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Abstract
This paper explores whether state provision of school meals in the 1980s crowded out private provision by examining two UK policy reforms that dramatically reduced school meal take-up. The paper examines whether this affected children's BMI, using a large, unique, longitudinal dataset of primary school children from 1972 – 1994. This period is characterized by –for some– relative scarcity of foods. The reforms placed further constraints on some families' already tight food budgets, leading to nutritionists expecting children to become malnourished. The findings however, show no evidence of any such effects. In addition, I find no support for the hypothesis of intra-household food reallocation. As some of those affected are relatively poor, and as sample sizes are often large with fairly precise estimates, the analysis should have been able to detect any effects. With no such evidence, this suggests that the state provision of school meals was crowding out private provision of similarly nutritious packed and home lunches.

Key words:
Crowd Out; School Meal Provision; Body Mass Index; Difference-in-Difference

JEL Codes:
H3, H4, I1

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1. Introduction

UK school meals were first introduced in the late nineteenth century, because children – especially those from low-income households – had problems concentrating in class. Several studies have since linked poor nutrition with poor cognitive performance (see for example Pollitt et al., 1998; Alaimo et al., 2001; Belot and James, 2011). Furthermore, a healthy diet is crucial for children's growth and development; unhealthy diets can increase the risk of a variety of health problems both in childhood (like dental health) and later in life (like coronary heart disease).

After the Second World War, the school meal service changed from one designed to benefit children's education, to a general service of lunchtime meals, intended to benefit all children. Most of the daily requirements had to be met by the school meal, because food was still rationed. But with the election of the Conservative administration in 1979, the government attitude to the service shifted. It was viewed as too expensive and the government wanted to introduce more choice and parental responsibility. Two Acts of Parliament were responsible for a substantial change in the school meal provision.

The first Act, in 1980, ended the fixed pricing of school meals for those not claiming benefits, i.e. the 'non-poor', leading to a rapid increase in the price of school meals. Figure 1a and 1b clearly show the effects of this reform on school meal take-up and on the consumption of packed lunches, such as sandwiches. The solid line represents those not on benefits – those affected by the Act – showing a large drop in the uptake of school meals at the time of the reform. The dotted and dashed lines are those not affected by the Act and remain relatively constant over the period. Figure 1b shows that the drop in school meal take-up is compensated by a large increase in the consumption of packed lunches.[1]

The second Act, in 1988, decreased the number of children eligible for free school meals by withdrawing certain benefit entitlements. The dashed line in Figure 1a illustrates the effects on school meal take-up for those affected by this reform, showing a substantial drop. The take-up among those not affected – the solid and dotted lines – remains constant. Figure 1b illustrates the simultaneous increase in the consumption of packed lunches for those affected by the reform.

These figures show that the response to the Acts was substantial: a big proportion of children decided against school meals due to the introduction of the reforms. The Acts sparked a large opposition, and lead to concerns among nutritionists who argued that, as school meals consisted of one-third of children's daily nutritional intake, withdrawing them would cause a decline in children's nutritional status. Hence, the main concern related to children's *under*-nutrition. When introducing

[1] Consumption of other types of lunches (including home lunches) also increased, but less so compared to packed lunches.

these changes, the government ignored reports by the Department of Education and Science (DES, 1975a, 1975b), which mentioned that some children come to school with little or no breakfast, and that there are still children whose only adequate meal of the day is their school meal. In fact, the opposition to these reforms was so large that the data used in this paper were specifically set up to investigate whether children's nutritional status showed any unforeseen effects due to changes in the provision of school milk and meals (see section 5). Despite this, no research has used these data to investigate the effects on child weight-for-height, applying the empirical methodology I use here.

The aim of this paper is therefore to explore whether the state provision of school meals substituted for, or crowded out, any private provision. In other words, I examine whether abolishing the state provision affected child weight-for-height, as proxied by their Body Mass Index (BMI, weight in kilograms divided by height in metres squared). With that, I shed light on a long and ongoing debate about public vs. private sector provision of goods and services.

I use a large, unique, longitudinal dataset of primary school children from 1972 – 1994, exploring the effects of two policy reforms that have not been examined in this context. Although these data are arguably not representative of today's society, it is a particularly interesting time period to study for two reasons. First, it allows me to examine whether there is any evidence of crowd out in a period where the general concern related to children's under-nutrition, but in which we simultaneously move from a shortage to a surplus of foods and experience rising levels of childhood obesity. The years in which the data are collected provide a unique opportunity to explore crowd out during a period of – for some – relative scarcity of foods. At the time of the reforms, nutritionists believed that parents were not able to provide similar nutritious lunches, leading to under-nourished children. There was a large opposition to the reforms, which was reinforced by the substantial drop in school meal take-up rates. The Acts placed further constraints on some families' already tight food budgets. Hence, if this affected their food purchases, the analysis should be able to detect this. If, for these vulnerable families, there is no evidence that it affected children's (and parents') BMI, it suggests that the government-provided school meals were substituting for similarly nutritious meals otherwise provided by parents.

Second, this is the period generally perceived to be the start of the obesity epidemic. Hence, if any changes to the home and school food environment that were caused by these Acts contributed to the longer term trend of childhood obesity, the long time-series of these data should be able to detect this.

Because the two reforms affect different groups of children and their families, I examine them separately. The two reforms are exogenous: first, they affect the take-up of school meals for one

group (the treated), but not the other (the controls). Second, there is no evidence that the introduction of the reforms is related to children's nutritional status. Additionally, they have been introduced in the whole country at the same time; there was no voluntary introduction. The findings show that the treated in both reforms did not gain or lose weight. To investigate a potential reallocation of resources within the household, I explore whether the reforms affected parental BMI. With no support for the hypothesis of intra-household reallocation, this suggests that state provision of school meals was crowding out private provision of similarly nutritious home or packed lunches.

The next section discusses the relevant literatures. Section three provides the institutional details and hypotheses, with section four setting out the econometric framework. The data are described in section five. Section six presents the results, and section seven concludes.

2. Literature

A number of different literatures are relevant to this paper. I discuss these in turn.

2.1. Crowd out

There is a large literature estimating the substitution of public for private coverage. An important area in economics research relates to the provision of public health insurance crowding out private coverage. For example, Cutler and Gruber (1996) estimate that approximately 50% of the increase in Medicaid coverage between 1987 and 1992 was associated with a reduction in private insurance coverage. Although much subsequent research estimates lower rates of crowd out, Gruber and Simon (2008) revisit the issue, incorporating a range of approaches to estimate crowd out and find considerable crowd out rates of 60% for public insurance expansions between 1996 and 2002.

Crowding out has also been investigated in other areas, such as unemployment insurance and charitable giving, mostly finding large estimates of crowd out. For example, Cullen and Gruber (2000) estimate that wives earn up to 73 cents less for each dollar of husband's unemployment insurance receipts. Similarly, Gruber and Hungerman (2007) investigate the extent to which the New Deal crowded out church charitable giving in the 1930s. Their findings show a 30% fall in benevolent church spending in response to the New Deal. In addition, they find that government relief spending can explain virtually all of the decline in charitable church activity observed between 1933 and 1939.

2.2. School Meals and Child Health

Despite the initial purpose of collecting these data, I am not aware of any studies that specifically explore the effect of the two Acts on child weight-for-height. Most studies instead only examine child height as a proxy for nutritional status. They generally find no association between height (gain) and the type of school meal consumed (Rona et al., 1983, Rona and Chinn, 1989). However, these studies do not consider the change in height due to the reforms: they focus on lunch consumption either before or after the Acts. Furthermore, they examine height differences for children who consume school, home, or packed lunches, where this choice of lunch is likely to be endogenous. I use BMI as the outcome of interest, examining whether the reforms affected children's weight-for-height.

The focus on child weights rather than heights is more recent. Over the past years, we have seen a substantial change in the general nutritional environment. There has been an increase in food choice and a relative decrease in food prices in the period studied here (Cabinet Office, 2008), leading to concerns about rising (childhood) obesity rates. With this, a slightly different strand of literature has emerged: one that examines the relationship between school meals and children's excess body weight. These studies mainly use US data and distinguish between the School Breakfast Program (SBP) and the National School Lunch Program (NSLP), but find somewhat conflicting results.

Some find a positive relationship between participation in these programmes and child weight. Whitmore-Schanzenbach (2005) analyses the impact of NSLP participation on obesity, observing children from kindergarten to first grade. Using a range of different approaches, she finds that children who take packed lunches are between 2 and 4 percentage points less likely to be obese than those who consume school lunches. Millimet et al. (2010) observe pupils between kindergarten and third grade and find some evidence of a positive association between SBP (but not NSLP) participation and child weight.

