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Going universal. The impact of free school lunches on child body weight outcomes☆

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ABSTRACT

We study the impact on young children's bodyweight of switching from means-tested to universal provision of nutritious free school meals in England, exploiting identifying variation in the timing of weight measurements. We show that exposure to high quality universal free lunches increases healthy weight prevalence and reduces obesity prevalence and BMI by the end of the first year of school. The effect seems driven by substitution of home-produced lunches with school meals among children not eligible under means-testing, with little evidence of income or parental labour supply effects. This suggests universal provision can improve the diets of relatively well-off pupils.

1. Introduction

Childhood overweight and obesity is one of the most serious worldwide public health problems, known to have serious implications for children's health which carry on into adulthood and cause significant healthcare and indirect productivity costs. Worldwide, 41 million children under the age of 5 and over 340 million children and adolescents aged 5–19 are estimated to be overweight and obese (WHO, 2021), a tenfold rise in the past four decades (NCD Risk Factor Collaboration, 2017). Childhood Body Mass Index (BMI) and obesity have been shown to be strongly persistent into adulthood (Singh et al., 2008; Simmonds et al., 2015), where obesity is well understood to be a risk factor for a wide range of diseases (OECD, 2019). Addressing the determinants of childhood obesity is therefore a policy priority for many governments worldwide. Because children consume a large fraction of their food energy at school, school meal provision is an obvious policy lever to increase rates of healthy weight among children (Davies, 2019).

This paper investigates whether providing free, high quality lunches to children in school can contribute to reducing childhood obesity. We study the Universal Infant Free School Meal (UIFSM) policy, implemented in England from September 2014, under which all children in state schools are eligible to receive a free lunch during their first 3 years of schooling, at age 4–7. Before 2014, children of all ages (about 18% of 4–7 year olds) were already eligible for free lunches of a high nutritional standard under a means-tested programme while children who were not eligible could purchase the same meal at cost. This setting allows us to study what happens to children's bodyweight outcomes when a means-tested school food programme is made universal across the whole country.

Traditionally, school food programmes use means-testing to target the children most in need of a free meal and to avoid the deadweight implied in subsidising meals for families who could afford to pay for them. However, in recent years there has been a move towards universal provision of free meals. The English policy is a case in point, and further examples can be found in the US, where Obama's 2010 Healthy Hunger-Free Kids Act allowed schools to provide free meals to all children in high poverty areas. Several large urban school districts

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including New York, Boston, Detroit and Atlanta, for example, have made school breakfasts and lunches free for all students (Leos-Urbel et al., 2013; Schwartz and Rothbart, 2020). These policies often aim to raise educational attainment, social skills and behaviour, and to ensure healthy eating in contexts where not all students take up school meals, including those eligible to receive them for free.

Making school meals universally available has been shown to address the potential stigma attached to receiving a free meal, and to send a signal that the school lunch is a desirable good, thereby raising participation amongst all students, not just those facing a change in price (Holford, 2015). There may also be economies of scale in lunch provision when participation increases under a universal scheme, and a reduction in the burden of administering means-testing. Further, if the meals provided in school are of high nutritional quality and appropriate energy content, they should be of benefit to all children whose counterfactual meal (that provided from home or shop-bought) is of lesser quality and of higher calorie content. It may be reasonable to assume that the poorest families have the least means to provide a nutritional meal from home. However, means-tested targeting works with essentially arbitrary threshold measures and is likely to miss children who would benefit from a free meal at the margin of eligibility.[1] Moreover, it has been shown that the rise in female employment in the last few decades is associated with less time spent on home food preparation, lower levels of fruit and vegetable consumption and consequently higher childhood obesity rates (Cawley and Liu, 2012; Bauer et al., 2012; Moser et al., 2012; Datar et al., 2014), suggesting that meal quality may be low even in high-income families.[2] Based on this, we may expect benefits from extending free meals from low income groups to all students.

Our analysis is based on anonymised school-level data from the National Child Measurement Programme (NCMP) from the 2008/09 to 2017/18 academic years. The NCMP was set up to monitor trends in growth patterns and obesity in England. It involves trained nurses collecting data on the heights and weights of children in primary school, at ages 4–5 and 10–11. Every primary school in England (approximately 16,000 in total) is visited once every academic year, and we have unique data on the timing of each school visit, school-level weight outcomes and a number of control variables for the universe of schools in England.

Using these data, we exploit the timing of the school visit to measure height and weight as a source of variation in duration of exposure to UIFSM at the time of measurement, among children in their first year of primary school (aged 4–5). Those children weighed and measured early in the school year will have had little exposure to free meals while those whose school was visited later in the year will have had access to a larger number of free lunches. This allows us to assess the impact of different lengths of exposure within the first school year. We compare the change in bodyweight outcomes of children throughout the school year in a school fixed-effect difference-in-difference style framework, where children observed in the years before the introduction of UIFSM in academic year 2014/15 serve as our control group and those observed after its introduction are treated for varying lengths of time. Our identifying assumptions are that conditional on controls and fixed effects, the timing of the data collection visits is unrelated to factors affecting bodyweight outcomes; and that child outcomes would have evolved in the same way over the school year in the post- as in the pre-reform years. This includes the effect of the school environment on children's bodyweight, which existing literature suggests is likely to be beneficial, and which we expect to be constant over time.[3] We present a number of tests that probe these assumptions and suggest that they hold.

We find that a larger 'dose', or longer exposure to UIFSM, has a beneficial impact on bodyweight outcomes. By the end of the school year (190 school days), on average a child exposed to UIFSM is 1.1 percentage points more likely to be of 'healthy weight', 0.7 percentage points less likely to be obese, and has body mass index (BMI) that is 4.1% of a standard deviation lower than a child not exposed to the policy. These are the intention-to-treat effects of UIFSM which suggest that expanding availability of free meals in school from about 18% of children to 100% of children leads to modest improvements of bodyweight outcomes in the short term.

Previous evidence suggests that reductions in BMI can persist where school-based interventions are maintained in the long-term (see Shaya et al., 2008; Driessen et al., 2014, for reviews).[4] While it is too early to see the longer-term effects of UIFSM, the literature indicates that even short-term improvements in bodyweight are likely to lead to long-term health benefits. The high persistence of bodyweight outcomes over the life course makes it difficult to identify the long-term effects of child obesity independently from the effects of bodyweight as an adult (Park et al., 2012). Studies that directly relate childhood bodyweight to adult outcomes include Fagherazzi et al. (2013) who showed obesity among prepubescent girls to be associated with an increased risk of breast cancer post-menopause, and Tirosh et al. (2011) and Field et al. (2005) who respectively found higher BMI in childhood to be associated with greater risk of coronary heart disease and hypertension as an adult, with the estimated effect size maintained or larger after controlling for BMI as an adult. Moreover, the *duration* of obesity over a lifetime has been clearly shown to increase the risk of cardio-vascular disease and cancer-related mortality (Abdullah et al., 2011) and early onset of type 2 diabetes (Everhart et al., 1992). Our results therefore suggest that UIFSM will indeed help mitigate the long-term impacts of obesity, making this evidence important both to health service providers and policymakers (Reilly and Kelly, 2011).

We explore the possible mechanisms driving improvements in bodyweight by using school census and household survey data. We find that children who experienced no lunch price change because they were already eligible for free meals increased take-up by about 3 percentage points while children affected by a price change increased take-up by about 50 percentage points. This indicates that while the policy may have reduced some of the barriers to taking up free meals for always eligible children, the impact on bodyweight outcomes is likely driven by changes in the diets of children that were not eligible for free meals before they were made universally available. This is supported

[1] Hobbs and Vignoles (2010) document that eligibility for Free School Meals is not a precise proxy for family income. While eligible children are on average in households with much lower incomes than not eligible children, many eligible children are not in the lowest income households, and many children in very low income households are not eligible.

[2] This is in line with Schwartz and Rothbart (2020) who study a shift from means-tested to universal free meals in New York City middle schools and find improvements in educational outcomes as well as some indication that bodyweight outcomes may be improved among the non-poor.

[3] Our identification strategy allows for duration of exposure to the school environment to have its own direct effect on bodyweight outcomes. von Hippel et al. (2007) show that BMI of American schoolchildren increases more slowly during Kindergarten and First Grade than during the summer vacation between them. Anderson et al. (2011) show that after accounting for endogeneity in school starting age, children of the same age with a year more schooling have (marginally) healthier bodyweight outcomes, conducting robustness checks to demonstrate that this treatment effect is strongest for those experiencing the biggest change in environment. Figs. 2 and 3 below also show that in the years prior to UIFSM introduction, children's bodyweight improved over the course of their first year in school.

[4] For example, Sanchez-Vaznaugh et al. (2010) and Taber et al. (2012) study the introduction by some US states of regulations on food sales 'competing' with those in the school, and find these suppressed increases in BMI and overweight prevalence for children tracked over at least 4 years, relative to those in other states. Manios et al. (1998) and Kafatos et al. (2005) study the effects of curriculum changes and physical activity worked into the regular school day of Cretan children for 6 years, finding significant effects on BMI during the programme and then 4 years after its cessation.

by heterogeneity analysis which shows that the impact of the policy is concentrated in schools with a low proportion (but not the very lowest) of children eligible for free school meals (FSM) pre-policy, and suggests that the diets of relatively well-off pupils can still be improved.

We also test whether the programme improved household finances through reduced food expenditure, thereby generating income effects on weight outcomes, but find that the savings were small and therefore unlikely to give rise to income effects. Further, because work disincentives associated with means-testing were effectively removed, we look at whether the policy increased hours worked, which may have impacted children's weight positively through increased income or negatively through time constraints on (healthy) food preparation. We find at most minimal increases in work hours, suggesting that impacts on bodyweight were mainly caused by increased take-up of meals.

We consider a number of threats to our empirical strategy. In particular we check whether the timing of school visits for children's height and weight measurement was related to characteristics that may affect children's bodyweight outcomes. Within-school shifts in the timing of school visits that are correlated with children's body weight and coincide with UIFSM introduction could bias our results. We find that any such shifts were small in magnitude, and we control comprehensively for other policies introduced during our analysis period as well as for differential trends within and between years by demographic and socio-economic characteristics. Simulations show that after controlling for observable characteristics, unobservable factors would have to have a considerable effect on timing to reverse our results. We also show that our results are likely to be valid for schools with a wide range of characteristics. Further, we show that the within-year trend in children's bodyweight we see after UIFSM were introduced was not apparent in the pre-policy years. These and further checks presented in the Appendix assure us that our findings are robust.

Most of the existing evidence on the effect of *means-tested* free school lunches on bodyweight outcomes, identified through income-eligibility 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).

The smaller literature on the effects of *universal* free school meal provision reaches a different conclusion. Schwartz and Rothbart (2020) and Rothbart et al. (2020) exploit shifts from means-tested to universal free meals in New York City and elsewhere in New York State respectively. Neither paper finds evidence of damaging effects on BMI on average. Schwartz and Rothbart (2020) find improvements for non-poor students in their study of middle schools, and Rothbart et al. (2020) for secondary-age pupils. Note that both studies focus on districts that introduced universal entitlement within relatively high-poverty schools, whereas our paper looks at a nation-wide switch.[5] More comparable to our setting is Alex-Petersen et al. (2021), who exploit reforms instituted through the 1950s and 1960s, to investigate the long-run effects of introducing free and nutritious school lunches on a universal basis in Swedish primary schools, eventually covering the whole country, on a range of outcomes. They find no effect on the probability of being overweight or obese at age 18, but substantial benefits to other health measures, educational attainment and lifetime earnings. Note that children in all of these papers are older than the children in our setting, and are at an age where they have more autonomy over the alternative meals consumed when not participating in school meal programmes.

The first contribution of this paper is specifically in relation to this literature: To evaluate the move from a high quality means-tested school meal programme to a free, universal school-based nutrition programme. This is a relevant policy option in a large number of countries that already run high quality, means-tested free meal policies. While several European countries provide free milk and/or fruit at school to some age-groups on a universal basis, very few offer a full free meal (Polish Eurydice Unit, 2016). Besides England, Scotland also provides a free lunch to all children aged 4–7, but only Sweden and Finland provide this throughout the whole of school. Our setting provides a unique opportunity to improve our understanding of the potential effects of a switch to universal provision (for some or all age groups) in the many countries that already serve meals at school and provide these free or at reduced cost on a means-tested basis, including major economies such as the United States, France, and Germany.

