eligibility* for both programmes *($D^K_{i,t}$, $D^C_{i,t}$)*, computed from the interaction of eligibility rules and the indicator of programme availability, as an instrumental variable that is correlated with beneficiary status, but conditionally exogenous and uncorrelated with the

outcome variables. We termed this the fixed-effect instrumental variable (FE-IV) approach which will solve the problem of partial compliance and non-random assignment and placement of the programmes.

Here we make use of the potential outcomes notation for the receipt of treatment ($B^K_{i,t}$, $B^C_{i,t}$) and the outcome ($Y^K_{i,t}$, $Y^C_{i,t}$). The instruments in this case are binary $D^K_{i,t} \epsilon \{0, 1\}$ and $D^C_{i,t} \epsilon \{0, 1\}$. Denote the level of the treatment received if the instrument takes on the value 0 and 1 respectively by $\{B^K_{i,t}(0), B^K_{i,t}(1)\}$ for the in-kind programme, and $\{B^C_{i,t}(0), B^C_{i,t}(1)\}$ for the cash programme. $B^K_{i,t}(1)$ and $B^C_{i,t}(1)$ tells us whether household i would become a beneficiary household given that it is eligible, while $B^K_{i,t}(0)$ and $B^C_{i,t}(0)$ tells us whether household i would become a beneficiary household if it is ineligible. As before, we only observe one of the potential receipts of treatment for each household depending on the instruments $D^K_{i,t}$ and $D^C_{i,t}$[10].

We made the following assumptions to identify the parameters of interest using the FE-IV approach.

- Assumption (1): Stable Unit Treatment Value Assumption (Rubin 1980). This means that programme participation and potential outcome of each household unit depends on the treatment received and are unrelated to the treatment status of other households. This rules out any spillover or general equilibrium effect from treated to non-treated households.

- Assumption (2): Conditional Independence of the Instruments Assumption. In this case, we assume that conditional on time-varying observables ($X_{i,t}$) and fixed unobservables (U_i), potential outcomes and potential treatment assignments are independent of the instruments which are household's FFE and PES programmes actual eligibility status ($D^K_{i,t}$, $D^C_{i,t}$).

$$\{Y^K_{i,t}(0), Y^K_{i,t}(1), B^K_{i,t}(0), B^K_{i,t}(1)\} \perp\!\!\!\perp D^K_{i,t} | X_{i,t}, U_i$$

$$\{Y^C_{i,t}(0), Y^C_{i,t}(1), B^C_{i,t}(0), B^C_{i,t}(1)\} \perp\!\!\!\perp D^C_{i,t} | X_{i,t}, U_i$$

- Assumption (3): Instrument relevance. There is a positive correlation between the instruments and treatment status. In this case, this means that there is non-zero relationship between eligibility status and beneficiary status of household in the programmes.

$$E[D^j_{i,t}, B^j_{i,t} | X_{i,t}, U_i] \neq 0 \, where \, j = K \, , \, C$$

- Assumption (4): Exclusion restriction. Formally, the exclusion restriction is

$$B^j_{i,t} = B^j_{i,t}(1)D^j_{i,t} + B^j_{i,t}(0)(1 - D^j_{i,t}) \mid X_{i,t,} U_i$$

$$Y^j_{i,t} = Y^j_{i,t}(1)B^j_{i,t} + Y^j_{i,t}(0)(1 - B^j_{i,t}) \mid X_{i,t,} U_i$$

$$where \, j = K \, , \, C$$

The instruments $D^j_{i,t}$ only appear in the first equation above which determine the beneficiary status $B^j_{i,t}$ but is absent from the second equation which determine the outcome $Y^j_{i,t}$. This means that conditional on time-varying observables and non time-varying unobservables the instruments only affect the outcome variable through its effect on the treatment status. Specifically to this case, eligibility status affect the beneficiary status of individual household but it doesn't have direct effect on

[10]The observed treatment status is therefore $B^K_{i,t} = B^K_{i,t}(1)D^K_{i,t} + B^K_{i,t}(0)(1 - D^K_{i,t})$ for the in-kind programme and $B^C_{i,t} = B^C_{i,t}(1)D^C_{i,t} + B^C_{i,t}(0)(1 - D^C_{i,t})$ for the cash programme.

household's calorie consumption controlling for $X_{i,t}$ and $U_{i,t}$. Household doesn't consume more or less calorie only just because it is eligible for either of the programmes. Notice that unlike the case with exogenous instrument, the instrument here is only exogenous and satisfied the exclusion restriction upon conditioning on observables and unobservables fixed effect.

- Assumption (5): Monotonicity. The instrument moved every household's responses in the same direction. In other words there is no household who behave as a defier i.e. household who would have participated in the programme if it is not eligible, and not participated in the programme if it is eligible. In this case monotonicity is assume to be

$$B^K_{i,t}(1) \geqslant B^K_{i,t}(0)$$

$$B^C_{i,t}(1) \geqslant B^C_{i,t}(0)$$

Given all of these assumptions hold, the FE-IV will identified the conditional Local Average Treatment Effect (LATE) instead of the average treatment on the treated. Unlike the ATT, LATE is the average treatment effect for those households who are moved by the instrument and is written formally as.

$$\begin{aligned} LATE^j &= \frac{E[Y^j_{i,t}|X_{i,t}=x,U_i\, D_{i,t}=1]-E[Y^j_{i,t}|X_{i,t}=x,U_i,\, D_{i,t}=0]}{E[B^j_{i,t}|X_{i,t}=x,U_i,\, D_{i,t}=1]-E[B^j_{i,t}|X_{i,t}=x,U_i,\, D_{i,t}=0]} \\ &= E[Y^j_{i,t}(1)-Y^j_{i,t}(0)|B^j_{i,t}(1) > B^j_{i,t}(0),\, X_{i,t}=x,\, U_i] \\ & \qquad where\, j = K\,,\, C \end{aligned}$$

As we needed the conditional independence of the instrument assumption for the strategy to work, the LATE we estimated is in fact a covariate-averaged LATE[11].

5.3 Local Average Treatment Effect (LATE)

In this subsection we gave formal interpretations to the LATE. Though we drop the superscript K and C, the discussion here applies to the parameters of both in-kind and cash programmes ($LATE^K$ and $LATE^C$).

Table 1 below shows all the possible sub-group of the population. Each group is classified by how the population moved with the instrument. Always-takers are those who would received treatment whether they are eligible or not. Never-takers are those that will never take up the treatment regardless of their eligibility. Compliers are those who take up the treatment when they are eligible and will not when they are ineligible. Lastly, we have defiers who behave in the opposite direction of the instrument they were assigned with, taking up the treatment when they are not eligible and not taking up the treatment when they are.

Table 8: Types of Population

	$D_{i,t}=1$	$D_{i,t}=0$
Always-takers	$B_{i,t}(1)=1$	$B_{i,t}(0)=1$
Compliers	$B_{i,t}(1)=1$	$B_{i,t}(0)=0$
Defiers	$B_{i,t}(1)=0$	$B_{i,t}(0)=1$
Never-takers	$B_{i,t}(1)=0$	$B_{i,t}(0)=0$

[11] Abadie (2003) provided the exact method to compute the covariate-averaged LATE which involve weighting the least square equation by "Abadie Kappa". Angrist (2001) however, showed that estimates based on Abadie's method are indistinguishable from two-stages least square estimates which are the estimates we will use in this paper.

LATE measures the treatment effect of the programme on compliers because the effect of the programme on always-takers and never-takers cannot be determined as the two sub-population treatment status never changed regardless of the value of the instrument. In addition, the *monotonicity* assumption made in the last section ruled out the presence of defiers in the population.

If we are willing to make a further assumption of one-sided partial compliance, that only those who are eligible can choose to not take up the programme but those who are not eligible for the programme will never be able to gain access to the programme, we will also be able to ruled out the existence of always-takers as well. This is because one-sided compliance imply that $B_{i,t}(0) = 0$ always, and as always-takers have $B_{i,t}(0) = 1$, it is impossible to have always-takers in this case. This seems like a plausible assumption to make in places where programmes are well and strictly implemented. As the ATT can be decomposed in to the programme effect on always-takers and compliers[12], ruling out the presence of always-takers means that those who are treated consists entirely of compliers. Therefore, If this assumption of "$B_{i,t}(0) = 0$ always" holds, the conditional LATE we identified in the last subsection using the FE-IV approach will, in this special case, be the same as the conditional ATT, which is the parameter of interest we originally wanted to identify[13].

However, this assumption is questionable in the Bangladesh data set used here. In this data set we do have households who are beneficiaries despite being ineligibles due to problems with mis-targeting which is common in this type of programmes in under-developed countries. The rate of mis-targeting is about 8% for both FFE and PES programmes. These figures are non-negligible, hence making the one-sided compliance assumption an invalid one. We therefore, have to be content with the LATE.

ATT which measure the effect of the programme of all those who received the benefits is more *externally valid* as a parameter than LATE. This means that the ATT is more useful in providing policy maker with an idea of the effect that similar programmes, implemented under different conditions, may generate based on the impact found in this programme. Although this is a very useful property, we cannot consistently estimate the ATT of both programmes due to data limitations. The ATT therefore, is not *internally valid*.

LATE, although not the original parameter we set out to estimate, is still useful. LATE measures the effect of the programme on the sub-population of compliers. Although this is less externally valid than the ATT, as the compliant sub-population may differ under different programme conditions or different instruments making the resulting effect of the LATE less generalisable, it is internally valid. We take the view here that an internally valid parameter with lower level of external validity is better than a more externally valid parameter that is not internally valid[14].

In this section, we identify two parameters which we are interested in estimating and comparing to each other, $LATE^K$ and $LATE^C$. We could obtain the estimates of both parameters by conducting two separate regressions, one for each programme. However, pooling the two regressions together is a more efficient way to both estimate and compare the two quantities. The next section discuss the issue of estimating the two parameters and provide details on the method.