Several other studies however, find no relationship or conclude that school food programmes improve children's diets. Hofferth and Curtin (2005) explore the effects of participation in SBP and NSLP simultaneously, but find no association with child weight. Bender (2006) also finds no relationship between SBP participation and child BMI. However, he finds that SBP participants consume significantly less total fat, saturated fat and cholesterol than non-participants. Similarly, Bhattacharya et al. (2006) and Bhattacharya and Currie (2000) find that school nutrition programmes improve the nutritional quality of the diet and do not affect the number of calories consumed.

2.3. Means-Tested Benefits and Child Health

Another strand of literature that is relevant to this paper is that that examines the relationship

between (means-tested) benefits and (child) health. The majority of these studies though, focus on the effect of *introducing* benefits, rather than *withdrawing* benefits. For example, Hoynes and Whitmore-Schanzenbach (2009) exploit variation in the month the Food Stamp Program (FSP) started operating in US counties. Using a difference-in-difference approach, they find that the introduction of food stamps leads to a decrease in out-of-pocket spending, an increase in overall food expenditures (which includes the value of food purchased with food stamps), and a decrease (though insignificant) in the propensity to take meals out. Almond et al. (2007) and Currie and Moretti (2008) explore the relationship between the introduction of a FSP and birth weight. Their results suggest that pregnancies exposed to a new FSP three months prior to birth yield deliveries with increased birth weight. The size of the effect decreases the longer the lag between the FSP introduction and birth. Similarly, Currie and Cole (1993) find that participation in AFDC (Aid to Families with Dependent Children) increases the birth weights of children born to poor white mothers.

3. Institutional details and Hypotheses

3.1. School Meal Provision

School meals make up a significant part of children's daily nutritional intake. Since the introduction of the school meals service in the UK, schools had a statutory duty to provide lunches for all children; the eligibility for *free* lunches was determined through the receipt of certain benefits, although Local Education Authorities (LEAs) were also allowed – at their discretion – to provide free meals to other low-income children. After the election of the Conservative administration in 1979, the school meal service changed. Two Acts of Parliament radically altered the school meals service: the 1980 Education Act and the 1988 Local Government Act.

3.1.1. The 1980 Education Act

The 1980 Act removed the statutory duty of LEAs to provide school lunches, except for pupils eligible for free school meals. Instead, schools had to provide areas where pupils could have a packed lunch. Eligibility for free meals was based on whether the family received certain types of benefits: Family Credit (FC) or Income Support (IS)[2]. LEAs were still allowed to provide free meals to children from other low-income families, though this only happened on a discretionary basis. By 1981, 27% of LEAs

[2] In 1980, FC and IS were known as Family Income Supplement and Supplementary Benefits respectively. They were renamed in 1988. FC is the predecessor of Working Families' Tax Credit (introduced in 1999). This paper refers to the benefits using the names as they were known in 1988; i.e. FC and IS. The differences in eligibility rules between these two benefits will be discussed in more detail below.

had decided to go no further than their statutory minimum obligations (Bissett and Coussins, 1982). A survey carried out by the Department of Health in 1983 reported that between 70 and 80% of free school meal children came from families receiving benefits. The others received them under this discretion granted to LEAs (DoH, 1989).

In addition, the 1980 Education Act abolished the minimum nutritional standards and ended the fixed pricing of school meals. Before the Act, there was a fixed national charge for meals, set at 35p per day. Prices rose quickly after 1980; the most common price charged for a school meal in 1981 was 50p, although there was much variation between LEAs ranging from 35p to 60p (Bissett and Coussins, 1982). For primary schools – the focus of this paper – this was the main effect of the Act: a price increase for those not eligible for free meals, i.e. those *not* claiming FC or IS. These are referred to as the treated; those on FC or IS are the controls.

With the implementation of the reforms, the government ignored the advice and concerns expressed by nutritionists. It was well recognised that children receiving free school meals on average weighed less and were shorter than those paying for, or not having school meals. Additionally, a large proportion of those on free meals had lower nutritional status, and it was argued that a withdrawal of school meals might prejudice children's future development (Rona et al., 1979).

Figure 2 presents a very simple graphical model, showing that households maximise their utility *U(c,f)*, where *c* and *f* refer to the consumption of non-food and food respectively. The straight solid line (BC) is the initial budget constraint (prior to 1980), with all school meals set at 35p per day. Point A is the optimal consumption bundle. The 1980 price increase caused both an income (IE) and substitution effect (SE), resulting in a steeper budget constraint (BC'). This simple illustration shows that, assuming non-negative income elasticities of food consumption, the implied income drop due to the price increase always leads to a decrease in the demand for food. Point B presents a potential outcome with positive income elasticity; point C shows the outcome with zero income elasticity.

Any effects of the reform on child weight-for-height however, are not only determined by the income and substitution effects, but also depend on various additional factors. First, the Act caused many of those affected by the reform to change from consuming school lunches to having packed or home lunches. Therefore, any effect will partly depend on the difference in nutritional value between school and home or packed lunches. As Rona and Chinn (1999) discuss, the diet provided by parents was unlikely to contain more energy, given the high fat content of school meals reported in most dietary surveys (e.g. Darke et al., 1980; DoH, 1989).

Second, any effect of the reform will depend on whether consuming free school meals introduces intra-household reallocation of resources. That is, do households reallocate their resources when

child-specific transfers are made, or does the transfer 'stick' to the child? The former would mean that (*a*) the policy of providing free school meals is neutralised by reallocation of other resources away from the child towards other members of the household, or (*b*) that withdrawing meals is offset by a reallocation to the child from other household members, leading to little or no effects on actual child consumption and body weight.

Studies that explore the *introduction* of child-specific transfers generally find no evidence of intra-household reallocation (see Jacoby (2002) or Afridi (2005) for evidence on developing countries). US research evaluating the effects of the NSLP and SBP on children's nutritional intakes also shows participants to have an increased 24-hour intake of nutrients (see e.g. Bhattacharya et al., 2006; Gleason and Suitor, 2003; Devaney and Fraker, 1989), suggesting that children's participation in these food programmes is not compensated by less nutrition in the home.

I am not aware of studies that examine the *withdrawal* of (child-specific) transfers. Related to this however, Wilde and Ranney (1998) find that adults receiving Food Stamps eat less in the last week of the month, with Food Stamps issued monthly, while children's intakes remain fairly constant. Similarly, Rose and Oliveira (1997) find a negative association between food insecurity and nutrient intakes among young adult women and the elderly, but not among children. This suggests that adults (and particularly women) give up food to ensure adequate nutrition for their children; something that has also been shown in several qualitative studies. For example, Burghes (1980) finds that, in times of food insecurities, parents go without to feed their children, and women go without to feed their husbands. Graham (1984) notes that mothers most often act as buffers: protecting the welfare of her family by absorbing shortages herself. These studies suggest that an *introduction* of (child-specific) benefits is not likely to lead to any intra-household reallocation, but the *withdrawal* of transfers (or insufficient availability of foods) can lead to a reallocation of resources towards children.

3.1.2. The 1988 Local Government Act

The second reform studied in this paper is the 1988 Local Government Act.[3] The main change in this Act was the tightening of eligibility rules: children in families receiving FC were no longer eligible for free meals. Instead, their benefit was increased slightly by way of compensation. This meant that eligibility for free meals was now restricted to those on IS. Thus, the treated in the 1988 reform are those claiming FC; the controls are those on IS. The eligibility rules for FC and IS will be discussed in

[3] The changes to the school meals service were laid out in the 1986 Social Security Act, but came into operation in April 1988, with the 1988 Local Government Act.

more detail below.

Simultaneously, LEAs were now required to charge for school meals in all other cases, meaning they lost their right to provide free meals on a discretionary basis. The Act also introduced Compulsory Competitive Tendering (CCT), meaning that LEAs were forced to put school meal services out to tender and invite bids from a range of caterers. They were then obliged to let the contract to the tender offering the cheapest price (UNISON, 2005).

In secondary schools, CCT drove down the price and quality of meals. However, the literature suggests that this mattered less for primary schools, as most LEAs retained the two-choice, two-course meal for the same price (Passmore and Harris, 2004). Hence, the main effect of the 1988 Act for primary schools was the withdrawal of *free* meals for those on FC.

