Journal of Health Economics 98 (2024) 102937

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/jhe

# Universal free school meals and children's bodyweight. Impacts by age and duration of exposure[☆]

Angus Holford[1], Birgitta Rabe[2,*]

*Institute for Social and Economic Research, University of Essex, UK*

ARTICLE INFO

*JEL classification:*
H42
I12
I14

*Keywords:*
School meals
Free lunch
Childhood obesity
Universalism
Difference-in-difference

ABSTRACT

We study the impact on children's bodyweight of switching from means-tested to universal provision of nutritious free school meals in England. We exploit the roll-out of universal provision across Local Authorities to estimate effects at different ages and durations of exposure, based on nurse-collected, population-wide health data for state school children. Exposure to universal free lunches reduces obesity prevalence and BMI among children aged 4–5 and 10–11, but older children's bodyweights are less responsive than younger children's. We find tentative evidence that effects may be cumulative, as impacts are largest for those exposed to universal free school meals throughout primary school.

## 1. Introduction

Childhood obesity is a serious worldwide public health problem. In England, the setting of our paper, one in ten children aged 4/5 and one in four aged 10/11 were living with obesity in 2021/22 (NHS Digital, 2022a).[3] Childhood Body Mass Index (BMI) and obesity are strongly persistent into adulthood, (Singh et al., 2008; Simmonds et al., 2015) a risk factor for a wide range of diseases in adulthood (OECD, 2019), and cause significant healthcare and indirect productivity costs (McKinsey Global Institute, 2014). Addressing childhood obesity is therefore a policy priority for governments worldwide, and a number of policies have been trialled, often with limited success. Children consume around one-third of their diet at school, making school meal provision a possible policy lever to improve weight outcomes among children (Davies, 2019).

In this paper we evaluate the impact of providing nutritious free school lunches on a universal basis on children's bodyweight outcomes. Specifically, we study the impact of Universal Free School Meal (UFSM) programmes run by four Local Authorities in

☆ The Nuffield Foundation has funded this project, but the views expressed are those of the authors and not necessarily those of the Foundation. Website: www.nuffieldfoundation.org; X (formerly Twitter): @NuffieldFound. The authors acknowledge additional support from the ESRC Research Centre on Micro-Social Change [ES/S012486/1], and the National Institute for Health and Care Research (NIHR) Applied Research Collaboration East of England (NIHR ARC EoE) at Cambridgeshire and Peterborough NHS Foundation Trust. The views expressed are those of the authors and not necessarily those of the NIHR or the Department of Health and Social Care. This work uses data provided by individuals and collected by the NHS as part of their care and support. Specifically, the paper uses data from the National Child Measurement Programme, supplied by NHS England, also known as the Health and Social Care Information Centre, part of the Government Statistical Service. The use of the NHS England statistical data in this work does not imply the endorsement of NHS England in relation to the interpretation or analysis of the statistical data. Research datasets may not exactly reproduce National Statistics aggregates.

* Correspondence to: Institute for Social and Economic Research, University of Essex, Wivenhoe Park, Colchester, CO4 3SQ, UK.
*E-mail address:* brabe@essex.ac.uk (B. Rabe).

1 University of Essex and IZA.

2 University of Essex, IZA and CESifo.

3 This was rise from pre-pandemic levels of 9.7% (at age 4/5) and 20.2% at age 10/11 in 2018/19.

https://doi.org/10.1016/j.jhealeco.2024.102937
Received 14 December 2023; Received in revised form 24 August 2024; Accepted 22 October 2024
Available online 12 November 2024
0167-6296/

London on children's BMI and prevalence of obesity and overweight at ages 4/5 and 10/11 in primary school. Universal provision represents a switch from a means-tested programme under which around one in four children in primary schools in England are already eligible for free lunches. This setting allows us to study what happens to children's bodyweight outcomes when a means-tested school food programme is made universal for primary-age children across whole Local Authorities, and to evaluate whether the impact differs by age of the child, by duration of exposure to the policy, or by school characteristics.

Nutritious free lunches have long been made available on a universal basis in Sweden, Finland and Estonia, but means-testing has been the dominant model for school food provision in major developed countries including the US, UK, France and Italy (Alex-Petersen et al., 2022). Means-testing is intended to target the children most in need, and avoid the deadweight implied in subsidising meals for families who could afford to pay for them. However, there has been a recent shift towards universal provision. The Local Authority-run schemes we exploit in this paper have been joined, since September 2014, by a free school meal programme for all children in state schools in England during their first 3 years of schooling. Similar schemes covering most or all primary school ages now operate in Scotland, Wales, and across London. In the U.S., several high poverty school districts including New York, Boston, Detroit and Atlanta have also made school lunches free for all students in recent years,[4] and they remain a policy option in other countries currently running means-tested free meal programmes. As more countries and local areas look to introduce universal free school meal schemes, it is important to evidence programme features that are likely to maximise benefits to children. Policy makers will – among other features – need to consider the age at which to start provision and how long to maintain it for. Our paper provides evidence on these questions.

We use data from the National Child Measurement Programme (NCMP, (NHS Digital, 2018)) which contains nurse-collected measures of children's height and weight at ages 4/5 and 10/11 for the universe of children in English state schools, aggregated to the school level. We exploit the roll-out, from 2010 onwards, of UFSM to different-aged children in four Local Authorities in London, to evaluate the impact of these schemes on children's bodyweights. To address biases in difference-in-difference estimation with two-way fixed effects in the presence of variable treatment timing and heterogeneous treatment effects, we adopt the imputation estimator and pre-trend placebo test proposed by Borusyak et al. (2024). We present results both comparing treated Local Authorities to the rest of London and to the rest of England.

We find that exposure to UFSM has a beneficial impact on children's bodyweight outcomes. Effects are larger for younger children, aged 4/5, than older children, aged 10/11, and are concentrated at the top of the weight distribution. Among children aged 10/11, effects on obesity prevalence and BMI are largest for children exposed throughout primary school, and we see tentative evidence that effects are cumulative. A child exposed to UFSM for 7 years is predicted to be 1-2 percentage points less likely to have obesity and to have a body mass index (BMI) that is 4%–7% of a standard deviation lower than a child not exposed to the policy. These are the intention-to-treat effects of UFSM which suggest that expanding availability of free meals in school from about 43% of children in the treatment Local Authorities[5] to all children leads to substantial improvements of bodyweight outcomes over the medium term.

There is only a small literature examining the effects of *universal* free school meal provision on children's health outcomes.[6] Alex-Petersen et al. (2022) exploit reforms in the 1950s and 1960s in Sweden to investigate the long-run effects of introducing free and nutritious school lunches on a universal basis in primary schools, eventually covering the whole country, on a range of outcomes. They find no effect on Body Mass Index at age 18, but substantial benefits to other health measures, educational attainment and lifetime earnings. Schwartz and Rothbart (2020) and Rothbart et al. (2023) exploit shifts from means-tested to universal free meals in New York City and elsewhere in New York State respectively, and find improved weight outcomes for non-poor students in middle schools (Schwartz and Rothbart, 2020), and for secondary-age pupils (Rothbart et al., 2023). For England, Holford and Rabe (2022) evaluate a universal free school meal policy for young children in primary school and find beneficial effects on children's bodyweights in their first year in school, at age 4/5.

We add to this literature in a number of ways. First, in our data we can observe bodyweights at two ages (age 4/5 and 10/11), allowing us to investigate, within the same policy setting and school environment, how the impact of universal meal policies differs by age of the child. Older children arguably have more autonomy over alternative meals consumed when not participating in school meal programmes as well as outside of school, suggesting that effects might not be constant across age (Cetateanu and Jones, 2014; Ianotti and Bush, 2014). The same reduction in portion sizes will also affect younger children more than older children, as they make up a larger share of their total calorie intake (Swinburn et al., 2006; Mahdi et al., 2023). Previous papers have not been able to compare the effects of universally free meals at different ages within the same setting. Rothbart et al. (2023) find a beneficial effect on secondary school children (aged 12 and 15) and null effect on primary school children (ages 5, 7, 9), but emphasise that

[4] The Healthy Hunger-Free Kids Act allowed schools to provide free meals to all children in high poverty areas, and several school districts including New York, Boston, Detroit and Atlanta have made school breakfasts and lunches free for all students (Leos-Urbel et al., 2013; Schwartz and Rothbart, 2020).