[12] $E[Y_{i,t}(1)-Y_{i,t}(0)|B_{i,t}=1,\ X_{i,t}=x] = E[Y_{i,t}(1)-Y_{i,t}(0)|B_{i,t}(0)=1,\ X_{i,t}=x]P[B_{i,t}(0)=1|B_{i,t}=1]+E[Y_{i,t}(1)-Y_{i,t}(0)|B_{i,t}(1)>B_{i,t}(0),\ X_{i,t}=x]P[B_{i,t}(1)>B_{i,t}(0),\ D_{i,t}=1|B_{i,t}=1]$, The effect of conditional average treatment on the treated is equal to the conditional average effect of treatment on always-takers times the probability of the subjects being an always-takers given that they are treated plus the conditional average effect of treatment on compliers times the probability of the subjects being a complier with the instrument taking value of 1 (complier will not be treated if instrument takes value of 0) given that they are treated.

[13] For a simple and direct proof of this result due to Howard Bloom (1984), please refer to Angrist and Pischke (2009)

[14] Imbens (2009) provides a good discussion of the usefulness of LATE.

6 Econometric Specification and Estimation

This section discusses the econometric specifications used in this paper to consistently estimate the parameters identified in section 5 and test the hypothesis of whether there is a difference in household outcomes under in-kind and cash transfers.

6.1 The Basic Specification: In-kind vs. Cash

In this section, we present the basic specifications that form the basis of the analysis of the impact of the FFE and PES programmes. The ultimate goal is to test whether there is a significant difference between the two programmes on households' behaviour and hence, the two types of transfer. We also present discussions of the problems that arise with this basic specification.

We are specifically looking at three major sets of outcomes; food consumption, expenditure shares and health status of children. The general specification is the same for all three outcomes and is given by equation 5:

$$y_{it} = \alpha + InKind_{i,2,t} \cdot \beta_{InKind} + Cash_{i,3,t} \cdot \beta_{Cash} + x_{it}\gamma + \epsilon_{it} \tag{5}$$

Where y_{it} is one of the three outcomes of interests; *per capita* calorie consumption, expenditure share of various food categories or health status of children for household $i = 1, ..., N$ at time $t = 1, ..., T$. $InKind_{i,2,t}$ is an indicator variable taking the value of 1 if household i is a beneficiary of the FFE programme at time t and zero otherwise. $Cash_{i,3,t}$, is similarly an indicator variable taking the value of 1 if household i is a beneficiary of the PES programme at time t and zero otherwise. x_{it} is a $(1 \times k)$ vector of both time-variant and time-invariant control variables, including household characteristics, expenditures per capita, prices, eligibility status and year; γ is the $(k \times 1)$ vector of corresponding coefficients; and ϵ_{it} is a mean zero error term, which is assumed to be orthogonal to the included regressors and has a variance of σ^2_ϵ.

Equation 5 is a reduced-form equation of the impacts of the two programmes. The two main coefficients of interest are β_{InKind} and β_{Cash} which respectively identify the impact on household i at time t that received the in-kind and cash benefit relative to receiving no benefits at all. As the PES programme is a continuation of the FFE programme, a household can never be a beneficiary of both programmes in the same period. β_{InKind} and β_{Cash} in this specification correspond to the naive estimator discussed in section 5. We are interested both in the individual magnitude of the coefficient β_{Inkind} and β_{Cash}, and also their magnitudes relative to each other. As both programmes target poverty, it is normal for the observed characteristics of beneficiary and non-beneficiary households to be different. These characteristics affect the outcomes of interest and, therefore, by omitting them, the estimated impacts of the programmes are going to be biased. It is therefore necessary to include these observable factors as control variables x_{it} in equation 5. We will discuss this in detail below.

6.1.1 Dependent Variables

There are three main sets of regression in this paper. In this subsection we describe the dependent variables that we use in each set of regressions.

Food Consumption Regression: the outcome variable y_{it} is daily per capita consumption of food measured in kilocalories.

Expenditure Share Regression: the outcome variables y_{it} of interest are the expenditure of various food and non-food items/categories as a share of household total expenditures.

Health Regression: The outcome variables y_{it} is a measure of the health status of children in a household. The two measures we concentrate on are the proportion of stunted and underweight children age 0 to 12 within a household.

6.1.2 Income Effect vs. Compositional Effect

The impact of both transfer programmes can be broken down into an income effect and a compositional effect. Referring back to the theory in Section 2, the income effect of both programmes is the impact of increasing income by T, causing households to spend more on food (foods are normal goods), regardless of the type of transfer. The compositional effect occurs when the food subsidy is extra-marginal, after controlling for any general equilibrium price effects of the programme. This compositional effect of the transfer causes changes in the composition of household food consumption resulting in households consuming more of some type of foods than others. If the transfer is both fully binding and extra-marginal as in point E or partially binding at any point along the kinked part of the budget line in Figure 1, its effect is not only to increase household wheat consumption but also to cause the household to consume more wheat than they would have otherwise, at the expense of other categories of food.

Both type of transfer have an income effect that will also be equivalent when both transfers are of equal value, while only the in-kind transfer has a compositional effect. To test for the difference between the impact of the two types of transfer on food, and hence, on calorie consumption, it is important that we control for the income effect of both programmes. This allows us to focus solely on the compositional effect of the transfer programme and determine the effect of an extra-marginal and binding transfer on various outcomes. For this purpose, the logarithm of per capita monthly expenditure is an appropriate wealth control for all specifications. By using expenditure that would have already incorporated changes in total spending out of the benefits received from the FFE and PES programmes, we can effectively control for income effects and focus on the compositional impact of both programmes.

6.1.3 Control Variables

In addition to the logarithm of real *per capita* monthly expenditure as a control for different levels of wealth between beneficiary and non-beneficiary households, there are several other factors for which we have controlled.

A logarithm of household size and the proportion of members in each age group is added as demographic controls that control for the compositional effect of household structure. Year dummies are added as control variables to capture all the effects that change with time. Prices of the in-kind transferred goods are also added to capture some of the general equilibrium effect. The eligibility status of households for both the FFE and PES programmes is added to control for the difference in

characteristics of eligible and ineligible households[15]. These eligibility variables will give a difference-in-difference estimator of the programme impact, especially in 2000 where there are programme and non-programme locations. The control variables are the same for all three sets of regression except that the price variables are excluded from the set of control variables in the health regression, as they should not matter in that context.

6.1.4 Conditionality of FFE and PES programmes

Both FFE and PES are conditional on children's enrolment in primary school. Therefore, theoretically, both programmes are in fact in-kind programmes. However, the cash and in-kind comparison between the two programmes was made possible by the fact that apart from the medium of transfer, the two programmes are almost identical in all other aspects. By controlling for all these aspects a comparison of differences in household behaviour under the two programmes will allow identification of the difference between the two types of transfers.

However, as both programmes were conditional on children's enrolment, they are likely to have a positive impact on children's enrolment in primary school among beneficiaries when compared to non-beneficiaries. It is possible for these enrolment effects to be correlated with the outcome of interest and, by ignoring this effect, the estimates of programme impact between beneficiary and non-beneficiary households will be biased. In addition, if the effect on enrolment is different between the two programmes, the estimate of the comparison between the two programmes will also be biased.

Estimation of both programmes effect on enrolment has found that both programmes have positive and statistically significant impacts on enrolment. In addition, the t-test of the two programmes found that they are statistically different, with the PES having bigger impact than the FFE programme. Failing to account for this means that the estimates of both the impact of in-kind and cash transfers on beneficiary households compared to non-beneficiary households, and compared to each other, will be biased. To solve this problem, we add the proportion of children enrolled in primary school for each household as a control variable in all three sets of regressions. The enrolment regression results can be found in Appendix A.1.

6.1.5 Controlling for Difference in the Values of Transfer

As shown in Figure 2, the real value of the transfer has been gradually declining since the year 2000. Therefore, differences observed in the effect between the two programmes, if there are any, could be due to differences in the size of the transfer under the two programmes.

To control for the differences in the value of the transfer, equation 5 with the appropriate control variables is estimated with changes made to the variable $InKind_{i,2,t}$ and $Cash_{i,3,t}$; these two variables will now measure the value of the in-kind and cash transfer received by the beneficiary household respectively, as opposed to being indicator (0/1) variables as before. There are two measures of these values for both types of transfer: the actual and the hypothetical value.

The actual values of the transfers are obtained directly from the questionnaire. It is the monthly quantity/value of the transfers that the household itself reports to be receiving from the programme. In-kind food quantity transfers were converted to their monetary value by multiplying by the price variable constructed from the food expenditure data. All monetary value variables are then converted

[15]Recall from Section 4 that the eligibility status also includes "hypothetical" eligibility; the eligibility status of household in either programmes had the programmes been available to them.

into their real 2006 value. The hypothetical values are constructed in a similar manner, with the only difference being that the values of the transfers received are assumed to be equal to what the programme promised. The assumed value for in-kind transfer is 15kg of wheat and the assumed value for cash transfer is 100 BDT for each household[16]. Both real actual and hypothetical values are then converted to *per capita* values, so as to generate sufficient exogenous variation for identification[17].

It is important to note that by controlling for expenditures per capita, the regressions already control for the differences in the income effect on consumption and so to a certain extent for the differences in the value of the two types of transfers. Therefore, it will not be necessary to use the value regression specification unless we believe that the size of the compositional effects of the programmes is proportional to the size of their income effects. In this case, the size of the transfers would matter. We will run the value specification for all three sets of outcomes for robustness check.