Pressure groups' and nutritionists' reactions to the Act were similar to the 1980 reform, especially since those affected by this reform were lower-income households. With the Act, approximately half a million children from low-income families lost their entitlement to free school lunches. McMahon and Marsh (1999) note that the compensatory amount added to the FC benefit was 44p per week: "insufficient to provide a packed lunch with the same nutritional value to the child". In addition, they argue there is no guarantee that the money would ever reach the child in question, given the tight budgets on which many low-income families live.

Hence, those affected by this reform differ from those affected by the 1980 reform. Although a simple graphical representation would look similar to Figure 2 discussed above, the treated in 1988 are low-income families on FC, and their reaction to a price increase is likely to differ from that of the better-off families in the 1980 reform.[4] A bulletin by the Family Policy Studies Centre (FPSC, 1986) notes that children from these low-income families "will start having to pay for their meals, bring packed lunches, or *do without* [emphasis added]".

As above however, any intra-household reallocation of resources may offset the potential negative effects of the 1988 Act. I examine this in the analysis below. Finally, note that the price change coincided with the introduction of CCT. As discussed above, the literature argues this mattered less for primary schools and therefore may be less of an issue for the analyses here, but if it did drive down the quality of primary school meals, it would have mainly affected those who remained eligible, i.e. those on IS. A decrease in quality might have lead to an increase in the intake of fats and sugars and simultaneously to a disproportionate increase in child BMI. Hence, the estimate of the effect of the 1988 reform may be driven by a weight decrease for those on FC, a weight increase for

[4] One might also expect families to react differently to a price *introduction* (as in 1988), compared to a price *increase* (as in 1980).

those on IS, or both. I examine this in more detail in the empirical application.

In short, the 1980 Act impacted on those not claiming benefits; the 1988 Act affected one group of benefit-claimers. Since crowd out is more likely in higher income populations, as they are more likely to be able to meet the expense of any additional costs, the crowd out would be expected to be larger for the 1980 reform.

3.2. UK benefits: FC and IS

The two reforms described above are directly linked to the UK benefit system, in particular to FC and IS. I will briefly summarise the main similarities and differences between the two types of benefits, since the second reform rests on distinguishing between them. Appendix A outlines any changes in IS and FC for the period 1970-1990, as this is the time span used in the empirical analysis.

Both FC and IS are aimed at low-income households and act as a 'passport' to other benefits, like free school meals and free prescriptions. The main difference is that FC is a *conditional* benefit, or an income *supplement*; it is only available to those in full-time employment. However, once FC is awarded, it is paid at the same rate for 12 months (6 months from 1988), regardless of any change in circumstances (Fry and Stark, 1993). In contrast, IS – the state 'safety net' – is available to all those not in full-time employment. In addition, FC is only payable to families with children, whilst eligibility for IS is independent of having children in the household.

4. Econometric framework

In studying the effects of the two reforms, I define children as treated or control based on their *eligibility* for free school meals. Eligibility in turn is derived from the family's benefit status. This differs from take-up; it is not the case that all eligible children consume free school meals.[5] Hence, this is an Intention To Treat (ITT) analysis, examining the effects of withdrawing the entitlement to free lunches for some, but not all, children on their BMI. The ITT analysis explores the effect of a specific policy (the Parliamentary Acts), rather than a specific treatment (changing the type of lunch consumed).

In fact, the actual take-up of school meals is a *decision*, and one that is likely to be related to other observed and unobserved child and family characteristics (like preferences and tastes), which may lead to biased estimates. The identification strategy therefore relies on the exogenous price change

[5] The two main factors that have been mentioned as determinants for take-up behaviour are stigma and ignorance of entitlement. Wilson (1981) showed that 71% of families who were eligible through receipt of 'passport' benefits were aware of their entitlement. However, 15% thought they did not qualify and 13% were unsure.

in school meals caused by the 1980 and 1988 Acts that differentially affected take-up rates for different groups of pupils. These reforms are exogenous; there is no evidence that its introduction is related to children's nutritional status. I use a difference-in-difference (DD) approach to account for fixed unobservable differences between the treated and controls. As the Acts affected different groups of children, I examine the two reforms separately.

4.1. The 1980 Reform

With the 1980 Education Act, all those not claiming benefits experienced an increase in the price of school meals. This exogenous shock caused a sudden drop in take-up. The DD compares the BMI of the treated (i.e. those not on benefits) in the years before and after 1980 to that of the controls (i.e. those claiming IS/FC), estimating the following OLS regression:

$$BMI_{it} = \alpha_0 + \alpha_1 D_i + \alpha_2 I_{it} + \alpha_3 D_i * I_{it} + X_{it}'\beta + u_{it} \quad (1)$$

where the subscripts *i* and *t* refer to child *i* at time *t.* D_i is the treatment indicator, equalling 1 if the child is not on benefits and 0 if it is. I_{it} is a dummy equalling 1 for the years post (and including) 1980. The analysis includes the years 1972 to 1986. Later years are omitted from this first analysis, as these coincide with the second policy reform. The parameter α_3 is the ITT estimator: the impact of the policy change on child BMI. The covariates included in the vector X_{it} will be discussed in section 5. All analyses use robust standard errors clustered by individual.

The DD setup makes several assumptions. First, it assumes a common time trend between the treated and controls. I examine this assumption in Section 5.2.3, comparing the average BMI by year for the treated to that for the controls. Second, there are no peer effects: i.e. the type of lunch consumed by child *i* is not influenced by other children's consumption.[6] Although the assumption is not testable, its violation biases the ITT, the extent of which depends on the nature and seriousness of the violation. Whether this assumption holds is debatable. It has been argued that some parents decide not to claim free school meals because of a concern that their children will be bullied (Bissett and Coussins, 1982). This would mean that some children in the control group are, in effect, also 'treated', leading to an underestimate of the ITT. The extent of this downward bias depends on the size of the control group affected by the peer effects.

[6] Also referred to as the Stable Unit Treatment Value Assumption (or SUTVA), meaning that the potential outcomes for person *i* depend on their treatment status only, and not on the treatment status of other individuals.

Third, no other policies that were introduced in this period affected the treated differently from the controls. Any changes to the benefit system that may have differentially affected the two groups in the period studied are described above and in Appendix A. One potential issue is the abolishment of the minimum nutritional standards in 1980, which would have affected those remaining on school meals: those receiving FC and IS. This is less likely to be a problem here however, as the analysis specifically examines the effect on primary school children, where the main change of the 1980 Act was the price increase.

There have also been various changes at the national level, such as the recession in the early 1980s. Some evidence suggests that working class men in manufacturing, mining and farming were hit hardest. To the extent that the working classes are differentially represented in the treatment and control groups, this may bias the empirical analysis. Indeed, the treated and controls are likely to differ in background characteristics. The former are likely to be wealthier and of higher social class than the latter, implying that issues like common support may be important to consider. In addition to the OLS estimation that may be extrapolating across observations with very different observables, I therefore also use propensity score matching (PSM) to match a group of non-claimants who are similar in observable characteristics to a group claiming benefits. The underlying assumption is that matching on observables will also match more closely on unobservables, decreasing the potential bias in OLS. Specifically, I use kernel matching with a Gaussian kernel. All analyses impose common support and use a bandwidth of 0.03. Using this more homogeneous set of children, I re-estimate the DD to obtain the ITT on the matched sample.

Finally, as mentioned in section 3, the extent of the 1980 school meal price increase varied by LEA. Bissett and Coussins (1982) show that the reactions to the reforms, in terms of the drop in school meal take-up, is strongly and positively related to the magnitude of this price increase. This implies that, for areas that do not increase their price after the reforms, we should see no large changes in take-up rates and with that, no effect on children's BMI's. Equation (1) averages over all areas, regardless of whether or not they introduced a price increase, and if they did, how large the increase was. Although the data do not allow me to extract each area's price increase, I do observe the proportion of children in each area deciding to move away from school meals due to the reforms. I rank this proportion to proxy the extent of the price increase, creating a categorical variable that indicates the quartiles of its distribution. I then re-estimate equation (1), interacting D, I, and $D*I$ with the four categories to explore any variation in the ITT by the extent of the price increase.

4.2. The 1988 Reform

With the 1988 Local Government Act, those claiming FC had to start paying for school meals, whilst those on IS did not experience price changes. The second analysis therefore uses the same estimation strategy as the above, but defines the treated as those on FC and the control as those on IS. I also proxy the extent of the price increase by each area's proportion of children that stop having school meals due to reforms. The analysis includes the years 1980 till 1994. Earlier years are omitted from this second analysis, as these coincide with the first policy reform.