Our analysis further contributes to the wider literature on the relative advantages of universalism versus targeting or means-testing. This has gained importance in other education and labour market policy areas. For example, the provision of free early years childhood education and care started out in the 1980s as a policy directed to families on low incomes but has in the last 20 years been expanded substantially with many countries now offering universal childcare support (OECD, 2001), essentially extending schooling universally provided in primary and secondary school to the earlier years. It has been argued, for example, that universalism benefits children from disadvantaged backgrounds through positive peer effects in childcare settings (Neidell and Waldfogel, 2010; Williams, 2019). However, for better-off households, the publicly provided good or service in question may be inferior to the privately-provided counterfactual, or crowd out other parental investments in children (Havnes and Mogstad, 2015), reducing the overall benefit–cost ratio of universal policies. Indeed, for the case of UIFSM in England, we find no effect on bodyweight outcomes for children in the most affluent 20% of schools. However, despite bodyweight outcomes being highly persistent, the secular increase in obesity with age in developed countries is such that 70% of obese adults were not obese in childhood. A universal scheme may therefore be more successful at reducing the burden of adult obesity than any intervention targeted specifically at obese or overweight children (Simmonds et al., 2015).

We also contribute to the larger debate on the role of in-kind transfers in promoting child welfare. Since in-kind transfers constrain household consumption choices, these are generally held to be weakly inferior to cash transfers (Currie and Gahvari, 2008), unless there is a specific justification for supporting the consumption of certain goods by vulnerable groups. Currie's (1994) survey shows that narrowly targeted in-kind transfers, such as the United States National School Lunch Program (or indeed the UK's pre-existing means-tested Free School Meals programme) can better serve the dietary intake (and other outcomes) of children from low-income families than cash or broad transfers of purchasing power such as Food Stamps. Griffith et al. (2018) also show that the UK's 'Healthy Start' scheme, which provides means-tested vouchers that can only be spent on fruit, vegetables and milk to pregnant women and parents of pre-school children in low income households, resulted in higher fruit and vegetable consumption and better overall nutrient composition than a cash transfer of the same value. Our results on the impact of UIFSM on bodyweight outcomes

[5] Schwartz and Rothbart (2020) also show that participation in universal free meals in their context improves test scores. Further papers studying the switch from means-tested to universal school meals and breakfast in US high-poverty school districts include Leos-Urbel et al. (2013) and Gordon and Ruffini (2021).

suggest that parents of young school-age children, across a wide range of socio-economic backgrounds, face either a time or an information constraint (Bhattacharya and Currie, 2001) in preparing their children's diets that would prevent the same gains being realised were cash of equivalent value transferred to them instead.

The paper proceeds follows. Section 2 describes the existing Free School Meal policy, the UIFSM reform, and the UK context; Section 3 presents the NCMP dataset and provides descriptive evidence on bodyweight outcomes of children in England. Section 4 describes how we identify the treatment effect of UIFSM, and Section 5 presents the results we obtain using this method, exploration of mechanisms as well as a battery of robustness checks. Section 6 concludes.

2. Background

The Free School Meal (FSM) policy in England has historically been means-tested, with a free meal being made available at lunch time to children of parents receiving certain qualifying out-of-work benefits (welfare payments). For the cohorts we study, this created an effective upper eligibility ceiling for gross annual household income of £16,190 (approximately $22,400) per year, though further households below this threshold were disqualified due to savings, unearned income, earned income, or working hours. Parents who receive a qualifying benefit are usually required to register for free meals online with their Local Education Authority (LEA), though some LEAs auto-enrol children of benefit recipients. Parents of prospective Reception children can register any time after their child has been allocated to a primary school, in April before school starts in September.

Children not meeting the criteria for FSM may purchase a school meal at cost (around £2.30 per meal, equivalent to about $3). Although some food-based standards for school meals have been in place since 2001, from September 2008, school meals were required to comply with both food-based standards, determining portion sizes and the frequency with which different types of food may be served; and with nutrient-based standards, which 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, 2014). In January 2015, updated food-based standards came into force, which were designed to make it easier for caterers to embed the existing nutrient-based standards (Department for Education, 2014). Compliance with these standards must be specified in each school or LEA's contract with their catering providers, who must provide evidence that their menus meet the requirements (Department for Education, 2019a). Moreover, the Department for Education provides a range of resources to help school principals and governing bodies monitor compliance with the standards (Department for Education, 2019b).

Students not having a school meal may bring a packed lunch. These lunches may be prepared at home or shop-bought and are not required to comply with school food standards, though individual schools may implement their own restrictions on what children are allowed to bring. The content of packed lunches, being the counterfactual to school meal consumption for those induced to switch by the UIFSM policy, are an important determinant of the effect of UIFSM on bodyweight outcomes. While a programme of school lunches complying with the standards should average 530 calories per day, audit studies in both 2006 and 2016 found at least 89% of packed lunches to exceed this level, averaging 624 and 591 calories respectively, with less than 2% of packed lunches meeting food school standards in terms of energy and nutrients (Evans et al., 2010, 2020).[6] Our prior is therefore that, other things equal, we would expect a reduction in children's bodyweight outcomes if they consume a school lunch rather than a packed lunch from home.

As we will show later in the paper, take-up of school meals among children for whom they were not free was relatively low at just over 30%, and among FSM-eligible children it was about 84%. Dietary preferences are one reason for not taking up school meals. Parents' primary concerns tend to be that the child actually eats sufficient food at lunchtime, and secondarily that the child likes what they eat, both of which parents have greater control over if they provide a packed lunch (Ensaff et al., 2018; Goodchild et al., 2017).

There are also social reasons for not taking up school meals. Before the introduction of universal entitlement, 44% of schools implemented separate dining tables or rooms for those taking packed lunches from those taking school meals (OCC Strategy Consultants, 2013; Haroun et al., 2012), which may discourage take-up among those with friends taking a packed lunch. Among FSM-eligible children stigma from being identifiable as claiming welfare entitlements also potentially deterred take-up, though by 2014 most schools in England had effectively anonymised those purchasing and claiming school meals through the introduction of cashless catering systems (Chambers et al., 2016).

Since September 2014 all infants (comprising the first three years in school, i.e. children aged 4–7) in state-funded schools in England have been eligible to receive a free school meal at lunchtime under the Universal Infant Free School Meals (UIFSM) policy.[7] The means-tested FSM system remains in place for children who are in their fourth year of school or beyond. The policy's stated aims are to improve children's educational attainment, social skills and behaviour; to ensure children have access to a healthy meal a day and develop long-term healthy eating habits; to help families with the cost of living; and to remove disincentives to work (Department for Education, 2013). The policy was announced in September 2013. Capital funding for necessary enhancements to kitchen and dining facilities, totalling £150 m, was allocated to LEAs in December 2013.[8] Revenue funding of £2.30 per universal infant free school meal served (equivalent to around $3, £437/$580 per year) is provided to schools, calculated based on take-up on a census day in January each academic year.

The then-government's case for UIFSM was largely based on evaluations of earlier pilot schemes for universal FSM entitlement in two LEAs in the 2009/10 and 2010/11 academic years, with Brown et al. (2012) showing a significantly faster improvement in educational attainment for pupils exposed to free lunches in these pilots. While children were more likely to eat vegetables at lunchtime there were no significant net changes in consumption of any types of food or drink over the whole day, with the exception of those exposed to free lunches becoming less likely to eat crisps. The authors found no evidence of changes in Body Mass Index for children exposed to free lunches but these results are based on small sample sizes. The authors also cautioned that their results may not be replicated in a national roll-out of free meals as the pilot took place in relatively deprived LEAs and was accompanied by a host of supporting activities around awareness and encouragement of take-up.

With the introduction of UIFSM the incentive of parents in receipt of qualifying benefits to register their child for free school meals was removed for the first three school years. However, the pre-existing pupil premium policy, under which schools receive additional funding for each FSM-registered pupil enrolled, means that schools retained an incentive to encourage and assist parents of FSM-entitled children to register them even after UIFSM was introduced.

[6] Moreover, in the 2006 study one-third of packed lunches surveyed contained a sweet snack, processed savoury snack *and* sweetened drink.

[7] In addition, the School Fruit and Vegetable Scheme has entitled every primary school aged child to a free piece of fruit or vegetable outside of lunch time every school day since 2004. This pre-existing universal scheme does not affect our analysis.

[8] Schools had a further opportunity to bid, through their LEA, for a share of a further £15 m in October 2014; and a further £10 m allocated (£8.5 m through LEAs, the rest directly to schools) ahead of the 2015/16 academic year.

3. Data

The National Child Measurement Programme has collected data on the heights and weights of children in every primary school in England each academic year since the 2005/06 school year.[9] The programme was set up in line with the government's strategy to tackle obesity, and aims to gather population-level data to allow analysis of trends in growth patterns and obesity, inform local planning and delivery of services for children and to be a vehicle for engaging with children and families about healthy lifestyles and weight issues (NHS Digital, 2018a). Prior to September 2013 commissioning and implementing NCMP measurements was the responsibility of the local NHS Primary Care Trust (PCT), but after this it became the responsibility of LEAs.

The bodies implementing the NCMP visits receive a detailed level of operational guidance both for arranging the visits and communicating with parents, and for taking the measurements themselves. They are also advised of the data quality checks that will take place, such as whether an unexpectedly large proportion of children are recorded with whole or half-kilogram weights, or whole-centimetre heights (see operational guidance in NHS Digital, 2018a). There were no restrictions on the timing of visits during the school year, save for the need agree a mutually convenient day with the school, to notify parents in advance, and to have filed all results with the NHS by August following the end of the school year. Parents did not need to consent to their child's participation, but did have the opportunity to opt their child out of measurement. Participation rates among Reception children rose from approximately 83% in 2006/07 to 90% in 2009/10, and have been stable between 93% and 95% since.[10]

Each visit entails recording the height and weight of each pupil in their first and last year of primary school (at ages 4–5 and 10–11), in order to derive their Body Mass Index and classify them as underweight, healthy weight, overweight or obese. These classifications are calculated according to the British 1990 growth reference charts for their age and sex, with 'underweight' corresponding to the 2nd percentile and below, 'overweight' to the 85th percentile up to less than the 95th, and obese to the 95th percentile and above (Cole et al., 1995). Schools and LEAs are encouraged to inform parents of their child's measurements in a confidential manner, but any treatment effect of this information will not be observed in our data. We do not expect any anticipation effects to vary with the timing of the school visit.

Our bespoke NCMP data extract covers academic years 2007/08 to 2017/18 and focuses on children in their first year in school, aged 4–5 (the UIFSM policy only covers children aged 4–7). The data is anonymised at the school level, documenting the date (week-commencing) of the visit, the percentage of children measured who fall in each weight category and the mean BMI 'z-score'. The latter measure reports the standard deviations above or below the British 1990 growth reference charts mean, adjusted for sex and age in months, among the children measured. In what follows 'standard deviations' refer to the 1990 age-adjusted distribution. We will use the BMI z-score as well as the percentage of children who are obese and healthy weight as our main outcomes.[11] Our data also include the percentage of children measured who are female and who are of Black ethnicity, but not any cross-tabulations of these characteristics.

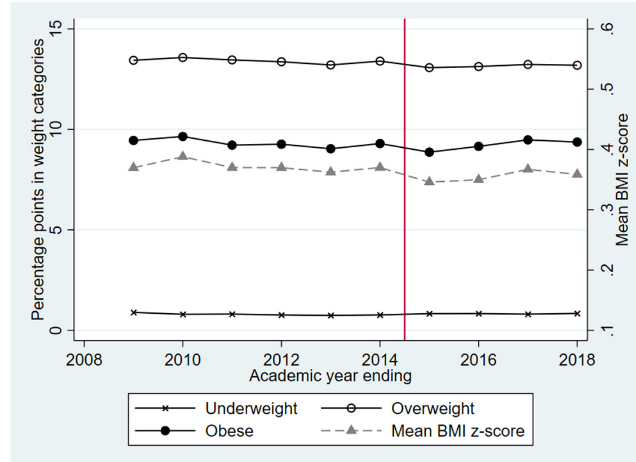

Fig. 1. Trends in mean school bodyweight outcomes across academic years.
Notes: National Child Measurement Programme. Unweighted means of within-school proportions of children measured in each bodyweight category and of the within-school mean BMI z-score (all accounting for age and sex of children measured), for each academic year. No additional controls.

We additionally supplied publicly available data on the characteristics of each school for each year of our data window to NHS Digital, who linked these to the NCMP data before releasing the extract. They include the school's involvement in a pilot scheme for universal or extended Free School Meals; and 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. Our data do not enable us to weight schools in our analysis in proportion to their size. Using data on school size and its variation across and within primary schools in England we will perform simulations which show that omission of weights is unlikely to lead to sizeable biases of our results (see Section 5).

We exclude school-years with missing bodyweight data, but include schools that closed or first opened during our observation period. We restrict our analysis sample to measurements taken in academic year ending 2009 and onwards to ensure that enhanced school food standards were fully in force, leaving us with 17,776 different schools and 154,169 visits. As a robustness check we estimate our specification using the balanced panel of schools open and visited in every year during our analysis period.

In Fig. 1 we show the mean across schools of the proportion of students aged 4–5 in each weight category, and the mean BMI z-score, for each academic year ending 2009–2018. Fig. 1 shows that overall, the proportion of children measured as being underweight, overweight or obese remains fairly constant over the period of the data, with 1%–2% underweight, 13% overweight and just under 10% obese. The remainder are 'healthy weight', not shown due to its larger magnitude. The secondary axis in Fig. 1 shows children's BMI z-score, which varies within the narrow range of only 4% of a standard deviation between its peak in 2010 and lowest point in 2015. There is a small downward movement in the latter three outcomes for the academic year-ending-2015 when UIFSM was introduced, but subsequently some reversion.