6.1.6 Estimating the Basic Specification

To simplify the exposition we rewrite equation 5 in another form stacking all of the independent variables $InKind_{i,2,t}$, $Cash_{i,3,t}$, x_{it}, and the intercept together to form a single $(1 \times K)$ vector, where δ is a $(K \times 1)$ vector of corresponding coefficients. The number of parameters in X_{it} and δ are equal to $K = k + 3$, where k is the number of parameters in x_{it} and γ is as defined earlier[18].

$$y_{it} \; = \; X_{it}\delta \; + c_i \; + \; \mu_{it} \tag{6}$$

Equation 6 decomposes the original error term from equation 5 in to $\epsilon_{it} \; = c_i \; + \; \mu_{it}$, where c_i is the unobserved household-specific time-invariant component, and μ_{it} is the unobserved household-specific time varying component. Equation 6 cannot be estimated directly, as the number of household fixed effects c_i increases with the number of sample size N. The estimation technique used to identify the estimates of interest therefore rest on how we deal with the household fixed effect.

The most basic estimation with panel data is the pooled ordinary least squares (OLS), which treats each observation as an independent unit ignoring the panel feature of the data. The pooled OLS regression will give consistent estimates of β_{InKind} and β_{Cash}, provided that the assumption $Cov[\epsilon_{it}\,, X_{it}] = 0$ holds as either $N \to \infty$ or $T \to \infty$. We estimate the pooled OLS regressions for all three outcomes set out previously. Given the assumption of exogenous regressors, the pooled OLS estimator $\hat{\beta}^{POLS}_{InKind}$ and $\hat{\beta}^{POLS}_{Cash}$ will give consistent estimates of ATT^K and ATT^Cdiscussed in section 5 respectively[19].

6.1.7 Problems with the Basic Specification

We now look at the validity of the crucial assumption made to obtain consistent estimators of β_{inkind} and β_{cash}. The assumption $Cov[\epsilon_{it}\,, X_{it}] = 0$ can be rewritten as $Cov[c_i + \mu_{it}\,, X_{it}] = 0$. To obtain a consistent estimate, it is crucial that the included regressors (especially $InKind_{i,2,t}$ and $Cash_{i,3,t}$) are not correlated with either the household fixed effect or the household-specific error term. However,

[16]Some households will be assign with a hypothetical 20kg of wheat or 125BDT if the actual quantity/value reported in the questionnaire exceeds 15kg or 100BDT. These are what the FFE/PES programmes promised to the household who send more than one child to school.

[17]This is valid as long as household size is not endogenous to programme availability, which it is not.

[18]More precisely $X_{it} = (1, InKind_{i,2,t}, Cash_{1,3,t}, x_{it})$ and $\delta' = (\alpha, \beta_{InKind}, \beta_{Cash}, \gamma)$

[19]In fact, β_{InKind} and β_{Cash} identify a variance weighted average $ATT^K$and ATT^C respectively as supposed to the unconditional $ATT^K$and ATT^C.

due to the nature of both the FFE and PES programmes, there are significant differences between the characteristics of beneficiary and non-beneficiary households. We have controlled for some of them but it is possible that there are other characteristics that we could not control for, such as household preferences for food consumption, which are correlated with the beneficiary status and also the outcomes of interest. These characteristics are unobserved, but are arguably fixed over time for each household, and are captured by c_i in equation 6. As a result, we have that $Cov[c_i,\, X_{it}] \neq 0$, and hence $Cov[\epsilon_{it}\,, X_{it}] \neq 0$. Failing to account for this correlation will lead to inconsistent estimation of the parameter of interest.

Although we may be able to control for the observable and time-invariant unobservable household characteristics, the assumption of strict exogeneity may still be violated. This occurs when the programme treatments are endogenous. The FFE programme has been intentionally placed in a relatively poorer region, both programmes also target poverty and suffer from issues of selection into treatment. In addition, the current data set also posed a problem with the determination of the actual beneficiary of the programme as discussed in Section 4, which results in measurement errors. For these reasons, it is very likely that the programme treatment variable will also be correlated with the unobserved error term μ_{it} and, hence, $Cov[\epsilon_{it}\,, X_{it}] \neq 0$. Ignoring this will also cause the estimates of interest to be inconsistent.

To deal with both problems, we designed a framework using both households fixed effects and instrumental variables. Due to the panel structure of the data set, it will be possible to control for the correlation between the unobserved household fixed effect and the regressor of interest. Using valid instruments will also solve the problem of endogeneity in programme treatment. Both methods are discussed in detail in the next two subsections.

6.2 Fixed Effect Estimation

As mentioned in the previous subsection, the correlation between unobserved household fixed effect and the regressor (especially the treatment) would lead to estimates of the programme effect being inconsistent. To solve this problem, we make use of the panel structure of the data, which allows the transformation of the basic specification that will help control for this correlation.

Here we will move on from the basic specification in Section 6.1 and make use of the fixed effect (or "within") model. The model estimates a transformed version of the basic equation 6. The transformed model removes the household fixed effect, which is thus treated as a nuisance parameter. The coefficient vector is then estimated by performing OLS on the transformed equation. The transformed equation can be written as follows:

$$y_{it} - \bar{y}_i = (X_{it} - \bar{X}_i)\delta + (\mu_{it} - \bar{\mu}_i) \tag{7}$$

for $i = 1, ..., N$ and $t = 1, ..., T$ where $\bar{y}_i = \sum_{t=1}^{T} y_{it}$, $\bar{X}_i = \sum_{t=1}^{T} X_{it}$, and $\bar{\mu}_i = \sum_{t=1}^{T} \mu_{it}$. As $\bar{c}_i = \sum_{t=1}^{T} c_i = c_i$, the fixed effect model transformation removed the individual household fixed effect parameter c_i. In addition to removing the individual household fixed effect, the transformation in equation 7 will also remove all time-invariant observable characteristics. As a result, coefficients on time-invariant regressors such as sex or adult education cannot be estimated in contrast to the pooled OLS perform on the basic equation 6.

The fixed effect estimator will be consistent as either $N \to \infty$ or $T \to \infty$ and $E[\mu_{it} - \bar{\mu}_i | X_{it} - \bar{X}_i] = 0$. Due to the presence of the averages $\bar{X}_i$ and $\bar{\mu}_i$, this condition is stronger than $E[\mu_{it}|X_{it}] = 0$ required

for consistency of pooled OLS estimators. A sufficient condition for $E[\mu_{it} - \bar{\mu}_i | X_{it} - \bar{X}_i] = 0$ is the strong exogeneity condition that $E[\mu_{it} | X_{i1}, ..., X_{iT}] = 0$, which means that the present error term cannot be correlated with past, present or future regressors. This precludes a fixed effect (within) estimation with lagged endogenous variables as regressors.

Assuming that strong exogeneity holds, estimating equation 7 will help solve the problem of endogeneity in the treatment. Given that none of the households in this study moved between unions, the fixed effect model also deals with endogeneity that is a direct result of the non-random placement of the two programmes. The fix effect estimators $\hat{\beta}^{FE}_{InKind}$ and $\hat{\beta}^{FE}_{Cash}$ provide consistent estimates for $ATT^K$and ATT^C in this case.

Another possible method to control for household fixed effect is by using the first-differences estimator. This is obtained by subtracting one period lagged equation from the original equation 6. This approach is less efficient than the fixed effect estimator if μ_{it} is iid. However, it does have advantages when we allow for the endogenous regressor. We will discuss in more details in the next section why this estimator is not a plausible one in this context.

6.3 Instrumental Variable Approach

The specification in the previous section helps control for unobserved household fixed effect. However, in the case where the included regressors (especially the treatment status regressor) are still endogenous, possibly due to measurement error or selection into treatment based on time-varying unobservables, the fixed effect estimator $\hat{\beta}^{FE}_{InKind}$ and $\hat{\beta}^{FE}_{Cash}$would still be inconsistent.

Participation in both programmes is affected by selection. There are two types of selection going on here, as discussed in the programme description in Section 3. First, an individual could self-select whether they want to participate in the programme by making a decision whether to enrol their child in school or not, and, if they do, whether to enrol in the school that participates in the programmes or not. Households may choose not to send their children to school for many reasons, including that the opportunity cost of enrolment may be too great as children are able to work otherwise. Households may choose to not enrol their children at a programme school, such a school could be of lower quality or too costly to travel to.

Secondly, if the programme is oversubscribed in some schools (i.e. over 40 percent of all its students are eligible), the school committee is responsible for choosing who will get the benefit and who does not. As there were no strict guidelines for selection of eligible pupils in this case, selection decisions are at the discretion of the school committee, which could be influenced by wealthier and more powerful parents. Contaminated by these selection effects, this beneficiary status will be correlated with the error term and is no longer an ideal regressor.

Another problem is measurement error, which in this case refers to the problems in identifying actual beneficiaries of both the FFE and PES programmes due to inconsistencies within the questionnaire (as discussed in Section 4). In the value regression, it is also very likely that the self-reported value of transfer received are reported with errors as well. Even if we have taken care of the fixed effect, the inconsistency of $\hat{\beta}^{FE}_{InKind}$ and $\hat{\beta}^{FE}_{Cash}$ due to measurement error would still remains.

IVs can help solve these problems as a result of selection bias and measurement errors. Choosing a valid instrument in a panel data setting is different to the same action in a cross-sectional setting and it depends heavily on the estimator used. If we use the first-differences estimator, we could use

an internal instrument in the form of lagged value or lagged difference. The difference-GMM and the System-GMM estimator, proposed by Arellano and Bond (1991) and Blundell and Bond (1998) respectively, make use of these instruments to correct for endogeneity in the first-differences equation. This requires that we have at least three years of data. Even though this condition is met, we only have one year of data from when the FFE programme was active and only two years of data for the PES programme. Given that it is the impact of both programmes relative to each other that we are interested in, we need to have both as a separate regressor ($InKind_{i,2,t}$ and $Cash_{i,3,t}$). This means that we will not have a sufficient set of lagged values to act as instruments. In addition, the first-differences coefficient of treatment effect would not allow the separation of in-kind and cash programme as we want, and all these reasons together are why we cannot use the first-differences specification.