One could argue that the control group should also include those who are never on benefits (i.e. the treated in the 1980 reform), as they do not experience any (price) changes either. However, I drop these children for two reasons. First, comparing those on FC to those on IS is likely to include children who are more similar to each other than a comparison that includes those not on benefits (see section 5). Second, the 1988 Act also introduced CCT. If this did lead to a change in the quality of meals in primary schools, we would expect this to be reflected mainly in the weights of IS-children as opposed to those not on benefits. I explore this in more detail below.

5. Data

As a result of the protests against Margaret Thatcher's plans to discontinue free school milk for children over seven years of age, the government agreed to carefully monitor the nutritional status of the population "with a view to detecting any unforeseen adverse effects which might arise from the change at a stage when they were mild and reversible" (Department of Health and Social Security, 1973). The National Study of Health and Growth (NSHG) was set up as part of the fulfilment of this undertaking (Rona and Chinn, 1999).

5.1. The National Study of Health and Growth

28 Geographical areas in England and Scotland were chosen by stratified random sampling. Within each area, medical and educational personnel were asked to select one or more primary schools 'representative of their area classification' to provide a total of at least 300 pupils. Thus complete primary schools were chosen within these areas, rather than individual children.[7] In 1981, the study-organisers decided they wanted the sample to include a higher proportion of children from ethnic minorities and inner city areas. To include these children, but simultaneously preserve the existing monitoring system, a 2-year cycle was introduced. The existing areas were visited in even years, and

[7] The data do not provide a unique school identifier after 1976, so this cannot be used in the analyses.

the inner city areas were surveyed each odd year from 1983 to 1993. As the two samples are different in various characteristics (see section 5.2), I examine them separately.

The NSHG is a longitudinal sample of primary school children (aged 4 to 11) nested within schools. Schools were revisited each year and children were eligible for as long as they attended any of the chosen schools in the study areas. This means that children in the oldest age group were lost the subsequent year, but simultaneously, that the sample was topped up with children who just started their first year of primary school. This allows us to analyse health and health patterns over time, across cohorts and age groups.

A big advantage of the NSHG is that there are enough observations to distinguish between families receiving FC or IS. Other studies that distinguish between different benefit-claimants typically only have very few observations, not allowing them to make robust inferences.[8]

At the start of the study, child height was seen as the main indicator for under-nutrition and therefore recommended as the key measure of nutritional status. However, the survey also collected children's weight and skinfold thickness in all survey years. I use the child's BMI (kg/m^2) as the dependent variable. All measures were taken by locally-employed nurses, supervised by a trained fieldworker. Measurements were made during one week in each area on school premises and absentees were measured on return to the school. The week in which measurements took place was kept the same from year to year so that children were examined approximately annually.

The NSHG contains a rich set of covariates. The main variable used in the analyses is whether the child is eligible for free school meals, measured by the family's benefit status. Although this eligibility is partly based on employment status – those on FC had to be employed full-time and those on IS could not be – benefit and employment status are not multi-collinear for two reasons. First, the data does not distinguish between part-time and full-time work. Second, the date that a family is registered as being eligible for benefits is different from the date when the questionnaire is filled out. As mentioned above, once FC is awarded, it is paid at the same rate for 12 months, regardless of any change in circumstances (6 months after 1988).

Thus, the analysis includes mother's and father's employment status as covariates. In addition, I include controls for child gender, birth weight, a binary measure indicating whether the child is non-white, the number of older and younger siblings, mother's and father's height, an indicator of the parents being married, and measures of family socio-economic status, including family's social class

[8] For example, Dorsett and Heady (1991) point out that 'not many attempts were made to model FC take-up rates due to small proportions of eligible families'. Using the Family Expenditure Survey, Fry and Stark (1993) note that their estimated FC take-up rates should be taken with caution, because of small sample sizes.

and maternal educational level.[9] In addition, the richness of the data allows us to control for a full set of year*child-age interactions. These will pick up any secular changes in BMI with age and time. I also include area fixed effects to allow for differences in mean BMI across areas. I deal with missing covariates using multiple (4) multivariate imputation (Royston, 2004).

5.2. Descriptive statistics

The majority of children are observed between one and three years (69%), but there is a substantial number with four to six observations in the first 10 years of the study. The sample contains approximately 700-1000 children of any age within any year, with slightly fewer aged 4 and 11. Hence, each year contains between about 5,000 – 7,000 children, as shown in Table 1. The different cohorts are presented diagonally. For example, the cohort that started in 1972, aged 4, included 137 children. The following year, this cohort increased in size to 776 (as children almost always start school in the month September after turning 4), to 758 in 1974, etc.

The first part of the analysis (the 1980 reform) explores the years 1972 – 1986, excluding the inner city sample (i.e. 1983 and 1985). The second part (the 1988 reform) uses 1980 – 1994 and examines the original and inner city sample separately.

5.2.1. Descriptive statistics by benefit status

Table 2 presents the descriptive statistics for the various different samples used. Column 1 and 2 present those for the treated (those not on benefits) and controls (FC or IS) in the analysis of the 1980 Act. Note that this analysis excludes the two inner city sample years (1983 and 1985). Those on benefits have slightly lower BMI's than those not on benefits. The controls are more likely to be non-white, have lower birth weights and more siblings. Almost all parents are married among non-claimants, while this proportion is much lower among those on benefits. Parents who claim benefits are shorter and of lower social class compared to non-claimants. A similar pattern is found for mother's education. Finally, fathers are much less likely to be employed among benefit claimants. These statistics shows that the treated and controls differ in observable characteristics and justifies exploring issues of common support, using propensity score matching (PSM).

Columns 3 and 4 show the descriptive statistics for the analysis of the 1988 Act, distinguishing

[9] Mother and father's weight is not observed in the years 1977-1981. I use the standard UK classification of social class based on occupation (professional (I), managerial and technical (II), non-manual skilled (IIInm), manual skilled (IIIM), semi-skilled (IV), and unskilled (V)). Maternal educational level is categorized into secondary or lower; commercial/technical; university/professional; and other.

between the treated (FC) and controls (IS) in the original sample. Columns 5 and 6 present these statistics for the inner city sample. Comparing families on these two types of benefits shows much smaller differences in background characteristics. The proportion of non-white children however, is larger among families receiving FC, and is highest in the inner city sample, as ethnic minorities were targeted for this sample. Children in the inner city sample are more likely to have three or more siblings compared to the original sample, and parents who receive FC are more likely to be married. The gradient in social class and mother's education found in columns 1 and 2 is now less clear, although we still observe the treated in both samples to be slightly better-off than the controls, and the inner city sample to be less wealthy compared to the original sample. Maternal employment is similar across the different samples, but paternal employment is higher among those on FC.

Figure 3 presents the trend in the number of individuals on FC and IS as a proportion of the full sample for the period 1972 – 1994. The left and right panel show this for the original and inner city sample respectively. The two vertical lines represent the two policy reforms. The figure shows an increasing proportion of claimants over time for both FC and IS, especially after 1980. However, this does not imply that take-up rates are endogenous to the policy; in other words, families did not start claiming benefits because of changes in eligibility rules for school meals. Instead, the increase in the number of beneficiaries is mainly due to a substantial rise in those living in poverty: the percentage of poor persons in the population rose sharply from about 6 percent in 1977 to over 20 percent in the early 90s. Poverty rates among households with children also increased: one in twelve children were poor in 1979, rising to one in four in 1995/6 (Burgess and Propper, 2002).

5.2.2. Trends in school meal consumption

Figure 1a shows the mean drop in school meal take-up rates due to the reforms. There is much variation however, around this average. With the drop in take-up rates positively related to the price increase of meals, I proxy the extent of the price increase by ranking the (area-level) proportion of switchers, creating a categorical variable that indicates the quartiles of its distribution. Table 3 presents the mean and standard deviation for each of these four quartiles. This shows that, on average, 0.4% of children decided against school meals in the first quartile, and 46% in the last, with an overall mean of 22.6%. The response was slightly larger for the 1988 reform.

5.2.3. Trends in BMI

To examine the common time trend assumption in BMI between the treated and controls, Figure 4a plots the average BMI's by year and treatment status for the 1980 reform. Although the control

group is slightly more volatile, as the DD will take the average BMI over the pre- and post-reform period, this volatility is not likely to affect the estimates. In fact, the linear prediction of BMI pre and post reform shows almost parallel lines, suggesting that the two groups have a common time trend. Figure 4b shows the same for the 1988 reform, combining the original and inner city sample for brevity.