[5] Note that means-tested eligibility for free school meals is higher in London than in the rest of the country on average.

[6] Most of the existing evidence on the effect of *means-tested* free school lunches on bodyweight outcomes, identified through income-eligiblity cutoffs or with bodyweights at school entry as a key control, suggest that these raise the prevalence of obesity (Frisvold, 2015; Hinrichs, 2010; Dunifon and Kowaleski-Jones, 2004; Schanzenbach, 2009; Millimet et al. 2010). This literature predominantly analyses participants in the United States' National School Lunch and School Breakfast Programs, which at the time were subject to less stringent and sometimes poorly enforced food standards compared to our setting (Schanzenbach, 2009). For the UK, von Hinke Kessler Scholder (2013) exploits a policy reform that restricted eligibility to means-tested Free School Meals but compensated those affected by the reform financially, finding no effect of these changes on child bodyweight outcomes. This study on the 1980s significantly predates the current UIFSM policy and the enforcement in 2008 of improved food and nutrient-based standards for school lunches (see Spence et al. 2013; Belot and James, 2011).

settings differ in terms of food served and autonomy to choose outside-school foods during school hours.[7] It is therefore unclear whether differences in effect sizes documented in the literature are due to age, or other factors such as the content of the free meals being offered in the school setting, or in the counterfactual they replace for those who switch to eating school lunches.

Second, we are able to assess treatment effect heterogeneity by years of exposure, allowing us to study whether any impacts of UFSM cumulate over the years. Alex-Petersen et al. (2022) find statistically significant impacts of school lunch exposure on adult height only for those exposed for at least three years as children, indicating that free meals may have a cumulative effect on height, but do not investigate corresponding exposure effects for BMI. Schwartz and Rothbart (2020) and Rothbart et al. (2023) do not report heterogeneity by duration for these outcomes. Further heterogeneity analysis by school-level obesity prevalence provides new insights into the wider environment that may be needed for UFSM to have the desired effects.

Third, by estimating age and exposure effects of universal free school meal polices we contribute to the wider literature on health production. The early health production literature relates health outcomes such as obesity to individuals' health capital (assumed fixed) and health inputs (Grossman, 1972). Epidemiologists have adopted a life-course perspective that acknowledges the dynamic effects and interactions of physical and social exposures during different life stages (Kuh et al., 2003). In economics, health production has more recently been formulated as a multistage technology, where inputs at each stage produce outputs that can augment the outputs gained in later periods and/or raise the productivity of investments at subsequent stages (Heckman, 2007). These perspectives provide a framework for understanding the cumulative effects of health inputs and studying critical periods for investment into children's health in empirical applications (Case et al., 2002; Goodman-Bacon, 2021b). Our findings of larger health effects of UFSM at younger ages and suggestive evidence of effects that increase with years of exposure add to this empirical evidence.

The rest of the paper proceeds as follows. In Section 2 we outline the institutional context of school meal provision in England and the Universal Free School Meal schemes we study in this paper. In Section 3 we introduce our dataset, in Section 4 explain our identification strategy, and in Section 5 show our results. Section 6 concludes.

## 2. School meals in England

Means-tested Free School Meals (FSM) have been available to children in England from low-income families receiving qualifying benefits since after World War II. In 2022/2023 24% of pupils in state-funded primary schools were registered to receive these. Pupils not eligible for a Free School Meal can purchase the same meal at cost. Alternatively, children who are not eligible for free meals under means-testing, and children who are eligible but do not want to have the school meal, can bring in a packed lunch from home.

For school meals to have beneficial health effects, their quality compared to the counterfactual, a packed lunch, is of primary importance. Since 2008, school meals in England are required to comply with limits on portion sizes (530 calories per day) and the frequency with which different types of food may be served, as well as with nutrient-based standards that specify maximum amounts of fat, sugars and sodium and minimum levels of intake of nutrients such as protein, fibre, vitamins A and C, calcium, iron and zinc, averaged over a three-week period (Spence et al., 2013; Spence and Matthews, 2014).[8] In contrast, packed lunches, which may be prepared at home or shop-bought, are not required to comply with school food standards, though individual schools may implement restrictions on what children are allowed to bring. Audit studies found that at least 89% of packed lunches exceed the average calorie content stipulated for school meals. Packed lunches contained 626 calories on average in 2006, and 591 in 2016 (Evans et al., 2020), which is 61–96 calories per meal more than the energy content of a school lunch. Less than 2% of packed lunches meet food school standards in terms of energy and nutrients (Evans et al., 2010, 2020). Lunches packed at home have also been found to have higher ultra-processed content than school meals (Parnham et al., 2022), which is associated with an increased risk of living with overweight or obesity. This suggests that, other things being equal, consuming a school lunch rather than a packed lunch from home could lead to a reduction in children's bodyweight outcomes.

In 2014 the Universal Infant Free School Meal (UIFSM) policy was introduced in England which made school meals free for all children in state-funded schools during their first three years of schooling, at ages 4–7. Holford and Rabe (2022) found that UIFSM significantly reduces children's bodyweights over the course of their first year in school, the Reception year, with an effect size of 4.1% of a standard deviation on mean BMI, and 0.7 percentage points on obesity prevalence. Evaluation of the longer-run impacts of UIFSM is hampered by disruption in data collection during Covid school closures.

Predating the national UIFSM policy, several Local Authorities in England implemented their own universal schemes for some or all primary school year-groups, which we exploit in this paper. This includes four London Local Authorities that started rolling out their schemes between 2010–2014 and two Local Authorities that ran free school meal policies in earlier years (Kingston-upon-Hull, 2004–2007, and Durham, 2010–2011). We focus our analysis on the London Local Authorities for which we have more complete data. All four Local Authorities stated aims relating to supporting households with children with the cost of living, as well as to improve children's health and educational outcomes (Rahman, 2013; London Borough of Newham, 2020; London Borough of Islington, 2019; London Borough of Southwark, 2011).[9]

[7] Holford and Rabe (2022) and Alex-Petersen et al. (2022) only observe bodyweight outcomes at one age, and Schwartz and Rothbart (2020) study middle-school children aged approximately 11–13, but do not report differential effects by age or grade.

[8] School meals have been subject to some food-based standards since 2001. Updated food-based standards came into force in January 2015, which were designed to make it easier for caterers to embed the existing nutrient-based standards (Department for Education, 2014).

[9] Newham's scheme was initially a pilot, co-funded for two academic years by the Department for Education and Department for Health (Nilufer Rahim and Mehul Kotecha and Meg Callanan and Clarissa White and Emily Tanner, 2012) and funded by Newham Council after. Islington, Southwark and Tower Hamlets funded UFSM through their own budgets, without any central government support.

**Table 1**
Rollout of Universal Free School Meal schemes.

| Acad. year | 03 | 04 | 05 | 06 | 07 | 08 | 09 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Newham | x | x | x | x | x | x | x | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 |
| Islington | x | x | x | x | x | x | x | x | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 |
| Southwark | x | x | x | x | x | x | x | x | x | 1 | 4 | 6 | 6 | 6 | 6 | 6 | 6 |
| Tower Hamlets | x | x | x | x | x | x | x | x | x | x | x | 1 | 6 | 6 | 6 | 6 | 6 |
| Never-treated LAs | x | x | x | x | x | x | x | x | x | x | x | x | 2 | 2 | 2 | 2 | 2 |

Notes: Academic years-ending 2003–2019. x = No provision. Numbers (1, 2, 4 or 6) indicate eldest school year-group provided with Universal FSM under each scheme. (Year 1 students are aged 5-6 and Year 6 aged 10-11).

Table 1 gives an overview of the timing of the rollout of the Local Authority schemes we exploit. Some LAs introduced to younger year-groups first, others made the whole primary school, running from Reception year to Year 6 (age 4–11) eligible all in one go. 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.