Our focus in this paper is on the development of bodyweight outcomes within the first school year. The school year in England comprises 190 teaching days spread over 39 weeks between early

[9] Collection was suspended in March 2020, when schools were closed due to the coronavirus pandemic.

[10] The NHS was concerned that selective non-participation would cause bias in estimates of bodyweight outcomes, so until 2011 reported on the relationship between participation rates and obesity prevalence. They concluded that while non-participation did result in underestimates of the obesity rate for students aged 10–11, it had a negligible relationship for children in their first year of school. From the 2011/12 academic year onwards, this analysis has not been conducted, because the response rate is considered sufficiently high. (Historical NCMP reports, including information on participation rates and national and LEA trends, can be found at NHS Digital, 2018b.

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

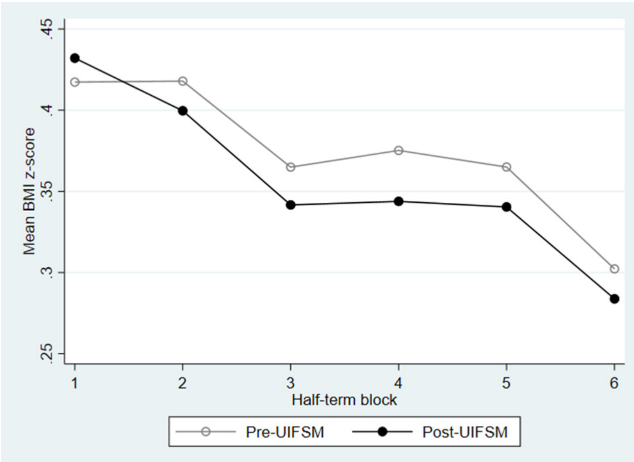

Fig. 2. Trends in BMI z-score over the school year, and pre- and post-UIFSM.
Notes: National Child Measurement Programme. Unweighted means of within-school mean BMI z-score (accounting for age and sex of children measured) for schools measured in each half-term block, by pre- (academic years ending 2009–2014) and post- (academic years ending 2015–2018) UIFSM. No additional controls.

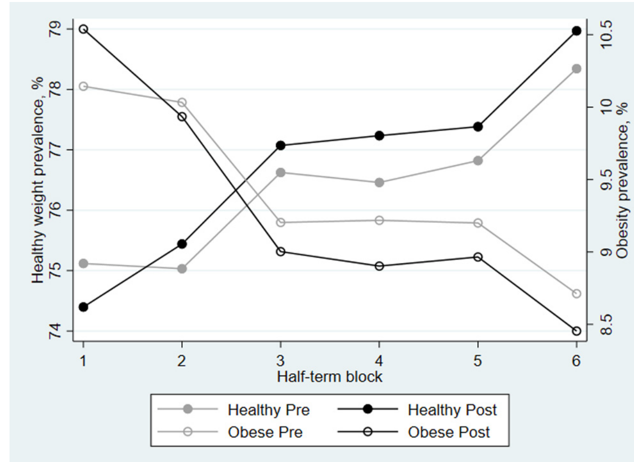

Fig. 3. Trends in healthy weight and obesity over the school year, and pre- and post-UIFSM.
Notes: National Child Measurement Programme. Unweighted means of within-school proportions healthy weight and obese (accounting for age and sex of children measured) for schools measured in each half-term block, by pre- (academic years ending 2009–2014) and post- (academic years ending 2015–2018) UIFSM. No additional controls.

September and late July.[12] The school year is divided into three terms, the autumn term starting in September and ending at Christmas, the spring term from January to Easter and the summer term from after the Easter holiday to the third week of July. Each of these three terms is broken up by a one week 'half-term' holiday in October, February and May so that the school year consists of six half terms each of roughly 6–7 week length, depending on when Easter falls in each year. We assign all measurements to half-term blocks. However, not all schools have exactly the same holidays; this depends on the policy of the LEA and can vary especially around the Easter holidays. Because we do not have an LEA identifier that would allow us correctly to assign every school visit to the correct half-term block, we assume that the Easter weekend is always incorporated into a school holiday. We allow Easter to be at the beginning, middle or end, and retain information on schools for which the timing of the visit by half-term cannot be established, controlling for these with an additional dummy variable and interaction with post-UIFSM in our regressions.

Fig. 2 presents descriptive evidence of the development of BMI across the six half terms of the school year, separately for the pre- and post-implementation years. Based on the raw data we see that already in the pre-policy years children measured later in the school year were lighter (for their height) than those measured earlier in the school year. As we show below, we find no evidence that this is driven by the characteristics of those measured at different times (see Section 4). Instead, it may be because the school environment is relatively beneficial for bodyweight outcomes, compared to the home or pre-school environment children were experiencing before starting school (von Hippel et al., 2007; Anderson et al., 2011),[13] or it may reflect seasonality in bodyweight. We do not have data on children's weight outcomes before their first year in school, so our analysis relies on within-year differences in bodyweight, comparing pre-policy to policy years.

Fig. 2 shows that while BMI was higher in the first half term block for the cohorts that benefited from UIFSM, and from the second half term block onwards BMI is consistently lower for children in receipt of UIFSM in the post-period, by around 1%–4% of a standard deviation.

Fig. 3 shows that similar relationships hold for our threshold measures, the percentage of children who are obese and who are healthy weight, with obesity rates being lower and healthy weight rates higher for children under UIFSM than pre-UIFSM. This indicates that the policy may have improved bodyweight outcomes for children in their first year in school. The pattern apparent in Figs. 2 and 3 whereby weight loss occurs relatively quickly and then stabilises over time is consistent with the medical literature. This predicts the largest absolute change in bodyweight in the early months after a change in diet.[14] There is consensus that the body either has a 'set point', or 'settling range', that will restrict the amount that an individual's bodyweight will change in practice. Individuals will reach the set point (or settling range) quite quickly after reducing their energy consumption, but further weight loss is prevented by a regulatory feedback mechanism of reduced energy expenditure in response to reduced energy intake (e.g. Müller et al., 2016). Of course the associations shown are not causal relationships and the next section will discuss how we go about identifying an effect that comes as close as possible to a causal one.

4. Methods

The UIFSM programme was introduced simultaneously across the whole of England in September 2014, so there is no experimental variation in exposure to UIFSM across schools, or 'control group' of similar schools recorded in our data which were not exposed to UIFSM. However, we do have information on the date schools were visited for height and weight measurement both before and after UIFSM was introduced. We expect the impact of UIFSM to depend on the 'dose' of free meals received, so that a greater effect should be observed for children at the end of the first year in school (after up to 190 meals) than for children just starting school for the first time. That is, for a school visited at the start of the school year in September, once accounting for other underlying trends there should be little difference in the BMI between a cohort of children entering reception in 2013/14 (pre-implementation) and 2014/15 (post-implementation), while if exposure to UIFSM does affect this outcome, the difference should be

[12] There are 5 'inset days' on which schools are closed, usually for teacher training, and usually timed at the start or end of a holiday.

[13] Possible reasons for this include less snacking during the day, a packed lunch/school meal that is better than the home lunch, or more physical activity, for example.

[14] Standard models of energy (im)balance entail convergence to new steady state with a half-life, so we would expect to see the largest absolute change in bodyweights in the earlier months after a change in diet, and this is faster for children than adults (Hall et al., 2011).

progressively larger in a school visited, say, in the spring and summer. Information on the timing of measurement allows us to observe the difference in outcomes between children who were exposed to the UIFSM policy for different durations at the time they were weighed and measured. We compare this with the difference in outcomes of children who were merely exposed to the pre-UIFSM school environment for different durations at the time they were weighed and measured.

Using this setup and the six half-term blocks described earlier to measure duration of exposure we formulate a 'difference-in-difference' model as follows:

$$\bar{Y}_{st} = \beta_1 + \sum_{h=2}^{6} \beta_h HTERM_{hst} + \tau_u UIFSM_t + \sum_{h=2}^{6} \tau_h (HTERM_{hst} \times UIFSM_t) + \gamma X_{st} + \mu_s + \varepsilon_{st} \quad (1)$$

where $\bar{Y}_{st}$ is the mean of the outcome recorded in school s in year t, $HTERM_{hst}$ is a dummy for the NCMP visit to school s in school year t taking place in half-term h (numbered 1 to 6), $UIFSM_t$ is a dummy variable that switches on for the UIFSM policy years, X_{st} is a vector of controls that varies across school and time, μ_s is a school fixed-effect, and ε_{st} a normally distributed error term.

In this setup β_h captures the effect of being exposed to the school environment up to half-term h, relative to half-term 1. The effect of UIFSM on bodyweight outcomes recorded in half-term 1 is captured by τ_u, and the effect of UIFSM for subsequent half-term blocks in this equation is given by $\tau_u + \tau_h$. The effect of the duration of exposure to UIFSM is captured by the interaction term coefficient τ_h. These are intention-to-treat effects, given that – as we show below – not all students take up free meals. We estimate Eq. (1) using linear models on our school-level data.

We include in X_{st} a comprehensive set of controls. Firstly, we control for other policies introduced during the observation period that may have had an effect on bodyweight outcomes. These include Department for Education pilot schemes for universal or extended means-tested entitlement to Free School Meals, and a number of other pilots run at the initiative of LEAs over the years preceding UIFSM. We characterise these using six dummy variable categories and also interact them with half-term block.

Further policies that could potentially affect children's bodyweight are the pupil premium and the physical education and sport premium. Both make extra funding available to schools, the former from 2013 onwards for each student eligible for free lunches plus a small number of other pupil groups (e.g. children adopted from care), and the latter from 2014 onwards, in most cases as a per-school block grant with a small additional per-pupil component. We control for provision and level of both, and allow pupil premium (which was only allocated to existing FSM-eligible children) to have differential effects pre- and post-UIFSM.[15]

Moreover, we control for the percentage of children measured at each visit who were girls and who were of Black ethnicity. We also include a cubic-time-trend interacted with both these variables to discount any differential growth in the prevalence of obesity or overweight between years by sex or by ethnic group, that may arise from these groups' different metabolic response to the same prevailing environmental changes. Likewise, we include a cubic time-trend specific to the neighbourhood 'Income Deprivation Affecting Children Index' (IDACI) quintile, to accommodate the widening of the gap in outcomes between schools in the most and least deprived neighbourhoods.[16] (The IDACI is time invariant within schools, so cannot be included as an independent regressor). We also include the means-tested FSM eligibility rate of the school (in quintiles), and a school fixed effect in our model to control for time-invariant school factors affecting outcomes.

Note that, as we will show, most schools are visited for measurement of children's bodyweight at different half-terms over our observation period. Identification of Eq. (1) comes both from schools that switch half-term of measurement, and from the policy switching on for the few schools that do not ever switch half-term of measurement. Each of the six treatment effects is identified from a different sample of schools, where the same school can contribute to identification of more than one treatment effect. For each half-term, identification also potentially relies on a different sample of schools pre- and post-UIFSM introduction. This is different from a classic difference-in-difference setup where the same treatment and control groups are observed both before and after a policy is introduced. We discuss identification at length below.

The direction of the expected effect is indeterminate. Children's BMI will increase if their energy intake relative to expenditure increases, and vice versa. Assuming no change in energy expenditure, the effect will depend on how calories consumed in meals prepared in school compare to those provided from home, usually in the form of a packed lunch. If the energy intake from a school lunch is higher than what children would otherwise have consumed in a packed lunch, net of any crowding out of calories provided by parents or in childcare at other times of the day, their BMI will increase, and vice-versa. Secondly, the effect will depend on the number and composition of children induced by the policy to eat a school lunch, rather than a packed lunch from home. We describe changes in take-up rates and discuss possible other mechanisms, including income effects and effects on work incentives below.

Identification

Our dose–response approach relies heavily on the timing of bodyweight measurement, and we need to consider that schools differ in observed and unobserved ways that may be related to the weight outcomes we are interested in. Each school is visited only once per academic year. The composition of schools visited at each half term for weight and height measurement will affect the observed weight outcome for that half term and therefore the inference we can draw from comparisons of children's bodyweight across the school year and between the pre- and post-policy period.

