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The impacts of Universal Free School Meal schemes in England

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Summary

Means-tested Free School Meals (FSM) have been available in England since 1944. These are intended to benefit children in terms of their health and ability to learn, and to ease financial pressures on their families. More recently, in some places the offer of a free school lunch has been extended to all children in some year groups. The Universal Infant Free School Meal (UIFSM) scheme was introduced across in England in September 2014, for children in their first three years of primary school, and all primary school children in London are being offered a free meal in the 2023/24 academic year. Before this, several local authorities in London had rolled out their own Universal Free School Meal (UFSM) schemes between 2010 and 2014 for children up to age 11.

In this report we evaluate the impact of these local authority UFSM schemes on:

- Take-up of school meals.
- Children's diet-related health, as measured by obesity prevalence and body mass index (BMI) classifications.
- Children's educational attainment, as measured by test scores in Reading and Maths at the end of primary school.
- Children's rates of absence from school.
- Patterns of household food expenditure.

The report builds on the authors' previous work funded by the Nuffield Foundation, which showed that the national UIFSM policy reduced obesity rates among children in their first year in primary school, and reduced household food expenditure (Holford and Rabe, 2020, 2022). Investigating the impacts of local authority-run UFSM schemes adds to the evidence by providing results for a wider range of outcomes, for children at older ages, and for children exposed to free meals over a longer time-period.

Data

We use a variety of existing secondary data sources appropriate to the outcomes being studied:

- For children's **BMI classifications** we use school-level data from the National Child Measurement Programme (NCMP).
- For **attendance or absences** (for health and other reasons) we use individual data on counts of half-day absences from school by reason, from the National Pupil Database (NPD).
- For educational **attainment** we use individual data on performance in Key Stage 2 (KS2) assessments at the end of primary school from the NPD.

For all of these we combine our data with school-level information from the Department for Education and local authority-level labour market and demographic characteristics from the Office for National Statistics (ONS).

- For **household expenditure** we use as *Understanding Society*, the UK Household Longitudinal Study (UKHLS), linked with local authority-level ONS data.
- For **take-up of means-tested FSM** we use school-level information from the Department for Education's 'Schools, Pupils and their Characteristics' releases.

Methods

All our results are based on the **difference-in-difference** method. This compares changes in outcomes from before to after UFSM was introduced in the 'treated' local authorities, with changes in outcomes over the same period in 'control' local authorities that never introduced UFSM. The key assumption of this method is of 'parallel trends': That the change in outcomes in the control group is a good guide to what the change in outcomes *would have been* in the treated group, had the UFSM schemes never been introduced.

The four treated local authorities are all in London. We show results for two alternative control groups: the rest of Greater London, and the rest of England. We show that for our key outcomes, the assumption of parallel trends holds after **controlling for observable characteristics** of each child, school and local authority, and either child, school or local authority '**fixed effects**' to control for unobservable differences that are constant over time.

All our results are 'intention to treat' estimates, meaning they represent the average effect of being entitled to a universal free school meal, compared to a situation where around 20% of children are eligible for means-tested free school meals. This does not distinguish between effects on those actually taking up a universal free school meal and not.

Main results

Take-up of school meals:

- Providing FSM on a universal, rather than means-tested basis, increased take-up of school meals among those already-eligible for FSM under means-testing by 1.3-1.6 percentage points.
- This is equivalent to shifting around 8% of non-takers into taking up their entitlement, despite this group facing no change in price, suggesting that the universal entitlement increased the attractiveness of taking a school meal for this group.
- Data on take-up of school meals by not-FSM-eligible children are patchy, but suggest an increase of around 50 percentage points.

- We estimate that introducing UFSM led to one in three children newly eating a school lunch.

Children's BMI classifications:

- In Reception, entitlement to UFSM reduces obesity prevalence on average by approximately 1-1.5 percentage points (from a base of 14% in the treated local authorities). This represents a 7-11% reduction in obesity prevalence.
- In Year 6, obesity prevalence is reduced on average by approximately 0.6-1.2 percentage points (from a base of 25%). This represents a 2-5% reduction in obesity prevalence.
- Effects are smaller in Year 6 than in Reception, despite Reception children all having received UFSM for less than a year at the time of weight measurement, while Year 6 children received UFSM between less than a year and up to seven years, depending on when UFSM were introduced in their local authority. This is consistent with bodyweights being harder to shift for older children.
- The effects of UFSM on Year 6 children are biggest (1.2-2.1 percentage point reduction in obesity) for those who received UFSM throughout primary school, from Reception onwards. This represents a 5-8% reduction in obesity prevalence.
- The beneficial impacts of UFSM did not extend to the schools with the highest pre-existing obesity prevalence. This indicates that additional support will be needed in more challenging environments.

Household expenditure:

- UFSM unambiguously helps households with the cost of living, because those who previously purchased a school meal no longer have to pay, and those who previously assembled a packed lunch no longer need to purchase the food items for the child's lunchbox. This does not account for the value of time saved by parents.
- Our analysis suggests that households reallocated these savings towards spending on non-food items, rather than increasing the quality or quantity of food purchased at supermarkets or eaten out.

Absences:

- The availability of UFSM did not materially affect children's absences from school, either in terms of days missed (overall or for illness or medical appointment) or longer-term engagement (share of children who are 'persistent absentees').

Educational attainment:

- Availability of UFSM improved children's Reading test scores at age 11 by approximately 4% of a standard deviation. This effect size is equivalent to approximately two weeks additional progress in school. This corresponds to a 'small' effect size in educational trials, but is not trivial, at around half the magnitude of the impact of two years' exposure to the 'Literacy Hour', for example.
- We do not find consistent evidence of an impact of UFSM on children's Maths test scores.
- Effects on Reading are similar for pupils registered for means-tested FSM, who see no change in the price of their lunches, and those not registered for means-tested FSM, who become newly entitled to a free lunch, and for whom the rise in take-up was likely to be much larger. Finding similar impacts for both groups supports there being benefits from universal provision not just driven by individual take-up. For example, the change in nutritional intake may mean children newly taking up a UFSM exhibit less disruptive behaviour. This may also benefit other children in their class.

Implications for policy and practice

In summary, universal provision of FSM throughout primary school helps ensure that all children have access to a meal of high nutritional standards, yielding health and educational benefits for these children, while also providing cost-of-living support to families with school-age children.

Our results on educational attainment and take-up show that the switch to the universal environment improves the welfare of already FSM-eligible children from low-income households, despite them not being the targeted beneficiaries.

Our results on obesity prevalence suggest that starting free meal provision early and maintaining it throughout primary schools would maximise the impact on cutting obesity rates and would thereby best contribute to lowering the long-term healthcare and indirect productivity costs of obesity. However, the beneficial impact on BMI classifications being smaller in schools with higher pre-existing obesity prevalence indicates that UFSM will not necessarily reduce health inequalities, and additional support will be needed in more challenging environments.

Longer-term benefits of UFSM will rely on the health benefits persisting and a continued high take-up of high-quality school lunches. This will entail efforts to maintain an attractive and social school dining environment, and ensuring that funding for school food is maintained in real terms (i.e. after inflation) at a level sufficient to provide hot meals meeting nutritional standards, and covering associated utility and staff costs.

Full report: The impacts of Universal Free School Meal schemes in England

1 Introduction

Providing children from low-income backgrounds with a free lunch at school has long been an important policy in the UK, intended to benefit children in terms of their health and ability to learn, and to ease financial pressures on families. Means-tested Free School Meals (FSM) have been available to children whose parents receive certain qualifying benefits and/or meet income thresholds nationally since 1944, and 24% of primary-age children in state-funded schools were registered to receive these in late 2022/23. Children not claiming a means-tested FSM must bring a packed lunch to school or purchase a school meal at a cost of approximately £2.40.

A Universal Free School Meal (UFSM) offer instead makes a free school lunch available to *all* children in a given year-group. This has potential benefits in ensuring that all children have access to a nutritious meal and the associated benefits, removing any stigma or informational barriers associated with claiming means-tested FSM, and changing the school environment to a default in which all children eat together. A universal offer does, however, cost more, since it provides meals for free even to children whose families could otherwise afford to pay.

The Universal Infant Free School Meal (UIFSM) scheme was introduced across in England in September 2014, and made free lunches available on a universal basis to all children in their first three years of primary school. UFSM is offered across Scotland for children in the first five years of primary school, intended to be rolled out to all children in primary school in Wales by 2024, and for the 2023/24 academic year is available to all primary school children in Greater London. Before this, several local authorities in London had rolled out their own UFSM schemes: Newham (from 2010), Islington (from 2011), Southwark (from 2012) and Tower Hamlets (from 2014). All four local authorities offered UFSM to all primary-age children from September 2014 at the latest, thus providing UFSM to children up to age 11, compared with age 7 in the rest of England.

In this report we evaluate the impact of these local authority UFSM schemes on:

- Take-up of school meals.
- Children's diet-related health, as measured by obesity prevalence and body mass index (BMI) classifications.
- Children's educational attainment, as measured by test scores in Reading and Maths at the end of primary school.
- Children's rates of absence from school.
- Patterns of household food expenditure.

In all cases, we identify the impact of *offering* free lunches on a universal basis, not the impact of actually taking up the meals.

Benefits to health and educational attainment can arise because making school meals universally free for all children may improve the nutritional intake of children newly taking up a school meal who previously brought in a packed lunch from home. Audit studies suggest that school meals on average have better nutritional and lower calorific and ultra-processed content than lunches packed at home (Evans et al., 2022, Parnham et al., 2022a, 2022b). Benefits may also arise from the financial saving to households who previously purchased a school meal, that can be spent on other things; and because of the financial and time savings for households who previously assembled a packed lunch. We might expect further benefits to educational attainment, plus reductions in absences from school and increases in school meal take-up because a universal system could make school and lunchtimes more enjoyable. A changed diet might improve the behaviour of children, benefiting even children who do not change what they eat.

This report builds on the authors' previous work funded by the Nuffield Foundation, which showed that the national UIFSM policy reduced obesity rates among children in their first year in primary school, and reduced household food expenditure (Holford and Rabe, 2020, 2022). Investigating the impacts of local authorities' own UFSM schemes allows us to a) investigate the longer-run effects of providing free meals to all children, b) evaluate the impact on children at older ages and c) to consider the impact on educational attainment.[1]

2 The local authority schemes in detail

All four local authorities we study offer a free meal at lunchtime every school day to all children within a given cohort. However, the schemes were introduced independently from each other, and the exact timing of introduction and decision to provide to all primary-age children immediately or to roll it out over several years, was determined by local political and budgetary decisions.

Table 1 and Figure 1 summarise the schemes available in the different local authorities over time. Some introduced UFSM to younger year-groups first, others all in one go, but all now provide UFSM to all primary school children (Reception – Year 6, Age 4-11). This timetable of roll-out creates variation in age at first exposure and cumulative duration of exposure to UFSM by age 11, when our main outcomes are measured.