6. Results

6.1. The 1980 Reform

The main results are reported in Table 4. In addition to the treatment indicator D_i and a dummy indicating the introduction of treatment I_{it}, the regressions control for all covariates discussed in section 5. Focussing first on column 1, the negative estimate suggests there is a decrease in BMI post-reform for the treated relative to the controls. However, the point estimate is small and it is not significantly different from zero.

The estimated ITT is the population effect of withdrawing school meals. However, the Local Average Treatment Effect (LATE) – the impact of the policy on those who changed their choice of lunch due to the price change – might also be of interest. This is obtained by dividing the ITT by the proportion of the treated that switch from school meals to other lunches, as given in Table 3.[10] This suggests that the BMI difference from pre to post treatment is 0.146 BMI-points (-0.033/0.226) smaller for those who changed their lunch-choice because of the reform, relative to the controls. For a child of average height, this is similar to 166 grams at age 4, to 302 grams at age 11; a trivial amount.

As shown in Table 2, the treated are different in terms of background characteristics compared to the controls. Because of the relatively small sample size of the control group, I use PSM to obtain a sample of treated that is more comparable to the controls, rather than obtaining a group of controls that is similar to the treated. Imposing common support in the kernel matching only slightly reduces the number of observations, but the weight given to the vast majority of those not on benefits is close to zero: 90% of observations were given a weight below 0.3. The largest weight given was 6, meaning that some of those not on benefits are weighed quite heavily in the analysis on the matched sample and thus that the analysis will be based on a specific group of children.

This can be problematic, because these children might in practice still receive school meals. As the Department of Health report (1989) mentions, between 20 and 30% of free school meal children in 1983 received them under the discretion granted to LEAs. This could mean that – if these children

[10] Table 3 gives the area-level proportion, though this is very similar to the proportion when using the individual-level data.

are mainly matched to those on benefits due to (presumably) very similar observable characteristics – the PSM is comparing children with the same eligibility. Given that an ITT analysis looks at effects of changes in eligibility rather than actual treatment, the ITT would be close to zero. Column 2 in Table 4 shows no large differences in the estimated ITT before and after matching, with a LATE ranging from 339 grams at age 4 to 622 grams at age 11.

Table 5, column 1, presents the results for the analysis in which D_i, I_{it}, and $D_i * I_{it}$ are interacted with a categorical variable proxying the extent to which the area experienced a price increase. The findings suggest a slight increase in the estimate for areas with larger price increases. However, all estimates are close to zero, with none being significant.

Hence, I find no evidence of a change in BMI following the withdrawal of fixed-price school meals for those not on benefits. Given the substantial drop in school meal take-up rates due to the reforms, this suggests that the lunches provided by parents were similarly nutritious compared to the previous government-subsidised school meals, with state-provided school meals crowding out private provision.

6.2. The 1988 Reform

The treated in the 1988 reform are those claiming FC; the controls are those on IS. This means that the sample sizes are smaller compared to the previous analysis, though still significantly larger than other studies that distinguish between these types of benefit-claimants (e.g. Dorsett and Heady, 1991; Fry and Stark, 1993).

The main results are reported in Table 6 for the original sample (column 1) and the inner city sample (column 2). Both show a negative ITT, indicating that the change in BMI for the treated is less compared to that of the controls. However, the point estimates are small and they are not significantly different from zero.

Columns 2 and 3 of Table 5 present the results for the analysis in which D_i, I_{it}, and $D_i * I_{it}$ are interacted with a categorical variable proxying the extent to which the area experienced a price increase. Although there are some significant differences, with the treated in the original sample gaining less weight compared to the controls, this is not found for the inner city sample, and there are no patterns in the data that would suggest that the reforms caused differential changes in BMI between the treated and controls in areas with differential price changes.

Note however, that the proxy for the price change in Table 5 is based on a *choice*: the proportion of children moving away from school meals. If the introduction of CCT in 1988 also caused a drop in the

quality of meals in primary schools, this may have affected the decision to switch to packed or home lunches. In other words, there may be multiple factors affecting the change in choice of lunch. A large drop in meal take-up rates may – for example – be due to a decrease in meal quality, as well as due to a price increase. A decrease in meal quality however, would have mainly affected those who remained eligible: those on IS. This may in turn lead to them gaining more weight compared to those on FC, rather than those on FC to lose weight or remain constant. Although the data does not allow me to examine the potential change in meal quality, I attempt to address the issue in a different way. First, I compare FC children to all those *not* on benefits (i.e. the treated-group in the 1980 reform), since they do not experience any change in school meal provision. Those on IS are excluded from this analysis, since they are neither treated, nor control. Second, I compare those on IS to all those not on benefits, omitting those on FC.

The results are shown in columns 1-2 (FC vs. not on benefits) and 3-4 (IS vs. not on benefits) of Table 7. They suggest that the BMI of FC as well as IS claimants increased slightly following the 1988 reform, with the inner city sample showing a significant increase for those on IS, relative to those not on benefits. The magnitude of the coefficient however, is small. Although this may suggest that children on both types of benefits gained slightly more weight compared to those not on benefits, this is unlikely to be due to a specific change in the school meal provision, as one group continues to consume school meals and the other switches to home and packed lunches. Hence, I find no evidence that the 1988 reform affected children's growth patterns. These analyses provide suggestive evidence that government-provided meals crowded private provision of lunches.

Another potential explanation for finding no differences in child BMI however, is that parents (and particularly mothers) may have cut back on their own food consumption to ensure adequate nutrition for their children. To specifically explore such intra-household reallocation, I investigate the effects of the reform on parental BMI, controlling for child BMI. As data on parental weight were not collected during the years 1977-1981, I can only examine this for the 1988 Act. However, this is likely to be the more relevant reform of the two, as both the treated and controls in 1988 are on benefits, and are therefore more likely to be affected financially by the reform.

The findings are presented in Table 8.[11] The first row gives the average ITT, showing that fathers' BMI decreased post reform in the original sample. However, this is not replicated in the inner city sample, and there is no evidence that mothers' BMI changed, neither for the original, nor for the inner city sample. The existing literature argues that it is mainly the mothers who give up food for their family. These analyses do not show any evidence that this occurred following the 1988 reform. In addition,

[11] Sample sizes are somewhat smaller due to missing values in parental BMI.

when distinguishing between the extent of the price increase, estimates show both positive and negative effects, without any changes or patterns in parental BMI that are consistent with the theory of intra-household reallocation of resources. These findings therefore do not suggest that any intra-household reallocation of foods can explain the non-result found for children. In other words, the government-provision of school meals may have been somewhat out of date, substituting for similarly nutritious foods provided by parents.

6.3. Robustness

I examine the robustness of the above findings to various different model/sample specifications and assumptions. First, I examine the timing issues related to the introduction of the reforms. More specifically, the effect of the Acts on child growth might not be measurable immediately. The Acts were introduced in April of 1980 and 1988. Schools were visited for data collection in the months April to December. Children who stop being eligible for free meals in April and are weighed and measured within the next few months may not have shown any changes relative to their peers who remain on free school meals. More generally: it takes time to gain or lose weight. Any effects of the policy change might, as a result, not be picked up by the DD described above. I attempt to deal with these timing issues by defining 1981 as the start of the treatment period (dropping 1980). Similarly for the 1988 reform, I define the start of the treatment period to be 1989, omitting 1988 from the regressions. The results (available upon request) are similar to those shown above.

Second, to investigate changes in children's BMI, I estimate an alternative DD model: one that explores whether there has been a change in the *trend* in BMI pre and post reform. The results again show no significant differences in trends for treated and controls.

Third, I use several subgroup analyses to examine potential heterogeneous effects across different groups. To explore whether the child's gender and age matter, I run the analyses separately by gender and age groups. In addition, I investigate whether there are differential effects for different lengths of exposure by interacting *D*, *I* and *D*I* with the exposure length. I also explore different dependent variables: child weight (controlling for height and height squared), the age and sex-adjusted overweight and underweight status, log-BMI, and tricep skinfold thickness. The results (available upon request) are similar.