The fixed effect (within) estimator discussed earlier is, therefore, the preferred estimator in this paper. However, the nature of the transformation as in equation 7 means that for an instrument Z_{it} to be a valid instrument it must satisfy the exogeneity assumption that $E(Z_{it},\, \tilde{\mu}_{it}) = 0$, where $\tilde{\mu}_{it} = \mu_{it} - \bar{\mu}_i$. This requires stronger condition of strong exogeneity $E(Z_{is},\, \mu_{it}) = 0\, for\, all\, t$, which means that the instrument cannot be correlated with any past, present or future period error. This is all due to the presence of $\bar{\mu}_i$ in the transformed error term. Therefore, the within transformation can only be used when the instrument is strongly exogenous.

We suspect that there are only two endogenous regressors, those indicating the beneficiary (treatment) status in both FFE and PES programme. Equation 8 shows the fixed effect IV specification used to obtain a consistent estimate of β_{InKind} and β_{Cash}; $\widetilde{y}_{it} = (y_{it} - \bar{y}_i)$ with $\tilde{e}_{i,FFE,t}$, $\tilde{e}_{i,PES,t}$, $\tilde{x}_{i,t}$, and $\tilde{\mu}_{i,t}$ defined in the same way representing the within transformation. The household fixed effect c_i has already been removed by the within transformation. The instrument used in this fixed effect IV regression is the interaction between the eligibility status of the household in both programmes, regardless of whether the programme was available or not ($e_{i,FFE,t}$ and $e_{i,PES,t}$) and an indicator of programme availability ($b_{i,FFE,t}$, $b_{i,PES,t}$). The creation of these variables was discussed in Section 4. The instruments $(e_{i,FFE,t} \cdot b_{i,FFE,t})$ and $(e_{i,PES,t} \cdot b_{i,PES,t})$ provide an alternative way to measure the beneficiaries of the programme. By strictly following the eligibility rule provided by the programme and identifying the eligibles, we managed to circumvent the selection problems previously mentioned, provided that the eligibility rule is exogenous. The focus is thus on measuring the impact of eligibility rather than participation (Morduch 1998).

$$\widetilde{y}_{it} = (e_{i,FFE,t} \cdot b_{i,FFE,t})\beta_{InKind} + (e_{i,PES,t} \cdot b_{i,PES,t})\beta_{Cash} + \tilde{e}_{i,FFE,t}\beta_3 + \tilde{e}_{i,PES,t}\beta_4 + \widetilde{x}_{i,t}\gamma + \widetilde{\mu}_{it} \quad (8)$$

Given the poverty-related nature of the eligibility rule, at first it seems unlikely that the instruments based on it will be exogenous. However, given that we have controls for many relevant household observables and fixed unobservable characteristics, the eligibility variable should now be exogenous from the transformed error term. We are making the assumption that both of the instruments are strongly exogenous conditional on $\tilde{X}_{it}$, i.e. $E[(e_{i,j,t} \cdot b_{i,j,t}),\, \tilde{\mu}_{it} | \tilde{X}_{it}] = 0$ where $j = FFE\, or\, PES$. These assumptions corresponds to those made in subsection 5.2.

There will only be a reason to believe that the instrument is not exogenous if households respond to the availability of both programmes and try to change their eligibility status as a result. Recall the four and five eligibility rules of the FFE and PES programmes respectively (see Section 3). It is

very unlikely that households would or could change their land holding or become a female-headed household (through divorce, or death of husband) in order to be eligible for a welfare programme. There might be more concerns that heads of households may change their occupations, or manage to get the official to misreport their occupation in order to change the eligibility status. If households do manipulate their eligibility status in response to the programme, this would lead to a potential problem, but only if the selectivity were linked to factors affecting outcomes of interest. However, given the small size of the transfer (which only represents around 5% of household expenditure on average), it is very unlikely that this will happen for enough people to cause the instrument to become endogenous and lose its validity entirely.

If all the assumptions stated above hold together with the monotonicity assumption, the fixed effect IV estimator, $\hat{\beta}^{FE-IV}_{InKind}$ and $\hat{\beta}^{FE-IV}_{Cash}$, will give consistent estimates of $LATE^K$ and $LATE^C$ discussed in section 5. Although, these parameters may be less externally valid than ATT^K and ATT^C, given by the pooled OLS and fixed effect estimator (if the required assumptions hold), they are the parameters that can be estimated with more credibility and more plausible assumptions needed for consistency than both the pooled OLS ($\hat{\beta}^{POLS}_{InKind}$ and $\hat{\beta}^{POLS}_{Cash}$) and the fixed effect estimator ($\hat{\beta}^{FE}_{InKind}$ and $\hat{\beta}^{FE}_{Cash}$). This makes the fixed effect IV estimator the most internally valid one. Here, due to data limitations, we sacrifice some degrees of external validity in favour of higher degree of internal validity. A household fixed effect IV regression will therefore be our preferred specification. We will test the result of this specification with the pooled OLS of the basic specification, regression with just the fixed effect estimator, and regression with just the IV estimator.

6.4 Standard Errors

The default assumption for all the estimators above is that the random error term μ_{it} is independent and identically distributed, with a mean of zero and a homoskedastic variance $\mu_{it} \sim iid\left[0\,,\,\sigma^2_\mu\right]$. Under this assumption, all the estimators discussed in the previous section will provide correct standard errors and, provided that we have consistent estimates, will allow us to conduct correct statistical inferences. It is reasonable to assume independence over individual i, but in a panel data setting the errors face two potential problems; heteroskedasticity and serial correlation .

Heteroskedasticity occurs when the variance is not constant across the observations; bias caused by heteroskedasticity could be in any direction. Using the White heteroskedastic consistent estimator allows for correction of certain forms of heteroskedasticity, but it still fails to take account of the serial correlation. Serial correlation occurs when the errors are correlated over t for given individual i, $Cov\left[\mu_{i,t},\,\mu_{i,s}\right] > 0\ for\ t \neq s$. Ignoring this serial correlation can lead to greatly underestimated standard errors, which lead in turn to over-estimated t-statistics. Controlling for fixed individual-specific effects can reduce serial correlation in the composite error term $\epsilon_{i,t}$, but it may not be completely eliminated.

Cluster-robust standard errors as proposed by Arellano (1987) are used for all the estimation set out in previous sections to account for both heteroskedasticity and serial correlation. The individual household has been chosen as the clustering unit, which is the appropriate choice here, given that we are looking for variations at the household level.

6.5 Attrition

As set out in Section 4, 25 households were lost from an original sample of 480 household due to attrition. This is an attrition rate of approximately 5.2 percent, which is relatively low in comparison to similar studies in low-income countries. However, non-random attrition, where households with certain characteristics which are correlated with the outcome of interest systematically leave the survey, will cause bias in the estimates.

To test for randomness of attrition, we run the attrition probit (Fitzgerald et al, 1998), in which the dependent variables take the value of one for households who had left the sample any time after the first wave and zero otherwise. Explanatory variables are baseline values for all variables that are believed to have affected the outcome of interest. Significance in some of the regressors may suggest that attrition is non-random. The attrition probit result is presented in Table 13 in Appendix A.2. Age of household head is the only important variable that explains attrition, with households with younger head more likely to leave the sample. This could be for many reasons, including that the younger heads are more likely to migrate to find a job. All other household characteristics strongly related to food consumption and health status of young children such as household size, household composition, level of education and income (proxy by asset and expenditure) are not significant predictors of attrition.

There are methods to correct for attrition bias depending on the assumption regarding the nature of attrition. If attrition occurs as a result of selection on observables, we can use the method of inverse probability weight (IPW) (Wooldridge, 2002) to correct for it. If attrition occurs as a result of selection on unobservables, a less restrictive assumption, an exclusion restriction which involves an exogenous instrument that is correlated with attrition but not the outcome of interest is needed. A common instrument for this is normally the interviewer's characteristics. Given that this information is not available, it is not possible to perform a correction for attrition based on selection on unobservables. IPW makes strong assumptions, which may create more noise without much gain. Given a small attrition rate and the evidence from the attrition probit, we concluded that attrition bias is small and it was therefore ignored.

7 Empirical Results

7.1 Effect on Consumption

Household calorie consumption is the starting point of comparison. As the theoretical model in Section 2 predicted, households who face extra-marginal and binding in-kind transfers will end up having to consume more of that transfer than they would have liked to do under an equal-valued cash transfer and this may shows up as higher calorie consumption.

Table 9 presents the impact of the FFE and PES programmes on household calorie consumption. The dependent variable in all specifications in Table 9 is the daily *per capita* calorie consumption by household measured in kilocalories. In regressions (1) to (4) we do not scale the impacts by the value of subsidies under the FFE and PES programmes. We relax this and allow for the difference in the size of transfers between the two programmes in the value regressions (5) and (6) to check for robustness of the results.