As we discuss later, we identify intention-to-treat effects of making UFSM available to primary school children. It is important to be clear about the underlying behavioural changes the policy may have induced. First, there is a direct effect on *take-up* of school lunches, changing what children eat at lunchtime. Data on take-up of school meals is patchy, and we can only measure change in take-up from before UFSM introduction to after rollout to all primary year-groups for Newham and Islington. There take-up rose by about 22 and 40 percentage points, respectively, between 2009 and 2012.[10] As expected, the increase in take-up is mostly among children not eligible for free meals under means-testing, as only their families experienced a price change.

Second, the policy will have an *income effect* through money saved paying for school meals or ingredients for packed lunches. If the savings are spent on health-related investments this may affect children's bodyweight outcomes. Thirdly, UFSM may increase *work incentives* because free meal receipt is no longer tied to low income or benefit receipt, and time preparing packed lunches is freed up. In previous work we provide evidence that income effects and work incentives are both small and therefore unlikely to be important drivers of the impact of UFSM (Holford and Rabe, 2022), and thus we assume here that effects mainly arise through increased take-up of school meals.[11]

## 3. Data

We use data from the National Child Measurement Programme (NCMP) which are collected by trained nurses visiting schools to weigh and measure all Reception (aged 4/5) and Year 6 (aged 10/11) children in state schools in England (see NHS Digital (2018)).[12] To avoid disclosure risk, our bespoke data extract is aggregated to the school level for each year, limiting our ability to carry out heterogeneity analysis at the individual level. We observe each school's average cohort size over the analysis period, which we use to weight the school-level observations. Data for school years with less than 20 children per cohort are suppressed from the dataset, and we exclude a small proportion of schools that *ever* had less than 20 children per cohort, resulting in an analysis sample of approximately 10,000 schools per academic year.[13]

Our observation window covers academic years-ending 2007–2019 for Year 6 children, 2019 being the last complete year of data collection before it was suspended due to Covid school closures in 2020. For Reception children we restrict our analysis to the period 2007–2014 because the national UIFSM scheme, introduced in the academic year ending 2015, covered Reception children.

Each child's raw height and weight are used to derive their Body Mass Index (BMI) (weight in kilograms/(height in metres)$^2$), which in turn is used to classify them as underweight, healthy weight, overweight or obese. These classifications are calculated according to the British 1990 growth reference 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 up to the 95th percentile are classified as overweight,

[10] 2009 figure taken from 'National Indicator 52a' (Department for Communities and Local Government, 2010) and 2012 from a School Food Trust survey (Nelson et al., 2010). No school or Local Authority-level data on take-up across all primary year-groups are available for 2014 and 2015 onwards, which would be necessary to derive the corresponding figures for Southwark and Tower Hamlets.

[11] The financial saving from offering free meals accrued to not previously eligible families only and amounted to £1 a day per newly eligible child; work hours among parents increased by 1 h per week.

[12] Coverage of data collection was 80% of children in 2006/07 and consistently above 90% since 2008/09 (NHS Digital, 2018, 2019). Children may be excluded from measurement because of parental opt-out or absence from school on the day of measurement.

[13] 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. We also exclude two Local Authorities that withdrew Universal Free School Meal schemes during this period: Kingston-upon-Hull (Universal FSM provided 2004–2007), and Durham (2010–2011). The potential for lagged impacts of Universal FSM mean these should not act as controls.

and those above the 95th percentile as obese (Cole et al., 1995).[14] We use as our main outcomes the percentage of children who are overweight-or-obese, or obese, as well as the BMI 'z-score' which reports the standard deviations above or below the British 1990 growth reference charts mean among the children measured, adjusted for sex and age in months.[15]

Our NCMP data extract includes the date (week-commencing) of the measurement visit, the percentage of children measured who are female, who are of Black ethnicity and whose ethnicity is missing, as well as a Local Authority identifier. Additionally, data on school characteristics supplied by us was linked to the extract. This includes the Income Deprivation Affecting Children Index (IDACI) for the neighbourhood where the school is situated, the rate of means-tested eligibility to Free School Meals and the rate of take-up of free lunches by FSM-eligible students across the whole school for all years, all converted into quintiles (across school-year points) to maintain anonymity of schools; a marker for the school's involvement in a universal free breakfast programme; and the school's average cohort size in bands of 10.

Table 2 shows descriptive statistics for the four London LAs that introduced UFSM and never-treated LAs in the rest of London and in the rest of England. These will be our control groups, as we will explain in the next section. Characteristics shown are for the common pre-treatment period for which we have complete data (years-ending 2007–2009). The four ever-treated LAs are characterised by higher proportions of Black ethnicity (except Tower Hamlets) and means-tested Free School Meal-eligible children, living in more densely populated and deprived neighbourhoods (as measured by the unemployment rate or Income Deprivation Affecting Children Index) than the rest of London or England. Children living in these Local Authorities had considerably higher prevalence of overweight and obesity than the rest of England, particularly in school Year 6.

## 4. Empirical strategy

We use the roll-out of Local Authority UFSM schemes described in Section 2 to identify the effect of UFSM on children's bodyweight outcomes. In this section we describe our estimation method and identifying assumptions.

### 4.1. Estimation methods

We use an adaptation of the two-way fixed-effects (2WFE) difference-in-difference estimation with year and school fixed effects. The basic model can be written as follows:

$$Y_{slt} = \beta UFSM_{lt} + \alpha \mathbf{X}_{slt} + \gamma_s + \mu_t + \epsilon_{slt} \quad (1)$$

where $Y_{slt}$ are means of children's bodyweights in school $s$, Local Authority $l$ at time $t$; $UFSM_{lt}$ is a dummy equal to one if the Local Authority is providing Universal Free School Meals at time $t$ and zero otherwise; $\gamma_s$ and $\mu_t$ are school and time fixed effects, respectively, and $\epsilon_{slt}$ is an error term.

The vector $\mathbf{X}_{slt}$ includes a rich set of pupil-level characteristics that are aggregated to school level, as well as school-level and local authority-level characteristics. Together these are designed to strip out the effects of differential trends, shocks, demographic changes and policies that potentially affect childhood obesity, and may be correlated with the introduction of the UFSM treatment. We describe these below.

Our coefficient of interest is $\beta$, the effect of UFSM availability on children's bodyweight outcomes. This is an intention-to-treat effect because we capture the effect of UFSM being available to children, rather than the effect of children taking up school meals. In our setting with variable treatment timing and potentially heterogeneous effects (e.g. because any benefits of policy exposure may cumulate over time), the 2WFE estimator will be biased towards zero (de Chaisemartin and D'Haultfœ uille, 2018; Goodman-Bacon, 2021a; Borusyak et al., 2024). For this reason, we use the two-way fixed-effects estimator only when allowing for separate treatment effects by duration of exposure (i.e. explicitly permitting heterogeneous effects). This model, as also employed by Alex-Petersen et al. (2022), can be written as follows:

$$Y_{slt} = \beta \mathbf{UFSMDUR}_{lt} + \gamma \mathbf{X}_{slt} + \gamma_s + \mu_t + \epsilon_{slt} \quad (2)$$

where $\mathbf{UFSMDUR}_{lt}$ represents a vector of treatment durations in LA $l$ at time $t$ (e.g. one year, two years, etc.) and the other notation is as before.

For our main estimates of the *average* intention-to-treat effect of offering universal free school meals we adopt the Borusyak et al. (2024) imputation method and show results using the method proposed by Callaway and Sant'Anna (2021) as well as the traditional 2WFE estimator for comparison.[16] The principle of this method is to use data from never-treated LAs and from treated LAs before

[14] 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).)

[15] Underweight has a very low prevalence in the population of 1%–2%. As such healthy weight prevalence mostly mirrors overweight-or-obese prevalence, since healthy versus overweight is the main discrete margin affected by underlying changes in the distribution of BMI.

[16] The Borusyak et al. (2024) estimator is suitable for our study because it can accommodate and control for the impact of post-treatment time-varying characteristics. It is important to control for the timing of measurement within the school year because there are strong seasonal bodyweight effects (as documented in Holford and Rabe (2022)) and for changes in the ethnic and gender composition of the children measured and in the economic conditions in the Local Authority, which are all likely to impact children's bodyweights but are unlikely to be endogenous to the treatment. By contrast, Callaway and Sant'Anna (2021) note that post-treatment covariates can potentially be affected by the treatment and their method is therefore designed to incorporate time-invariant or pre-treatment characteristics only.