Finally, I use the 1970 British Cohort Study (BCS) to explore whether the withdrawal of fixed-price school meals in 1980 affected children's nutritional intake. The BCS follows up everyone living in the

UK who was born between 5 and 11 April 1970.[12] Their height, weight and benefit status are observed at age 10 (in 1980) and age 16 (1986). Assuming that it takes time to gain or lose weight, I code the years 1980 and 1986 as pre- and post-treatment respectively. Those not claiming benefits are the treated; those claiming FC and/or IS are controls. I cannot study the second policy reform with this cohort, as the majority of children will have left school by 1988 and thus are not consuming school meals. Although the BCS only includes a limited number of children on benefits, the results (available upon request) do not show evidence of differential growth between treated and controls.

7. Conclusion

The withdrawal of fixed price and free school meals in 1980 and 1988 respectively caused a considerable drop in school meal take-up among those affected by the reforms. Children responded to these Acts by substantially increasing their consumption of packed and home lunches. The main concern about these reforms at the time related to children's *under*-nutrition: as school meals consisted of one-third of children's daily nutritional intakes, nutritionists expected to see a decline in children's nutritional status.

The aim of this paper is therefore to explore whether the state provision of school meals substituted for, or crowded out, any private provision. In other words, I examine whether abolishing state provision affected child BMI. With that, I shed light on a long and ongoing debate about public vs. private sector provision of goods and services.

As these data are arguably not representative of today's society due to the large changes in the nutritional environment since the 1980s, I do not wish to extrapolate these results to the current context. In addition, there may have been other advantages of the government-provided school meals that I cannot examine here, such as better student concentration and cognitive performance. However, I argue that the years in which the data were collected provide a unique opportunity to explore crowd out. The period studied was a one of – for some – relative scarcity of foods. At the time of the reforms, nutritionists believed that parents were not able to provide similar nutritious lunches, leading to under-nourished children. There was a large opposition to the reforms, which was reinforced by the substantial drop in school meal take-up rates.

The Acts placed further constraints on some families' already tight food budgets. Hence, if this affected their food purchases, the analysis should be able to detect this. The findings however, show that the treated in both reforms did not gain or lose weight quicker than the controls. In addition, I find no consistent evidence of intra-household reallocation of resources. As some of those affected

[12] For a more thorough description of these data and documentation, see: http://www.cls.ioe.ac.uk/

by the reforms are relatively poor, and as the sample sizes are often large with fairly precise estimates, the analysis should have been able to detect any effects on child and parent BMI. With no such evidence, this suggests that the state provision of school meals was crowding out any private provision of similarly nutritious packed and home lunches.

However, the period studied here is also generally perceived to be the start of the obesity epidemic. Hence, changes to the food environment in the treated and/or control group may have concealed any effect of the Acts. For example, there has been an increase in food choice and a relative decrease in food prices over the period studied here. This would have shifted households' budget constraints to the right, allowing them to increase their food consumption and compensate for any potential declines in food intake due to the Acts. If changes in the food environment relating to the reforms contributed to the longer term trend of childhood obesity however, the long time-series of these data would have been able to pick this up. I find no evidence of this, suggesting that the Acts did not decrease, neither did they increase children's and parents' BMI.

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Appendix A: Income Support and Family Credit

Income Support

Supplementary Benefits (SB), the predecessor of Income Support (IS), is the state 'safety net' and was in place between November 1966 and March 1988. Eligible claimants – those whose income ('*resources'*) is less than their needs ('*requirements'*) – receive the difference between the two. *Resources* consist of earnings net of tax, National Insurance and work expenses. Other contributory benefits, like unemployment benefits, also count as resources. *Requirements* consist of three elements. First, a part that varies with the number of adults and the ages of children. Second, a component consisting of net mortgage interest payments, water rates and ground rent. And third, an element consisting of small additions for heating for the over-65s, those with young children and the sick (Fry and Stark, 1993).

SB was replaced by IS in 1988. The targeting of IS moved resources towards families with children, and away from pensioners, single people and others without children. For IS, this was achieved through higher child scales, balanced by reductions in housing benefits. The abbreviation 'IS' is used to refer to both SB and IS.

Although the amount of the allowance depends on the number of adults and children in the family, a rough calculation can be given. For a two-parent family in 1980, both unemployed and with one 9-year-old child, the weekly allowance would be £41.90.[13] Any extra benefits received (like sickness benefits or child benefits) are subtracted from this amount.

Take-up rates for IS remained very stable in the 1980s. In 1984 and 1987, this was 82 and 81% respectively, with the highest take-up for families with children. Rates decreased somewhat post-1988: out of those eligible for IS in 1989 and 1990, an estimated 73 and 75% took them up. Breaking these figures down by family type however, shows that the decline in take-up was greatest for those without children (Fry and Stark, 1993).

Family Credit

Family Income Supplement (FIS), the predecessor of Family Credit (FC), was in use between August 1971 and March 1988. The abbreviation 'FC' is used to refer to both FIS and FC. FC is payable if (joint) gross income falls short of a 'prescribed amount', which depends on the number and (from

[13] Ignoring any housing costs, heating additions and other benefits for simplicity. A married couple would receive £34.60 per week as 'normal requirements' plus £7.30 for a child under 10.

November 1985) ages of children. Income excludes child benefit, one-parent benefit and housing benefit. FC is 50% of the difference between income and the 'prescribed amount', up to a maximum weekly payment, which itself depends on the number and ages of children. With the government's aim in 1988 to target families with children, the generosity of FC increased, balanced by reductions in housing benefits.

To give a rough idea of the amount of the benefit, we take a similar two-parent family with one 9-year-old child. The head of the household is in full-time employment, but earns less than the 'prescribed amount' (£67 per week for a one-child family in 1980). The maximum weekly payment for this family is £17.[14]

For 1984 and 1987, Fry and Stark (1993) report an FC take-up rate of 56 and 57%. However, they note that these numbers should be taken with caution, as the sample sizes in their data are small. Take-up in 1989 was estimated to be 50%.

[14] A simple back-of-the-envelope calculation for the effect of the 1988 Act shows an approximate annual increase in costs for households on FC of £83.40 per child (based on a 40-week school year, school meals costing 50p per day (£2.50 per week), and a compensatory amount added to the FC benefit of 44p per week: £100 - £17.60 = £ 83.40). This is almost five times the maximum weekly FC payment for this two-parent family.

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Tables and Figures

Table 1: Sample sizes by year and age

	Age								
Year	4	5	6	7	8	9	10	11	Total
1972	137	932	908	1,009	977	918	1,001	646	6,528
1973	116	776	756	777	832	768	748	533	5,306
1974	94	727	758	745	771	792	750	502	5,139
1975	98	676	720	721	736	742	763	522	4,978
1976	118	719	702	723	757	716	757	520	5,012
1977	205	1,027	1,136	1,097	1,159	1,152	1,170	769	7,715
1978	196	1,035	1,076	1,214	1,163	1,250	1,216	782	7,932
1979	178	955	1,060	1,118	1,243	1,173	1,233	775	7,735
1980	189	831	901	1,008	1,038	1,136	1,073	736	6,912
1981	138	774	809	865	977	972	1,094	709	6,338
1982	172	833	913	913	966	1,063	1,098	778	6,736
1983	142	913	928	971	1,017	1,057	1,103	912	7,043
1984	207	1,024	993	975	1,010	1,042	1,063	748	7,062
1985	171	1,023	1,063	1,031	972	929	998	837	7,024
1986	189	1,051	1,131	1,026	1,035	960	981	666	7,039
1987	182	1,006	1,098	1,080	1,045	968	911	706	6,996
1988	262	1,097	1,135	1,183	1,159	1,087	1,046	647	7,616
1989	214	908	979	1,056	1,031	1,005	911	581	6,685
1990	164	1,082	1,115	1,108	1,119	1,165	1,123	690	7,566
1991	171	983	1,101	1,033	1,034	1,071	1,045	716	7,154
1992	210	1,092	1,083	1,087	1,031	982	1,002	684	7,171
1993	179	980	1,037	1,002	1,001	914	937	684	6,734
1994	224	994	1,011	1,045	982	912	945	647	6,760
Total	3,956	21,438	22,413	22,787	23,055	22,774	22,968	15,790	155,181

Table 2: Means (standard deviations) for the different samples by treatment status