Results for pooled OLS regression on the basic specification set out in equation 5 is presented in Table 9, column (1). Households under the in-kind programme consume significantly more calories

Table 9: Daily Per Capita Calorie Consumption Regression

	(1)	(2)	(3)	(4)	(5)	(6)
Calorie per capita	Pooled	FE	IV	FE-IV	FE-IV	FE-IV
In-Kind	**107.164****	**11.145**	**160.766***	**50.361**		
	(48.878)	(57.690)	(97.425)	(196.518)		
Cash	**-7.685**	**-98.313****	**-582.695***	**-401.224**		
	(43.692)	(42.154)	(319.649)	(475.887)		
PC value FFE					**1.336**	
					(4.932)	
PC value PES					**-15.334**	
					(17.884)	
PC hypo.value FFE						**1.554**
						(4.012)
PC hypo.value PES						**-13.158**
						(15.416)
Log household size	-228.106**	-365.940	-174.509*	-359.075	-407.131*	-408.785*
	(92.100)	(249.467)	(97.641)	(243.691)	(228.881)	(228.850)
Log pc expenditure	1117.606***	1291.465***	1081.338***	1291.780***	1286.266***	1288.849***
	(86.275)	(108.747)	(89.900)	(108.904)	(107.852)	(106.645)
Children 0-4	-521.133**	309.461	-485.110**	345.643	361.953	362.495
	(216.857)	(662.454)	(225.790)	(649.500)	(628.388)	(629.239)
Children 5-9	-312.837*	262.960	-175.631	326.326	310.129	306.787
	(185.124)	(600.551)	(208.953)	(581.411)	(567.331)	(570.713)
Children 10-14	131.725	617.867	301.730	767.044	721.296	744.206
	(159.292)	(585.073)	(193.653)	(551.453)	(546.907)	(545.896)
Adult 15-54	252.149**	611.094	247.198*	687.883	660.598	669.392
	(128.012)	(530.142)	(129.345)	(506.085)	(506.328)	(502.712)
Wheat price	-16.213*	3.133	-16.714	4.863	1.490	2.167
	(9.523)	(10.825)	(10.353)	(11.054)	(10.792)	(10.781)
Rice price	-88.155***	-40.183	-89.028***	-43.526	-41.992	-39.396
	(13.758)	(26.824)	(14.590)	(30.511)	(28.786)	(28.228)
Eligible for FFE	36.904	204.369	138.923	271.751	258.212	258.964
	(149.743)	(223.679)	(174.947)	(274.491)	(263.426)	(265.798)
Eligible for PES	-79.412	-241.460	-125.693	-277.623	-269.548	-275.938
	(152.088)	(218.543)	(170.696)	(245.972)	(235.019)	(238.941)
year==2003	261.292***	115.324	551.042***	261.400	241.408	224.342
	(58.038)	(78.798)	(162.057)	(204.477)	(173.356)	(157.832)
year==2006	401.089***	-144.413	669.204***	-22.261	-50.207	-62.970
	(152.434)	(217.186)	(218.172)	(318.834)	(260.865)	(255.731)
Child enrolment	-91.784	105.928	223.593	274.896	223.228	213.673
	(150.071)	(159.288)	(261.897)	(393.188)	(335.124)	(325.952)
Constant	-3581.252***	-5699.823***	-3611.700***			
	(544.539)	(870.026)	(558.655)			
Observations	1365	1365	1365	1365	1365	1365
InKind=Cash	**0.057****	**0.129**	**0.012****	**0.186**	**0.244**	**0.247**

Robust-Clustered standard error in parentheses; main statistics for InKind=Cash are the p-values
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

than households who received no benefit, the omitted category in this case. The coefficient for the cash programme is negative, implying that a beneficiary household of the cash programme consumes less food than a household who received no benefit. However, the coefficient is not statistically significant. $\hat{\beta}^{POLS}_{InKind}$ and $\hat{\beta}^{POLS}_{Cash}$ are the naive estimators of programmes effect which will give ATT^K and ATT^Conly when both can be estimated consistently. Simple t-test of equality between in-kind and cash indicates that the two programmes are statistically different in their effects on household calorie consumption. This is the first evidence that there may be a constraint on the consumption behaviour of households when transfers are given in-kind.

The coefficients on other regressors included as control variables have reasonable interpretations in the pooled OLS setting. An increase in household size results in lower per capita calorie consumption. The wealthier the household (measured by higher real per capita expenditure), the higher their calorie consumption (holding other factors constant). Both household size and income are significant in explaining calorie consumption. The composition of households also plays a major role, with children eating less calories while young and middle age adults eating more calories relative to the omitted categories (i.e. adults of the age over 55 years). The rice price is also significant in explaining calorie consumption, which mainly stems from the fact that rice is a staple and makes up a major part of the household diet (as shown in Table 6). The indicator variables for 2003 and 2006 also show positive signs, suggesting that households in this time period tend to consume more calories than households in the year 2000.

However, as previously discussed, there are numerous problems with the endogeneity of the regressors in the basic specification estimated by pooled OLS which could all cause inconsistencies in the pooled OLS estimates. As explored earlier, these problems can be corrected for using the fixed effect (within) estimator that exploits the panel structure of the data and an IV approach. Both methods are attempted separately and then together as the FE-IV estimator. Regression (2) in Table 9 shows the result of the within estimation as shown in equation 7 in Section 6. In the fixed effect estimation, coefficients on both in-kind and cash still have the same sign as the pooled OLS estimates, but both decrease in magnitude. This is consistent with the selection into treatment based on household unobservable characteristics, which could result in an upward bias in the pooled OLS estimates. The coefficient for the in-kind programme loses significance, while the coefficient for the cash programme is now more negative and statistically significant, suggesting that households under the cash programme consume less calories than non-beneficiaries. However, the t-test for difference between in-kind and cash transfer is no longer significant. This suggests that, when we allow for the household fixed effect, the difference between the two programmes disappears. It could be that eligible households that select themselves for the food transfer are those who have a preference for food consumption and are therefore likely to benefit the most from food transfers. $\hat{\beta}^{FE}_{InKind}$ and $\hat{\beta}^{FE}_{Cash}$ here are consistent estimators of ATT^K and ATT^Crespectively when selection bias are caused by observable $X_{i,t}$ and time-invariant unobservables c_i only which, as argued earlier, is not the case in this setting.

The IV specification in regression (3) provides different results to the fixed effects specification. In comparison to the pooled OLS regression (1), the coefficients of both the in-kind and cash programmes increase in magnitude, but in an opposite direction. The coefficient for the in-kind programme is more positive while the coefficient of the cash programme is more negative with both coefficients becoming statistically significant. The t-test for differences between the in-kind and cash programmes is significant once more. The standard error on both estimates increased, which is a usual characteristic

of the IV specification. $\hat{\beta}^{IV}_{InKind}$ and $\hat{\beta}^{IV}_{Cash}$ are estimators of $LATE^K$ and $LATE^C$. Both are only consistent if the instruments are relevant and exogenous[20]. Applying the Kleibergen and Paap rk statistic (a generalisation of the Cragg-Donald Wald statistic) to the case where the errors are not iid, and comparing it to the Stock-Yogo critical value (Stock and Yogo, 2005), we were able to reject the null hypothesis that the instrument is weak. As the IV regression in this case is exactly-identified, it is not possible to perform an over-identifying restriction test to look for instrument exogeneity[21].

As discussed earlier, it is possible for the instrument to fail the exogeneity assumption without controlling for the unobserved household fixed effect. Despite the instrument being calculated from the exogenous eligibility rule that households have no control over, the poverty-targeting nature of the programme means that these eligibility criteria will be correlated with many of the poor household characteristics. In this case, the instruments not only act through the endogenous variable but also directly affect the outcome as well. Depending on the nature of the correlation of the instruments with the unobserved household fixed effect, the IV estimates could be positively or negatively biased.

The FE-IV is used to solve the above problem. By controlling for all the observable and unobservable fixed effects, the exogeneity assumption of the IV can arguably be satisfied. The FE-IV, which is the preferred specification, should give consistent estimates of both in-kind and cash programme variables.$\hat{\beta}^{FE-IV}_{InKind}$ and $\hat{\beta}^{FE-IV}_{Cash}$ here are consistent estimators of $LATE^K$ and $LATE^C$. However, due to the fact that the fixed effect estimation requires a transformation of the original equation, it could potentially reduce the first stage correlation between the instrument and the transformed regressor, resulting in a problem of weak instruments. However, the Kleibergen and Paap rk statistics reject the null hypothesis that the instrument is weak. From regression (4) in Table 9, the coefficient of the in-kind programme decreases in magnitude, becoming less positive, and has lost its significance when compared to the pooled OLS result. The coefficient on the cash transfer is more negative than with the pooled OLS estimate and also is not statistically significant. More importantly, with the preferred FE-IV estimation, the t-test still found no difference between the two programmes.

As mentioned in Section 6, the size of transfers under both programmes are not equal, with the cash programme receiving both less stipend and also facing the effect of rising inflation eroding the value of the transfer. If the compositional effects of the programmes were proportional to the size of the transfers, our comparison of the two programmes' estimates in regression (4) would be incorrect. To solve this, we use the transfer value of each programme as regressors instead of the indicator variable used in regressions (1) to (4). Regression (5) uses the actual values received by household, while regression (6) uses the hypothetical value that the household should receive based on the promise of the programmes. Both regressions were estimated using the FE-IV approach and both provide similar results to regression (4) with positive in-kind and negative cash coefficients. Neither coefficient is statistically different from zero, and again the t-test for the test of difference between the two programmes fails to reject the null hypothesis that there are no difference in both regressions. The first stage result for regression (3) to (6) in table 9 are shown in table 14 in the appendix A.3.

Apart from the pooled OLS and IV regression, which both suffer from endogeneity problems, there is no statistical difference between the level of daily *per capita* consumption of calories among

[20] In addition, we also need the monotonicity assumption for the identification of $LATE$ as well.

[21] The critical values due to Stock and Yogo were only originally computed for using with the Cragg-Donald Wald statistics where the error are assume to be iid. However, there are no other critical values computed for non-iid error as is the case here. Using this critical value however, is still informative and is better than nothing at all but the test cannot be taken to be 100% accurate.

beneficiaries of the in-kind and cash transfer programmes. Even when the difference in the value is taken in to account, both programmes still have statistically the same effect on food consumption. The lack of difference between the two programmes on calorie consumption is robust to different specifications.

There are three possible explanations for these results. The first is the issue mentioned in Section 6.1.2 regarding income and the compositional effect of the transfer. By only looking at the compositional effect of the transfer, we can conclude that both in-kind and cash programmes do not have any effect on the calorie composition of beneficiary compared to non-beneficiary households, and that the compositional change between the two programmes was not significant either. This means that, while either programme could have led to increases in overall consumption of calories as a result of an income effect, neither have led to compositional changes in spending that shift calorie consumption enough to be identified as statistically significant from non-beneficiary households and each other. This explanation helps to reconcile the small and even negative coefficient observed for households under the cash programme, but it still fails to explain why there is no difference in effect between the two types of transfer from each other.