Ideally we want each half-term measurement to be representative of the population of children in England. This could be achieved if each year, every school was randomly assigned a half-term block for measurement. This would yield treatment estimates that would be unbiased and externally valid, i.e. a valid average for schools of any composition. If instead each school was assigned a visiting date once and then revisited at this same half-term every year, our difference-in-difference type estimates would yield unbiased treatment effects of UIFSM for each half-term, but these effects would only be valid for the types of schools that contributed to each half-term measurement. For example, consider two periods within the school year, early vs late, and timing such that in each year high-obesity schools (that is, schools with observable and unobservable characteristics that are associated with high obesity) were visited early and low-obesity schools late in the

[15] Pupil premium funding per eligible student increased uniformly across the country, but non-linearly year-by-year from £430 in the academic year 2012/13 to £1320 in 2017/18. We approximate the average premium across all the pupils in the school in each year, calculated using the mean proportion of free lunch eligible children in each of the quintile bands and the size of the pupil premium amount. Physical education and sports premium was provided from 2014 at a level of £500 each for the first 16 pupils and £5 per student thereafter, doubling in 2018 to £1000 each for the first 16 pupils and £10 thereafter. Since we do not observe the school size and the policy was designed to account for large economies of scale, we simply control linearly for the two levels of provision.

[16] We choose a cubic trend because it is more flexible than linear or quadratic specifications in that it does not impose rigid assumptions about how outcomes evolve within or between years that may lead to a poor fit. Substituting linear or quadratic trends produces qualitatively similar results.

Table 1
Timing of NCMP visits by half-term block of the school year.

	Percent of visits in school year, during half-term block:							
Acad. year	1	2	3	4	5	6	Not classifiable	N schools
2008/09	2.2	9.8	20.8	32.3	19.1	15.3	0.5	15,197
2009/10	2.6	11.2	24.0	31.0	21.1	9.7	1.3	15,106
2010/11	3.0	12.4	31.9	31.0	12.7	8.5	0.5	15,169
2011/12	2.8	15.8	28.9	30.8	16.2	5.0	0.6	15,409
2012/13	4.1	15.1	31.2	25.6	16.2	7.6	0.4	15,303
2013/14	4.4	15.7	29.7	29.1	11.6	8.7	0.7	15,389
2014/15	6.5	20.6	27.6	23.5	14.2	7.3	0.4	15,543
2015/16	7.1	17.4	27.9	21.1	19.9	6.2	0.5	15,707
2016/17	7.3	20.2	26.8	25.9	12.8	6.9	0.1	15,713
2017/18	5.6	19.1	24.2	23.7	19.0	8.3	0.2	15,633
Total	4.6	15.8	27.3	27.4	16.3	8.3	0.4	154,169

Notes: National Child Measurement Programme.

year. The downward slope in children's bodyweight across the school year seen in Fig. 2 could reflect such a visiting pattern. If we were to find that UIFSM reduces obesity, this inference would be for low-obesity schools, but we could say nothing about the effect of UIFSM in high-obesity schools.

In practice, the timing of NCMP visits was set up according to local considerations of the organisations in charge of implementing the programme rather than following an experimental design and could potentially change year-on-year.[17] In this setup we must consider whether schools visited at each half-term differ systematically in observed and unobserved characteristics relating to bodyweight outcomes, potentially limiting the external validity of our results. If our 'natural experiment' led to any differences between schools visited at different half-terms, a particular concern would be if this pattern changed at the same time as the UIFSM policy was introduced as this could bias our estimates. For example, if NCMP teams visited high-obesity schools early in each academic year and low-obesity schools late, and they reversed this pattern to early visits for low-obesity schools and late for high-obesity schools at UIFSM introduction, our results would understate the beneficial effect of the policy.

In what follows we examine the timing of school visits across our observation window to check whether we can expect our results to be unbiased and externally valid. We start off by describing the timing of visits and contrasting observed visits with yearly random timing (model 1) and yearly recurring visits at the same time (model 2). Next we investigate (a) how the timing of school visits relates to observed characteristics, and (b) whether there was any shift in timing of school visits at the time of UIFSM introduction in 2014. The results of (a) will be informative about the external validity of our estimates, and the results of (b) will be informative about any biases affecting our results.

Timing of school visits

Table 1 displays the distribution of NCMP school visits across half-term blocks for each academic year during our observation period. The Table shows that schools were visited across all of the half-term blocks, albeit with more visits in the middle of the school year than at the beginning and end. We also see that the pattern of visits has changed slightly over the years, with a shift towards visits earlier in the year.[18]

Fig. 4 plots patterns of the timing of school visits. In each panel, the grey bars plot what is observed in the data. The red hollow bars plot what would be observed (on average) if schools were randomly assigned to one of the observed visiting slots each year (model 1). These data points are derived from 1000 simulations following this assignment rule. Panel A shows the distribution of year-on-year changes in the timing of visits, ranging from zero (no change) to a maximum of 5 half-terms. Our data exhibits slightly more inertia than random allocation, with a higher proportion of schools exhibiting no or only one half-term change in year-on-year timing than under random allocation. Under model 2, yearly recurring visits at the same time each year, we would have zero changes in every school (not shown). Panel B of Fig. 4 displays within-school serial correlation between timing of visits at lags of 1 up to 9 years. Schools are considerably more likely to be visited in exactly the same half-term as in the preceding year than would happen if timing was randomly re-determined every year, but the majority of schools still experience a shift in any given year. This auto-regressive pattern leads to a slightly smaller number of total switches per school than under randomisation (see panel C) and to a narrower range between the school's earliest and latest visit observed in our estimation sample (see panel D). Under model 2 (visit at same half-term every year) we would see a serial correlation in timing of 1 for all lags in panel B, zero observed switches in the timing of visits in panel C and a range between earliest and latest visit of zero in panel D.

In summary, the description of timing patterns shows that schools were not randomly assigned visit dates every year (model 1), nor were they visited at the same half-term each year (model 2). The observed patterns are in between these models and appear closer to model 1 than 2. Next we investigate how the timing of measurement and any shifts in timing at UIFSM introduction relate to observable school characteristics.

Relationship between timing and school characteristics

Table 2 summarises the school-level observable characteristics available to us by half-term block. These include the main factors persistently shown by the NCMP to be associated with children's overweight and obesity, namely gender (boys are more likely to be obese than girls of the same age), disadvantage and deprivation (there is a monotonic gradient in children's bodyweight outcomes by neighbourhood deprivation decile, with those in the most deprived decile more than twice as likely to be obese as the least deprived — 13.3% to 6.0% in 2020) and Black ethnicity (the clear outlier, with 15% obese and 69%–70% healthy weight in both 2009 and 2020, compared with prevalences among White, Mixed and Asian ethnicity pupils of 9%–11%

[17] According to the NCMP implementors, the timing of visits was organised around local considerations such as staff and school availability, coordinated to coincide with other health checks in the school, and not systematically linked to criteria such as disadvantage. Source: Email by NCMP programme support manager dated 2 April 2019.

[18] One of the reasons for this change in pattern may have been that over time a minority of schools initially using staggered school entries (such that those born later in the school year start school in January or after Easter rather than in September) switched to a school start for all children in September. Visits for height and weight measurement could therefore move to earlier in the school year without losing coverage. Among our robustness checks we implement a specification excluding all observations prior to 2012, by which time the vast majority of these switches had been completed. This does not change the interpretation of our results (see Appendix 2).

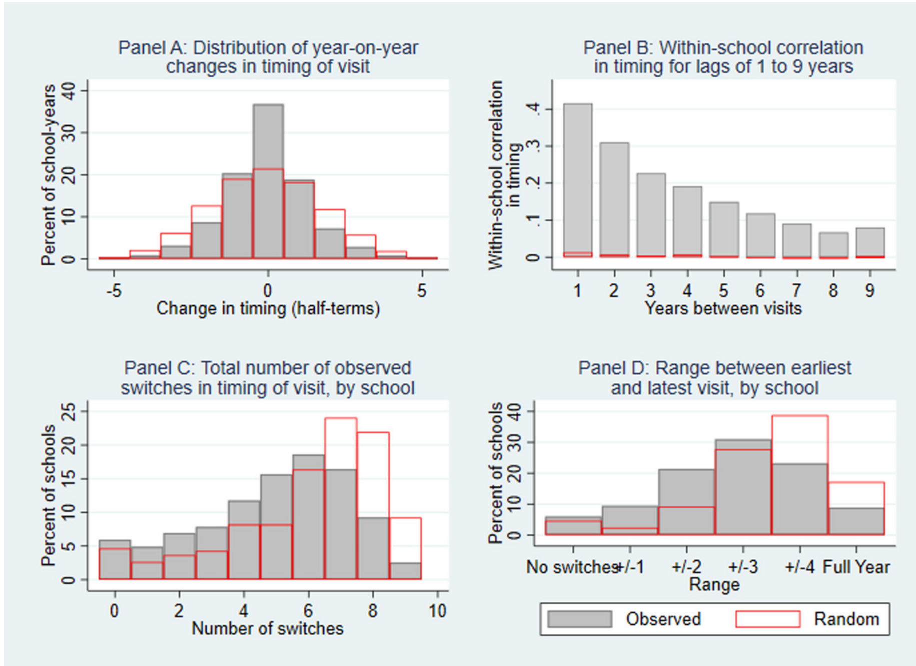

Fig. 4. Timing of school visits.
Notes: National Child Measurement Programme, academic years ending 2009–2018. In all panels, the grey bars indicate the changes in timing of school visits observed in the data. Panel A shows the distribution of year-on-year changes, Panel B the within-school correlation in timing of visits, Panel C the total number of switches observed for schools and Panel D the range between the earliest and latest visit within a school. The hollow red bars represent the means of the outcomes obtained from 1000 simulations, in which within each year schools are randomly shuffled to re-assign across the observed visiting dates. Panel A: N = 134,403 year-on-year transitions from 17,772 schools. Panel B: N_1 (Number of school-transitions observed at lag of one year) = 16,164; N_2 = 15432; N_3 = 15,102; N_4 = 14,681; N_5 = 14410; N_6 = 13,369; N_7 = 13,073; N_8 = 12,832; N_9 = 12,492. Panels C and D: N = 17,772 schools.

and 76%–79% respectively), see NHS Digital (2009, 2020). Ethnicity and deprivation have been shown independently to affect outcomes (Strugnell et al., 2020) as well as obesogenic behaviours (Falconer et al., 2014) so it is important to account for both. In our model we control for proportion female, proportion Black, and FSM eligibility quintile, which all vary over time within schools. Our model also interacts our sex, race and deprivation quintile variables (the latter is time-invariant) with cubic time trends. This follows closely the specification shown by Banks et al. (2016) to produce an excellent fit for the modelling of trajectories of BMI over time. Together with our school fixed effects, that control for all time-invariant heterogeneity across schools, this suggests that there should be little room for unobservable characteristics to bias our results.

The p-values in square brackets in Table 2 test whether the mean of the characteristic in a particular half-term is statistically different from the mean across all half-terms. The Table shows that there are some noticeable differences in the characteristics of schools visited in different half terms. For example, the proportion of Black students is 0.6%pts below the overall mean in the first half-term starting September, and approx. 0.2%pts above it in the second and fifth, but slightly below the mean in the third and sixth half-term. Several of these differences are statistically significant but they are small in magnitude, with the maximum deviation from the overall mean at 6.6% of a standard deviation for the proportion of Black students. Several other characteristics have statistically significant but small differences between particular half-terms and the overall mean.

Note that observable differences do not seem to be responsible for the downward trend in children's bodyweight over the school year shown in Fig. 2. The change in composition of Black students from the first to second half-term would promote an upward, rather than downward trend in the early part of the school year, for example, as Black ethnicity is associated with higher bodyweight per height. Similarly, we might expect higher bodyweight per height in schools with high proportions of FSM eligible children but we see a higher-than-average proportion of both the highest and lowest quintile of FSM eligibility quartile visited in the last half-term of the year (starting in June). There is no pattern discernible whereby schools with characteristics associated with lower bodyweight are visited later in the school year, suggesting that other factors such as lower calorie intake and/or more exercise when at school are likely to explain the pattern in Fig. 2.

Another way to check the extent to which the characteristics of schools visited in different half-term blocks are similar is to summarise all our observable characteristics using principal component analysis. We extract the principal component from a pooled sample of all half-terms and years and standardise this to a pooled-sample mean of 0 and standard deviation of 1 (see notes to the Figure for more details). Fig. 5 shows kernel density plots of the standardised principal component by half-term block. As already apparent in Table 2 there are some differences between half-term blocks, but on the whole the plots present a good picture of common support, suggesting that our estimated treatment effect should have external validity.

A third way to check whether schools visited in different half-terms differ in observable characteristics is to regress the timing of visit (here measured from half-term block 1 to 6) on school characteristics. Column (1) of Table 3 shows results from a school random-effects regression for all years. It shows that the proportion of Black children in schools has no association with timing of visit, but schools in the very highest FSM eligibility quintile (most deprived) are on average measured 10% of a half-term (3.5 school days) later than those in the lowest quintile, and those in IDACI quintiles 2–5 are all visited a similar margin earlier than those in the least deprived quintile. These margins are very small, and the small within- and between-school R^2 values show that school characteristics collectively have very little explanatory power for the timing of visit.

Column (2) of Table 3 shows the same regression but, like our main specification (Eq. (1)), includes school fixed effects and so focuses on within-school changes in characteristics. This shows that increasing the proportion of Black or FSM-eligible students over time in a school is

Table 2
School characteristics by timing of visit.