**Table 2**
Characteristics of treated and control LAs in pre-treatment period (academic years-ending 2007–2009).

| | Treated LAs | | | | Control LAs | |
|---|---|---|---|---|---|---|
| | Newham | Islington | Southwark | Tower H. | London | England |
| **School-level BMI classifications** | | | | | | |
| **Reception** | | | | | | |
| Overweight or obesity prevalence, % | 25.8 | 23.6 | 27.8 | 24.7 | 23.3 | 22.7 |
| Obesity prevalence, % | 14.3 | 11.2 | 14.0 | 14.0 | 10.9 | 9.4 |
| Mean BMI z-score | 0.313 | 0.358 | 0.480 | 0.273 | 0.353 | 0.338 |
| N school-years | 182 | 127 | 191 | 184 | 3,902 | 30,281 |
| **Year 6** | | | | | | |
| Overweight or obesity prevalence, % | 39.2 | 38.0 | 40.8 | 38.5 | 35.7 | 32.3 |
| Obesity prevalence, % | 24.7 | 22.8 | 26.2 | 24.1 | 20.8 | 18.0 |
| 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 |
| **Child-level characteristics** | | | | | | |
| **Reception** | | | | | | |
| Black ethnicity, % | 24.9 | 15.8 | 37.7 | 4.7 | 17.3 | 3.7 |
| Ethnicity missing, % | 2.5 | 34.4 | 18.3 | 34.1 | 19.5 | 40.7 |
| Girls, % | 49.4 | 48.8 | 49.1 | 50.9 | 49.2 | 48.7 |
| N school-years | 182 | 127 | 191 | 184 | 3902 | 30,281 |
| **Year 6** | | | | | | |
| Black ethnicity. % | 26.8 | 17.3 | 32.1 | 6.1 | 17.4 | 3.8 |
| Ethnicity missing, % | 1.2 | 32.9 | 34.1 | 33.1 | 21.2 | 39.4 |
| Girls, % | 49.4 | 49.4 | 49.3 | 47.9 | 49.8 | 48.4 |
| N school-years | 183 | 127 | 191 | 187 | 3860 | 29,225 |
| **School-level characteristics** | | | | | | |
| IDACI Quintile 1 (lowest depriv.), % | 0.0 | 0.0 | 0.0 | 0.0 | 6.4 | 15.7 |
| IDACI Quintile 2, % | 0.0 | 0.0 | 5.4 | 0.0 | 11.9 | 17.5 |
| IDACI Quintile 3, % | 0.0 | 0.0 | 7.7 | < 5.0 | 17.4 | 20.5 |
| IDACI Quintile 4, % | 26.5 | 5.6 | 15.0 | < 5.0 | 28.2 | 23.9 |
| IDACI Quintile 5 (highest depriv.), % | 73.5 | 94.4 | 72.0 | 94.7 | 36.0 | 22.5 |
| Universal Breakfast (ever), % | 14.8 | 39.1 | 12.6 | 23.0 | 9.1 | 4.5 |
| Per-pupil sports premium (2014–2019), £ | 27.2 | 42.3 | 37.7 | 37.4 | 34.1 | 33.5 |
| Average cohort size | 78.2 | 44.2 | 51.0 | 55.4 | 58.2 | 57.4 |
| N school-years | 195 | 127 | 206 | 197 | 4515 | 35,247 |
| **Local Authority-level characteristics** | | | | | | |
| Unemployment rate, % | 4.28 | 4.10 | 3.85 | 5.81 | 2.94 | 2.81 |
| Pop share age 5-9,% | 7.06 | 4.87 | 5.37 | 6.13 | 5.94 | 5.70 |
| Fast Food outlets/1000 pop | 0.360 | 0.670 | 0.499 | 0.572 | 0.470 | 0.456 |
| N school-years | 195 | 127 | 206 | 197 | 4515 | 35,247 |

Note: *IDACI = Income Deprivation Affecting Children Index. Sources: School-level BMI-classification outcomes from National Child Measurement Programme. All school and Local Authority-level characteristics weighted by school's average cohort size. Local Authority-level characteristics plus ward-level unemployment rates from Office for National Statistics. School characteristics derived from Department for Education website, except Universal Breakfast provision, provided by Magic Breakfast. Sample sizes differ because some schools do not serve both age-groups. Excludes schools that ever had cohort size below 20 between 2007 and 2019. Universal Breakfast was provided in less than 0.1% of schools by 2009. School sports premium introduced in academic year-ending 2014.

treatment to predict what treated children's bodyweights would have been in the absence of the policy. The treatment effect then is the difference between the predicted and observed outcomes. The imputation first estimates Eq. (1) without a treatment dummy, based on data for all never-treated schools and pre-treatment observations of treated schools, extracts the school-specific and year-specific fixed effects from this equation, and then predicts each treated school's outcome based on the parameters of the model, observed (time-varying) characteristics, and estimated fixed effects. The difference between the actual (observed) and predicted outcome is the school-year-specific intention-to-treat effect. This is averaged across school-by-cohort observations, weighted by the school's average cohort size to arrive at the average intention-to-treat effect. Standard errors are clustered at the school level.

### *4.2. Parallel trends*

Our main identifying assumption is that, conditional on control variables, the treatment and control group have parallel counterfactual trends in the outcome such that outcomes in treated LAs would have evolved in the same way as untreated LAs in absence of UFSM.

We compare treated LAs to two alternative control groups, (i) the rest of Greater London (comprising 29 Local Authorities) and (ii) the whole of the rest of England. We use London as one of our control groups because pupil characteristics such as ethnicity (important for BMI classification), baseline bodyweight and area deprivation markers linked to obesity differ markedly to other regions, as shown in Table 2. We want to allow for the possibility that there may be a London-specific trend which diverges from the rest of the country, as has been documented for other domains, such as children's educational attainment (Ross et al., 2020). On the other hand, choosing the rest of England as control group minimises the risk that LA-specific shocks and alternative policies will cause significantly different conditional trends to the treated group, and we show below that we can reject non-parallel pre-trends with more certainty for this control group than for the rest of London.[17]

Following recent evidence that, with heterogeneous treatment effects, estimates of deviations from parallel pre-trends that are jointly estimated with post-treatment effects will be contaminated (Sun and Abraham, 2021; Roth, 2022), we use the 'placebo' estimator proposed by Borusyak et al. (2024) to test the parallel trends assumption. This entails estimating a fixed-effects regression on never-treated and pre-treated observations only:

$$Y_{slt} = \sum_{t=-p}^{-1} \beta_t^{pre} COUNTDOWN_{lt} + \lambda \mathbf{X}_{slt} + \gamma_s + \mu_t + \epsilon_{slt} \tag{3}$$

where the dummy variables $COUNTDOWN_{lt}$ are equal to one for schools in eventually-treated Local Authorities in the $t$th period before the treatment is introduced. There is a dummy variable for each of $p$ pre-treatment periods. Our objects of interest are the $\beta_t^{pre}$, which indicate any deviation from parallel trends for a later treated school in period $t$. If pre-trends are conditionally parallel, $\beta_t^{pre}$ should not be significantly different from zero in any pre-treatment period. The relevant test statistic is the joint significance of all the $\beta_t^{pre}$ using an F-test, which we implement with a specification with clustered and heteroscedasticity-robust standard errors.

Results from estimating Eq. (3) for the chosen control groups are shown in Table 3, separately for bodyweight outcomes in Reception and Year 6. Note that $p = 2$ is the correct test as complete pre-treatment observations for all treated LAs are only available for this time-period after using pre-treatment year 2007 as the reference period. We show coefficients and standard errors for each pre-treatment period over this horizon in the top panel of the table, and the key summary test statistics below this, for 2–5 pre-treatment periods $p$. Note that tests for the longer pre-treatment periods are unbalanced and rely more heavily on data for the late adopters of UFSM. F-tests and p-values in Eq. (3) show that we never reject parallel pre-trends for Year 6 outcomes, or for obesity or mean BMI z-score in Reception. For Reception children's overweight prevalence and comparing with the rest of London only, we would marginally reject parallel trends at the 10% significance level ($p$-value = 0.096, upper panel, column 1) when considering a time horizon of 2 years.