A second explanation for the lack of difference between the two programmes both from non-beneficiaries and each other would be the small size of the transfer given out in both programmes. On average, the in-kind and cash transfers are only approximately 5% of household total expenditure. Though the programme may have an impact, and outcomes under cash and in-kind transfers could be significantly different, we were unable to observe such an outcome, as the initial transfers to households were too small to have a noticeable impact. Lastly, we cannot rule out the possibility that there is really no difference between the two programmes, and, hence the two types of transfer. Nevertheless, an inspection of the direction of the coefficient suggests that in-kind beneficiary households shift more of their consumption towards food, hence the positive coefficient in comparison to the cash programme, where households shift more of their consumption away from food resulting in the observed negative sign.

7.2 Effect on Expenditure Share

Despite the result above, the direction and magnitude of the coefficients suggest that the in-kind food transfer did lead beneficiary households to shift more of their consumption towards food than under the cash programme. It is interesting to investigate this further in order to determine where the opposing shift seen in two the programmes may have originated from and, if there is a constraint, why it did not show up more strongly in the overall food calorie consumption. This is done by looking deeper into various expenditures categories.

Table 10 shows the result of the regression of the impact of both the in-kind and cash programmes on various expenditure categories. The specification in all of the regressions in Table 10 is the preferred FE-IV regression. Similar to before, $\hat{\beta}^{FE-IV}_{InKind}$ and $\hat{\beta}^{FE-IV}_{Cash}$ here are consistent estimators of $LATE^K$ and $LATE^C$. The only difference is that the dependent variable is now the expenditure on various food, non-food items/categories, all computed as a **share of household total expenditure**. Though we do not report the value regressions, the results in this section are robust to using the value specification as with the calorie regressions earlier.

Only a few of the more relevant items and categories are presented in Table 10. Wheat and rice

Table 10: Expenditure Shares Regression

	(1) Wheat	(2) Rice	(3) Cereal	(4) Meat & Fish	(5) Fruit & Veg	(6) Food	(7) Cloth	(8) Education	(9) Kid	(10) Smoke
In-Kind	**0.056*****	**-0.030**	**0.018**	**-0.041**	**0.009**	**-0.011**	**0.039***	**0.010**	**0.030****	**-0.006**
	(0.008)	(0.026)	(0.027)	(0.030)	(0.021)	(0.042)	(0.023)	(0.010)	(0.013)	(0.008)
Cash	**-0.007**	**-0.025**	**-0.055**	**-0.085**	**-0.015**	**-0.137**	**0.133****	**0.047***	**0.078****	**-0.033***
	(0.021)	(0.061)	(0.064)	(0.077)	(0.052)	(0.106)	(0.065)	(0.027)	(0.037)	(0.017)
Log household size	0.001	-0.013	-0.004	0.039***	0.003	0.024	0.020**	-0.006	0.016***	0.001
	(0.003)	(0.012)	(0.013)	(0.014)	(0.010)	(0.017)	(0.010)	(0.007)	(0.006)	(0.003)
Log pc expenditure	-0.005*	-0.140***	-0.133***	0.106***	-0.003	-0.006	0.008	-0.004	0.003	-0.005***
	(0.003)	(0.008)	(0.008)	(0.012)	(0.008)	(0.017)	(0.009)	(0.004)	(0.004)	(0.002)
Children 0-4	0.007	0.027	0.013	0.003	-0.020	0.013	-0.026	-0.003	0.003	-0.007
	(0.014)	(0.046)	(0.049)	(0.049)	(0.036)	(0.060)	(0.038)	(0.021)	(0.022)	(0.011)
Children 5-9	0.003	0.031	0.004	0.075	-0.047	0.057	-0.022	-0.007	0.010	0.002
	(0.012)	(0.045)	(0.046)	(0.047)	(0.033)	(0.061)	(0.038)	(0.020)	(0.023)	(0.010)
Children 10-14	0.001	0.101**	0.082*	0.046	-0.024	0.087	-0.069*	0.023	-0.010	0.010
	(0.013)	(0.044)	(0.047)	(0.050)	(0.035)	(0.062)	(0.038)	(0.020)	(0.022)	(0.010)
Adult 15-54	0.002	0.113***	0.089**	-0.008	-0.029	0.041	-0.044*	0.024	-0.018	0.012*
	(0.009)	(0.034)	(0.037)	(0.038)	(0.028)	(0.045)	(0.026)	(0.017)	(0.015)	(0.007)
Wheat Price	-0.001	-0.001	-0.000	0.001	0.001	0.006**	-0.000	-0.000	-0.000	-0.001*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.000)
Rice price	0.003*	0.003	0.006	-0.013***	0.010***	0.009	-0.004	0.003*	-0.001	-0.001
	(0.002)	(0.004)	(0.004)	(0.005)	(0.003)	(0.006)	(0.004)	(0.002)	(0.002)	(0.001)
year==2003	0.005	-0.001	0.015	0.014	-0.010	-0.006	-0.049*	-0.024**	-0.018	0.013**
	(0.009)	(0.025)	(0.026)	(0.030)	(0.022)	(0.044)	(0.030)	(0.011)	(0.016)	(0.007)
year==2006	-0.003	-0.011	-0.026	0.039	-0.059*	-0.099	-0.021	-0.021	-0.005	0.020*
	(0.015)	(0.039)	(0.040)	(0.043)	(0.034)	(0.062)	(0.043)	(0.018)	(0.024)	(0.010)
Eligible for FFE	-0.006	0.035	0.044	0.063*	0.028	0.087*	-0.054**	-0.013	-0.033**	0.007
	(0.007)	(0.032)	(0.034)	(0.035)	(0.020)	(0.047)	(0.025)	(0.011)	(0.014)	(0.006)
Eligible for PES	0.007	-0.033	-0.035	-0.038	-0.026	-0.058	0.038*	0.008	0.024**	-0.007
	(0.006)	(0.029)	(0.031)	(0.031)	(0.017)	(0.038)	(0.020)	(0.009)	(0.012)	(0.005)
Child Enrolment	0.007	0.042	0.072	-0.016	0.024	0.058	-0.093*	-0.025	-0.051*	0.028**
	(0.016)	(0.050)	(0.051)	(0.060)	(0.044)	(0.078)	(0.048)	(0.020)	(0.028)	(0.013)
Observations	1365	1365	1365	1365	1365	1365	1365	1365	1365	1365
InKind=Cash	**0.002*****	**0.909**	**0.124**	**0.450**	**0.542**	**0.116**	**0.060***	**0.047****	**0.094***	**0.002*****

Robust-Clustered standard error in parentheses; main statistics for InKind=Cash are the p-values

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

share were selected, as the in-kind food transfers were given out in terms of these two items. Cereals, meat, fish, fruit and vegetables are the other food categories presented in Table 10; these are not an exhaustive list of categories but represent around 80% of all food expenditure and are sufficient for the analysis. The total food share is also presented, along with other non-food categories. Selected categories are clothing expenditure share for the household (adults and children), expenditure share on education, expenditure share on cigarette, and finally all expenditures on children as a share of total expenditure. It must be noted that expenditures on children's clothing are also a part of expenditure on children (though expenditures on education are not), but we chose to present the overall spending on children in addition to the clothing expenditure so as to illustrate a possible intra-household allocation story.

From Table 10 it is evident that the FFE programme leads to more consumption of wheat than the PES programme, with beneficiaries who received the in-kind food transfer ending up with a wheat share 5.6% higher than households who received no benefits. The wheat share fell for beneficiaries of the cash programme, although this fall is not statistically significant. The t-test for the difference between the two programmes is highly significant. The findings reflect the fact that the wheat transfer was extra-marginal and binding, and beneficiaries of the in-kind programme are constrained to consume more wheat than they would have chosen to do under an equal-valued cash transfer. This provides clear evidence of a constraint on the composition of household food expenditure.

The rice share is lower for beneficiary households under both programmes when compared to those households that received no benefits. Both coefficients were not statistically significant and the t-test of the difference found that there are no differences between programmes. This either indicates that most in-kind rice transfers were infra-marginal or the fact that households in this round of the survey received most of their benefits in wheat rather than rice. The coefficient for the FFE programme provides an interesting story. The fall in rice share is bigger for the in-kind programme in comparison to the cash programme, even though some of the in-kind food transfers were supposed to be given out in rice. Regression (2) confirms the findings in the survey that most of the in-kind food transfer was given out in the form of wheat and that, in response to the constraint that households faced in higher wheat consumption, they substitute away from rice consumption. This type of substitution is predicted by the theory of rationing (Neary and Roberts 1980; Deaton 1981).

This substitution story is confirmed in regression (3), when we look at expenditure on cereal share. Both rice and wheat fall under the cereal category and in fact make up around 92% of households' cereal consumption on average. Though the coefficient of the in-kind programme is positive, suggesting that in-kind beneficiary households did consume more cereal than non-beneficiary households, the increase was not statistically significant. This indicates that beneficiary households were able to substitute away from rice in favour of wheat to leave themselves less constrained overall. Therefore, we cannot find the impact of the in-kind programme on calorie consumption despite the fact that the wheat transfer did constrain people to consume more wheat. Households under the cash programme choose to consume less cereal than non-beneficiary households but again the difference between the two programmes is not statistically significant, which strengthens our argument that in-kind beneficiary households were able to adjust their behaviour and substitute effectively. It is important to note that, while the constraint in wheat consumption caused by the FFE programme is clear, wheat on average represents only a small part of households' total expenditure. Consequently, it could easily be offset by household-level adjustments intended to decrease other parts of their food expenditure in order to free themselves from

the constraint.

The next two regressions show the result of both programmes' impact on consumption of meat and fish and fruit and vegetables, which are the other major parts of households' food consumption. The findings on both categories are similar in that none of the coefficients of the in-kind and the cash programmes are individually significant, although it is interesting to note that the magnitude of the coefficient on the meat and fish categories is large and negative. A lower consumption of meat and fish could provide further evidence of the substitution within household consumption, where in-kind transfer beneficiary households consume less meat and fish in favour of more cereal. The t-test for the difference between the in-kind and the cash transfer programmes fails to reject the null hypothesis that there are no differences between the two transfers for both categories of food. Given that the categories of food presented in this table made up the majority of households' food consumption, we can safely conclude that, while they may be evidence of some constraints as a result of the FFE programme, households were able to shift their consumption among food items around so that the overall constraint is minimal.