		Half-term block					
	All periods	1	2	3	4	5	6
Girls	48.9	48.96	48.87	48.91	48.85	48.96	48.84
		[0.638]	[0.720]	[0.799]	[0.499]	[0.359]	[0.500]
Black	4.19	3.50	4.43	4.06	4.21	4.39	4.07
		[0.000]	[0.000]	[0.514]	[0.155]	[0.001]	[0.708]
IDACI deprivation quintile							
Q1	19.74	17.35	17.39	19.32	20.8	21.13	20.9
		[0.000]	[0.000]	[0.570]	[0.000]	[0.000]	[0.001]
Q2	20.3	21.52	20.5	20.73	20.02	20.48	19.1
		[0.055]	[0.779]	[0.265]	[0.495]	[0.009]	[0.205]
Q3	19.6	20.42	20.88	20.02	19.65	17.98	18.58
		[0.138]	[0.000]	[0.133]	[0.820]	[0.000]	[0.013]
Q4	19.8	19.26	20.61	20.48	19.5	18.5	19.59
		[0.475]	[0.006]	[0.006]	[0.573]	[0.000]	[0.877]
Q5	20.06	21.11	20.21	19.10	19.31	21.99	20.59
		[0.229]	[0.598]	[0.000]	[0.000]	[0.000]	[0.636]
FSM eligibility quintile							
Q1	16.83	16.71	15.78	16.56	17.5	17.3	17.53
		[0.745]	[0.000]	[0.196]	[0.064]	[0.088]	[0.066]
Q2	21.39	21.32	21.33	21.61	21.71	20.9	20.97
		[0.975]	[0.934]	[0.223]	[0.100]	[0.146]	[0.360]
Q3	21.38	22.82	22.41	21.78	21.12	20.41	20.12
		[0.007]	[0.001]	[0.196]	[0.200]	[0.000]	[0.000]
Q4	20.46	20.55	21.68	20.67	19.93	19.99	19.87
		[0.846]	[0.000]	[0.390]	[0.039]	[0.116]	[0.121]
Q5	17.58	16.71	16.86	17.44	17.31	18.57	18.56
		[0.095]	[0.020]	[0.608]	[0.297]	[0.001]	[0.011]
N obs	154,912	7,072	24,301	42,073	42,183	25,072	12,821

Notes: National Child Measurement Programme. Figures in square brackets indicate p-values from test of significant difference of mean prevalence of school characteristic in this half-term from mean across all periods. Overall mean includes 1390 school visits where half-term timing is indeterminate.

Table 3
Timing of visits and shifts in timing of visits between pre- and post-UIFSM implementation.

	Timing of visits across all periods			Shifts in timing of visits: UIFSM relative to pre-UIFSM period	
	Random effects (1)	Fixed effects (2)		Random effects (3)	Fixed effects (4)
Girls (%)	0.000	0.000	UIFSM × Girls (%)	0.000	0.000
	(0.000)	(0.000)		(0.001)	(0.001)
Black (%)	0.001	0.002**	UIFSM × Black (%)	−0.007***	−0.008***
	(0.000)	(0.001)		(0.001)	(0.001)
FSM Q2	0.001	0.005	UIFSM × FSM Q2	0.010	−0.006
	(0.012)	(0.013)		(0.021)	(0.021)
FSM Q3	−0.026	−0.013	UIFSM × FSM Q3	0.072**	0.036
	(0.014)	(0.016)		(0.023)	(0.022)
FSM Q4	0.008	0.060**	UIFSM × FSM Q4	0.053*	0.004
	(0.016)	(0.020)		(0.025)	(0.023)
FSM Q5	0.101***	0.209***	UIFSM × FSM Q5	0.098***	0.034
	(0.019)	(0.025)		(0.029)	(0.025)
IDACI Q2	−0.089***		UIFSM × IDACI Q2	−0.023	
	(0.018)			(0.019)	
IDACI Q3	−0.118***		UIFSM × IDACI Q3	−0.062**	
	(0.019)			(0.021)	
IDACI Q4	−0.114***		UIFSM × IDACI Q4	−0.072**	
	(0.020)			-(0.023)	
IDACI Q5	−0.109***		UIFSM × IDACI Q5	−0.107***	
	(0.022)			-(0.026)	
			UIFSM period	−0.177***	−0.204***
				-(0.035)	-(0.034)
Within-school R^2	0.0027	0.0034		0.015	0.015
Betw.-school R^2	0.0157	0.0006		0.026	0.007
N obs.	153,522	153,522		153,522	153,522
N schools	17,772	17,772		17,772	17,772

Notes: National Child Measurement Programme. Dependent variable is half-term of school visit (1–6). Columns (1) and (3) are school random-effect regressions, columns (2) and (4) school fixed effect regressions. Columns (3) and (4) include exhaustive interactions between UIFSM period and all explanatory variables. Standard errors in parentheses. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$.

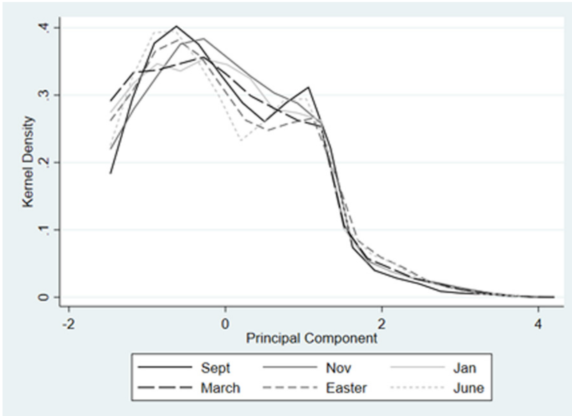

Fig. 5. Common support: Principal component analysis.
Notes: Source: National Child Measurement Programme. Principal component derived from factor analysis on pooled sample of all school-visits in years-ending 2007–2018 (N=138,410), with observable characteristics percent Black, percent Girls, FSM eligibility quintile, IDACI quintile all treated as linear measures of latent deprivation or propensity for poor bodyweight outcomes. Observations with missing Black prevalence or FSM or IDACI quintile are dropped. This is standardised to a mean of zero and standard deviation 1 in the pooled sample of all school-visits and half-terms. Kernel densities plotted for pooled sample of all school-visits for each half-term, with density estimated at 20 points.

associated with later visits. Again, these effect sizes are very small: one within-school standard deviation increase in the proportion of Black children - 4%pt — resulting in a visit less than a third of a school day later; a shift from fourth to fifth FSM-eligibility quintile resulting in a visit approximately 5 school days later.

In summary, we find that while there are differences in observable characteristics between schools visited for bodyweight measurement at different times of the year, these differences are small and there is good common support across all six half-terms, suggesting our results are likely to be valid for schools with a wide range of characteristics. Given the high explanatory power of the observable characteristics available to us, it seems unlikely that unobservable characteristics could change this finding.

Shifts in timing pre- and post-UIFSM introduction

Although we have shown that, over the whole period of study, the timing of visit is not strongly related to any school characteristics we can observe, a potential threat to identification occurs if there is a significant change in the association between school characteristics and timing of visit, between the pre- and post-UIFSM periods. Columns (3) and (4) of Table 3 report results from school random- and fixed-effect regressions with exhaustive interactions between a dummy for the UIFSM period and all observable school characteristics. The bottom row of the table shows that schools were on average visited slightly earlier in the post-UIFSM period than before (approx. 0.2 half-terms, or 7 school days).

Column (3) of Table 3 shows that the tendency observed in column (1) for high-FSM eligibility schools to be visited later and high deprivation (IDACI) schools to be visited earlier was slightly stronger in the post-UIFSM period, but also that schools with more Black children tended to be visited earlier. Moving to the fixed-effects regression however reveals that any shift in the association between FSM eligibility and timing of visit is accounted for by unobserved time-invariant school characteristics, that we also control for in our main regressions with school fixed-effects. The interaction term on the percent of Black ethnicity children shows that, relative to before UIFSM, increasing the proportion of Black children in the school by one within-school standard deviation would predict the school being visited approx. 0.032 half-terms, or 1.1 school days earlier. Although statistically significant, the magnitude of this shift is very small, and altogether these results do not suggest any change in visiting scheme being associated with observable characteristics.

This does not rule out a change in the visiting scheme based on *unobservable characteristics* related to children's bodyweight. We carry out a simulation exercise which investigates how strongly associated an unobservable characteristic must be with timing of visit in the post-treatment period to eliminate the treatment effect at the end of the year in the sixth half-term, and the difference between the treatment effects at the start and end of the year, between the first and sixth half-term (as these are our headline findings). This simulation exercise is presented in Appendix 1. It shows that an unobserved factor that affects bodyweight similarly to that of a school's proportion of Black students would have to have a strong (0.1) correlation with timing of school visits post-UIFSM introduction while having no correlation with timing before the introduction of the policy to reverse our main results presented in the next section. As apparent from column (4) of Table 3, none of the time-varying observable characteristics known to affect children's bodyweight have an association anywhere near this magnitude.

Parallel trends

As noted, our estimation strategy does not compare the same treatment and control schools before and after the policy was introduced as would be the case in the usual difference-in-difference setup. Instead, schools move into and out of being high-dose schools (visited later in the year after more meals were provided to children) and low-dose schools (visited earlier in the year). This prevents us from comparing pre-trends between schools that receive low or high doses of treatment after UIFSM introduction.[19] However, the preceding analysis has shown only small associations between school characteristics and the timing of school visits, suggesting that mean bodyweight in a sample of schools visited in a particular half-term should be representative of the bodyweight in all schools in that half-term (after comprehensively controlling for school observable characteristics, a school fixed effect and any other policies introduced in the observation window). Then we can describe the data across pre-policy years to examine whether the change in children's bodyweight over the school year (i.e. the downward gradient in Fig. 2) was already steepening in the years leading up to UIFSM introduction. This is not a classical parallel trends test but serves a similar purpose.

We would be concerned if we were to see a systematic relative improvement in bodyweight outcomes for 'high-dose' children over 'low-dose' children already having begun in the pre-treatment period. This would suggest there is another unobserved time-varying factor serving to increase the beneficial effect of the school environment on child bodyweight outcomes that may be driving our results. To test this, we run a school fixed-effect regression of bodyweight outcomes on an exhaustive set of half-term-by-year interactions for the pre-treatment period:

$$\bar{Y}_{st} = \sum_{h=1}^{6} \sum_{t=2009}^{2014} \theta_{ht}(HTERM_{hst} \times YEAR_t) + \gamma X_{st} + \mu_s + \varepsilon_{st} \tag{2}$$

For each year t and half-term block h the difference in coefficients $(\theta_{ht} - \theta_{1t})$ provides a measure of the conditional improvement in observed bodyweight outcomes since the start of the school year. In Fig. 6 we show the difference-in-difference $(\theta_{ht} - \theta_{1t}) - (\overline{\theta_h} - \overline{\theta_1})$ between September and all the specified half-terms, with confidence intervals, for our three bodyweight outcomes. We show these results smoothed for shocks in individual years or seasons by plotting two-year moving averages, and all changes are relative to the pre-treatment averages. The figure shows

[19] Only 6% of schools were visited always in the same half-term, and 3% were visited always in the same half-term and observed in each of the six pre-UIFSM years.

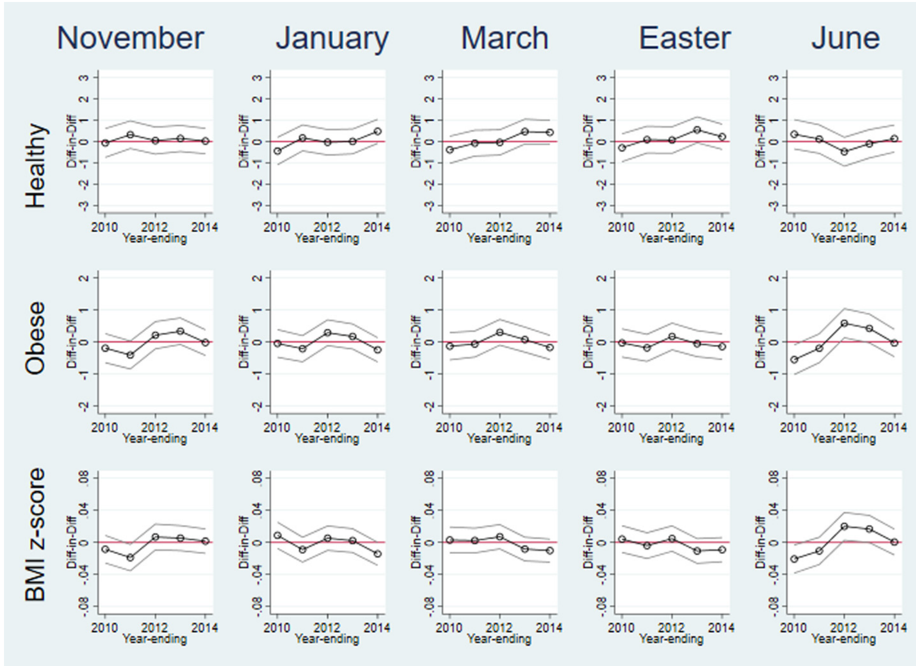

Fig. 6. Tests of pre-treatment parallel trends in bodyweight outcomes.
Notes: National Child Measurement Programme. Two-year moving averages of change in bodyweight outcomes between the first and subsequent half-terms, relative to pre-treatment average, with 95% confidence intervals. Treatment effects derived from exhaustive half-term block by academic year interactions in school fixed-effect regressions. Additional controls: exhaustive half-term block by pilot scheme interactions; percent of measured students Black ethnicity, percent Black missing, percent of students measured girls, FSM-eligibility quintile (including missing dummy), pupil premium per pupil in school, school sports premium level, cubic year trend interacted with IDACI quintile, percent Black, percent Black missing, percent girls; half-term dummies interacted with within-school demeaned percent Black, percent Black missing and percent girls.

that the pre-trends are different from zero only in a very small number of year/outcome combinations. We are also reassured by the fact that there appear to be no trends across years that are consistent across all half-term blocks. (See Fig. 6.)