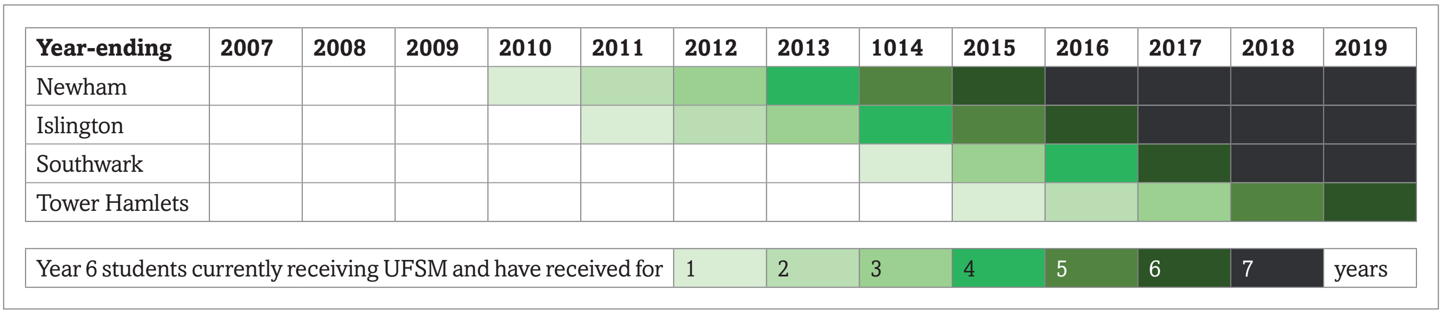
1 An early evaluation of a Department for Education Universal Primary Free School Meal (UPFSM) pilot, run in Newham and Durham in academic years ending 2010 and 2011 (Brown et al., 2012) showed that offering UFSM improved educational attainment at Key Stages 1 and 2, with pupils in the pilot local authorities making between four and eight weeks' more progress.

Table 1 **Local authority-based UFSM schemes**

Local authority	Academic years ending	Form of provision	Provider		Coverage
			DfE	LEA	
Newham	2010-2011	DfE pilot, UPFSM	R-6	–	R-6
	2012-2014	LEA scheme, UPFSM	–	R-6	R-6
	2015-present	LEA scheme, UJFSM (DfE UIFSM top-up)	R-2	3-6	R-6
Islington	2011-2014	LEA scheme, UPFSM	–	R-6	R-6
	2015-present	LEA scheme, UJFSM (DfE UIFSM top-up)	R-2	3-6	R-6
Southwark	2012	LEA scheme, UFSM for R-1	–	R-1	R-1
	2013	LEA scheme, UFSM for R-4	–	R-4	R-4
	2014	LEA scheme, UPFSM	–	R-6	R-6
	2015-present	LEA scheme, UJFSM (DfE UIFSM top-up)	R-2	3-6	R-6
Tower Hamlets	2014	LEA scheme, UFSM for R-1	–	R-1	R-1
	2015-present	LEA scheme, UJFSM (DfE UIFSM top-up)	R-2	3-6	R-6

Notes DfE: Department for Education. LA: local authority. R: Reception, the first year of primary school, and other figures for subsequent school years. UIFSM, UJFSM and UPFSM: Universal Infant (R-Year 2), Junior (Years 3-6) and Primary (R-Year 6) FSM respectively.

Figure 1 **Exposure of Year 6 children to UFSM for different durations**



Funding

Newham implemented a two-year pilot scheme for Universal Primary Free School Meals (UPFSM) in 2010, funded by the Department for Education and Department for Health (Callanan et al., 2012). Newham Council continued to fund the scheme after the matched funding ended. **Islington**, **Southwark** and **Tower Hamlets** introduced UFSM funded through their own budgets, without any central government support.

Aims

All four local authorities stated aims relating to **supporting households with children with the cost of living**. **Newham's** continuation of the scheme was to support 'significant numbers of families living in poverty, but who did not qualify for benefits related Free School Meals' (London Borough of Newham, 2020). **Islington** aimed to 'to ease the pressure on hard working families' (London Borough of Islington, 2019); **Southwark** to 'reliev[e] child poverty by putting money in parents' pockets' (London Borough of Southwark, 2011); and **Tower Hamlets** to support a population with high levels of child poverty, in an environment with reduced generosity of other central government-funded support (Rahman, 2013).

All four local authorities aimed to **improve children's health and educational outcomes**. The stated aims of the **Newham** pilot were to 'test the impacts of extended entitlement on pupils' eating habits at school and at home; pupils' Body Mass Index (BMI) and general health and well-being [and] pupils' behaviour, absences and academic performance'. (Brown et al., 2012, p. 14), and the programme was continued in the expectation that the healthier content of school meals improved children's concentration, performance and attainment (Gold, 2021). **Southwark** stated the policy was 'aimed at tackling [Southwark]'s extremely high levels of child obesity' (London Borough of Southwark, 2011). **Tower Hamlets** initially 'guarantee[d] a healthy lunch to get [children] off to a good start' (Rahman, 2013) and extended the scheme to ensure 'equal access to a nutritional meal at lunchtime, in order to promote a healthy lifestyle' (McCulloch-Graham, 2014) and **Islington's** stated aims were 'to support children's diet [and] to support children's learning' (London Borough of Islington, 2019).

3 Data and methods

Data

We use a variety of existing secondary data sources appropriate to the outcomes being studied:

- For **take-up of means-tested FSM** we use school-level information from the Department for Education's 'Schools, Pupils and their Characteristics' releases.
- For **children's bodyweights**, we study their **BMI** classifications using school-level data from the National Child Measurement Programme (NCMP) for 2006/07 to either 2013/14 (Reception) or 2018/19 (Year 6), supplied by NHS England. We do not study Reception outcomes from 2014/15 onwards, because for these years all children received UFSM through the national UFSM scheme. NCMP data are collected by trained nurses visiting schools to weigh and measure children in Reception (aged 4-5) and Year 6 (aged 10-11).[2] Data for school years with less than 20 children per cohort are suppressed from this dataset to minimise the risk of disclosure about small numbers of individual pupils. In our analysis we exclude schools that ever had less than 20 children per cohort, resulting in an analysis sample of approximately 10,000 schools per academic year.[3] We weight observations in proportion to the school's average cohort size over the analysis period.

All outcomes are defined with respect to the age-and-sex specific UK 1990 growth charts. We use thresholds for population monitoring, such that children with BMI above the 85th percentile among children of the same age and sex in 1990 are classified as overweight, and those above the 95th percentile as obese.[4] Each child's BMI is also converted to a 'z-score' indicating how many standard deviations their BMI is above or below the 1990 mean BMI for children of the same age and sex. The dataset also contains information on the timing of measurement within the school year, the ethnic and gender composition of children measured, and the school's quintile of FSM-registration and quintile of Income Deprivation Affecting Children Index (IDACI).

- For **educational attainment** we use individual data on scores achieved in Reading and Maths tests taken as part of Key Stage 2 (KS2) assessments, recorded in the National Pupil Database (NPD) for school years 2002/03 to 2018/19. In this analysis we use the population of children participating in each test. We link these data to the School Census in the NPD, which contains child-level characteristics such as ethnicity, sex, and eligibility for FSM.
- For **absences** from school we use individual data on counts of half-day absences, from the NPD for 2006/07 to 2018/19, for children in school years 1-6. We sum up each child's count of 'absence for any reason' and the sum of 'absences for illness' and 'absence for medical appointments' across the first five half-terms of the school year.[5] We convert this to the percentage of possible half-day sessions they could have attended that each child misses for any reason, and for illness or medical appointments. We also create a marker for the child being a 'persistent absentee', meaning that they missed more than 10% of sessions in the academic year. We also link these data to child-level characteristics from the School Census.
- For **household expenditure** we use the UK Household Longitudinal Study (UKHLS), also known as *Understanding*

2 Coverage was 80% in 2006/07 and consistently above 90% since 2008/09 (NHS Digital, 2018, 2019). Children may be not measured because of opt-out, absence from school on the day of measurement.

3 These exclusions mean our final estimation sample includes approximately 95% of schools in the treated local authorities, attended by approximately 97% of children in the treated local authorities. Our control groups include approximately 95% of schools in the rest of Greater London, and 76% in the rest of England. Our results are therefore representative of schools attended by the vast majority of treated pupils, but we cannot rule out that the impacts of UFSM would have been different in smaller schools.

4 We follow the practice of the Office for Health Improvement and Disparities and its predecessor Public Health England in using population monitoring thresholds as our outcome measure to evaluate the impact of UFSM across whole local authorities. Clinical thresholds for individual overweight and obese status are instead set at the 91st and 98th percentiles respectively. The population thresholds are designed to 'capture children in the population in the clinical overweight or obesity BMI categories and those who are at high risk of moving into the clinical overweight or clinical obesity categories [to] ensure that adequate services are planned and delivered for the whole population'. (Office for Health Improvement and Disparities, 2023. See also Public Health England, 2018).

5 Absences for the sixth and final half-term of the school year are not consistently recorded in the NPD for before 2012/13, so we exclude this half-term from our analysis.

Society, waves 1-11 (February 2009 to February 2020), restricting our sample to households with at least one child aged 0-15. The UKHLS measures expenditure on supermarket shopping (which includes some non-food items) and on eating out (which includes any money spent on meals at school), over the last four weeks, and records a rich set of household characteristics.

We combine all the above data sources with school and local authority-level data that helps us rule out that any observed impacts on children's outcomes are driven by changes other than UFSM eligibility:

- School-level information that is publicly available from the Department for Education, including shares of pupils eligible for FSM and of different ethnic backgrounds.
- Local authority-level information from the Office for National Statistics, including unemployment rates, population density, population shares of children aged 5-9, and prevalence of fast-food restaurants.

The exact set of control variables used in each model are documented in Appendix A2.

Descriptive statistics

Table 2 shows descriptive statistics of our outcome variables, and Table 3 shows descriptive statistics, from these combined sources of child, school and local authority-level characteristics of the treated London local authorities and comparison local authorities (the rest of London and the rest of England), in the period before any London local authority had introduced UFSM, for which we have complete data (years ending 2007-2009).

Table 2 indicates that in the pre-policy period, children in the local authorities that went on to introduce UFSM had educational attainment below those in the rest of London or

Table 2 Outcomes in London local authorities that implemented UFSM and comparison local authorities that never introduced UFSM

Local authority	London local authorities that later implemented UFSM				Comparison local authorities that never implemented UFSM	
	Newham	Islington	Southwark	Tower Hamlets	Rest of London	Rest of England
Educational attainment for Year 6 children						
Standardised Reading	-0.286	-0.150	-0.162	-0.166	-0.007	0.006
N children	*10,690*	*5,007*	*8,013*	*7,796*	*206,520*	*1,620,630*
Standardised Maths	-0.137	-0.104	-0.136	-0.038	0.031	0.003
N children	*10,848*	*5,032*	*8,041*	*7,825*	*207,396*	*1,627,617*
Absences for children in school Years 1-6						
Absences for any reason, % of all sessions	5.69	6.21	5.55	5.76	5.54	5.25
Absence for illness or medical appointments, % of all sessions	2.86	2.92	2.44	2.93	2.79	2.70
Persistent absentees, % of children	16.8	18.6	16.5	16.5	15.4	13.6
N child-years	*69,055*	*32,328*	*51,240*	*50,649*	*124,910*	*964,061*
School-level BMI classifications						
Reception overweight or obesity prevalence, %	25.8	23.6	27.8	24.7	23.3	22.7
Reception obesity prevalence, %	14.3	11.2	14.0	14.0	10.9	9.4
Reception mean BMI z-score	0.313	0.358	0.480	0.273	0.353	0.338
N school-years	*182*	*127*	*191*	*184*	*3902*	*30,281*
Year 6 overweight or obesity prevalence, %	39.2	38.0	40.8	38.5	35.7	32.3
Year 6 obesity prevalence, %	24.7	22.8	26.2	24.1	20.8	18.0
Year 6 mean BMI z-score	0.597	0.608	0.747	0.559	0.547	0.464
N school-years	*183*	*127*	*191*	*187*	*3860*	*29,225*

Notes Child-level variables from National Pupil Database (NPD). Absences for any reason or for illness or medical appointments, are expressed as percent of all possible sessions missed. Persistent absentees are defined as children missing more than 10% of possible sessions in the academic year. For non-treated children, absence rates and persistent absenteeism are shown for 10% random of sample of population. School-level BMI-classification outcomes from National Child Measurement Programme, weighted by school's average cohort size.