Regression (5) shows the results for food share. Given the substitution argument earlier, it is not surprising that the coefficient of the in-kind variable is both close to zero and statistically insignificant. The effect of the cash programme on overall food share is also not statistically significant. However, the size of the coefficient suggests that cash beneficiary households have around a 12% lower food share than non-beneficiaries, a relatively large magnitude especially when compared to 1.1% for the in-kind beneficiary households. Though the t-test of differences between the two programmes cannot reject the null hypothesis that they are the same, there are some indications that households under the cash programme choose to spend less on food than households under the in-kind transfer programme. This provides some evidence that, once free from the constraint of the in-kind transfer, households choose to spend their money differently, allocating more of their funds towards non-food items.

If the cash-receiving households were free from the constraint faced by the in-kind households, and choose to spend less on food, it is important for policymakers to know what they choose to spend the extra money on. Paternalistic fears would be confirmed if these households choose to spend the extra cash on goods deemed to be undesirable by society, such as cigarettes or alcohol. However, if households in fact spend their extra money positively on goods that are deemed beneficial (such as children's education), then paternalistic fears would be unjustified.

Another important issue in the planning of redistribution programmes is the issue concerning the indirect targeting of a specific group of the population, such as young children. Transfers that are aimed at children are often given through parents and as a result it is possible that, if given in cash, such transfers could be diverted by parents to spend on things that do not improve children's welfare. As a result, a paternalistic government would provide the benefits in-kind by making the transfer conditional or by giving out the benefits in in-kind goods that benefit the children directly, such as school uniforms, school lunch or food. Both the FFE and PES programmes are conditional on children's education and, hence, aim to improve the welfare of young children in Bangladesh. It is therefore also important to determine whether the benefits from both programmes are being sufficiently passed on to young children whose welfare, apart from school enrolment, is only indirectly targeted.

Regression (7) looks at the impact of both transfer programmes on households' spending on clothing. Both In-kind and cash beneficiary households are found to have spent more on clothes than non-beneficiaries. However, the t-test of the difference between the two coefficients rejects the null

hypothesis and shows that there are significant differences between the two programmes at the 10% level with cash programme households spending larger share of their income on cloth than in-kind households. Regression (8) looks at the impact of both transfer programmes on households' spending on children's education. Beneficiary households from the in-kind programme spend no more on children education than household who received no benefits at all. On the contrary, cash beneficiary households spend more on children education and this is marginally significant. More importantly, the t-test of the difference between the two coefficients found that there is significance different between the two programmes at the 5% level.

In addition, regression (9) also provides evidence that counters paternalistic fears concerning transfer programmes in general; the indirect targeting of children. Both in-kind and cash programme beneficiaries were found to have spent more on children than non-beneficiary households. This finding provides evidence for the "flypaper" effect (Jacoby 2002), where if a benefit is aimed at a specific member of the household, the benefits seem to stick to that member even though the household could have reallocated the benefits across all members. In this case, as the transfers were only obtainable as a result of children attending school, parents might view the benefits received as a transfer to their children and therefore ended up spending more on their children. The effect is different between the two programmes with cash beneficiary households able to spend more on children, but only marginally significant. This should eliminate the paternalistic fear regarding indirect transfers to children possibly being reallocated away under cash transfers, though we must remember that even though the mode of the transfer is different, both are conditional transfer programmes and are both technically in-kind, so we cannot draw the conclusion here too strongly.

Regression (10) shows that cash beneficiary households spend statistically less on cigarettes than households that received no benefits, while in-kind beneficiary households spent no differently on smoking than non-beneficiary households. The t-test found that there is a difference between programmes. It is evident that cash beneficiary households spend less on smoking than in-kind beneficiary households.

It is possible to drawn from these results a conclusion that cash transfer beneficiary households who were not under any constraints were able to spend their extra money directly on children's education or useful personal items, such as clothes while spending no more on vices. This should ease the paternalistic fear that poor households make bad decisions once given the option of spending freely. Households under in-kind programme however, are already facing constrain with in-kind goods (wheat), and can only manage to substitute between food categories to a certain extent. As a result, they were not able to generate enough leftover cash to spend on non-food items as much as cash beneficiary households can.

7.3 Effect on Health Status

In the last section, we established that in-kind transfer lead to a constraint in the behaviour of the recipient. Households under the cash transfer were able to spend more on clothes and children's non-food consumption. In this section, we now look at how these constraints in the behaviour may have an effect on household welfare.

One measure of household welfare is the health status of the members of households. We look specifically at two measures of health status of young children between the ages 0 to 12 years old;

proportion of stunted and underweight children within a household. Anthropometric measures were taken only in the 2000 and 2006 rounds, so only data from these two rounds is used in the regressions. In addition, we can only investigate these effects for households who have children age 0 to 12 in both rounds of the survey. The overall sample sizes are therefore reduced from 1365 to 632 observations. Again, the specification in all of the regressions in Table 11 is the preferred FE-IV regression, $\hat{\beta}_{InKind}^{FE-IV}$ and $\hat{\beta}_{Cash}^{FE-IV}$ here are consistent estimators of $LATE^K$ and $LATE^C$ on health outcomes.

Regression (1) in Table 11 shows the impact of both the FFE and PES programmes on the proportion of stunted children within a household. Neither programme had an impact in reducing the number of stunted children within households, as the coefficients on both programmes' variables are not statistically different from zero, due to the relatively large standard error that indicates that both variables were not precisely estimated. Looking at the magnitude and direction of each coefficient, the coefficient on in-kind transfer – though very small – is negative and indicates that children within the beneficiary households are less stunted than non-beneficiaries. The coefficient of the cash programme is large in magnitude and positive. Despite the large difference in magnitude, a t-test of the difference between the two programmes' effects fails to reject the null of no difference.

Regression (2), which looks at the programmes' impact on the proportion of children who are underweight, tells a similar story. The effects of both programmes are not statistically significant from non-beneficiary households and are also not statistically different from each other. However, if we look at the magnitude and the direction of the coefficient, the in-kind coefficient is negative though small. Surprisingly, the cash coefficient is relatively large and negative, indicating that nearly 78% of children in cash households are not underweight. Overall, the regression evidence shows that two programmes led to no difference in the health outcomes of young children within households. As before, we do not report the outcomes of the value regressions, but the results in this section are robust to using the value specification.

It must be noted however, that health outcomes were only measured in 2000 and 2006. Given the panel aging, it is most likely that the health outcome measures at the two spots in time were of different individual children. In addition, it is almost impossible to immediately observe changes in health variables such as height or weight over a short period of time. Therefore, it is hard to draw a definite conclusion about the programmes' impact on long-term health outcomes of children when we are not sure if the same individuals were measured and the initial period of food transfer only lasted for two years before it was abandoned in 2002.

Table 11: Health Regression

	(1) Stunted	(2) Underweight
In-Kind	**-0.002**	**-0.053**
	(0.160)	(0.179)
Cash	**0.123**	**-0.780**
	(0.685)	(0.765)
Log household size	0.019	-0.061
	(0.112)	(0.120)
Log pc expenditure	0.030	-0.037
	(0.072)	(0.075)
Children 0-4	0.082	-0.850*
	(0.425)	(0.435)
Children 5-9	-0.078	-0.374
	(0.407)	(0.455)
Children 10-14	-0.147	-0.122
	(0.417)	(0.459)
Adult 15-54	-0.328	-0.081
	(0.428)	(0.464)
year==2006	-0.005	0.249
	(0.209)	(0.234)
Eligible for FFE	0.228	0.027
	(0.153)	(0.176)
Eligible for PES	-0.309**	-0.009
	(0.152)	(0.167)
Child Enrolment	-0.183	0.292
	(0.387)	(0.441)
Observations	632	632
Inkind=Cash	**0.830**	**0.263**

Robust-Clustered standard error in parentheses; main statistics for InKind=Cash are the p-values
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

7.4 Summary

The results show that the in-kind wheat transfer is extra-marginal and binding. Consistent with the theory, this in-kind transfer did lead to a higher wheat share among beneficiaries of the in-kind transfer than they would have chosen if they were given equal-valued cash transfer. Though there were statistically no difference between the total food share of in-kind and cash beneficiary households, the large difference in the magnitude (-0.14 versus -0.01) gives an indication that in-kind transfer households may be forced to consumed more food than households with cash transfers.

Cash households who face no constraints divert more spending to non-food consumption than in-kind households. Contrary to the usual paternalistic fears, households spend their extra cash income positively on children's education, clothes, children-focused non-food items, and spend no more on negative consumption such as smoking. Despite the constraint imposed by the in-kind programme, the substitution behaviours by in-kind households, in the end, lead to no statistical difference in the calorie consumption between households under the two programmes, which illustrates that households were able to adjust effectively. Neither programme had a significant effect on the health status of children in Bangladesh. The lack of effects could be because of the small size of the transfer, which only represents around 5% of total expenditure on average, but also the difficulty of measuring long-term health outcome must be noted. More importantly, there is no significant difference in the measured health outcomes between the two programmes, which further proves that paternalism could lead to unnecessary distortions and constraints that lower household utility without achieving better social or household longer-term welfare.

8 Conclusion and Policy Implications

Paternalistic in-kind transfers intentionally cause a distortion to recipients' behaviour in the hope of generating a positive social welfare gain not possible under cash transfers. This paper attempts to provide empirical evidence contributing to the debate between in-kind versus cash transfers and also seek to address whether such distorting paternalistic acts are really necessary given the cost involved. We makes use of a natural experiment, where the same sets of household in Bangladesh experience an exogenous change in the nature of the transfer they received, from in-kind to cash. In this paper, we look for differences in households' behaviour under each programme as a result of the change in the type of transfer.