5. Results

5.1. Main results

Table 4 presents the treatment effects of UIFSM on the prevalence of healthy weight and obesity, and the mean BMI z-score for each half-term of the school year, estimated using linear regression of Eq. (1). For each bodyweight outcome, column (1) shows results when controlling for free meals pilot schemes, column (2) adds other policies, demographic characteristics and time trends, and column (3) subsequently adds interactions of demographic characteristics with time trends and half-term blocks. The estimated coefficients are relatively stable across these different specifications with few marginally significant changes. As expected, in the first half-term of the school year when there has been little exposure to UIFSM, columns (3) show no statistically significant treatment effect of UIFSM on bodyweight outcomes. For every later half-term, UIFSM has a beneficial effect on bodyweight (positive for healthy weight, negative for obese and BMI z-score) which for all cases is statistically significant at the 1% level.

The size of the treatment effect does not get significantly larger after the second half-term block in November for the remainder of the school year, either in statistical terms or quantitative importance. This suggests that while the differential between children's calorie intake and expenditure is initially negatively affected by UIFSM, in line with the medical literature referenced earlier, they reach a new steady state fairly quickly. The estimated effects show that by the end of the school year children eligible for UIFSM are 1.1 percentage points more likely to be a healthy weight (relative to a pre-policy average of 76%), 0.7 percentage points less likely to be obese (relative to a pre-policy average of 9.4%); and have a BMI 4.1% of a standard deviation lower (relative to a pre-policy average that is 37% of a standard deviation above the 1990 average). To put this into context, a 4.1% standard deviation reduction in BMI z-score corresponds to about 63 g of absolute weight change for boys and 73 g for girls of this age.[20]

While this effect on children's weight does not seem large in absolute terms, it is considerable if compared to other school-based bodyweight reduction interventions that have been trialled in the UK. For example, an education-based intervention involving 16 lessons on healthy eating, physical activities and reducing sedentary activities had no effect on BMI (Kipping et al., 2008). Similarly, a physical activity programme in Scotland comprising 3×30 min of high-intensity physical activity per week for 24 weeks for 4 year olds found no overall reduction in BMI (Reilly et al., 2006). The 'Daily Mile', which entails primary school children walking or running outside for 15 min each day improved physical fitness and reduced body fat proportion but reduced BMI by only 0.8% of a standard deviation over the course of an academic year (not statistically significant), so it appears to generate

[20] Our measure of BMI is provided as the mean 'z-score' (i.e. standard deviations from the mean) with respect to the British 1990 Growth Reference Charts. The coefficient of variation (in percentage points) at age five-and-a-half for these charts is 7.6 for boys and 9.25 for girls, for a mean BMI of 15.5 kg/m^2 (Cole et al., 1995). This implies standard deviations $\sigma_{boy} = 0.076 \times 15.5 = 1.178$ kg/m^2 and $\sigma_{girl} = 0.0925 \times 15.5 = 1.43375$ kg/m^2. At median heights of 113.1 cm for boys and 111.8 cm for girls, this means a one-standard deviation change in BMI corresponds to the following change in weight, $\Delta W_{boy} = 1.178 \times 1.131^2 = 1.507$ kg and $\Delta W_{girl} = 1.43375 \times 1.118^2 = 1.792$ kg. This means that 1% of a standard deviation change in BMI, or a change in the BMI z-score of 0.01, corresponds approximately to a change in weight of 15 g for boys and 18 g for girls.

Table 4
Treatment effect of UIFSM by half term block.

	Healthy weight			Obese			Mean BMI z-score		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
September	−0.515*	−0.313	**−0.253**	0.128	−0.123	**−0.157**	0.001	−0.009	**−0.011**
	(0.273)	(0.299)	**(0.305)**	(0.184)	(0.201)	**(0.205)**	(0.007)	(0.008)	**(0.008)**
November	0.625***	0.820***	**0.857*****	−0.375***	−0.615***	**−0.608*****	−0.022***	−0.031***	**−0.032*****
	(0.176)	(0.214)	**(0.222)**	(0.119)	(0.144)	**(0.149)**	(0.005)	(0.005)	**(0.006)**
January	0.466***	0.652***	**0.682*****	−0.389***	−0.621***	**−0.619*****	−0.016***	−0.025***	**−0.026*****
	(0.155)	(0.196)	**(0.205)**	(0.104)	(0.132)	**(0.138)**	(0.004)	(0.005)	**(0.005)**
March	0.894***	1.063***	**1.085*****	−0.577***	−0.801***	**−0.800*****	−0.028***	−0.037***	**−0.038*****
	(0.159)	(0.199)	**(0.208)**	(0.107)	(0.134)	**(0.140)**	(0.004)	(0.005)	**(0.005)**
Easter	0.568***	0.732***	**0.775*****	−0.324***	−0.542***	**−0.554*****	−0.023***	−0.032***	**−0.034*****
	(0.180)	(0.216)	**(0.225)**	(0.121)	(0.146)	**(0.151)**	(0.005)	(0.006)	**(0.006)**
June	0.905***	1.089***	**1.127*****	−0.425***	−0.654***	**−0.667*****	−0.030***	−0.038***	**−0.041*****
	(0.227)	(0.256)	**(0.264)**	(0.152)	(0.172)	**(0.177)**	(0.006)	(0.007)	**(0.007)**
Pilot schemes	Yes	Yes	**Yes**	Yes	Yes	**Yes**	Yes	Yes	**Yes**
Other policies; Demographics; Cubic year trend	x	Yes	**Yes**	x	Yes	**Yes**	x	Yes	**Yes**
Demographics × cubic year trend and half-term block	x	x	**Yes**	x	x	**Yes**	x	x	**Yes**
Pre-treatment sample mean	**76.368**			**9.411**			**0.373**		
… between school SD	**6.312**			**4.209**			**0.178**		
… within school SD	**8.689**			**5.689**			**0.224**		
N schools	17,776	17,776	**17,776**	17,776	17,776	**17,776**	17,776	17,776	**17,776**
N obs.	154,169	154,169	**154,169**	154,169	154,169	**154,169**	154,169	154,169	**154,169**

Notes: National Child Measurement Programme. Treatment effect by each half-term block of exposure to UIFSM (academic years-ending 2015–2018, relative to pre-UIFSM period 2009–2014). Treatment effects derived from exhaustive half-term block by academic year interactions in school fixed-effect regressions. Additional controls: exhaustive half-term block by pilot scheme interactions; percent of measured students Black ethnicity, percent Black missing, percent of students measured girls, FSM-eligibility quintile (including missing dummy), pupil premium per pupil in school, school sports premium level, cubic year trend interacted with IDACI quintile, percent Black, percent Black missing, percent girls; half-term dummies interacted with within-school demeaned percent Black, percent Black missing and percent girls. Standard errors in parentheses. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$.

benefits of at most 20% the size of UIFSM (Chesham et al., 2018).[21] That universal meal provision appears to outperform previously trialled physical activity or education-based activities at reducing children's excess bodyweight is a clear and important implication of these results. We discuss the treatment effects on compliers implied by the estimated intention-to-treat effects in the next section when we analyse the change in take-up induced by the UIFSM policy.

Fig. 7 presents the estimated treatment effects obtained from specification (3), with full controls, over the course of the school year and with 95% confidence intervals. This reveals a pattern in the effects across half-term blocks, in which the treatment effect is smaller in the first half-term block of each term (i.e. those beginning September, January and Easter) than in the corresponding second half-term block of each term (November, March and June). The second half-term blocks of each term follow short, one-week holidays, whereas the first half-term blocks follow holidays of at least 2 week length. Though these differences are not statistically significant, this seems to suggest that there is some reversion in holidays, and a benefit from longer or less interrupted exposure to UIFSM.

We check that our estimated treatment effects are robust to variations on samples and years. We also carry out a simulation to check whether our results are sensitive to the fact that we cannot weight them by school size as this is not available in our data. These checks can be found in Appendix 2.

Cost–benefit calculation

A back-of-the-envelope calculation for the overall costs and benefits of UIFSM can be conducted as follows. We assume the UIFSM policy costs £1337 per child over the three years of eligibility.[22] We also assume that the intention-to-treat effect, reducing obesity prevalence by 0.7 percentage points at the end of the first year of school, persists throughout these children's lives. This would mean no further reduction in obesity prevalence in caused by the second and third year of UIFSM provision (a cautious assumption), but long-term persistence of the effect (possibly a strong assumption).

Dividing the cost-per-person of provision (£1337) by the treatment effect (0.7) produces an estimate of a cost of £191,000 per person no longer obese later in life as a result of the policy. It is estimated that the NHS annually spends £6.1bn on overweight and obesity-related ill-health (Public Health England, 2017). This is equivalent to £377 per obese person (approximately 24% of the population, including children). If such expenditure is required for a lifespan of 80 years, the total benefit of the UIFSM policy to the NHS from reduced obesity can be calculated at £30,160. This (undiscounted) benefit is less than the cost, so UIFSM falls short of representing value for money by this metric. However, the overall cost of obesity to the UK economy from direct medical expenditure plus productivity-related factors has been estimated at £60bn per year (McKinsey Global Institute, 2014, cited in Davies, 2019). This is £3708 per obese person per year, or £296,640 over 80 years. This benefit exceeds the cost in nominal terms, and in present-value terms when future benefits are discounted at a rate of 1.2% per year or less. Note that this calculation hinges on assumptions about the longer-term effects of the policy which we cannot observe, given our data.

[21] Better results were found for the Healthy Schools Network scheme in Denmark, involving schools sharing best practice over health and physical educations and a measurement programme. This achieved a 0.010–0.015 reduction in BMI (albeit not statistically significant) and reduced the prevalence of obesity by 1% (Greve and Heinesen, 2015).

[22] We assume constant revenue funding of £2.30 per meal (£437 per pupil per year), and capital funding for improved or expanded kitchen facilities of £175 m allowed to depreciate over 10 years. The £175 m figure comprises a total of £150 m initially allocated through LEAs for the 2014/15 academic year; £15 m allocated across 233 individual schools assessed as having the greatest need later that year and £10 m allocated through LEAs for the 2015/16 academic year. Just over 2 million infant pupils were recorded participating in UIFSM in 2017 (Department for Education, 2017). If this equipment is replaced after 10 years, this increases the average total cost by only £8.75 per pupil per year (£175 m ÷ 10 years, ÷ 2 m children), to £445.74.