Table 3 **Characteristics of London local authorities that implemented UFSM, and comparison local authorities that never introduced UFSM, academic years-ending 2007-2009.**

Local authority	London local authorities that later implemented UFSM				Comparison local authorities that never implemented UFSM	
	Newham	Islington	Southwark	Tower Hamlets	Rest of London	Rest of England
Child-level characteristics						
English as Alternative Language %	70.6	42.1	41.2	75.2	34.9	11.7
Male %	50.5	51.1	51.3	51.0	51.2	51.2
FSM eligible %	37.8	42.3	33.5	58.9	24.0	15.8
IDACI score*	0.495	0.524	0.443	0.647	0.316	0.219
Black African, %	18.5	16.2	30.8	6.1	11.5	2.4
Black Caribbean, %	5.9	7.8	12.4	2.4	3.8	1.4
Bangladeshi, %	17.2	6.4	2.8	63.1	2.1	1.0
Pakistani, %	11.7	0.5	0.5	2.4	3.6	3.2
Indian, %	10.0	1.0	0.6	0.7	5.7	2.3
Mixed, %	5.7	11.9	8.4	3.4	7.6	3.6
Other Non-White, %	11.7	9.1	12.4	4.7	14.5	3.1
Special Educational Needs, %	25.6	34.9	32.4	26.1	27.4	24.5
N children	*11,486*	*5,310*	*8,482*	*8,254*	*218,486*	*1,710,519*
Selected local authority level controls						
Unemployment rate, %	4.28	4.10	3.85	5.81	2.94	2.81
Pop. density, per hectare	76.3	129.5	95.9	117.7	56.0	17.7
Pop. share age 5-9, %	7.06	4.87	5.37	6.13	5.94	5.70
Fast Food outlets/1,000 pop.	0.360	0.670	0.499	0.572	0.470	0.456
N children	11,486	5,310	8,482	8,254	218,486	1,710,519
Selected school-level controls						
School exp. per pupil, £000s	4.66	5.47	5.48	5.99	4.27	3.69
Pupil-teacher ratio	20.81	21.27	19.34	20.94	22.21	22.13
N children	11,486	5,310	8,482	8,254	218,486	1,710,519

Notes *IDACI: Income Deprivation Affecting Children Index. Sources: Child-level characteristics from National Pupil Database (NPD). Local authority-level characteristics plus ward-level unemployment rates from Office for National Statistics, and school expenditure and pupil-teacher ratios from Department for Education website. Local authority and school characteristics merged with NPD to produce school-level figures weighted by number of Year 6 children in each.

England and tended to have slightly higher absence rates and share of persistent absentees. Among Reception children all treated local authorities had higher overweight and obesity prevalence than the comparison local authorities, though Newham and Tower Hamlets in fact had a lower mean BMI z-score. Among Year 6 children, all four London local authorities that introduced UFSM had higher prevalence of overweight and obesity, and higher mean BMI z-score, than either comparison group.

Table 3 shows that the four London local authorities have markedly higher levels of disadvantage, as measured by means-tested FSM eligibility and by the Income Deprivation Affecting Children Index than the rest of London and England. They are ethnically diverse in distinct ways, with Tower Hamlets having a large Bangladeshi and Southwark a large Black African population, for example. Greater levels of disadvantage mean that these schools received higher per-pupil funding and could sustain a higher pupil teacher-ratio.

Methods

All our results are based on the **difference-in-difference** method. This compares changes in outcomes from before to after UFSM was introduced in the treated local authorities, with changes in outcomes over the same period in control local authorities that never introduced UFSM. The key assumption of this method is of parallel trends: That the change in outcomes in the control group is a good guide to what the change in outcomes would have been in the treated group, had the UFSM schemes never been introduced.

We present results obtained by comparing the treated local authorities with **two alternative control groups: the whole of the rest of Greater London, and the whole of the rest of England**. These are chosen to be sufficiently wide that the results are not sensitive to any unobserved policy changes or shocks affecting a single or small numbers of local authorities; to not use any arbitrary cut-off of local authorities we will use or ignore; to take into account the 'London Effect' (the phenomenon of different trends in educational outcomes observed in Greater London compared to the rest of the country). See Appendix A3 for more information.

Tables 1 and 2 have highlighted that local authorities that introduced UFSM schemes are quite different from the rest of the country. While difference-in-difference methods do not require the treated and control groups to be similar, we account for these differences and their changes over time by controlling for a wide range of **observable characteristics** of each child, school and local authority. These include indicators for ethnicity, sex, and socio-economic background. For household expenditure and children's BMI outcomes, we also control for timing of measurement, because we may expect seasonal effects. (The full set of control variables included in each specification is listed in Appendix A2). We also use '**fixed effects**' estimation at the child, school or local authority level (depending on what we can observe in the data sets to study the outcome in question) which wash out unobservable differences that are constant over time. Tests of the parallel trends assumption (after controlling for these characteristics) are presented in Appendix A3. They show that trends are parallel for all the outcomes we consider and for both control groups we use.

The recent literature on difference-in-difference methods has highlighted that the classic two-way fixed-effects (TWFE) method yields biased results in cases where the treatment was introduced at different times and where the effects cannot be expected to stay stable over time. Both issues affect our analysis, as participating local authorities introduced UFSM at different times, and we might expect effects on some outcomes to get bigger the longer the children are exposed to the policy. To address this, we use an alternative estimation method proposed in the literature by Borusyak et al., (2023). This entails using data on pre-treated and never-treated children to predict what the outcomes of treated children would have been, had the policies never been introduced (see Appendix A1 for details on the methods used). The difference between the observed outcomes of treated children and their outcomes as predicted using the data on non-treated children in the Borusyak estimator is the treatment effect of the policy. This relies on the same parallel trends assumption as the classic difference-in-difference method.

We also present results where we distinguish effects according to the number of years children were exposed to UFSM. In this case we can make use of the classic TWFE difference-in-difference method. We estimate each possible duration of exposure to UFSM as a different treatment effect, with its own treatment dummy included in the model, so that we do not have to require effects to remain stable over time.

All our results are 'intention to treat' estimates, meaning the average effect of being exposed to a school environment in which school meals are provided for free for everyone, compared with a situation where school meals are free only for children who are eligible under means-testing (between 33 and 59% of children in the four treated local authorities), and is an average across children who never take school meals, those who always do, and those who switch their lunch arrangements. In order better to understand the magnitude and mechanisms for the impacts on other outcomes, we next assess the impact of UFSM on take-up of school meals.

4 Effects of UFSM on take-up of school meals

The intended *direct* beneficiaries of UFSM are not-FSM-eligible children, who become newly entitled to a free meal. Those who previously paid for a school meal will benefit from their household no longer having to pay, but will not experience any change in nutritional intake. Those who previously did not take a school meal but switch into doing so, will benefit from their household no longer needing to purchase and assemble a packed lunch, and from any nutritional differences between their school meal and what they would have eaten instead.

The available data on take-up among not-FSM-eligible students are somewhat patchy and not sufficient to evaluate the impact of UFSM on take-up by not-FSM-eligible students. Therefore we cannot establish the size of the group that switches into newly having a school lunch.[6] However, a back-of-the-envelope calculation comparing reported overall primary-school take-up of school meals in Newham and Islington in 2012 (Nelson et al., 2012), when these local authorities had completed their roll out of UFSM to the whole of primary school, with the pre-treatment year 2009 (Department of Communities and Local Government, 2010), suggests a 47 percentage point rise in take-up.[7] This is similar to the 50 percentage point rise among not-FSM-eligible

6 Specifically, primary school take-up of school meals is reported at the local authority level in 'National Indicators' from 2008 to 2010, and in the statistical release of School Food Trust Surveys in 2011 and 2012. Data are missing for local authorities with low response rates. Take-up among infants is reported in Department for Education records at the school-level from 2015 onwards, after the introduction of the national Universal Infant Free School Meal policy.

7 This figure is derived from the 22 and 40 percentage point rises in overall take-up in Newham and Islington respectively, then scaling by the proportion of children not eligible for FSM in each local authority and assuming a uniform 1.5 percentage point rise in take-up among the FSM-eligible.

infants after introduction of the of national UIFSM scheme documented by Holford and Rabe (2022).

The introduction of UFSM could potentially also increase take-up among the already-FSM-eligible, despite this group experiencing no change in entitlement or price. This may happen by removing stigma (to the extent that FSM status can be inferred by others when taking it up) or making it more desirable to participate, for example if the logistics of school lunch-hours meant that children eating school were separated from those with packed lunches (Holford, 2015). Providing meals for free on a universal basis potentially overcomes these barriers and helps ensure that children in greatest financial need take their entitlement.[8]

Table 4 shows estimates of the impact of UFSM on take-up by FSM-eligible pupils using the Borusyak et al., (2023) imputation method, and school-level data. It shows a statistically significant increase in take-up by FSM-eligible pupils of either 1.6 or 1.3 percentage points. This is equivalent to shifting around 8% of non-takers into taking up their entitlement, despite this group facing no change in price. A 1.5 percentage point increase in take-up by primary-age FSM-eligible children nationally would correspond to an extra 16,700 children from low-income households eating a school meal, on top of the approximately 900,000 who already do so (source: calculation from 2022/23 Schools, Pupils and Characteristics dataset). It suggests that the UFSM programmes might have slightly reduced the stigma and social or informational barriers to taking up entitlements or improved the overall attractiveness of the school meal environment. We can combine this result with some additional information and assumptions to distinguish four groups of children according to their school lunch take-up behaviour, and approximate the size of each group as shown in Table 5 (see the notes of the table for the assumptions made).

Table 5 shows that for roughly half of the children in London local authorities we expect the introduction of UFSM to have had no direct effect because they were either already having free meals or they continued to have packed lunches after their introduction. We expect that one in five children kept eating a school meal but no longer had to pay for it, and only roughly one in three children switched from eating a packed lunch to eating a school meal. Any effects on outcomes we estimate are an average across all children, with only about one in three of the treated children changing what they eat. Note that the proportions in the table are approximate as we have to rely on several assumptions. Moreover, there may be indirect effects of the policy – like an improved eating environment or spillover effects in the classroom due to improved behaviour of children who have switched their lunch choice, for example – that would accrue to all children.