Using the FE-IV approach which allows us to control for the unobserved time-invariant individual-specific effects, such as household preference, via the fixed effect (within) estimator and control for the endogeneity of programme participation and possible measurement errors via the instrumental variables, we find that the in-kind food grains transfers, which was completely infra-marginal in terms of total food, as predicted by the theory, has no effect on overall calorie consumption when compared to equal-valued cash transfer. However, in the analysis of individual food items and categories, the in-kind wheat transfer was both extra-marginal and binding, forcing beneficiary household to consume more wheat than they would have otherwise chosen. Households, however, were able to substitute effectively out of similar foodstuffs such as rice, keeping the overall cereal consumption around the same level in order to ease the constraint and minimise distortions. However, households under cash transfers who faced no constraint were able to substitute more effectively out of all food categories to finance expenditure on non-food items. These substitutions did not result in a significant difference in calorie consumption or consumption of specific categories of food. Moreover, the extra cash drawn out of spending on food by households under the cash programme was spent positively on children's education, clothes and children's non-food items, with no increase in spending on consumption of undesirable items like cigarettes. It would seem that households are capable of making good rational decisions on their own without paternalistic intervention.

The findings in this paper provide minimal evidence in support of the paternalistic motivation behind the in-kind food transfer and are similar to a finding in a study in Mexico by Cunha (2010), which cleanly estimates the impact of cash versus in-kind transfers using a randomly assigned experiment. Cunha found little evidence of differences between cash and in-kind households and similarly concludes that the evidence supporting the paternalistic motivations behind in-kind transfers is weak. However, the findings of no programme effects in this paper contradict earlier findings by Ahmed (2005), who concluded that households under the in-kind FFE programme consumed more calories than non-beneficiary households, while the same is not true for cash PES beneficiary households, hence concluding that there were differences between the two programmes. However, these differences could have been the result of failing to control for the unobserved household fixed effect, which results in a bias in the estimates.

In addition to the theoretical discussion of paternalistic in-kind transfers versus cash transfers, we must not forget the policy implications of empirical research in this particular subject. In-kind transfer programmes involve higher administrative costs in both the storage and distribution of in-kind goods such as food grains. These costs may be justified if the provision of in-kind transfers leads to a substantial increase in welfare in comparison to its cash counterpart. Such an increase may outweigh

these administrative costs on top of the cost to recipients' utility whose consumption was distorted by the programme. The precise comparison was not possible in this paper due to data limitations. Given the sizable cost saving that could result from replacing in-kind transfers with cash transfers, more empirical evidence looking more closely at the cost benefit analysis of cash versus in-kind transfer is needed.

Despite the conclusion here that paternalism may not be a sufficient justification for provision of transfer in-kind over cash, there are other valid justifications recently being investigated and these make for interesting and important findings. The pecuniary effect may provide an important justification, as recent findings show that the general equilibrium effect on the prices of in-kind and cash transfers works in an opposite direction, which could lead to different outcomes for beneficiary households (Jayachadran, Cunha and De Giorgi 2010). Other considerations, such as the self-targeting induced by in-kind transfers and the political economy explanation, are also important and will provide the opportunity for a great deal of further research on the issue of in-kind versus cash transfers and the design of transfer programmes in general.

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A Appendix

A.1 Enrolment Regression

Table 12: Enrolment Regression

	(1) FE-IV Proportion of Children enrolled in primary school
In-Kind	**0.086****
	(0.039)
Cash	**0.250*****
	(0.089)
Log household size	-0.023
	(0.019)
Log pc expenditure	0.015
	(0.013)
Children 0-4	0.021
	(0.075)
Children 5-9	0.316***
	(0.080)
Children 10-14	0.239***
	(0.082)
Adult 15-54	0.010
	(0.051)
Wheat Price	0.002
	(0.003)
Rice price	0.002
	(0.007)
year==2003	-0.121***
	(0.039)
year==2006	-0.146**
	(0.067)
Eligible for FFE	-0.003
	(0.044)
Eligible for PES	0.068*
	(0.037)
Observations	1365
InKind=Cash	**0.016****

Robust-Clustered standard error in parentheses; main statistics for InKind=Cash are the p-values
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.2 Attrition

Table 13: Attrition Probit Result

		(1) Attrition
Attrition		
Household head age	-0.043***	(0.002)
Household head age$\hat{2}$	0.003***	(0.001)
Household Head education	-0.000	(0.999)
Household size	-0.126	(0.120)
Children age 0 to 4	0.603	(0.737)
Children age 5 to 9	0.520	(0.765)
Children age 10 to 14	0.224	(0.903)
Adult age 15 to 54	0.923	(0.530)
Asset	0.038	(0.719)
Per capita Expenditure 2000	0.026	(0.939)
Per capita Calorie 2000	0.000	(0.451)
upazilas1==Nilphamari Sadar	0.000	(.)
upazilas2==Mohadebpur	-1.283***	(0.001)
upazilas3==Sherpur Sadar	-0.717*	(0.068)
upazilas4==Modhupur	-1.075***	(0.008)
upazilas5==Kalia	-1.137***	(0.002)
upazilas6==Agailjhara	-0.709*	(0.074)
upazilas7==Haziganj	-0.437	(0.198)
Constant	-0.348	(0.890)
Observations	420	
Psuedo R$\hat{2}$	0.193	
Wald Test	55.774	

main statistics are coefficients; p-value in parentheses
Dependent variable takes value 1 if household left the sample after 2000, and 0 otherwise
Independent varaibles are all baseline value in year 2000
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.3 First Stage Regression

Table 14: First Stage Regression of Table 9

	(3.1) In-Kind	(3.2) Cash	(4.1) In-Kind	(4.2) Cash	(5.1) In-Kind	(5.2) Cash	(6.1) In-Kind	(6.2) Cash
Inkind Instrument	**0.626*****	**-0.031****	**0.613*****	**-0.127*****	**0.613*****	**-0.127*****	**0.613*****	**-0.127*****
	(0.035)	(0.014)	(0.036)	(0.040)	(0.036)	(0.040)	(0.036)	(0.040)
Cash Instrument	**0.219*****	**0.199*****	**0.194*****	**0.121****	**0.194*****	**0.121****	**0.194*****	**0.121****
	(0.036)	(0.038)	(0.037)	(0.051)	(0.037)	(0.051)	(0.037)	(0.051)
Log household size	-0.006	0.069**	-0.020	-0.023	-0.020	-0.023	-0.020	-0.023
	(0.017)	(0.028)	(0.035)	(0.052)	(0.035)	(0.052)	(0.035)	(0.052)
Log pc expenditure	-0.045***	-0.075***	-0.024	-0.011	-0.024	-0.011	-0.024	-0.011
	(0.014)	(0.021)	(0.024)	(0.035)	(0.024)	(0.035)	(0.024)	(0.035)
Children 0-4	-0.041	0.066	-0.249*	0.206	-0.249*	0.206	-0.249*	0.206
	(0.073)	(0.098)	(0.139)	(0.184)	(0.139)	(0.184)	(0.139)	(0.184)
Children 5-9	-0.025	0.197**	-0.239*	0.227	-0.239*	0.227	-0.239*	0.227
	(0.073)	(0.095)	(0.129)	(0.171)	(0.129)	(0.171)	(0.129)	(0.171)
Children 10-14	-0.016	0.244***	-0.227**	0.403***	-0.227**	0.403***	-0.227**	0.403***
	(0.060)	(0.091)	(0.111)	(0.149)	(0.111)	(0.149)	(0.111)	(0.149)
Adult 15-54	-0.022	-0.005	-0.176**	0.220*	-0.176**	0.220*	-0.176**	0.220*
	(0.039)	(0.054)	(0.085)	(0.115)	(0.085)	(0.115)	(0.085)	(0.115)
Wheat price	0.002	-0.003	-0.003	-0.001	-0.003	-0.001	-0.003	-0.001
	(0.004)	(0.006)	(0.006)	(0.008)	(0.006)	(0.008)	(0.006)	(0.008)
Rice price	0.008*	0.001	-0.005	-0.016	-0.005	-0.016	-0.005	-0.016
	(0.004)	(0.009)	(0.013)	(0.018)	(0.013)	(0.018)	(0.013)	(0.018)
Eligible for FFE	-0.059	0.204***	-0.003	0.277***	-0.003	0.277***	-0.003	0.277***
	(0.045)	(0.077)	(0.080)	(0.097)	(0.080)	(0.097)	(0.080)	(0.097)
Eligible for PES	-0.205***	-0.226***	-0.283***	-0.212**	-0.283***	-0.212**	-0.283***	-0.212**
	(0.064)	(0.077)	(0.092)	(0.101)	(0.092)	(0.101)	(0.092)	(0.101)
year==2003	-0.239***	0.333***	-0.197***	0.350***	-0.197***	0.350***	-0.197***	0.350***
	(0.037)	(0.040)	(0.046)	(0.058)	(0.046)	(0.058)	(0.046)	(0.058)
year==2006	-0.280***	0.308***	-0.150	0.365**	-0.150	0.365**	-0.150	0.365**
	(0.066)	(0.085)	(0.110)	(0.149)	(0.110)	(0.149)	(0.110)	(0.149)
Child enrolment	0.244***	0.537***	0.308***	0.572***	0.308***	0.572***	0.308***	0.572***
	(0.062)	(0.085)	(0.082)	(0.112)	(0.082)	(0.112)	(0.082)	(0.112)
Constant	0.417***	0.207						
	(0.135)	(0.158)						
Observations	1365	1365	1365	1365	1365	1365	1365	1365
R^2	0.490	0.316	0.555	0.327	0.555	0.327	0.555	0.327
F	29.888	41.356	29.948	38.816	29.948	38.816	29.948	38.816

b coefficients; Robust-Clustered standard error in parentheses
equation 3.1 and 3.2 are first stage of euqation(3) in table 9
equation 4.1 and 4.2 are first stage of euqation(4) in table 9
equation 5.1 and 5.2 are first stage of euqation(5) in table 9
equation 6.1 and 6.2 are first stage of euqation(6) in table 9
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$