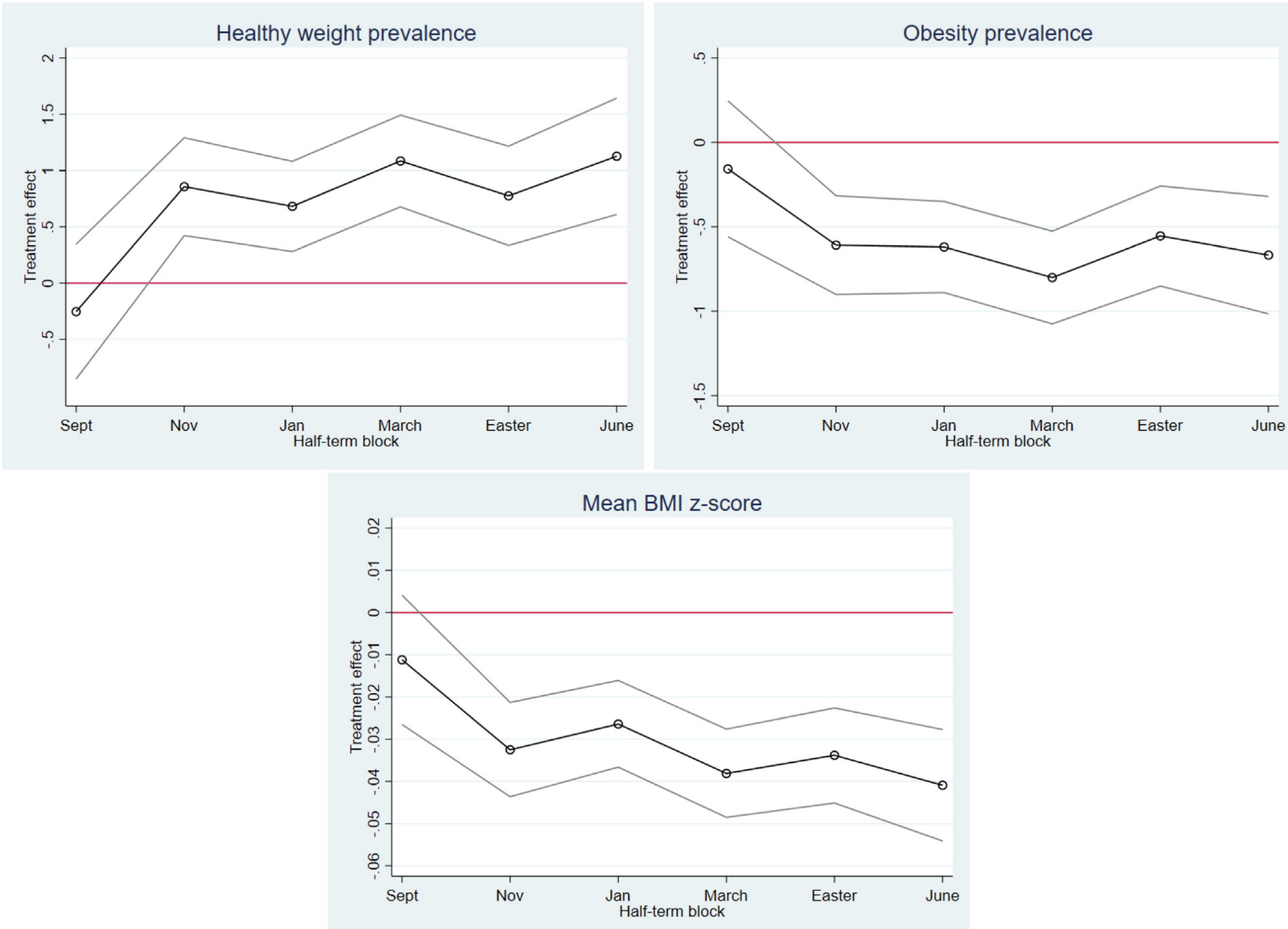

Fig. 7. Treatment effects of UIFSM by half-term block.
Notes: National Child Measurement Programme. Estimated treatment effect of exposure to UIFSM (academic years ending 2015–2018, relative to pre-UIFSM period 2009–2014). Derived from school fixed effect regression controlling for exposure to UIFSM pilot schemes, pupil premium and sport premium, proportion measured Black (and missing indicator), proportion measured girls, cubic year-trend interacted with IDACI quintile and demeaned proportion Black and girls, half-term block dummies interacted with demeaned proportion Black and girls.

5.2. Mechanisms

As discussed earlier, we may expect benefits of moving from a targeted, means-tested school food programme to universal provision of free meals to arise from different sources. They can come from increasing take-up among children who would have been eligible under means-testing but not taking up their meal, for example because of the stigma sometimes associated with targeted benefits. They can also come from previously not eligible children taking up school meals because the policy makes them free. We also consider that our results could be driven by an improvement of household finances as a result of reduced food expenditure, or by an effect on work incentives which may give rise to income and time effects, all of which can affect children's weight status. We investigate these mechanisms here.

Take-up

We first show in Fig. 8 how take-up of free meals has changed for children eligible and not eligible for free meals in academic years 2006/07 to 2017/18.[23] There are no consistent data on take-up for not FSM-eligible children in the pre-policy years, but there have been different surveys and LEA-level data returns run over the years so that each data point is from a different source (see notes to the Figure for details). In the pre-policy years take-up among not eligible children was just over 30%, documented across the different data sources. Once meals became free for everyone in academic year 2014/15 around 85% of children were eating them, an increase of more than 50 percentage points. For FSM-eligible children we have take-up rates among all free meal eligible students in the school for the years before the UIFSM policy was introduced (spanning Reception year to Year 6 in most schools) and among eligible students in the first 3 years of schooling for the post-UIFSM years. This should give a correct picture if take-up patterns do not vary across primary school years. The Figure shows that take-up among FSM-eligible students was about 84% in the pre-policy years, rose by around 3 percentage points to 87% in the first year UIFSM was introduced, and remained stable in the next three years.

Next we explore the extent to which the small increase in take-up among FSM-eligible students might be driven by removing the stigma sometimes associated with being seen to eat school meals under means-tested provision. To this end we show in Fig. 9 take-up of school meals by FSM-eligible children across our observation period,

[23] Note that eligibility for free meals is still recorded post-UIFSM introduction because school funding allocations depend on children's Free School Meal status, among other factors. Schools therefore pushed parents to register for free meals, but registration rates dropped from an average of 19.2% among 4–7 year olds in the 3 years preceding the policy to 15.2% in the 3 post-implementation years.

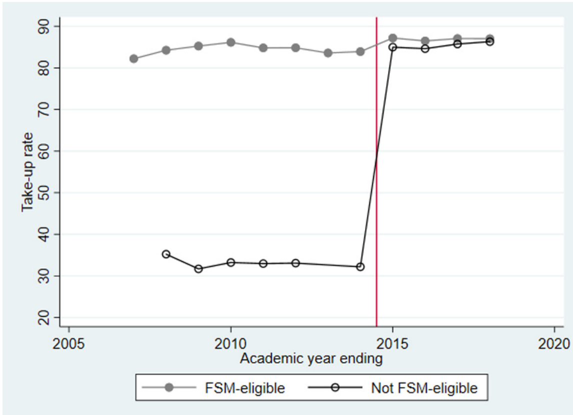

Fig. 8. Take-up of school meals among free meal eligible and not eligible children.
Notes: FSM-eligible series: Academic years ending 2007–2014: School-level 'Schools, pupils and their characteristics' data issued by Department for Education (2020), with take-up rates weighted by the number of FSM-eligible primary school aged children; 2015–2018: Individual-level Spring School Census with take-up rate equal to the proportion of all FSM-eligible infant-age pupils taking a school lunch. Not FSM-eligible series: 2008–2010: 'National Indicator 52a' from the Department for Communities and Local Government; 2011–2012: Take-up surveys by School Food Trust (Nelson et al., 2011, 2012); 2014: Take-up survey by Department for Education (Wollny et al., 2015). (Combining these figures for overall take-up by primary-age children at the LEA level, with the proportions FSM-eligible and the FSM-eligible take-up known from the 'Schools, pupils and their characteristics' series, enables the proportions of primary-age not-FSM eligible children taking school meals to be derived). 2015–2018: Individual-level Spring School Census, with take-up rate equal to the proportion of all not FSM-eligible infant-age pupils taking a school lunch.

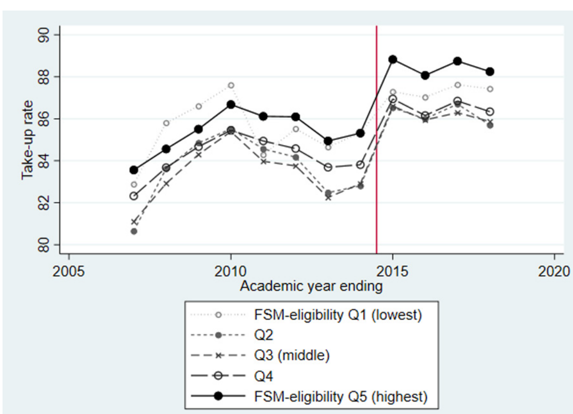

Fig. 9. Take-up of school meals among free meal eligible children, by school-level eligibility quintiles.
Notes: Academic years ending 2007–2014: School level 'Schools, pupils and their characteristics' data issued by Department for Education (2020), with take-up rates weighted by the number of FSM-eligible primary school aged children. 2015–2018: Individual-level Spring School Census, with take-up rate equal to the proportion of all FSM-eligible infant-age pupils taking a school lunch. School FSM-eligibility quintile is fixed over time, based on registration rates for the academic year-ending 2014.

separately by the proportion of children eligible for free meals in the school, measured in quintiles. Stigma effects could be lower in schools where many children are eligible for free meals, but this is not reflected in differential take-up rates. Both in the pre- and post-policy years there is no clear pattern as to whether take-up is higher in schools with higher or lower proportions of children on free meals. Moreover, the small increase in take-up does not seem to differ by the proportion of children eligible for free meals, suggesting that stigma effects were not driving the changes in take-up (though we note that stigma may prevent older students from taking up free meals).

While not FSM-eligible children increase their take-up of free meals by considerably more once UIFSM was introduced than FSM-eligible children, this does not necessarily imply that the main impact of the policy was on not eligible children. It could be that all the benefits of the policy were concentrated on the few FSM-eligible children who were induced by the policy to take up meals. One way to investigate this is by analysing treatment effects by the proportion of FSM-eligible children in the school. If the impact of UIFSM was concentrated in high FSM-eligibility schools that would indicate that benefits accrued to FSM-eligible children. In Fig. 10 we present the end-of-year treatment effects by the school's FSM-eligibility quintile. In the first quintile, between 0 and 4.4% of children are FSM-eligible. In the fifth quintile at least 27%, and an average of 38%, are FSM-eligible. This means that even in the fifth quintile, most of the rise in take-up will still be accounted for by not-eligible children.[24] As shown in Fig. 10, for all three outcomes we find a zero treatment effect for both the lowest and the highest FSM-eligibility quintiles. The middle three FSM-eligibility quintiles have differing effect sizes depending on outcome which generally go in the expected direction but in some instances are not statistically different from zero (e.g. healthy weight prevalence in the 3rd quintile).

Our consistent finding across these outcome variables that children in schools in the lowest quintile of FSM-eligibility do not benefit from UIFSM is in line with Alex-Petersen et al. (2021) who found benefits from free, nutritious school lunches in Sweden for all households except the richest. Our finding cannot be explained by the absence of a rise in take-up, so must instead reflect the counterfactual meals of children in these schools being very similar in energy content to the free school meals. This suggests that households in the least deprived schools have sufficient income, time and/or education to be able to produce balanced lunches at home, in contrast to those in more deprived schools where income, time or information constraints are more likely to bind. The peaking of the treatment effect in the second quintile across all outcomes suggests that the diets of relatively well-off pupils can still be improved. The lack of a beneficial treatment effect on obesity in the poorest (highest FSM-eligibility) schools suggests that there is a subset of income-constrained or low-educated households in which parents respond to the UIFSM transfer by reducing the quality of the food provided to the affected children during the rest of the day.

Treatment effects on compliers

We can use the changes in take-up rates for a back-of-the-envelope calculation of the treatment effects on the treated. This approach assumes that effects are entirely on compliers – those who take school meals because of the policy – so there are no spillover effects on never-takers or always-takers. Take-up increased by roughly 40% across all children (combining the 50%pt increase among non-FSM-eligible and the 3%pt increase among FSM-eligible children, weighted by the relative size of these groups). Dividing our intention-to-treat estimates from Table 4 by 0.4 would indicate that among children taking school meals because of the UIFSM policy, the policy reduced average BMI by 10.3% of a standard deviation by the end of the first year in school on average, equivalent to approximately 158 g and 182 g for a median-height boy and girl respectively.

Applying the same calculation to derive estimates of the treatment effects on the treated for our threshold measures, we find the policy increased the likelihood to be of healthy weight by 2.75 percentage points (reducing the proportion in unhealthy ranges by 11.5%), and reduced the likelihood to be obese by 1.75 percentage points (reducing the proportion obese by 18.6%). These are large proportional reductions that

[24] Assuming within-group rises in take-up of 3 and 50 percentage points respectively, on average in the fifth quintile the FSM-eligible and not-eligible groups will contribute rises in overall take-up of 0.76 and 31 percentage points respectively).