	1980 Act (1972 – 1986)		1988 Act Original Sample (1980 – 1994)		1988 Act Inner City Sample (1980 – 1994)	
	(1) **Treated:** No benefits	(2) **Controls:** FC/IS	(3) **Treated:** FC	(4) **Controls:** IS	(5) **Treated:** FC	(6) **Controls:** IS
Body Mass Index (BMI)	16.13	15.93	16.10	16.08	15.84	15.90
	(1.87)	(1.65)	(1.96)	(1.85)	(2.19)	(2.07)
Girl	0.490	0.495	0.504	0.483	0.519	0.490
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Non-white	0.014	0.044	0.127	0.064	0.567	0.475
	(0.12)	(0.20)	(0.33)	(0.24)	(0.50)	(0.50)
Birth weight	3330	3237	3266	3232	3151	3130
	(528)	(551)	(552)	(558)	(567)	(578)
1 older sibling	0.352	0.286	0.308	0.312	0.282	0.274
	(0.48)	(0.45)	(0.46)	(0.46)	(0.45)	(0.45)
2 older siblings	0.139	0.163	0.178	0.152	0.183	0.169
	(0.35)	(0.37)	(0.38)	(0.36)	(0.39)	(0.37)
3 or more older siblings	0.084	0.175	0.151	0.128	0.225	0.224
	(0.28)	(0.38)	(0.36)	(0.33)	(0.42)	(0.42)
1 younger sibling	0.407	0.364	0.377	0.396	0.333	0.358
	(0.49)	(0.48)	(0.48)	(0.49)	(0.47)	(0.48)
2 younger siblings	0.168	0.228	0.299	0.322	0.300	0.306
	(0.37)	(0.42)	(0.46)	(0.47)	(0.46)	(0.46)
3 or more younger siblings	0.045	0.124	0.241	0.203	0.341	0.297
	(0.21)	(0.33)	(0.43)	(0.40)	(0.47)	(0.46)
Married	0.963	0.622	0.773	0.564	0.773	0.581
	(0.19)	(0.48)	(0.42)	(0.50)	(0.42)	(0.49)
Mother's height	162.4	161.4	162.5	162.3	162.2	161.9
	(7.03)	(7.41)	(7.58)	(7.70)	(8.26)	(8.29)
Father's height	175.2	173.6	173.6	174.0	170.3	170.7
	(7.58)	(8.29)	(8.43)	(8.79)	(9.70)	(9.60)
Social class II	0.212	0.091	0.119	0.108	0.083	0.057
	(0.41)	(0.29)	(0.32)	(0.31)	(0.28)	(0.23)
Social class III non-manual	0.092	0.059	0.076	0.060	0.053	0.044
	(0.29)	(0.24)	(0.27)	(0.24)	(0.22)	(0.21)
Social class III manual	0.438	0.444	0.442	0.440	0.347	0.330
	(0.50)	(0.50)	(0.50)	(0.50)	(0.48)	(0.47)
Social class IV	0.139	0.194	0.217	0.184	0.252	0.217
	(0.35)	(0.40)	(0.41)	(0.39)	(0.43)	(0.41)
Social class V	0.035	0.135	0.075	0.115	0.070	0.115
	(0.18)	(0.34)	(0.26)	(0.32)	(0.26)	(0.32)
Armed Forces	0.029	0.057	0.059	0.078	0.186	0.228
	(0.17)	(0.23)	(0.24)	(0.27)	(0.39)	(0.42)
Mother's educ: Comm/Techn	0.135	0.069	0.127	0.110	0.103	0.089
	(0.34)	(0.25)	(0.33)	(0.31)	(0.30)	(0.28)
Mother's educ: Univ/Prof	0.109	0.056	0.047	0.054	0.029	0.020
	(0.31)	(0.23)	(0.21)	(0.23)	(0.17)	(0.14)
Mother's educ: Other	0.018	0.020	0.009	0.006	0.000	0.000
	(0.13)	(0.14)	(0.09)	(0.08)	(0.02)	(0.02)
Mother works	0.377	0.388	0.661	0.615	0.591	0.476
	(0.48)	(0.49)	(0.47)	(0.49)	(0.49)	(0.50)
Father works	0.928	0.506	0.700	0.469	0.694	0.487
	(0.26)	(0.50)	(0.46)	(0.50)	(0.46)	(0.50)
N	74019	10413	3497	8961	3739	15070

Table 3: The mean (standard deviation) change for each of the four quartiles of the (area-level) distribution of the proportion of treated moving away from school meals due to the reforms

	(1) 1980 reform		(2) 1988 Reform, Original sample		(3) 1988 Reform, Inner city sample	
Q1, No change	0.004	(0.087)	0.014	(0.103)	0.050	(0.061)
Q2, Small change	0.139	(0.033)	0.299	(0.016)	0.200	(0.025)
Q3, Medium change	0.282	(0.054)	0.359	(0.029)	0.357	(0.054)
Q4, Large change	0.463	(0.080)	0.502	(0.045)	0.497	(0.061)
Overall mean	0.226	(0.191)	0.309	(0.191)	0.283	(0.178)

The "No change", "Small change", "Medium change" and "Large change" refer to the 1st, 2nd, 3rd, and last quartile of the distribution of the drop in school meal take-up rates by area.

Table 4: DD estimates, no benefits (treated) vs. FC/IS (controls), the 1980 reform

	(1) No PSM	(2) PSM
ITT	-0.033	-0.067
	(0.041)	(0.054)
R^2	0.15	0.16
Observations	84432	84408

Notes: All regressions include a dummy for being treated (*D*) and an indicator for the introduction of treatment (*I*). The analyses also control for: area fixed effects, all interactions between age and year, gender, birth weight, a dummy for being non-white, married, indicators for having 1, 2, and 3 or more older and younger siblings, mother and father's height, social class, mother's education, and mother's and father's work status. *p<0.10, **p<0.05, ***p<0.01; robust clustered standard errors in parentheses.

Table 5: DD estimates by the extent of the price change, proxied by the proportion of children changing lunch

	(1) 1980	(2) 1988 Original sample	(3) 1988 Inner city sample
ITT, No change	-0.025	0.273	-0.149
	(0.064)	(0.168)	(0.163)
ITT, Small	-0.141	-0.687***	0.147
	(0.115)	(0.240)	(0.250)
ITT, Medium	0.072	-0.080	0.214
	(0.115)	(0.233)	(0.214)
ITT, Large	0.078	-0.374*	-0.060
	(0.112)	(0.221)	(0.209)
R^2	0.15	0.17	0.20
Observations	84432	12458	18809

Clustered standard errors in parentheses; the analyses include the covariates mentioned in the note to Table 4. The "No change", "Small", "Medium" and "Large" refer to the 1st, 2nd, 3rd, and last quartile of the distribution of the drop in school meal take-up rates by area.

Table 6: DD estimates, FC (treated) vs. IS (controls), the 1988 reform

	(1) Original sample	(2) Inner city sample
ITT	-0.008	-0.075
	(0.081)	(0.075)
R^2	0.17	0.20
Observations	12458	18809

Clustered standard errors in parentheses; the analyses include the covariates mentioned in the note to Table 4.

Table 7: DD estimates, FC vs. no benefits, and IS vs. no benefits, the 1988 reform

	FC vs. no benefits		IS vs. no benefits	
	(1) Original sample	(2) Inner city sample	(3) Original sample	(4) Inner city sample
ITT	0.079	0.048	0.069	0.099**
	(0.072)	(0.075)	(0.048)	(0.045)
R^2	0.18	0.20	0.18	0.19
Observations	54239	26566	59703	37897

Clustered standard errors in parentheses; the analyses include the covariates mentioned in the note to Table 4.

Table 8: DD estimates for parental BMI, average and by the extent of the price change

	(1) Original sample		(2) Inner city sample	
	Mother's BMI	Father's BMI	Mother's BMI	Father's BMI
Average ITT	-0.046	-0.435**	0.142	-0.117
	(0.240)	(0.213)	(0.204)	(0.200)
R^2	0.09	0.08	0.08	0.07
ITT, No change	-0.088	-0.184	0.236	-0.745
	(0.480)	(0.486)	(0.471)	(0.454)
ITT, Small	0.172	0.159	0.188	1.545**
	(0.713)	(0.669)	(0.651)	(0.603)
ITT, Medium	0.363	-0.201	-0.462	0.988
	(0.698)	(0.641)	(0.594)	(0.604)
ITT, Large	-0.170	-0.811	-0.095	0.115
	(0.629)	(0.611)	(0.603)	(0.575)
R^2	0.10	0.13	0.09	0.17
Number of observations	8377	6320	11949	9193

Note: The dependent variable is maternal or paternal BMI. Clustered standard errors in parentheses. the analyses control for the child's BMI and include the covariates mentioned in the note to Table 4. The "No change", "Small", "Medium" and "Large" refer to the 1st, 2nd, 3rd, and last quartile of the distribution of the drop in school meal take-up rates by area.