Table 4 Impacts on take-up of means-tested FSM, %

Control group	(1) London	(2) England
UFSM current	1.618*** (0.632)	1.321*** (0.590)
N treated	1,605	1,605
N underlying registered	19,374	184,263

Notes Source: Department for Education 'Schools, Pupils and Characteristics' dataset, school-level data. Borusyak, Jaravel and Spiess imputation method with school and year fixed-effects. 'The treated' are all FSM-registered pupils exposed to UFSM. *, **, *** indicates statistical significance at the 10%, 5% and 1% significance levels.

Table 5 Who benefits directly from UFSM?

Group	Description	Approx. proportion of treated population	Direct effect of UFSM
A	Already FSM-eligible and taking a school meal.	36%	None
B	Previously taking a packed lunch and keep doing so.	14%	None
C	Previously paid for a school meal, now receive one for free	18%	Income effect
D	Previously taking a packed lunch, switch to a school meal	32%	Change nutritional content of meal consumed at school; income effect.

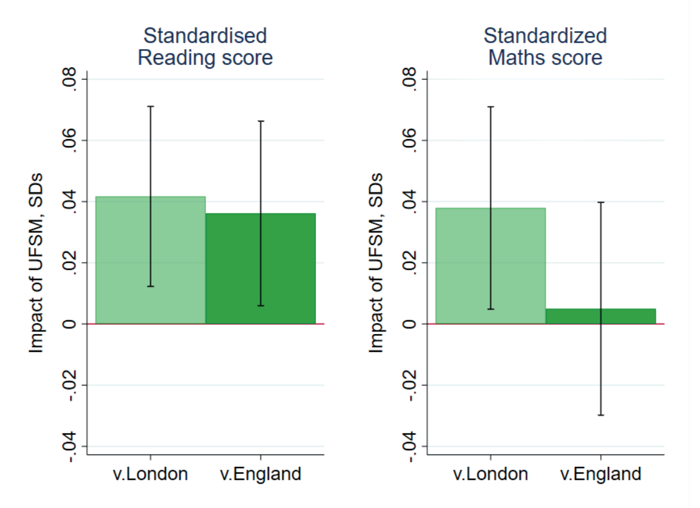
Notes Source: Population proportions estimated from pre-treatment FSM eligibility rate in treated local authorities (42.6%, weighted average of populations of the four treated local authorities, in Table 3) and assumptions of 85% take-up of means-tested FSM and 86.5% take-up of UFSM, and 31% take-up of paid-for school meals (guided by findings from UIFSM policy; Figure 1, Holford & Rabe, 2020, and above estimate of impact of UFSM on take-up by FSM-eligible); and assuming that nobody who takes a school meal under means-testing switches to packed lunches under Universal entitlement.

5 Effects of UFSM on educational attainment

Figure 2 shows the estimated average impact of UFSM (for all children exposed to UFSM for at least one year) on KS2 scores in Reading and Maths, for our two alternative control groups. Estimates use the Borusyak et al., (2023) method to account for time-varying treatment effects. For this and subsequent Figures

8 Registration for means-tested FSM may have reduced as a result of parents losing the direct financial incentive to register their children. In Holford and Rabe (2020) we document that the national UIFSM scheme reduced registration for means-tested FSM by around 1.5 percentage points in the affected year-groups.

Figure 2 Impacts of UFSM on Year 6 children's Reading and Maths test scores



Notes Source: National Pupil Database, 2002/03 to 2018/19. Borusyak, Jaravel and Spiess (2023) imputation method. N children in underlying regression for rest of Greater London = 1,096,484 (Reading) and 1,097,805(Maths). N children in underlying regression for rest of England = 7,507,121 (Reading) and 7,520,942 (Maths). N treated children: 76,148 (Reading), 76,183 (Maths). Capped lines indicate 95% confidence intervals.

in this section, estimates obtained using the rest of London as the control group are indicated with lighter bars, and those obtained using the rest of England with darker bars. Capped lines indicate 95% confidence intervals.

We find a positive treatment effect of 4% of a standard deviation on Reading outcomes, based on both control groups. This effect size corresponds to two weeks, or 10 school days, of additional progress in Reading. This effect size is small according to commonly applied rules of thumb for education trials (Sanders et al., 2020 and citations therein). However, the impact of UFSM is still quantitatively important, when compared with the impact of other interventions such as the Literacy Hour, which explicitly targeted Reading ability and had an effect size of +8% of a standard deviation over two years (Machin and McNally, 2008).

The estimated impact on Maths outcomes is more sensitive to the choice of control group. The two estimates are not significantly different from each other but would lead to conclusions either of an effect of close to 4% of a standard deviation, or of zero. We interpret this as not providing consistent evidence of a benefit of UFSM for Maths outcomes. Next, in Table 6, we show separate treatment effects for each possible duration of exposure to Universal FSM, for children's KS2 scores based on tests taken in Year 6. These estimates rely on the traditional difference-in-difference estimator with time and school fixed-effects as they allow for different effects by treatment duration. Each coefficient is estimated less precisely than our main effects in Figure 2 because each relies on fewer observations. We find no significant positive effects for any specific duration, and the treatment effect fluctuates from period to period such that there is no evidence of a cumulative impact on children's reading scores that gets bigger the longer than children are exposed.

Figures 3 and 4 show results when we split the sample according to four key characteristics, by sex, ethnicity, FSM-eligibility of the child[9] and the FSM eligibility rate of the school. We divide the latter into 'high FSM' schools, with means-tested FSM registration rates above the treated local authorities median in 2009, and 'low FSM' schools that are below the median.

With the exception of observing somewhat larger beneficial impacts on South Asian ethnicity students than Black (and compared with the population on average), the impacts of UFSM do not seem to differ between groups of the population. Point estimates are qualitatively and statistically very similar for boys and girls, FSM and not-FSM children, and high FSM and low FSM schools.

We might have expected effects to be larger in low-FSM schools and among not-FSM eligible children, given that newly eligible children increase take-up of school meals considerably more than children already eligible for FSM (see section on take-up of school meals above). Finding similar impacts of UFSM for both groups could suggest that small increases in take-up can lead to sizeable improvements in education outcomes for FSM-registered pupils. It could also support there being benefits from universal provision not just driven by individual take-up. For example, children newly taking up a UFSM may exhibit less disruptive behaviour, which would benefit children in their class who do not change what they eat at lunchtime. Similarly, the change in the school environment to a norm of children in the class all eating together may improve social and emotional skills.

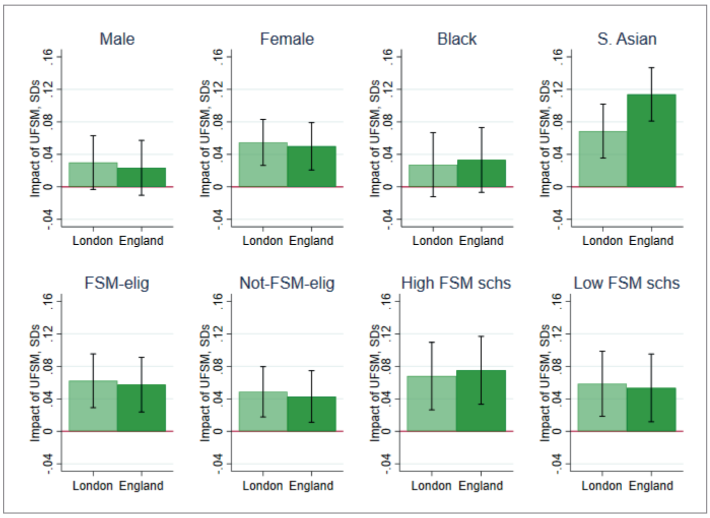
9 Note than since parents may not continue to register their children for means-tested FSM and pupil premium after UFSM is introduced, this characteristic is not completely predetermined.

Table 6 Impacts of UFSM on Year 6 children's Reading and Maths test scores by duration of exposure

	Reading		Maths	
Control group	(1) Rest of London	(2) Rest of England	(3) Rest of London	(4) Rest of England
Child's duration of exposure				
One year	0.017 (0.028)	0.032 (0.028)	0.014 (0.031)	0.004 (0.027)
Two years	0.022 (0.038)	0.034 (0.036)	0.021 (0.036)	0.013 (0.034)
Three years	-0.021 (0.028)	-0.022 (0.030)	-0.031 (0.030)	-0.053** (0.025)
Four years	-0.009 (0.046)	-0.002 (0.047)	0.003 (0.050)	0.017 (0.049)
Five years	0.036 (0.044)	0.022 (0.052)	0.026 (0.042)	-0.018 (0.057)
Six years	0.010 (0.066)	-0.006 (0.070)	0.025 (0.070)	-0.021 (0.063)
Seven years, whole of primary school	0.046 (0.079)	0.023 (0.086)	0.043 (0.080)	0.006 (0.096)
N observations	905,637	5,925,109	904,772	5,928,902

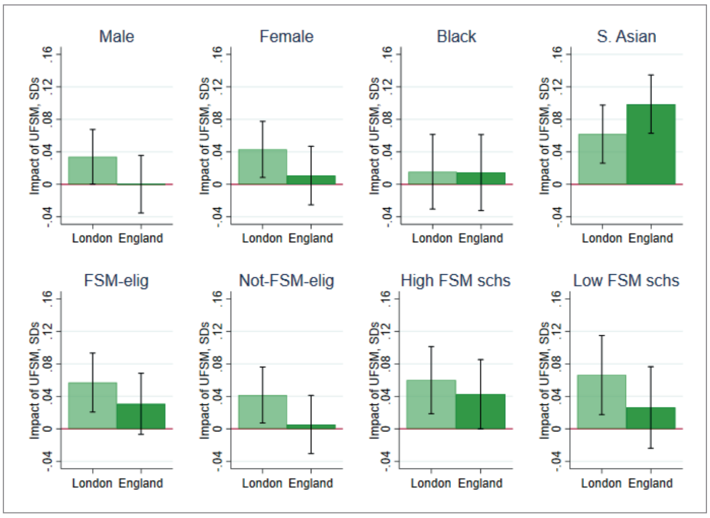
Notes Source: National Pupil Database, 2002/03 to 2018/19, individual-level data with school fixed-effects. Two-way fixed-effects with separate treatment dummies for each duration of exposure. *, **, *** indicates statistical significance at the 10%, 5% and 1% significance levels.