### 4.3. Trends, shocks and other policies

We include in our regressions a comprehensive set of characteristics to control for trends, shocks and concurrent policies that may affect children's bodyweight outcomes and could be correlated with the introduction of UFSM. School and year fixed-effects account for unobserved time-invariant school characteristics and common economic and social shocks to obesity, and half-term of academic year-fixed effects account for seasonal variation in children's bodyweights (Anderson et al., 2011; Holford and Rabe, 2022). We control for changes in ethnic and gender composition in schools by including the proportion of children measured who are Black, whose ethnicity is not recorded, and who are girls. We interact each of these variables with dummies for half-term of measurement and with cubic year-trends to account for differential metabolic responses to season and economic and social trends. Overweight and obesity calculations in the British 1990 growth reference charts are by age and sex but not ethnicity, which means that controlling flexibly for ethnicity is particularly important.

We account for differential trends by neighbourhood deprivation by including in our regressions the Income Deprivation Affecting Children Index (IDACI) of the neighbourhood in which schools are located, and their interactions with cubic year-trends. At the Local Authority-level we control for the following variables that we treat as exogenous: the unemployment rate, affecting households' budgets; the density of fast food outlets per 1000 children, affecting the local food environment; and the share of the population aged 5–9.

Further, we control as far as possible for policies active in the same time-period that may affect children's bodyweight. First, we include a marker for the school implementing a universal free breakfast scheme funded by the Magic Breakfast charity or the government's National School Breakfast Programme. Second, we account for the implementation of the national School Sports Premium policy, which from 2013/14 onwards provides funding to schools to facilitate improvements in quality of physical education or activity within a school, by including the per-pupil funding received by each school in each academic year.

Note that Local Authorities (or before 2013, Primary Care Trusts) have a statutory duty to attempt to improve public health outcomes, including child obesity. Control Local Authorities in England will have implemented a variety of policies or interventions to do so, and the implementation or retention of UFSM in the treated Local Authorities may have reduced their budget for such other interventions. These could include active travel schemes and infrastructure, access to leisure centres and activities, food education and health promotion in schools, and weight management services for those already living with overweight or obesity (National Institute for Health and Care Research, 2022). We do not observe these interventions. Our estimates of the treatment effect of Universal FSM should therefore be thought of as the impact of Universal FSM relative to what was implemented elsewhere or would have been implemented in the treated Local Authorities instead of UFSM, rather than compared to no health interventions.

To assess the possibility of any residual time-varying differences between the treatment and comparison groups that should not be affected by the treatment but might be related to bodyweight outcomes, we track movements in house prices, and in adult body

[17] We considered selecting a smaller group of very similar LAs as control group and tested the sensitivity of the parallel trends assumption to the inclusion/exclusion of control LA's. This exercise is presented in online Appendix A and shows that choosing a narrow control group is more likely to result in non-parallel pre-trends than wider control groups. It also risks arbitrary cut-offs for the number of control LAs being used.

**Table 3**
Test for parallel pre-trends in children's bodyweight outcomes.
*Source:* National Child Measurement Programme.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Overweight or obese, % | | Obesity prevalence, % | | Mean BMI z-score | |
| Control group: | London | England | London | England | London | England |
| **Reception** | | | | | | |
| 2 year horizon | | | | | | |
| β 1 year before | 1.015 | 0.186 | −0.066 | −0.425 | 0.021 | −0.003 |
| | (0.673) | (0.625) | (0.490) | (0.453) | (0.021) | (0.020) |
| β 2 years before | 1.300 | 0.732 | 0.453 | 0.153 | 0.041** | 0.024 |
| | (0.617) | (0.594) | (0.442) | (0.425) | (0.019) | (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 |
| 3 year horizon | | | | | | |
| F | 1.770 | 0.597 | 0.585 | 0.765 | 1.631 | 0.846 |
| p(F) | 0.151 | 0.617 | 0.625 | 0.514 | 0.180 | 0.468 |
| 4 year horizon | | | | | | |
| F | 1.419 | 0.456 | 0.452 | 0.58 | 1.532 | 0.696 |
| p(F) | 0.225 | 0.768 | 0.771 | 0.677 | 0.190 | 0.594 |
| 5 year horizon | | | | | | |
| F | 1.141 | 0.411 | 0.363 | 0.466 | 1.484 | 0.716 |
| p(F) | 0.336 | 0.841 | 0.874 | 0.802 | 0.192 | 0.612 |
| N | 11,572 | 81,938 | 11,572 | 81,938 | 11,572 | 81,938 |
| **Year 6** | | | | | | |
| 2 year horizon | | | | | | |
| β 1 year before | 1.072 | 0.759 | −0.26 | −0.41 | 0.021 | 0.009 |
| | (0.655) | (0.612) | (0.601) | (0.573) | (0.020) | (0.019) |
| β 2 years before | 0.860 | 0.621 | 0.349 | 0.261 | 0.022 | 0.012 |
| | (0.669) | (0.647) | (0.610) | (0.594) | (0.018) | (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 |
| 3 year horizon | | | | | | |
| F | 1.159 | 0.826 | 0.463 | 0.691 | 0.778 | 0.609 |
| p(F) | 0.324 | 0.479 | 0.708 | 0.557 | 0.506 | 0.609 |
| 4 year horizon | | | | | | |
| F | 1.491 | 1.632 | 0.452 | 0.684 | 0.677 | 0.833 |
| p(F) | 0.202 | 0.163 | 0.771 | 0.603 | 0.608 | 0.504 |
| 5 year horizon | | | | | | |
| F | 1.374 | 1.340 | 0.445 | 0.578 | 0.569 | 0.667 |
| p(F) | 0.231 | 0.244 | 0.817 | 0.717 | 0.724 | 0.649 |
| N | 18,213 | 130,307 | 18,213 | 130,307 | 18,213 | 130,307 |

Estimated on never-treated LAs and pre-treatment observations of schools in treated LAs only. Standard errors in parentheses. *, **, *** indicates statistical significance at the 10%, 5% and 1% significance levels. Additional controls in underlying regression: Half-term of measurement; school and year fixed effects; percent measured Black, percent measured missing ethnicity, percent measued girls, all demeaned within-schools and interacted with half-term of measurement and with four IDACI quintile dummies; Cubic year-trends by IDACI quintile; 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.

weights, adult smoking, and infant mortality over time. Respectively these capture potential shocks to wealth or housing costs that could affect households' resources, and three outcomes which are important indicators of wider public health. We present these in Figure B1 in online Appendix B. We find no evidence that wealth or indicators of health were improving in the treated Local Authorities relative to their comparators.

### *4.4. Cross-border mobility of schoolchildren*

We check for cross-border mobility of children. Families might move house across Local Authority borders to take advantage of UFSM, as school places are primarily allocated according to the place of residence. If such families differ from the general population in ways that affect bodyweight this would bias our results. While we cannot provide direct evidence of the types of children crossing Local Authorities to attend school, we can look for evidence of parents moving house at the start of children's schooling into LAs that offer UFSM, and assume that these children are positively selected, given the resources required to move.

The left panels of Fig. 1 plot the estimated resident populations of 5 year-old boys and girls over time, relative to the academic year-ending 2008, for our four treated LAs, for the LAs which border them, and for the eight most similar LAs (identified using the procedure described in online Appendix A). Growth in these populations in treated Local Authorities follows a similar pattern, but typically with slower growth than the comparison Local Authorities, suggesting that cross-border house moves are unlikely to affect our results.