Figure 1: Trends in the take-up of school meals and packed lunches by benefit status

Figure 1a

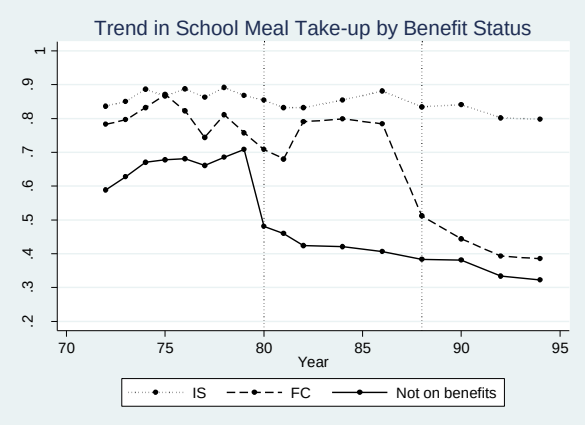


Figure 1b

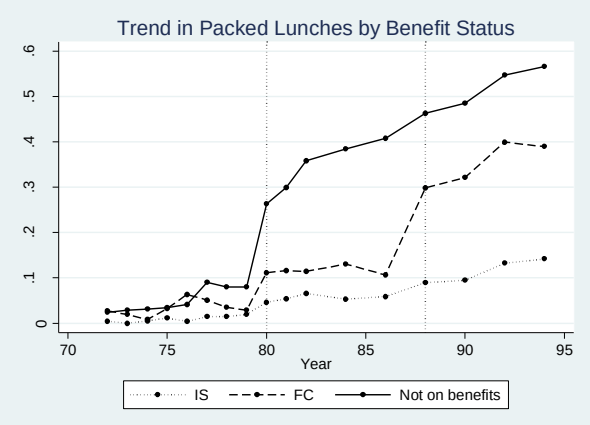


Figure 2: Theoretical impact of a price increase on food consumption

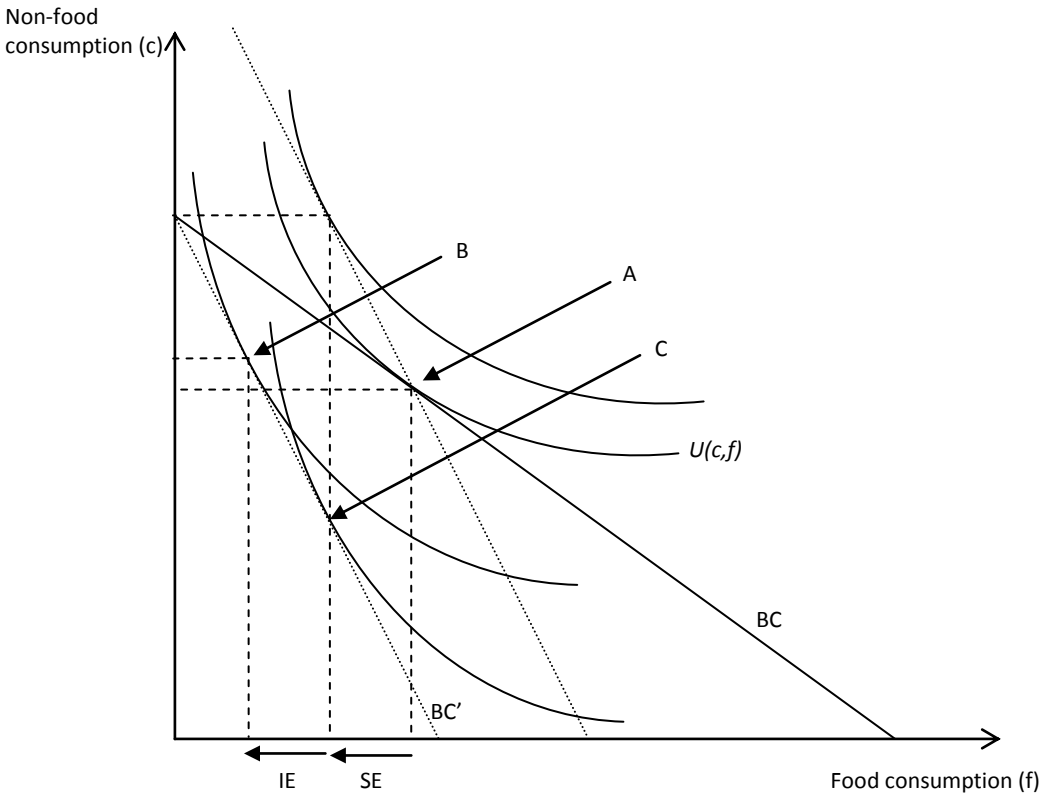


Figure 3: Proportion of IS and FC recipients over time

Figure 3a

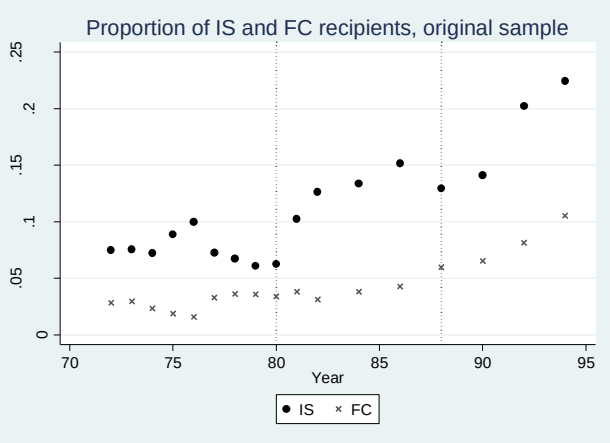


Figure 3b

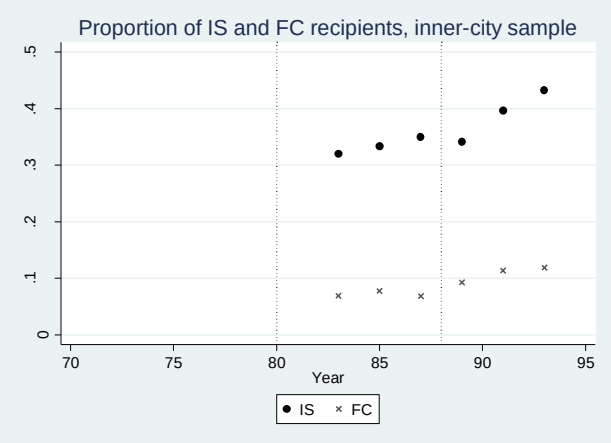


Figure 4: BMI by year and benefit status

Figure 4a

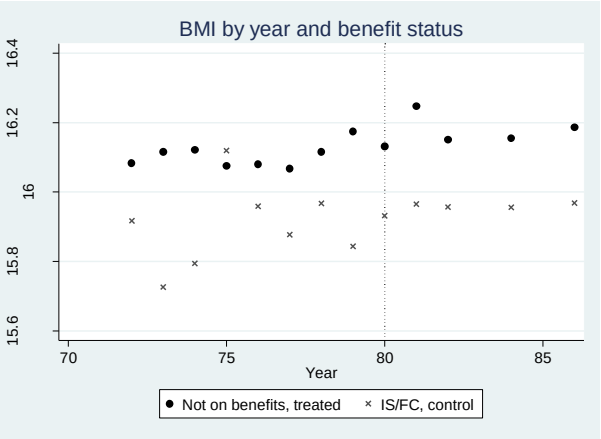
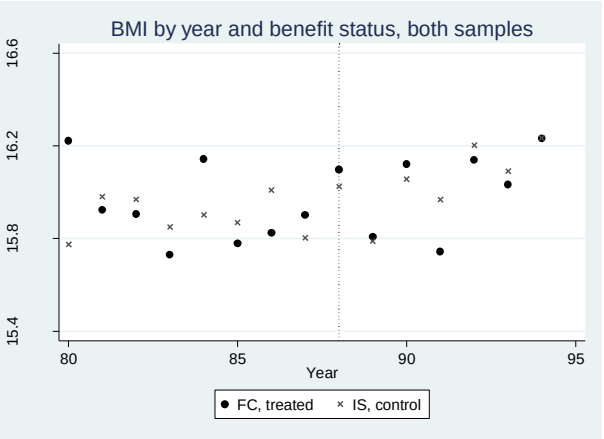


Figure 4b



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