Figure 3 Impacts of UFSM on Reading test scores by individual and school characteristics



Notes Source: National Pupil Database, 2002/03 to 2018/19. Borusyak, Jaravel and Spiess (2023) imputation method. N children in underlying regressions for Greater London = 553,234 (Male), 544,571 (Female), 262,423 (Black), 204,401 (South Asian), 253,535 (FSM-eligible), 544,270 (Not-FSM-eligible), 224,211 (High FSM schools), 832,265 (Low FSM schools). N children in underlying regression for whole of England: 3,809,467 (Male), 3,711,475 (Female), 482,454 (Black), 735,148 (South Asian), 1,206,567 (FSM-eligible), 6,314,375 (Not-FSM-eligible), 621,383 (High FSM schools), 6,574,585 (Low FSM schools). N treated children: 38,081 (Male), 38,102 (Female), 23,438 (Black), 27,097 (South Asian), 23,322 (FSM-eligible), 52,861 (Not-FSM-eligible), 33,697 (High FSM schools), 42,229 (Low FSM schools). 'High FSM schools' are those with FSM eligibility above the treatment group median in academic-year-ending 2009. Capped lines indicate 95% confidence intervals.

Figure 4 Impacts of UFSM on Maths test scores by individual and school characteristics, for different control groups



Notes Source: National Pupil Database, 2002/03 to 2018/19. Borusyak, Jaravel and Spiess (2023) imputation method. N children in underlying regressions for Greater London = 553,234 (Male), 544,571 (Female), 262,423 (Black), 204,401 (South Asian), 253,535 (FSM-eligible), 544,270 (Not-FSM-eligible), 224,211 (High FSM schools), 832,265 (Low FSM schools). N children in underlying regression for whole of England: 3,809,467 (Male), 3,711,475 (Female), 482,454 (Black), 735,148 (South Asian), 1,206,567 (FSM-eligible), 6,314,375 (Not-FSM-eligible), 621,383 (High FSM schools), 6,574,585 (Low FSM schools). N treated children: 38,081 (Male), 38,102 (Female), 23,438 (Black), 27,097 (South Asian), 23,322 (FSM-eligible), 52,861 (Not-FSM-eligible), 33,697 (High FSM schools), 42,229 (Low FSM schools). 'High FSM schools' are those with FSM eligibility above the treatment group median in academic-year-ending 2009. Capped lines indicate 95% confidence intervals.

6 Effects of UFSM on children's body mass index classifications

Figure 5 shows the average treatment effects of UFSM on children's BMI classification outcomes in Reception and Year 6. Estimated treatment effects are slightly bigger when comparing with the rest of England (darker bars) than the rest of London (lighter bars), but we find statistically significant beneficial impacts on obesity in both cases and for both age groups.

Impacts on obesity prevalence are similar in magnitude for both Reception and Year 6 children, at 0.6-0.9 percentage points reduction compared with the rest of London, or 1.3-1.4 percentage points reduction compared with the rest of England. These effects are proportionally larger in Reception, where the baseline prevalence of obesity is 14% and the effect represents a 7-11% reduction, than in Year 6, where baseline obesity prevalence is 25% and the effect represents a 2-5% reduction.

UFSM also significantly reduced average BMI z-score among Reception children by 4.5% of a standard deviation (compared with the rest of London) or 7% (compared with the rest of England). The impact on mean BMI z-score in Year 6 is only significant and of a quantitatively important magnitude for the larger (rest of England) control group, at 2.6% of a standard deviation.

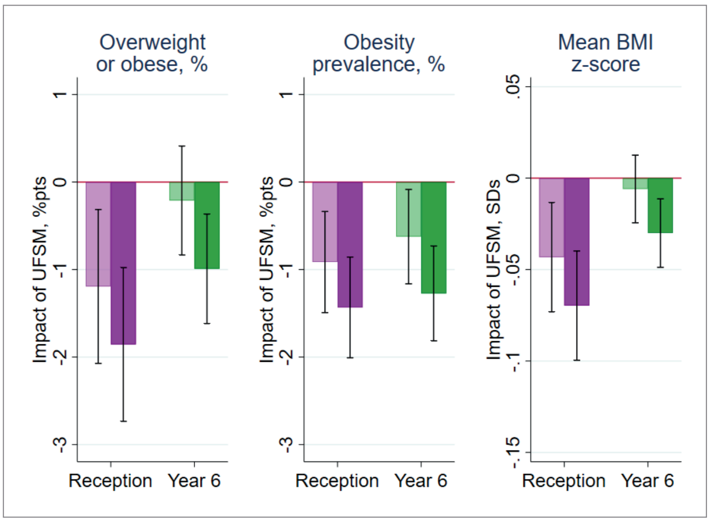
This effect size for Reception children, all of whom are exposed to UFSM for less than a year, is remarkably similar to that found for the UIFSM policy (Holford and Rabe, 2022). This underlines that younger children's BMI can respond to changes in intake and environment quite quickly, whereas the BMI of older children may be harder to shift.

We next test whether the impacts of UFSM on Year 6 children are bigger for those who have received UFSM for longer. All children weighed and measured in Reception had received UFSM for less than one academic year. For Year 6 children their duration of exposure depends on how long the local authority had been providing UFSM. Some could have received UFSM for the first time in Year 6, others could have received it throughout primary school (for seven years).

Figure 6 shows estimated treatment effects on Year 6 children by duration of exposure. Estimates are less precise here because there are fewer school years of data for each duration. This means few individual treatment effects or differences between them are statistically significant.

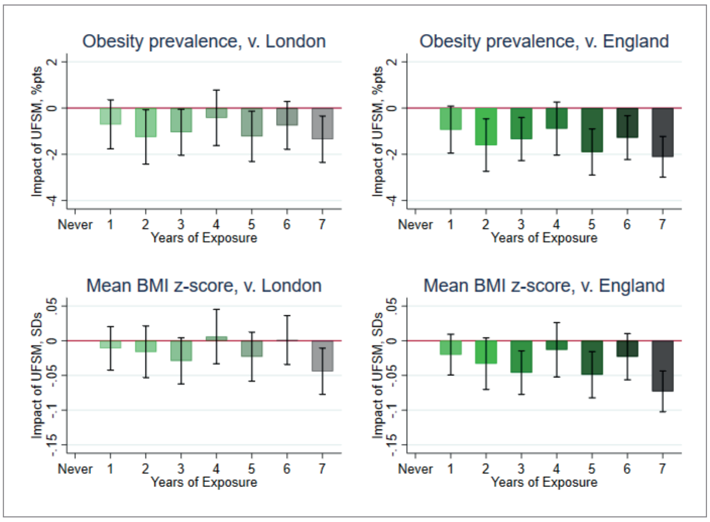
Nonetheless, the figure shows that the impact of UFSM is always statistically insignificant for Year 6 children receiving UFSM for the first time during the academic year of measurement (one year of exposure) and a smaller effect than newly receiving UFSM in Reception. This again indicates that the BMIs of older children are harder to shift in the short term. This could be because school meals comprise a smaller proportion of older children's total energy intake, or perhaps

Figure 5 Impacts of UFSM children's BMI classifications



Notes Source: National Child Measurement Programme. Borusyak, Jaravel and Spiess imputation method with school and academic-year fixed-effects. **Light bars use rest of London as control group, dark bars use rest of England as control group**. For Reception outcomes we restrict the sample to academic years ending 2007-2014, to avoid conflating local authority-based UFSM with national UIFSM policy. N treated school-years = 710 (Reception), 1653 (Year 6). N school-years in underlying regression: 11,572 (Reception, v. London), 81,938 (Reception, v. England), 18,213 (Year 6, v. London), 130,307 (Year 6, v. England). Capped lines indicate 95% confidence intervals.

Figure 6 Heterogeneous treatment effects of UFSM on Year 6 children's BMI classifications, by duration of exposure



Notes Source: National Child Measurement Programme. Pooled two-way school and academic-year fixed-effect regression with separate treatment indicators for each duration of exposure. N = 19,908 school-years (v. London), 132,002 school-years (v. England). Capped lines indicate 95% confidence intervals.

due to older children having more autonomy over what they eat outside of school.

Impacts on children who have been exposed to UFSM for seven years, throughout the whole of primary school, are always statistically significant and represent the largest point estimates in the Figures for both outcomes and both control groups. However, the magnitude of the point estimates fluctuates across the durations because each is sensitive to year- and local-authority-specific shocks that are averaged out across our overall effects.

The results in Figure 6 do suggest that providing UFSM throughout primary school is more beneficial than not providing it, or a 'starting late' model, in which UFSM is only provided from Year 3 onwards or in the final year, for example. However, our method (and the available data and timing of policies) cannot tell us the relative effectiveness of a UPFSM scheme for promoting health benefits in Year 6 versus the UIFSM scheme currently implemented nationally: this is because we have no observations on children exposed to UFSM for the first three years of primary school, from whom the scheme was then withdrawn.

In summary, our results show that starting free meal provision early and maintaining it throughout primary schools achieves a significant impact on cutting obesity rates, or at least delays the onset of obesity.

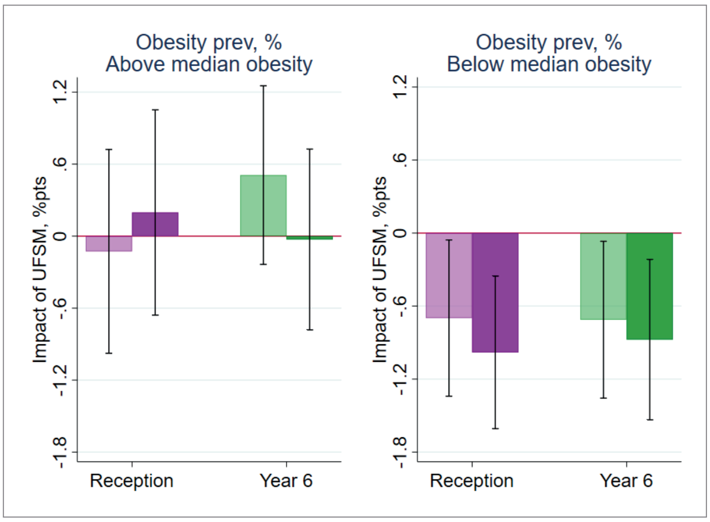
These results can be put into context by comparing them with the impact of other interventions targeting children's BMI outcomes. Chesham et al., (2018) studied the impact of a year-long trial of the 'Daily Mile' (15 minutes walking or running per school day) across all ages in primary school and found this reduced BMI z-scores by 0.8% of a standard deviation on average. Here the impact of UFSM on Reception children (who are exposed for an equivalent duration) are approximately five times larger, while results on Year 6 children receiving it for the first time that year are statistically indistinguishable both from zero and from the impact of the Daily Mile. While we cannot rule out that longer exposure to the Daily Mile would result in larger impacts, this comparison underlines that UFSM is especially effective at shifting younger children's BMIs quickly.

Next we check whether the results differ by school characteristics (pupil-level information is not available to us). Figure 8 shows impacts on obesity prevalence for schools when we divide the analysis into schools with high pre-existing obesity prevalence (above the treated local authorities' median between 2006/07 and 2008/09) and low pre-existing obesity prevalence (below the treated local authorities' median between 2006/07 and 2008/09).