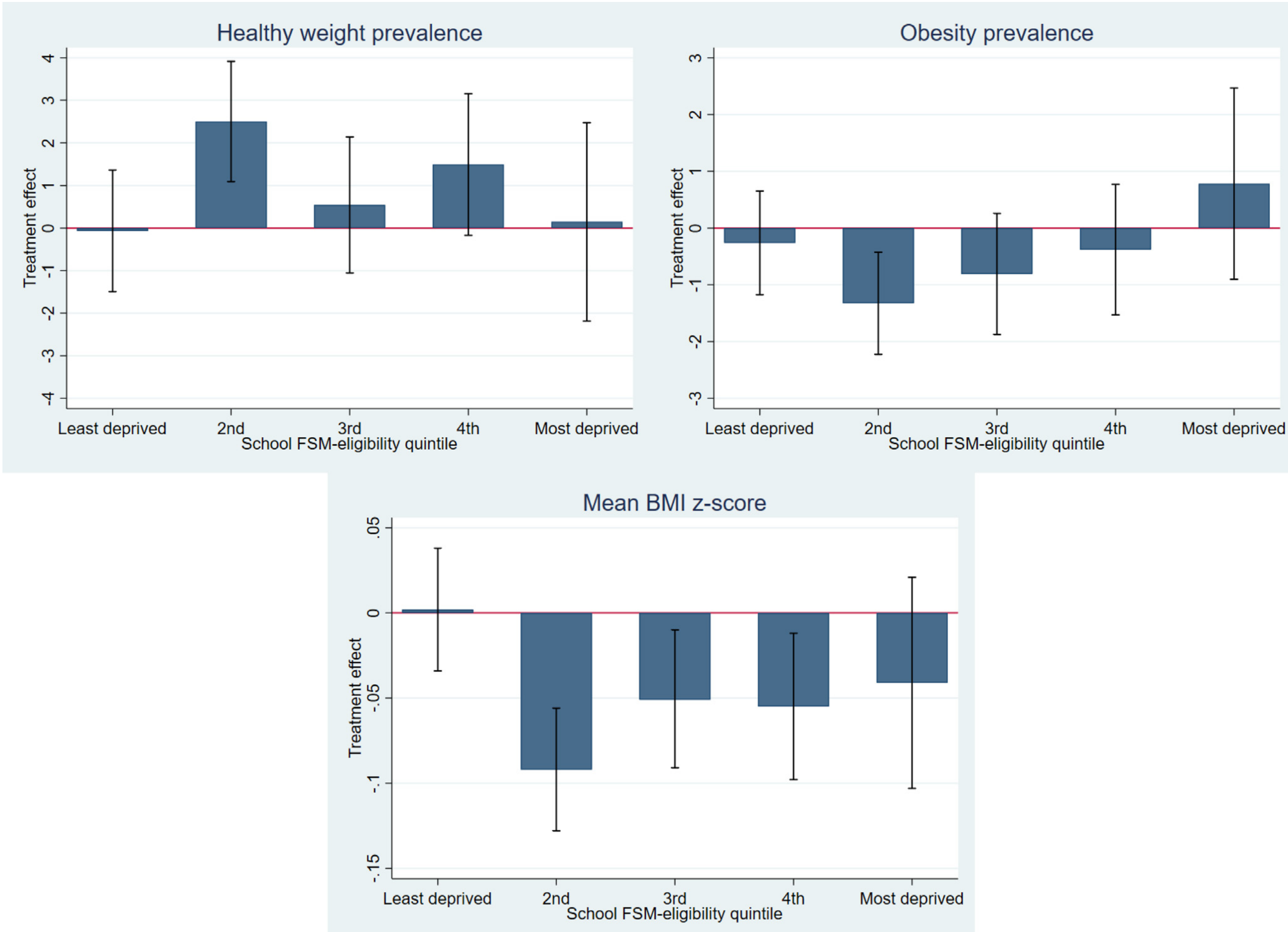

Fig. 10. Treatment effects of UIFSM for June half-term block by School FSM-eligibility quintile.
Notes: National Child Measurement Programme. Estimated treatment effect of exposure to UIFSM (academic years ending 2015–2018, relative to pre-UIFSM period 2009–2014) for sixth half-term of the school year. Derived from school fixed effect regression controlling for exposure to UIFSM pilot schemes, pupil premium and sport premium, proportion measured Black (and missing indicator), proportion measured girls, cubic year-trend interacted with IDACI quintile and demeaned proportion Black and girls, half-term block dummies interacted with demeaned proportion Black and girls. School FSM-eligibility quintile is fixed over time, based on registration rates for the academic year-ending 2014.

should be important for policymakers and public health practitioners, particularly for obesity, which is the more extreme outcome.

It is of course possible that the effects on mean BMI are not uniformly distributed across children. For example, if the effects on mean BMI were driven *entirely* by the 2.75% of the population shifting from overweight to healthy weight and the 1.75% shifting from obese to overweight, these compliers (4.5% of the population) would need to have lost 3.51 kg (boys) and 4.04 kg (girls) each on average.[25]

Income effects and work incentives

Another possible mechanism driving the beneficial effect of UIFSM on bodyweight outcomes is an income effect, accruing to families not previously eligible for free lunches for whom there is a drop in expenditure for school meals or packed lunches brought from home. Any savings can be spent on health-related investments that improve children's bodyweight, such as taking part in sports clubs or improving diet at home through purchasing higher quality food. While we do not have access to data on exercise-related expenditure, we can use waves 1–9 (2009–2018) of *Understanding Society*, the UK Household Longitudinal Study (University of Essex et al., 2019), to assess how expenditure on food has changed as a result of making school meals free for everyone. We would expect food expenditure to remain unchanged or increase if families saving on school meals shop for higher quality food and to reduce if they do not.

We evaluate the impact of UIFSM on expenditure for supermarket shopping for food and groceries and for eating out using a difference-in-difference model, estimated using households with any children aged 0–11, and excluding those interviewed during the school summer holidays. We estimate the following pooled OLS regression:

$$EXPENDITURE_{it} = \alpha_1 + \alpha_2 TREAT_{it} + \alpha_3 UIFSM_t + \beta(TREAT_{it} \times UIFSM_t) + \gamma X_{it} + \varepsilon_{it} \quad (3)$$

Here $EXPENDITURE_{it}$ is real expenditure (measured in 2015 pounds) of household i at time t on supermarket shopping for food and groceries and on eating out, respectively, both in the last four weeks, and equivalised for household composition using the OECD equivalence scale. $TREAT_{it}$ is the number of children in the household who are eligible for UIFSM and $UIFSM_t$ an indicator equal to one for the periods when UIFSM is available. The treatment effect is given by β, the coefficient on their interaction. X_{it} is a vector of time-invariant and time-varying individual and household characteristics (see notes to Table 5 for details) and ε_{it} is an idiosyncratic error term.

We run equivalent regressions on the same data set to assess whether UIFSM had an effect on weekly normal working hours (of all parents in the household and of mothers, respectively). If so, this could imply the existence of income and/or time effects on bodyweight. For example, additional income could be spent on higher quality

[25] Calculation for boys: $158g/0.045 = 3.51$ kg; girls: $182/0.045 = 4.04$ kg).

Table 5
Effect of UIFSM on food expenditure and work hours.

	All families (1)	Non-FSM-eligible (2)	FSM-eligible (3)	All families (4)
	Supermarket food			Work hours (parents)
Coef.	−5.731***	−6.207***	−3.152	1.036*
SE	(1.815)	(1.971)	(4.535)	(0.618)
N	31,999	26,954	5,045	22,254
Mean	165.6	169.63	143.92	34.41
	Eating out			Work hours (mothers)
Coef.	−2.204**	−3.023**	2.546	0.612
SE	(1.099)	(1.234)	(2.238)	(0.382)
N	32,010	26,967	5,043	21,121
Mean	41.4	44.11	25.24	14.97

Notes: UKHLS waves 1–9. Sample of families with any children aged 0–11 and interviewed outside the summer holidays. Estimated treatment effect of exposure to UIFSM (September 2014 onwards) relative to pre-UIFSM period. Treatment is the number of UIFSM-eligible children in the family. Outcome is 2015 real expenditure for supermarket shopping (food and groceries) and eating out, equivalised for household size, and total normal weekly working hours of parents and of mothers, respectively. Estimates derived from a difference-in-difference regression with year and month fixed effects, controlling for urban/rural, household tenure, age of youngest child, number of lone parents in household, nine dummies for household composition, number of household members in work (only for expenditure regressions). Working hour regressions additionally control for eligibility for childcare for any children aged 2–4 in the household, where eligibility for 2 year olds depends on lagged benefit receipt in addition to children's ages. FSM status of children is derived by applying the FSM eligibility criteria to parents' survey information on receipt of benefits. Standard errors in parentheses. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$.

food, reducing bodyweight, but time available to produce healthy food could be constrained, increasing bodyweight. In these regressions we control carefully for the availability of free childcare for any pre-school children in the household to account for policies newly phased in during our observation window.[26]

Table 5 shows in column (1) that the introduction of UIFSM reduced both supermarket expenditure and expenditure for eating out across all families. Splitting the sample into families that would be eligible for FSM and those that would not, columns (2) and (3) show that the monthly savings are statistically significant for not eligible families only. We do not expect significant changes in expenditure among FSM-eligible families as there was very little change in take-up among them. For the not FSM-eligible families the reduction in expenditure suggests that savings were not invested in higher quality food.

The estimated equivalised values can best be interpreted by looking at the savings in a 'typical' family. In a household of two adults and two children that is not eligible for free meals, having an additional child exposed to UIFSM reduces total household supermarket shopping expenditure by £13.03 (about $16.80) and eating out expenditure by £6.35 (about $8.20) over four weeks.[27] This is a saving of about £1 ($1.30) per weekday and child among not FSM-eligible families and of about £2 for each not FSM-eligible family taking up the free lunch per child as a result of the policy (about 50% do, according to our take-up analysis). It is possible that families invested these savings into expenditure related to physical activities, which we do not observe, but given the small amounts of money it seems unlikely that the UIFSM treatment effects are driven entirely by income effects, rather than an increase in take-up.

Column (4) of Table 5 shows the impact of universal free meals on parents' and mothers' working hours. The combined weekly normal working hours of all parents in the household increased by 1 h for each child in the household eligible for UIFSM (significant at the 10% level). Among mothers, who more often work part-time than fathers do and thus have ability to adjust working time upwards, the increase is by just over half an hour per week, but this is imprecisely estimated. This analysis suggests that time constraints and additional income will have had, if anything, a minimal impact on children's body weight, suggesting that eating a nutritious and calorie constrained school meal instead of a packed lunch provided from home is the main mechanism behind our results.

6. Conclusion

In this paper we have evaluated the effect of cumulative exposure to Universal Infant Free School Meals (UIFSM) over the course of the first year of school of children in England on bodyweight outcomes. We find evidence that by the end of the school year, those exposed to UIFSM have significantly better bodyweight outcomes then they otherwise would, in terms of being more likely to be healthy weight (1.1 percentage points), less likely to be obese (0.7 percentage points) and have a lower BMI (4.1% of a standard deviation). Our results largely contrast with earlier evidence on free school lunch provision, mainly from the United States, which has found these tend to increase obesity prevalence and BMI, but our context differs in that we are evaluating the nationwide switch from means-testing to a universal programme with rigorous nutritional requirements. This is a policy-relevant margin for the many countries that run means-tested nutrition programmes with high nutrition standards. Our results do not extend to low-quality food programmes which may have different effects.

Analysis of changes in take-up of school meals before and after the policy was introduced shows that children from families not previously eligible for free meals increased their take-up considerably whereas children previously eligible for free lunches increased their take-up by little. Heterogeneity analysis suggests that children from fairly affluent families can benefit from making meals free, whereas children in the most and least affluent 20% of schools do not benefit, presumably because their counterfactual meal provided from home is similar in terms of calories provided or because the positive effects lead to worse nutrition at other times of the day. We also find that families newly eligible for free meals under the UIFSM policy subsequently reduce expenditure for food shopping, suggesting that they do not use savings for higher quality food at home. UIFSM eligibility has negligible effects on hours worked by parents.

The size of the effects compares favourably with existing estimates of physical activity or education-based programmes that have been implemented in the UK and elsewhere. Once the cost of obesity to the economy is factored in, UIFSM appears to be cost-effective if evaluated on its bodyweight benefits alone. While our data only allow us to evaluate the short-run effects of the policy within the first year of school, bodyweight outcomes can be persistent for sustained school-level interventions. Even if the onset of excess weight is merely delayed, there is evidence both that shortening an individual's accumulated lifetime duration of obesity, and reducing BMI in childhood for the same realised BMI as an adult, reduce the risk of a range of metabolic, cardio-vascular, cancer-related conditions.

Our analysis suggests that there were no pronounced stigma effects associated with taking up means-tested free school meals before the policy was introduced: take-up did not rise much among eligible students after making free meals universally available. Universalism should however benefit groups of low-income children who were missed by criteria used to define eligibility, and we show that it does benefit higher income students from families where time and/or information constraints may prevent preparation of healthy packed lunches. This suggests that universalism has a role to play in an environment where school meals address mal-nourishment more than under-nourishment. Our results therefore imply that in the face of time or information

[26] Free childcare for 2 year-olds was rolled out from September 2013 for disadvantaged families, defined similarly to free school meal eligibility. We use lagged disadvantage to define eligibility. Free childcare for 3–4 year-olds was increased from 15 to 30 h from September 2017.

[27] The estimated coefficient of 6.207 is multiplied by 2.1, giving a weight of one to the first adult in the household, 0.5 to the additional adult household member and 0.3 to each child.

constraints it is justified to provide meals as in-kind benefits rather than cash transfers. While potentially being cost-effective, perhaps unsurprisingly the move from means-tested to universal school meals does not seem to have made huge progress in reducing socio-economic inequalities in bodyweight outcomes between students, as benefits are concentrated in schools in the middle range of deprivation. This arguably is an inherent feature of universal systems that cater to everyone, regardless of background.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix 1 and 2. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.pubecp.2022.100016.

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