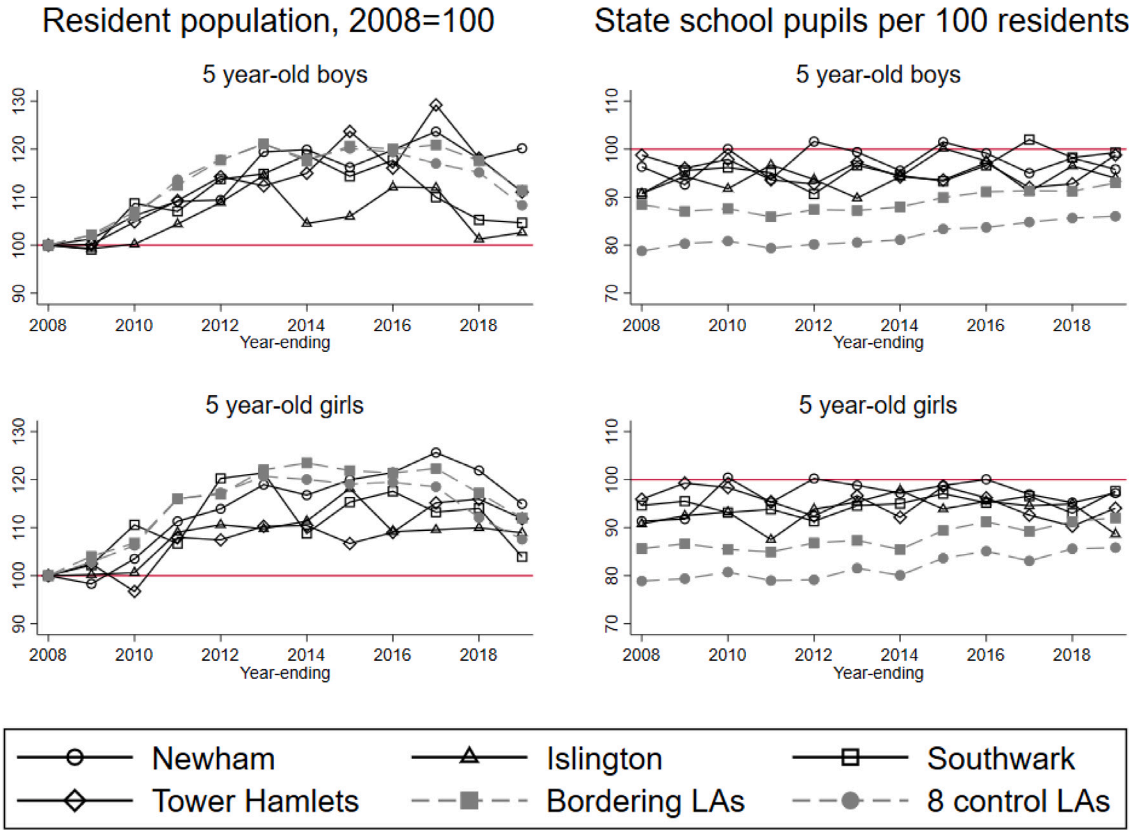


**Fig. 1.** Investigating mobility of children across Local Authorities.
Notes: Department for Education 'Schools, Pupils and Characteristics' datasets, and Office for National Statistics Population estimates — local authority by single year of age. Bordering LAs are: Hackney, Lambeth, Redbridge, Barking and Dagenham, Camden, Haringey, Waltham Forest, Lewisham, and Greenwich. The 8 control LAs are: Hackney, Lambeth, Kensington and Chelsea, Barking and Dagenham, Camden, Hammersmith and Fulham, and City of Westminster.

We further check for evidence of mobility into UFSM Local Authorities by tracking the ratio of state school pupils to the resident population over time. An increase in this ratio in the treated LAs and decrease in the contiguously neighbouring LAs that coincides with the introduction of UFSM would indicate that parents are sending children to attend school across LA borders or that they switch from private schools to state schools in treated LAs to benefit from UFSM. This would be an issue if such pupils differed in unobserved characteristics from other pupils, but we do not find such a pattern.

## 5. Results

### 5.1. Main results

Fig. 2 shows the average intention-to-treat effects of UFSM availability on children's bodyweight outcomes in Reception (age 4/5) and Year 6 (age 10/11), estimated using the Borusyak et al. (2024) imputation method. For each outcome and age, the light bars on the graph show results based on using the rest of London as the control group, whereas the dark bars use the rest of England as the control group.

Starting with the results for obesity in the middle panel of Fig. 2, we find statistically significant beneficial impacts of UFSM on obesity prevalence for both Reception and Year 6 children, and based on both control groups. Impacts on obesity prevalence are similar in magnitude for both Reception and Year 6 children, at a 0.6–0.9 percentage points reduction compared with the rest of London, or a 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.[18] Note that the confidence intervals on the estimates derived using the rest of London substantially overlap those derived for the rest of England here and in all the results that follow.

[18] We show results from a parallel pre-trend test for the *difference* between Year 6 and Reception outcomes in the same school, in Table C1 in online Appendix C. We find no significant differential that can help explain the distinction between the impacts we find on children of different ages.

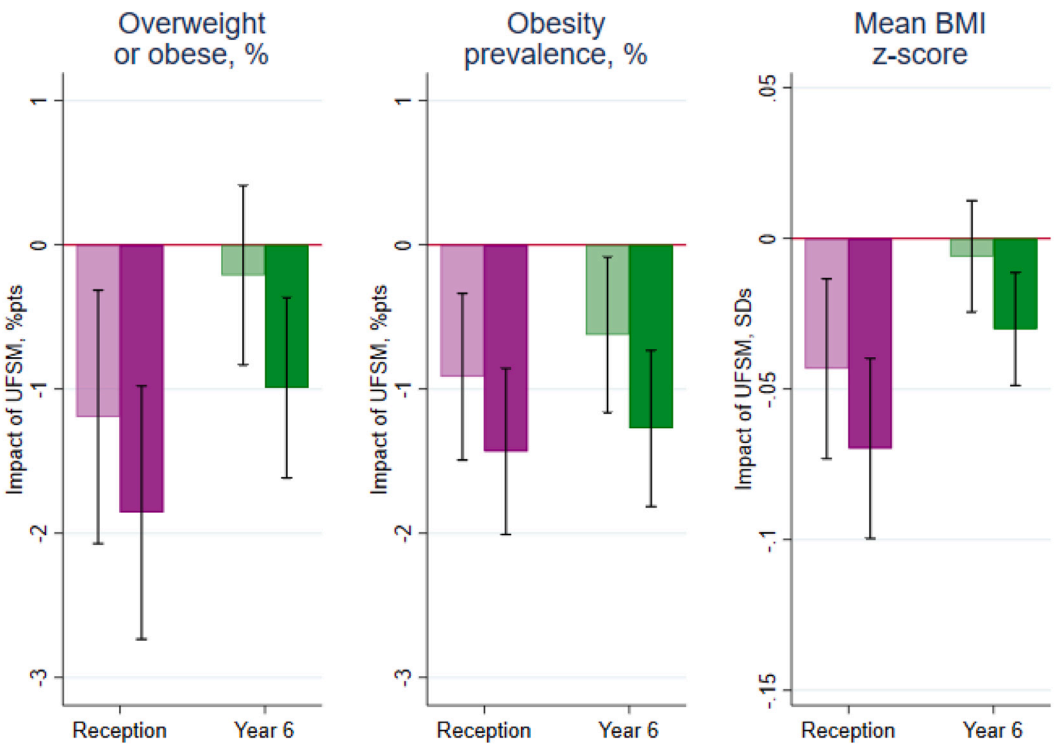


**Fig. 2.** Treatment effects of Universal FSM on bodyweight outcomes.
Notes: Within year groups, light bars use rest of London as control group, dark bars use rest of England as control group. Borusyak, Jaravel and Spiess imputation method with school and academic-year fixed-effects. For Reception outcomes 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. Additional controls in underlying regression: Half-term of measurement; school and year fixed effects; percent measured Black, percent measured missing ethnicity, percent measured girls, all demeaned within-schools and interacted with half-term of measurement and with four IDACI quintile dummies; Cubic year-trends by IDACI quintile; 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.
*Source:* National Child Measurement Programme.

The underlying estimates and how they change when successively adding controls are shown in online Appendix D, Table D1. The estimated benefits to Year 6 children's outcomes are sensitive to omitting school and Local Authority-level controls, the intuition for which is that this fails to account for the prevailing faster increase in obesity prevalence in more deprived areas of England (NHS Digital, 2022b), among which our treated schools are predominantly situated (see Table 2). We show in online Appendix D, Table D2, that our results are of similar magnitude when varying the specifications further, and when using the Rios Avila et al. (2021) implementation of Callaway and Sant'Anna (2021) or the traditional Two Way Fixed Effects estimator.