In both Reception and Year 6 we find statistically significant benefits for children in schools with initially lower obesity prevalence. In contrast, we find no statistically significant impacts for children in schools with the highest pre-existing obesity. The fact that all the benefits of UFSM on bodyweight outcomes are concentrated in low-obesity schools cannot be explained by school characteristics that we can observe: The treated high and low-obesity schools are spread across all four treated local authorities and are very similar according to observable characteristics accounted for in the model. For example, over 80% of both high and low obesity schools are in the top quintile of the IDACI. The difference in the impact of UFSM across schools could be because children in schools with lower obesity rates have here-unobserved school, neighbourhood and home environments that make it easier for UFSM to affect children's bodyweight. For example, schools initially placing a higher priority on food and nutrition education may achieve higher take-up of the healthier options. They may have better physical education facilities or be located in neighbourhoods that have a lower density of fast-food outlets or more green space in which to exercise safely.

These results means that UFSM alone will not necessarily reduce health inequalities, and additional support will be needed in more challenging environments. More positively, this may suggest that average impacts would be greater if a UPFSM were rolled out across the country, where the obesity crisis, while serious, is less entrenched than in our four treated London local authorities.

Figure 7 Treatment effects on BMI classifications, by prior obesity prevalence in the school



Notes Source: National Child Measurement Programme. Borusyak, Jaravel and Spiess imputation method with school and academic-year fixed-effects. **Light bars use rest of London as control group, dark bars use rest of England as control group**. Above and below median obesity indicates schools with pre-treatment obesity prevalence above and below the pre-treatment median within the treated local authorities only. Reception analysis excludes academic years-ending 2015 onwards due to national UIFSM. N treated school-years = 350 (Above median)/ 360 (Below median) (Reception), 806/847 (Year 6). N school-years in underlying regression: 3,223/8,193 (Reception, v. London), 13,428/66,177 (Reception, v. England), 4,469/12,975 (Year 6, v. London), 13,814/106,665 (Year 6, v. England). Capped lines indicate 95% confidence intervals.

Table 7 **Impacts of UFSM on primary school children's absences from school**

	Absences for any reason, %		Absences for illness and medical appointments, %		Persistent absentees, % of child-years	
Control group	(1) London	(2) England	(3) London	(4) England	(5) London	(6) England
Impact of UFSM	-0.115** (0.047)	0.035 (0.052)	0.067 (0.041)	0.143*** (0.046)	-0.944*** (0.234)	-0.106 (0.259)
N treated child-years	572,874	572,874	572,874	572,874	572,874	572,874
N underlying regressions	902,567	4,328,354	902,567	4,328,354	902,567	4,328,354
Dep-var mean	4.61%	4.53%	2.34%	2.38%	11.04%	10.32%
Standard deviation	(5.20)	(5.13)	(2.70)	(2.73)	(31.34)	(30.42)

Notes Source: National Pupil Database, 2006/07-2018/19. All children in treated local authorities in school years 1-6. 10% random sample of population in non-treated local authorities. Borusyak, Jaravel and Spiess (2023) imputation method, with school and year fixed effects, and controls as documented in Appendix A2. Standard errors in parentheses below impact of UFSM. *, **, *** indicates statistical significance at the 10%, 5% and 1% significance levels.

7 Effects of UFSM on absences from school

In this section we estimate the impact of UFSM on absences from school. We consider three outcomes:

- Absences for any reason, which can include holidays, religious observance, exclusions, illness, and medical appointments, as well as lateness beyond the point in the morning that the register is taken. We interpret the overall absence rate as an indicator for the attractiveness of school attendance.
- Absence for illness and medical appointments, which we treat as an indicator for child health.
- Persistent absence – this is defined as the child missing more than 10% of possible sessions over the whole year, and we interpret this is an indicator of low engagement with school.

Results are shown in Table 7.[10] Coefficients are not statistically different from zero in some of the specifications, and where they are the effect sizes indicate very small effects (less than 0.2% changes in absence rates and 1% of absentee rates). These results suggest that UFSM had essentially no impacts on absences, either at the margin of attractiveness of attending school on a given day, through child health, or on long-term (dis)engagement. These results are consistent with the findings of the Department for Education's UFSM pilot evaluation (Brown et al., 2012) in showing that the gains to children's educational attainment from UFSM are not driven by a reduction in absences. They also align with a recent local-authority-by local-authority assessment of the impact of Universal FSM on absences (Thomson, 2023b).

8 Effects of UFSM on household expenditure

The UFSM policy provides savings for households taking up the free offer who previously purchased a school meal which they no longer have to pay for, and for families who previously assembled a packed lunch and no longer need to purchase the food items for the child's lunchbox. For families already eligible for and taking FSM under means-testing, UFSM provides no savings and does not change what children eat.

Families previously purchasing a school meal save £33.50 in a four-week window per child (based on £2.30 per child per day for 190 school days per year). Children in such families make up about 18% of all children so that the saving equates to £5.96 averaged across all children (see Table 5 for approximate group sizes and underlying assumptions). How much families who previously packed a lunch for their child save depends on the cost of the ingredients. Assuming the same cost as a school meal, the saving is £33.50 in a four-week window, equating to £10.87 per child when averaged across all children (considering that about 32% of children are in this group). If the cost of a packed lunch was £1.50, say, then the saving per child would be lower at £21.84 in a four-week window, or £7.09 when averaged across all children.

Households can use these savings in different ways. They could spend all or part of the savings on a higher quality or quantity of food. Alternatively, they could spend more on non-food items that may or may not benefit the child's health or education, or they could build up savings. Which of these strategies prevails will shed light on the mechanisms driving the benefits of UFSM for children's health and educational attainment we find.

10 To reduce the computer processing time to produce the estimates without major loss of precision, we retain the population of the treated areas but use a 10% random sample of pupils in the control group(s).

While we do not have access to data that allows us to examine fully how households use the savings, data from the UK Household Longitudinal Survey, waves 1-11, from 2009 up to February 2020 (University of Essex, Institute for Social and Economic Research, 2021) allows us to observe families' expenditure on supermarket shopping for food and groceries, and expenditure on eating out, which includes takeaways and money spent on paid-for school meals.

We assess how this expenditure changed for families resident in the treated local authorities, when children in their household become eligible for UFSM (because the policy was introduced by that local authority for their year-group, or because the child started primary school when the policy was already in place), or lose eligibility for UFSM (when the child moves from primary to secondary school). We compare these changes in expenditure with those of households in non-treated local authorities with at least one child.

Table 8 shows the impact of UFSM on real monthly household expenditure in February 2020 prices. Across all eligible children we find a reduction of £11.53 in expenditure on supermarket food per four weeks and a reduction of £9.46 in expenditure on eating out per four weeks, the two items totalling £20.98. This is very similar to the impact of the national UIFSM scheme on food expenditure found by Holford and Rabe (2020).

The reduction in expenditure on supermarket food is roughly the same amount as or higher than the average savings families face for no longer assembling packed lunches, depending on how much they were spending on the lunch ingredients. This indicates that families do not opt to use the saved amount to buy more, or more expensive food in the supermarket. Instead, they will be using the saved money for something else. The reduction in expenditure on eating out, which includes purchasing school meals, is higher than the saving families have for not having to pay for school meals. It might be that the group switching from packed lunches to taking a universal free lunch are reducing their expenditure on (non-school food) eating out, possibly because school meals are more likely to be hot and nutritious, so that children are (perceived to be) less hungry when they come out of school.

Together, these results suggest that families do not spend the savings from UFSM on increasing the quality or quantity of food purchased in supermarkets. If anything, they reduce spending on 'eating out', such as takeaways. This suggests that the benefits on children's health and learning are not driven by their food consumption at home, which according to expenditure data does not change much. There may be benefits to children and their families for having more disposable income to spend on other things that we do not observe. This is in addition to the time parents save by no longer needing to shop for and assemble packed lunches. This time could be spent on other activities that benefit the parent, child and household more widely.

Table 8 Impacts of UFSM on household food expenditure

	Expenditure in last four weeks		
	(1) Supermarket food	(2) Eating out and takeaways	(3) Total (1)+(2)
UFSM-eligible children	-11.53* (6.327)	-9.46* (5.108)	-20.98** (9.292)
N	46,581	46,581	46,581
Sample Mean	395.86	10.63	497.49
(Standard deviation)	(238.83)	(101.63)	(289.52)

Notes Source: UKHLS waves 1-11. Sample of families with any children aged 0-15 interviewed. Estimated treatment effect of exposure to UFSM based on local authority of residence and age of children. Treatment is the number of UFSM-eligible children in the family who are not eligible for national UIFSM programme. Outcome is real expenditure for supermarket shopping (food and groceries) and eating out, based on February 2020 prices. *, **, *** indicates statistical significance at the 10%, 5% and 1% significance levels.

9 Conclusions and recommendations for policy and practice

Our main results and conclusions are as follows:

- We estimate that introducing UFSM led to one in three children newly eating a school lunch.
- Data on take-up of school meals by not-FSM-eligible children are patchy, but suggest an increase of around 50 percentage points among children in this group.
- The 1.3-1.6 percentage point **increase in take-up of means-tested FSM** shows that providing UFSM reduces barriers that prevent already-eligible pupils from taking up their entitlement, potentially improving their welfare despite not being the targeted beneficiaries.
- The **beneficial impacts of UFSM on Year 6 children's Reading scores** suggests that UFSM aids children's concentration and ability to learn.
- The **beneficial impacts on Reception children's bodyweight outcomes** builds on previous evidence showing that the current UIFSM improves healthy weight prevalence and reduces obesity among children in their first year of school.
- The **beneficial impact on Year 6 children's bodyweight outcomes and test scores** are an average across children who have received UFSM for up to seven years. Impacts on bodyweight outcomes of Year 6 children who are only receiving UFSM for the first time that year are very small. This suggests that starting free meal provision early and

maintaining it throughout primary schools would maximise the impact on cutting obesity rates or delaying the onset of obesity. This would thereby contribute to addressing the significant long-term healthcare and indirect productivity costs of obesity.

- The beneficial impact on bodyweight outcomes is smaller in schools with the highest pre-existing obesity prevalence. This means that UFSM will not necessarily reduce health *inequalities*, and additional support will be needed in more challenging environments.
- We find **no impact on absences from school**, either at the margin of number of days missed or on the share of children persistently absent.
- The benefits of UFSM for educational attainment accrue both to children who are newly eligible for free meals and those already eligible, suggesting that universal entitlement improves the learning environment for all children
- Exposure to UFSM saved money for both households previously assembling packed lunches and previously purchasing school meals. Our analysis suggests that households reallocated these savings to non-food items, rather than increasing the quality or quantity of food purchased at supermarkets or eaten out.

In summary, universal provision of FSM throughout primary school helps ensure that all children have access to a meal of high nutritional standards, yielding health and educational benefits for these children, while also providing cost-of-living support to families with school-age children. There are also likely to be time saving for households no longer shopping for, and assembling, packed lunches that we cannot capture. We also do not take into account any employment benefits, due to the increased number or hours of catering staff required to facilitate the increase in take-up of school meals. This may also reduce Government outlay on Universal Credit.