Our main results can be put into context by comparing them with the impact of other interventions targeting children's bodyweight outcomes. First of all, the effect size for Reception children is remarkably similar to that found for the English Universal Infant Free School Meal policy for children of the same age, estimated based on a different identification strategy (Holford and Rabe, 2022). Chesham et al. (2018) studied the impact of a year long trial of the 'Daily Mile' (15 min walking or running per school day) across all ages in primary school and found this reduced BMI z-score 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 only slightly larger than 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 bodyweights.

As discussed, our results are intention-to-treat effects of making UFSM available in primary school. The policy – very roughly – led to one in three children newly taking up a school meal rather than bringing a packed lunch from home, while it was unlikely to lead to sizeable income and work incentive effects (see Section 2). If effects are therefore driven mainly by changes in take-up, this suggests that the average treatment effect on the treated could be in the region of three times larger than our estimated effect.

In contrast to the results on obesity, the effect of UFSM on overweight *or* obesity prevalence ("overweight or obese") shown in the left panel of Fig. 2 is markedly larger in Reception at 1.2 percentage points (compared with the rest of London) and 1.9 percentage points (compared with the rest of England) than in Year 6, where the reduction in prevalence is not different from zero when using the rest of London as control group and 1 percentage point when using the rest of England. As for obesity, the baseline prevalence of overweight/obesity is lower in Reception (25%) than in Year 6 (39%). The effects in Reception represent a 5%–8% reduction and the effect in Year 6 at most (based on the comparison with the rest of England) a 2.5% reduction in overweight/obesity. The larger effect at the obesity than overweight threshold suggests that the beneficial impacts of UFSM are concentrated among children towards the top of the bodyweight distribution.[19]

The pattern is similar for the BMI z-score, shown in the right hand panel. UFSM significantly reduces the average BMI z-score among Reception children by 4.3% 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 statistically significant and of a quantitatively important magnitude for the rest of England control group, at 3.0% of a standard deviation. For Reception children, a 1% reduction in BMI z-score implies a lower weight of 15 g for boys and 18 g for girls (Holford and Rabe, 2022), implying that boys in Reception have between approximately 65 g and 105 g lower bodyweight and girls between about 75 g and 125 g lower bodyweight than they would have had in absence of UFSM. An average change of this magnitude would be feasible within the period between starting school and timing of measurement, even if entirely driven by the approximately 14% of children initially living with obesity: Pitt County Pediatric Dietitians and Nutrition Educators (2020) advise deducting 125 calories per day to achieve 114 g of weight loss per week among already-obese 6 year-olds. Halving this calorie reduction to approximately reflect the expected difference between school meals and the counterfactual meal in Evans et al. (2020), a back of the envelope calculation suggests our estimated weight loss would be achieved in between 8 and 16 weeks.

Taken together, our results indicate that younger children's BMI and the thresholds derived from it react more strongly to changes in food intake and environment than older children's (age 10/11). Effects are smaller in Year 6 than in Reception, despite Reception children all having been eligible for UFSM for less than a year at the time of weight measurement, while Year 6 children had been eligible for UFSM between less than a year and up to 7 years, depending on when UFSM were introduced in their LA. This suggests that bodyweights of older children are harder to shift. This may be because they have more autonomy over what they eat inside and outside of school (Cetateanu and Jones, 2014; Ianotti and Bush, 2014). It may also be because school meals comprise a smaller proportion of older children's total energy intake,[20] or because older children eat a larger share of calories later in the evening, and other things equal these calories have a bigger impact on bodyweight than those eaten earlier in the day (Mahdi et al., 2023).

### 5.2. Heterogeneous effects

*Duration of exposure*

We test whether the impacts of Universal FSM on Year 6 children are larger for those who have received UFSM for longer, depending on when their LA first started providing UFSM. We estimate Eq. (2) which allows for separate treatment effects 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. Fig. 3 shows results for obesity prevalence and BMI z-score when using the rest of London as control group on the left hand side, and equivalent results when using the rest of England as control group on the right hand side.

Across both specifications and outcomes the Figure shows that there is no significant impact on children who received UFSM for the first time in Year 6 (for up to 1 year). This provides evidence that bodyweights of older children are not as responsive within their first year of exposure to UFSM availability as those of younger children at the start of primary school, in line with what we found earlier.

Though a number of estimates in Fig. 3 are not statistically significantly different from zero, particularly when using the smaller London control group, the Figure broadly shows a relationship where the impact of UFSM is the smallest for Year 6 children receiving UFSM for the first time and largest for Year 6 children who have been exposed to UFSM throughout primary school. This suggests that while Year 6 pupils do not respond quickly to UFSM introduction, there seems to be a cumulative impact of UFSM availability that builds up over the years of entitlement, though the evidence is suggestive only. For the children who were exposed to UFSM throughout primary school we find a 1–2 percentage points reduction in obesity prevalence, and 4%–7% of a standard deviation reduction in the BMI z-score. We do not suggest this tentatively-cumulative relationship reflects a period of transition to a new settling point for BMI, which we would expect to occur more quickly (Müller et al., 2016). Rather, this could reflect children's long-term habits and preferences being more malleable if first-exposed at a younger age (Birch, 1980) or where the initial intervention coincides, at the start of primary school, with a wholesale change in the child's and peer group's entire routine and social environment (Kwasnicka et al., 2016).

*School environment*

We are also interested in how the effects of UFSM differ in schools with greater or lesser pre-existing prevalence of obesity, for example when considering explicitly targeting schools with high obesity prevalence. Here, for each year-group, we split the sample into schools with pre-treatment obesity prevalences (residualised by timing of measurement) that are above or below the median among schools in treated Local Authorities during the years-ending 2007–2009, before the first Universal FSM scheme was

[19] We cannot estimate effects at different points of the BMI distribution because our data are aggregated to school level.

[20] Swinburn et al. (2006), shows that the relationship between calorie intake and steady state bodyweight can reasonably be modelled as invariant to age and height, meaning that the same absolute difference in calorie intake will make less of a difference to BMI for children with higher initial intake.

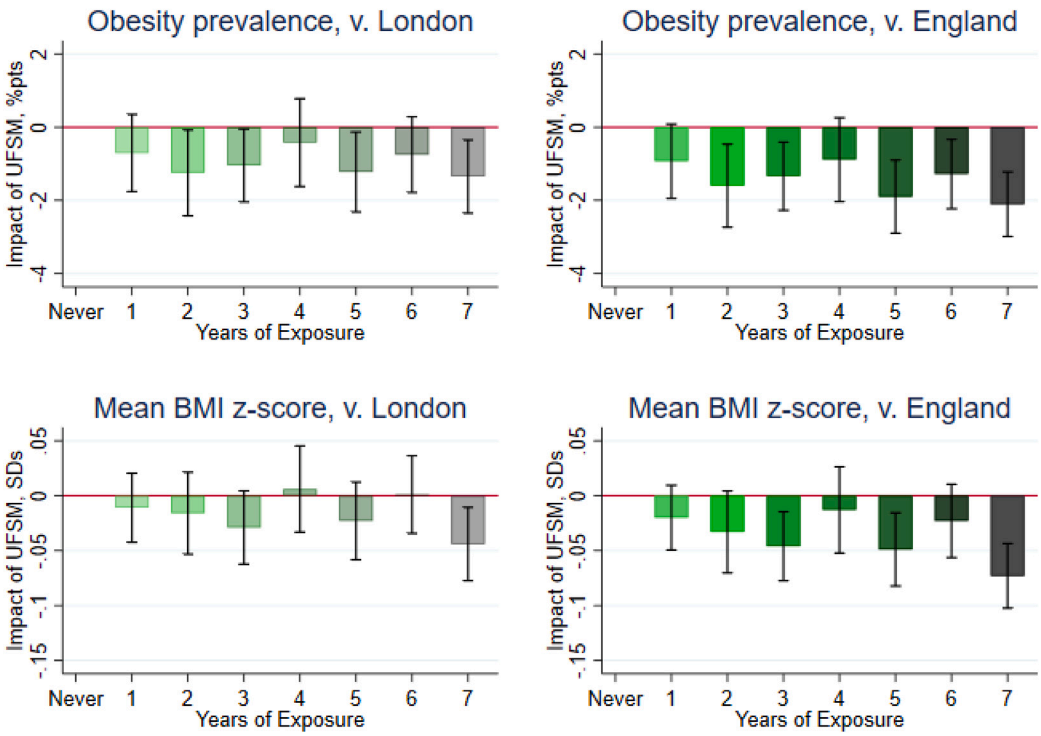


**Fig. 3.** Heterogeneous treatment effects of Universal FSM on Year 6 children's bodyweight outcomes, by duration of exposure.
Notes: Light bars use rest of London as control group, dark bars use rest of England as control group. Pooled two-way school and academic-year fixed-effect regression with separate 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. See Fig. 2 for the list of additional controls in underlying regression.
*Source:* National Child Measurement Programme.

introduced. Note that the median is defined within the treated Local Authorities. As suggested by Table 2, the vast majority of these are above the national median.