The impacts of providing UFSM that we have identified are measured close-in-time to UFSM provision, but are likely to yield long-term benefits to individuals and to society. An increase in reading ability at age 10 has been shown to promote earnings throughout adults' working lives (Machin and McNally, 2008). Childhood BMI has been shown to be somewhat persistent into adulthood (Singh et al., 2008, Simmonds et al., 2015). If effects on obesity are sustained, this will affect healthcare costs and productivity. Total NHS and productivity costs of obesity to the UK economy have been estimated at £60bn per year, or £3,708 per person with obesity (McKinsey 2014). Even where impacts do not persist, there is evidence that reducing the prevalence of obesity in childhood, or the lifetime duration of obesity, reduces risk of later-life diseases (Fagherazzi et al., 2013; Tirosh et al., 2011; Field et al., 2005; Abdullah et al., 2011), suggesting there will be further benefits of reduced childhood obesity through improved length of life and quality of life. Reductions in overweight prevalence and downward shifts in BMI are likely to also have positive effects on productivity, healthcare spending and individuals' health which are less often quantified.[11]

Enabling continued high take-up of high-quality school lunches among both FSM-registered and not-FSM-registered pupils throughout primary school will be key to embedding the short-term benefits of the scheme observed for children's bodyweight and attainment. This could entail efforts to maintain and create an attractive and social school dining environment through the use of staggered lunchtimes and improvements or enlargements of kitchen and dining spaces, or emphasis of school meals as 'healthy' rather than free, and an integral part of the school day.

Parnham et al. (2022a, 2022b) caution that the benefits of UFSM on obesity could be improved with higher-quality meals, while budgetary pressures due to food and energy-price inflation may result in quality being reduced. This poses a threat to the continued benefits of both national and local authority schemes. Funding for school food should be sufficient to provide hot meals meeting nutritional standards and cover associated utility and staff costs.

Finally, to establish the longer-term impacts of both the national UIFSM and local authority UFSM schemes, consistent and comprehensive data are required. In particular, data on take-up of school meals should routinely be collected and published by the Department for Education separately for infant and junior school students, and within each of these age-groups separately for FSM-registered and not-FSM-registered pupils. At present, school-level take-up statistics are available on all infants, and on all FSM-eligible children. They do not differentiate take-up among infants by FSM-registration status, and do not differentiate take-up among FSM-eligible children by infant/junior status. Individual-level Census data do not include school lunch take-up for any non-infants. At present this lack of data limits research into the effectiveness of school meal policies.

11 For a prospective cost-benefit analysis, we refer to Impact on Urban Health's (2022) study estimates of the benefits of UFSM to economic Gross Value added.

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This work uses research datasets which may not exactly reproduce National Statistics aggregates.

Technical annex

A1 Estimation methods

Educational outcomes and obesity

All models discussed here are an adaptation of the two-way fixed-effects (2WFE) (i.e. year-fixed-effects, and school fixed-effects), difference-in-difference estimation, with controls for individual characteristics (each child is observed only once), and time-varying school and local authority characteristics. Standard errors are clustered by local authority.

The basic model can be written as follows, where is a dummy equal to one when the local authority is providing UFSM:

$$Y_{islt} = \beta\, UFSM_{lt} + \boldsymbol{\alpha YEAR_t} + \boldsymbol{\mu_s} + \boldsymbol{\gamma IndivChars_{islt}} + \boldsymbol{\theta SchoolChars_{slt}} + \boldsymbol{\lambda LAChars_{lt}} + \varepsilon_{islt} \quad (1)$$

The main coefficient of interest in this model is β, the average effect of exposure to UFSM across all children exposed to UFSM for at least one year. We also estimate a version permitting heterogeneous effects by duration of exposure, or permitting *past* exposure to have an effect. This model can be written as follows, where $\boldsymbol{UFSMDUR_{lt}}$ represents a vector of current or past treatment durations (e.g. one year, contemporaneous; two years, contemporaneous; one year of exposure, one year ago; two years of exposure, two years ago, etc):

$$Y_{islt} = \boldsymbol{\beta_{dur}\, UFSMDUR_{lt}} + \boldsymbol{\alpha YEAR_t} + \boldsymbol{\mu_s} + \boldsymbol{\gamma IndivChars_{islt}} + \boldsymbol{\theta SchoolChars_{slt}} + \boldsymbol{\lambda LAChars_{lt}} + \varepsilon_{islt} \quad (2)$$

Here $\boldsymbol{\beta_{dur}}$ represents the average effect of exposure to UFSM for those exposed for exactly the number of years represented by *dur*.

The two-way fixed effect method has been shown to be biased in cases of heterogeneous effects[12] and/or variable treatment timing.[13] We expect both to be an issue here: the impact of UFSM will be greater the longer that children are exposed to it (heterogeneous effects); while each treated local authority introduced UFSM at its own discrete point in time (variable treatment timing).

We implement the **Borusyak, Jaravel and Spiess (2023) imputation (BJS) method** which is designed to address both issues. The principle of this method is to estimate equation (1) (without a treatment dummy) for all never-treated schools and all pre-treatment observations of treated schools; extract the school-specific and year-specific fixed effects from this equation after controlling for their observable characteristics; and predict each treated child's (or school's) outcome based on the parameters of the model, their observed characteristics, and estimated fixed effect. The difference between their actual and predicted outcome is their individual Treatment Effect on the Treated.[14]

Our parameter of interest is the Average (mean) Treatment Effect on the Treated, and we cluster standard errors of this measure at the school level.

Pre-trends tests

To test the credibility of the parallel trends assumption we use the 'placebo' estimator proposed by Borusyak et al., (2023). This entails estimating a fixed-effects regression on never-treated and pre-treated observations only

$$Y_{islt} = \textstyle\sum_{t=-p}^{-1} \beta_t^{pre}\, COUNTDOWN_{lt} + \boldsymbol{\mu_s} + \boldsymbol{\gamma IndivChars_{islt}} + \boldsymbol{\theta SchoolChars_{slt}} + \boldsymbol{\lambda LAChars_{lt}} + \varepsilon_{islt} \quad (3)$$

Here the dummy variables $COUNTDOWN_{lt}$ are equal to one for schools in eventually treated local authorities in the period before the treatment is introduced. There is a dummy variable for each of p pre-treatment periods. Our objects of interest are the β_t^{pre} coefficients which indicate any deviation from parallel trends for a later treated school in period t. If pre-trends are conditionally parallel, β_t^{pre} should not significantly differ from zero in any pre-treatment period, and there should be no systematic movement of this conditional difference in either direction. We test for the joint significance of all the β_t^{pre} using an F-test.

For our obesity and absence results, for our main tests (Tables 9 and 11 in section A3) we use $p = 2$. This is maximum number of available periods, since there are three pre-treatment periods in the data for the earliest introducer of UFSM (Newham), and the first of these is required as a reference period, against which any deviations are defined. In Figure 8 (section A3) we also show the p-values obtained for $p = 3,4,5$, leaving at least observations for 2007 as the reference period, but otherwise using the maximum number of 'countdown' periods available for each treated local authority. The conclusions drawn from any of these options are the same. For our educational performance results (Table 10), we consistently use $p = 3$.

Household food expenditure:

For household food expenditure estimate a linear regression with local authority and timing fixed effects:

12 This problem occurs in two-way fixed-effects because the estimate of is identified by units changing from non-treatment to treatment. This means that the 'already treated' units (in both periods) are effectively used as controls in the same way as 'never treated' units (in both periods). This means that (for example) if the benefit of UFSM is getting bigger with a longer duration of exposure, the two-way fixed-effect method will pick this up as an improvement in outcomes not driven by the treatment, and this will bias the estimated treatment effect downward.

13 This problem occurs because the two-way fixed-effect estimator gives a bigger weight to units on which it has most information about in both untreated and treated states.

14 This method does not use already-treated units as controls, and gives each treated observation an equal weight, unrelated to the timing of exposure.

$$Y_{ilt} = \beta_1 N\_UFSM_{ilt} + \beta_2 N\_UIFSM_{ilt} + \beta_3 (N\_UFSM_{ilt} \times N\_UIFSM_{ilt}) + \boldsymbol{\gamma HouseholdChars}_{ilt} + \boldsymbol{\lambda LAChars}_{lt} + \boldsymbol{\mu}_l + \boldsymbol{\alpha TIMING}_t + \varepsilon_{ilt} \quad (4)$$

The main explanatory variable *N_UFSM* is number of children in household eligible for UFSM, because they are the right age and live in the right local authorities. Including the interaction between number of children eligible for local authority UFSM, and number of children exposed to the national UIFSM programme ensures the estimate represents the impact of an additional child eligible through the local authority programme.

We assume children are in youngest possible school year group for their age on date of interview. This means we mis-assign some secondary-age as treated, and bottom primary-age as untreated.

A2 Outcome and control variables

Outcome variables

- Our educational outcomes are 'year-standardised Key Stage 2 Reading and Maths test scores'. This is calculated as

  $$z_{it} = \frac{y_{it} - \overline{y}_t}{\sigma_t}$$

  where y_{it} is a child's raw test score, $\overline{y}_t$ is the national average of children's raw test scores in year t, and σ_t is the standard deviation of all children's raw test scores in year t. This removes the influence of year-to-year fluctuations in the difficulty of the test that both raised or lowered the average raw score, and the differences in terms of marks between high and low performers. It means that all treatment effects can be interpretated as a proportion of a standard deviation.
- Our BMI classification outcomes are all measured at the school level, and are defined with respect to age- and sex-adjusted growth tables for the UK population in 1990.
  - A child's BMI z-score is the number of standard deviations their BMI (Weight in kg/Height in metres^2) is above or below 1990 mean of children the same age and sex. We use the mean BMI z-score among children in the school.
  - Obesity prevalence is defined as the percent of children in the school with a BMI above the 1990 95th percentile.

Our expenditure categories are 'Supermarket food and groceries', which includes some non-food household items; and 'Eating out', which include takeaways and food purchased at school and work. For both, we convert prices to 2020 levels.

Overall absence is defined as the percent of possible school sessions (half-days – usually out of 380) missed for any reason in an academic year. We also show the share of possible sessions missed for reasons attributed to 'illness' or 'medical appointments'. Persistent absenteeism is defined as a child missing 10% or more of the possible school sessions in a given academic year. We re-scale our regression coefficients so that they show the impact of UFSM on the percentage of children persistently absent.

Control variables

We control for the following individual, school and local authority level variables. These are designed to account for the impact of differential changes in the composition of pupils in treated and control local authorities; differences in trends across different kinds of pupils; even where their composition would stay the same; and other policies and environmental factors that are likely to affect the outcomes and potentially change differently across treated and control local authorities.

Education and absences

- Individual control variables:
  - Year-Standardised KS1 average point score (education outcomes only)
  - Gender
  - Pupil premium entitlement, £1000s
  - IDACI score of Lower Super Output Area (LSOA) of residence
  - Ethnic group (18 dummy variables, omitted is White British)
  - Special Educational Needs – dummy for any provision
  - Dummy for first language is not English, or unknown.
  - Dummy for being a year-group too old or young
  - Relative age (11 for September births down to 0 for August births).
- School-level control variables:
  - Percent eligible for free school meals
  - Magic Breakfast provision.
  - Percent first language not English
  - Percent ethnic minority
  - School total expenditure per pupil
  - Pupil-teacher ratio
  - Claimant count unemployment rate of the ward in which the school is located
  - School type (so that effects of transitions from community to academy, for example, can be captured).

- Local authority level controls:
  - Population density
  - Population share aged 5-9.