Test statistics for parallel pre-trends are shown in Table C2 in online Appendix C. For Reception children in below-median obesity schools there is one significant positive (i.e. unfavourable) coefficient and joint significance of countdown coefficients ($p$ = 0.057) when compared with the rest of London, and we bear this in mind when interpreting our results, below. For Year 6 children all four tests pass (minimum $p$-value 0.551), and no coefficients are statistically different from zero, nor the corresponding coefficients for above and below median schools different from each other.

Fig. 4 shows results for high-obesity schools on the left hand side and low-obesity schools on the right hand side. The lighter shaded bars again represent results estimated when using the rest of London as control group and the dark bars the rest of England. Both for Reception and Year 6 and based on both control groups we find statistically significant benefits of UFSM availability for children in schools with initially lower obesity prevalence. The pre-trends test fails for Reception children compared with Rest of London, but with obesity increasing in the treated LAs in the countdown period, this should bias us towards finding no benefit (see Table C2 in online Appendix C). 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 LAs 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 Income Deprivation Affecting Children Index (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

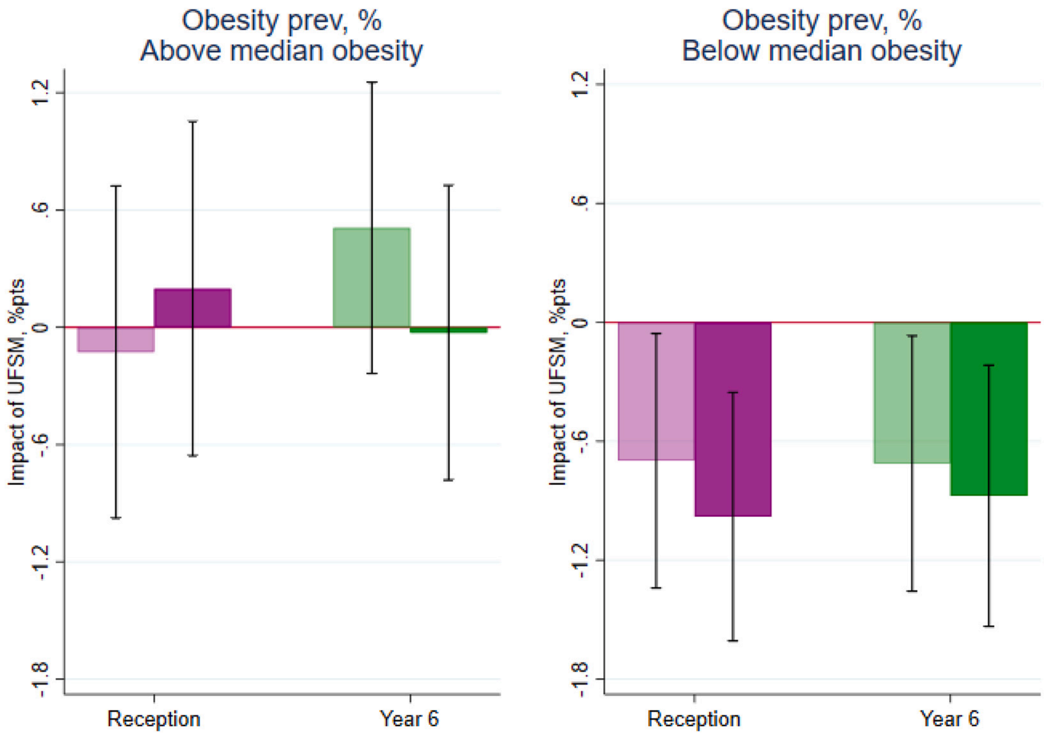


**Fig. 4.** Heterogeneous treatment effects of Universal FSM on Year 6 children's bodyweight outcomes by pre-treatment school-level bodyweight outcomes.
Notes: Within year groups, light bars use rest of London as control group, dark bars use rest of England as control group. Borusyak, Jaravel & Spiess imputation method with school and academic-year fixed-effects applied separately to each distinct population. 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: 3223/8193 (Reception, v. London), 13,428/66,177 (Reception, v. England), 4469/ 12,975 (Year 6, v. London), 13,814/106,665 (Year 6, v. England). Capped lines indicate 95% confidence intervals. See Fig. 2 for the list of additional controls in underlying regression. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)
*Source:* National Child Measurement Programme.

impacts would be greater if a Universal Primary FSM scheme were rolled out across the country, where the obesity crisis, while serious, is less entrenched than in our four treated London Local Authorities.[21]

## 6. Conclusions

This paper examines Universal Free School Meals programmes run in London Local Authorities to explore their effect on children's bodyweight and how this varies by the age of the child, the duration of exposure, outcome measured, and obesity prevalence in the school. In doing so, we reveal some of the programme features that may maximise the effects on children's weight status. More broadly, from a health production perspective, this analysis contributes to understanding how health inputs (nutritious meals) contribute to producing health outcomes (children's healthy weight), and specifically how productivity of inputs varies by age and effects accumulate over time.

Based on nurse-collected data for the universe of state school children in England, we find that UFSM schemes reduce the prevalence of childhood obesity. Effects are larger at younger than older ages, with obesity prevalence reducing by 7%–11% at age 4/5 and 2%–5% at age 10/11. This is despite the younger children having been exposed to UFSM for less than a year at the time of weight and height measurement, and older children between less than a year and up to seven years. This is consistent with children's bodyweights being harder to shift at older ages. Evidence tentatively suggests that effects are cumulative and are the largest for children who were exposed to UFSM throughout primary school. Results also indicate that UFSM are most productive for children at the higher end of the weight distribution, but not in schools with high obesity prevalence, where obesity is more entrenched.

[21] We repeated this exercise, instead splitting schools into terciles or quartiles. We do not report results here because the confidence intervals on the treatment effects are somewhat larger, as we would expect given the smaller sample, and the pool of comparable control-area schools for the higher-obesity treatment schools is very small. We do not find either a monotonic pattern in point estimates across obesity levels, or a non-monotonic pattern that replicates across year groups or control groups. However, we do consistently find quantitatively important negative point estimates for the lowest-obesity schools, which bolsters our interpretation that UFSM is not a panacea but will have beneficial effects when complementing a relatively favourable environment.

As more countries and local areas look to introduce universal free school meal schemes, it is important to evidence the programme features that are likely to maximise any educational and health benefits to children. Our suggestive finding of a cumulative effect over time of providing free meals, together with evidence on the relative difficulty of shifting bodyweights of older children in a short period of time, indicates that starting free meal provision early and maintaining it throughout primary schools may maximise the impact on cutting obesity rates and thereby contribute to addressing the significant long-term healthcare and indirect productivity costs of obesity. These effects depend on the school meals being of higher quality and less calorie dense than the counterfactual meal in each setting. As we have shown, this is not sufficient to realise the potential of UFSM however. We find an important role for the school and/or home environment, suggesting that without additional support UFSM will not necessarily reduce health inequalities.

## CRediT authorship contribution statement

**Angus Holford:** Writing – review & editing, Writing – original draft, Visualization, Validation, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Birgitta Rabe:** Writing – review & editing, Writing – original draft, Validation, Methodology, Investigation, Funding acquisition, Conceptualization.

## Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jhealeco.2024.102937.

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