For attainment outcomes we also control for academic year fixed-effects, and school fixed-effects.

For absences we always control for academic year fixed-effects, and the school year the child is in (Year 1, Year 2, etc). We show results with both school fixed-effects and individual (child) fixed-effects.

Control variables: body mass index classification outcomes:

- School-level controls:
  - Half-term of measurement
  - Percent Black, Percent Ethnicity Missing and Percent Girls measured, and their interactions with half-term of measurement
  - Cubic year-trends by IDACI quintile, Percent Black, Percent Ethnicity Missing, and Percent Girls
  - Universal School Breakfast provision; per-pupil funding via the School Sports Premium.
- Local authority level:
  - Unemployment rate
  - Density of fast food outlets per 1000 children:
  - Population share aged 5-9.

We also control for academic year fixed-effects and school fixed-effects.

In section A3 below, we test the sensitivity of the parallel trends assumption to a range of possible control groups. For this purpose, we rank 'control' local authorities by their similarity to the treated local authorities according to the probability that each local authority would have introduced UFSM, based on their similarity to our treated local authorities. We estimate the propensity score using the follow limited set of control variables, measured between 2007 and 2009:

- Share of school population who are girls.
- Share of school population who are Black or with ethnicity not reported (NCMP); or who are Black, South Asian or another or not reported ethnicity (NPD)
- School quintile of Income Deprivation Affecting Children Index, of FSM eligibility, and take-up of means-tested FSM
- Average school cohort size
- School has a universal free breakfast club
- 2007-2009 school average level of standardised Reading and Maths test scores
- Average level of outcomes (Reading and Maths; obesity and mean BMI z-score respectively) in 2007-2009
- Change in outcomes between 2007 and 2009 (obesity and mean BMI z-score) and between average of 2003-2005 and average of 2007-2009 (Reading and Maths).

Household food expenditure

We use all the local authority level controls described above, plus local authority fixed-effects, and the following household-level controls:

- Number of children eligible for the national UIFSM programme
- Interaction between number of children of eligible for local authority UFSM and the national UIFSM.
- Month of interview fixed effects
- Urban-rural indicator
- Housing tenure
- Equivalised real household income.
- Parent ethnicity (18 categories)
- Age of eldest child
- Share of female children
- Presence of lone parent
- Household structure (eight dummies)
- Number of adults in work
- LSOA-level unemployment rate.

A3 Selection of control groups

The treated local authorities are Newham, Islington, Southwark, and Tower Hamlets. For most outcomes, our control local authorities are either the rest of Greater London, or the rest of England. For household food expenditure, we only consider the comparison with the rest of England, because statistical power is an issue with survey data and the sample based in London is small.

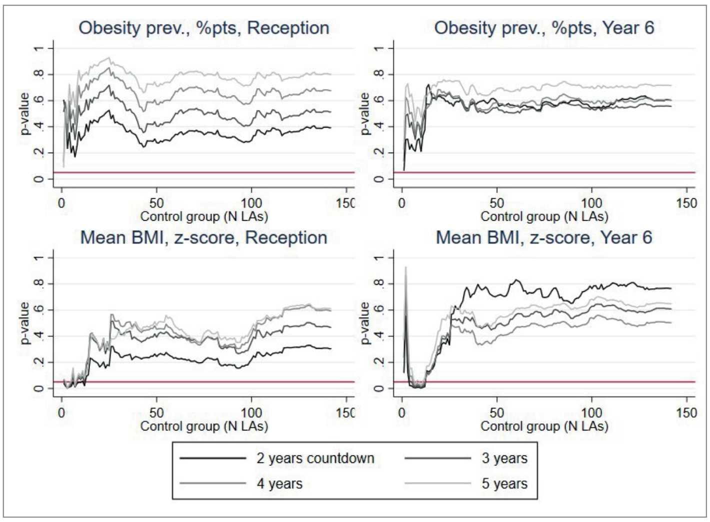
The essential characteristic of a control group is that (conditional on control variables) the treatment and control group have parallel counterfactual trends in the outcome. A standard approach to constructing a control group for this assumption to be credible would be to use observed characteristics and other contextual knowledge to select a group of similar, often neighbouring, local authorities (e.g. Belot and James, 2011). This could make sense in our context, since all four treated local authorities are in London, in which the composition of pupils by characteristics including ethnicity (important for both BMI classification and educational outcomes) and first language differs markedly to other regions. Trends and inequalities in pupil outcomes such as absence and attainment also differ from the rest of the country (Farquharson et al., 2022, Ross et al., 2020, Thomson, 2023a)

This would suggest using just the most similar London local authorities as our control groups. To test the sensitivity of our results to the definition of the control group, we estimated a propensity score for the probability that a school was 'ever-treated' using a probit regression of school and local authority characteristics, based on the three years prior to the first treatment being introduced (2007-2009). We took the within-local-authority average of the propensity score, and ranked local authorities from highest to lowest. We then estimated Borusyak et al's (2023) test of parallel trends first using the narrowest possible control group (one most similar local authority), then two most similar local authorities, and so on up to the whole of England. Figure 9 shows the p-values for the joint significance of the 'countdown' dummy variables. The red horizontal line marks the p-value of 0.05, below which indicates a statistically significant deviation from parallel pre-trends.

Figure 8 shows that in this application, at least for mean BMI z-score, choosing a narrow control group is more likely to result in non-parallel pre-trends than wider control groups. This may result from local authority-specific shocks and possible alternative policies, which have a bigger impact on the control group trend, the fewer control local authorities there are. To avoid arbitrarily choosing a cut-off for similarity of control LAs, we show results comparing our treated local authorities with (i) the rest of Greater London (29 other local authorities) and (ii) the whole of the rest of England. With this strategy any shocks or policy successes of any other local authority do not overly influence the control group trend. Note that everywhere in the country, local authorities (or for health outcomes before 2013, Primary Care Trusts) have a statutory duty to attempt to improve public health outcomes and children's educational attainment, and they will have implemented a variety of policies or interventions to do so. Our estimates of the treatment effect of UFSM on all outcomes should therefore be thought of as the impact of UFSM relative to what was implemented elsewhere, rather than versus 'do nothing'.

For BMI classifications, Table 9 shows for both of these chosen control groups we never reject parallel pre-trends for obesity or mean BMI z-score, except very marginally for overweight prevalence with London as the control group.

Figure 8 Outcomes of parallel pre-trends check for narrowest up to widest possible control groups



Notes Source: National Child Measurement Programme, Borusyak, Jaravel and Spiess (2023) test for parallel pre-trends on never-treated and not yet-treated local authorities. N LAs = Number of local authorities. Minimum N school-years (for narrowest control group of one local authority) = 1930 (Reception), 2784 (Year 6). Maximum N school-years = 81,938 (Reception), 130,307 (Year 6).

For education outcomes we have seven pre-treated periods for the earliest treated local authority, so could estimate up to six parameters on countdown dummies, but to avoid saturating the model we estimate three. Table 10 shows that no individual coefficient is ever significant, and nor are the coefficients jointly significant.

For absences we have only three pre-treated periods for the earliest-treated local authority, so we estimate coefficients on two countdown periods. Table 11 shows that again, no individual coefficients are ever significant, and for no outcome or control group are they jointly significant.

For all education and absence variables, we are therefore satisfied that the parallel pre-trends holds, and will maintain the assumption of counterfactual parallel post-treatment trends.

Table 9 Pre-trends tests for bodyweight outcomes for different control groups

	Overweight		Obesity		Mean BMI	Z-score
Control group	(1) London	(2) England	(3) London	(4) England	(5) London	(6) England
Reception						
One year before	1.015 (0.673)	0.186 (0.625)	-0.066 (0.490)	-0.425 (0.453)	0.021 (0.021)	-0.003 (0.020)
Two years before	1.300 (0.617)	0.732 (0.594)	0.453 (0.442)	0.153 (0.425)	0.041** (0.019)	0.024 (0.018)
F	2.349	0.818	0.872	0.935	2.261	1.190
P(F)	0.096	0.441	0.418	0.393	0.105	0.304
N observations	11,572	81,938	11,572	81,938	11,752	81,938
Year 6						
One year before	1.072 (0.655)	0.759 (0.612)	-0.260 (0.601)	-0.410 (0.573)	0.021 (0.020)	0.009 (0.019)
Two years before	0.860 (0.669)	0.621 (0.647)	0.349 (0.610)	0.261 (0.594)	0.022 (0.018)	0.012 (0.018)
F	1.677	0.985	0.391	0.504	1.011	0.267
p(F)	0.187	0.374	0.676	0.604	0.364	0.765
N observations	18,213	130,307	18,213	130,307	18,213	130,307

Notes Source: National Child Measurement Programme, school-level data. Borusyak, Jaravel and Spiess, 'placebo' estimator for deviations from parallel pre-trends using never-treated and pre-treated observations only. *, **, *** indicates statistical significance at the 10%, 5% and 1% significance levels.

Table 10 Pre-trends tests for Reading and Maths test scores for different control groups

	Reading		Maths	
Control group	(1) London	(2) England	(3) London	(4) England
Child's duration of exposure				
One year before	0.011 (0.027)	0.026 (0.026)	-0.003 (0.027)	-0.008 (0.027)
Two years before	0.023 (0.026)	0.023 (0.026)	0.021 (0.025)	0.005 (0.025)
Three years before	0.034 (0.021)	(0.022) (0.021)	0.005 (0.022)	-0.024 (0.022)
F	0.919	0.482	0.564	0.922
p(F)	0.431	0.694	0.639	0.429
N observations	1,096,484	7,507,121	1,097,805	7,520,942

Notes National Pupil Database, individual-level data 2002/03 to 2018/19. Borusyak, Jaravel and Spiess (2023) 'placebo' estimator for deviations from parallel trends using never-treated and pre-treated observations only. School and year fixed-effects and controls as documented in Appendix A2. Standard errors in parentheses. *, **, *** indicates statistical significance of individual coefficients at the 10%, 5% and 1% significance levels.

Table 11 **Pre-trends tests for absences for different control groups**

	Absences for any reason, %		Absences for illness and medical appointments, %		Persistent absentees, % of child-years	
Control group	(1) London	(2) England	(3) London	(4) England	(5) London	(6) England
One year before	0.086 (0.087)	0.073 (0.087)	0.081 (0.054)	-0.002 (0.050)	0.333 (0.539)	0.215 (0.551)
Two years before	0.069 (0.053)	0.091 (0.054)	0.055 (0.053)	0.008 (0.051)	0.223 (0.282)	0.226 (0.280)
F	0.895	1.408	1.195	0.018	0.370	0.337
p(F)	0.408	0.244	(0.303)	0.982	0.691	0.714
N observations	902,657	4,328,354	902,657	4,328,354	902,657	4,328,354

Notes National Pupil Database, individual-level data. Borusyak, Jaravel and Spiess (2023) ‘placebo’ estimator for deviations from parallel trends using never-treated and pre-treated observations only. School and year fixed-effects, and controls as documented in Appendix A2. Standard errors in parentheses. *, **, *** indicates statistical significance of individual coefficients at the 10%, 5% and 1% significance levels.

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