

Going universal - The impact of free school lunches on child body weight outcomes¹

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Abstract

Since September 2014 school lunches – previously free for very low income students - are available to all children in England in their first three years in school free of charge. We draw on data from the National Child Measurement Program (NCMP) to evaluate the effect of switching from targeted to universal free lunch provision on the body weight outcomes of children aged 4-5, showing how the treatment effect evolves over the school year as the cumulative dosage of exposure to free meals increases. By the end of the school year, on average a child exposed to free lunches is 1.2 percentage point more likely to be of ‘healthy weight’ and 0.7 percentage points less likely to be obese, and has body mass index (BMI) that is 4.3% of a standard deviation lower than one who is not. This effect seems driven by children not previously eligible for free meals taking them up, suggesting that the diets of relatively well-off pupils can be improved.

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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, 2018), a tenfold rise in the past four decades (Ezzati et al., 2017). Childhood Body Mass Index (BMI) and obesity have been shown to be strongly persistent into adulthood (Singh et al, 2008). This has made it difficult to identify the long-term effects of child obesity independently from the effects of their bodyweight as an adult, though individual studies have indeed shown such an effect: Fagherazzi et al, (2013) showed obesity among prepubescent girls to be associated with an increased risk of breast cancer post-menopause, and Tirosh et al (2011) found higher BMI among adolescents to be associated with greater risk of coronary heart disease as an adult. Nevertheless, whether the effects are direct or indirect, adult obesity is well understood to be a risk factor for a wide range of diseases (OECD, 2019). 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). 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.

This paper investigates whether providing free, high quality lunches to children in school can contribute to tackling 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, about 18% of children in these year groups were already eligible for free lunches under a means-tested program while children who were not eligible could purchase the same meal at cost. This setting allows us to study what happens to children's body weight 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 point in case, 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 have made school breakfasts and lunches free for all students (Leos-Urbel et al., 2013). 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 mean-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 counter-factual meal (that provided from home) 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 home meal. 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.³ 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 (Caeley 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. 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 data on the timing of each school visit, school-level weight outcomes and a number of control variables for the whole country, avoiding issues of external validity.

Using these data, we use the conditionally exogenous timing of the school visit to measure height and weight as a source of variation in duration of exposure to UIFSM 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, allowing us to assess the impact of different lengths of exposure within the first school year, assuming that the timing of the measurement is conditionally exogenous. We compare the change in bodyweight outcomes of children throughout the school year in a difference-in-difference 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.⁴

³ 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 lowest income households are not eligible.

⁴ The existing literature suggests that exposure to school is likely to be beneficial to bodyweight outcomes. Von Hippel et al. (2007) show that BMI of American schoolchildren increases more slowly during Kindergarten and First Grade than

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 1.2 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 4.3% 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 all children lead to modest improvements of bodyweight outcomes in the short term.

We study the mechanisms driving these improvements by using school census and survey data to describe changes in take-up induced by the policy. We find that children who were always eligible for free meals increased take-up by about 2 percentage points and newly eligible children increased it 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 body weight 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 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 FSM pre policy, and suggests that the diets of relatively well-off pupils can still be improved. We also test whether the program improved household finances through reduced food expenditure but find that the savings were small and therefore unlikely to generate income effects on weight outcomes.

Most of the existing evidence on the effect of free school lunches on bodyweight outcomes suggests that these raise the prevalence of obesity, but this is almost exclusively identified from the effects on a small group of marginal recipients in the United States’ National School Lunch and School Breakfast Programmes (Frisvold, 2015; Hinrichs, 2010; Dunifon and Kowaleski-Jones, 2004; Schanzenbach, 2009; Millimet et al., 2010). For the UK, von Hinke Kessler Scholder (2013) shows no effects of means-tested free school meal provision on child bodyweight outcomes, but this study on the 1980s significantly predates the current policy, and the enforcement in 2008 of improvement food and nutrient-based standards for school lunches (see Spence et al, 2013). Alex-Petersen et al. (2017) investigate the long-run effects of introducing free and nutritious school lunches in Swedish primary schools on a range of outcomes. They find no effect on the probability of being overweight or obese at age 18. Our first contribution 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.

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 body weight outcomes, conducting robustness checks to demonstrate that this treatment effect is strongest for those experiencing the biggest change in environment. Based on the data used in this paper Holford and Rabe (2019) show that pre UIFSM introduction children’s bodyweight improved over the course of their first year in school.

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 meal (Polish Eurydice Unit, 2016). Besides England, Scotland also provides free lunch to children aged 4-7, and only Sweden and Finland provide this throughout the whole of school. A switch to universal provision for some age groups is a relevant policy option for 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 contributes to the wider literature on the relative advantages of means-testing vs universalism which has gained importance in policy areas such as early years policy. For example, the provision of free early childhood education and care started out in the 1980's 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.

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. Our finding of an improvement in bodyweight outcomes for treated cohorts show that UIFSM represents an occasion in which universal in-kind transfer can be supported on these grounds. Parents of all socio-economic backgrounds appear to face either a time or an information constraint (Bhattacharya and Currie, 2001) in constructing their children's diets that would prevent the same gains being realised were cash of equivalent value transferred instead. Finally, we conduct cost-benefit analyses of this intervention in the context of school-based health and bodyweight-reduction initiatives, and the value of reduced prevalence for healthcare spending and productivity. We conclude that UIFSM has a larger treatment effect but poorer cost-effectiveness than the physical activity or information programmes surveyed.

The paper proceeds follows. Section 2 describes the free school meal policy 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, heterogeneity analysis as well as a number of robustness checks. Section 6 concludes.

2. Background to Universal Infant Free School Meals (UIFSM)

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) or with a sufficiently low household income. Running in parallel, the School Fruit and Vegetable Scheme has entitled every primary-school age child to a free piece of fruit or vegetable, outside of lunch time, every school day since 2004.

Children not meeting the criteria for FSM may purchase a school meal at cost (around £2.30 per meal). 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 and minimum levels of intake of different nutrients, 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 embed the existing nutrient-based standards (Department for Education, 2014b). Compliance with school food standards is assessed as part of the short-notice inspections made by the Office for Standards in Education (Ofsted), which in England is the main mechanism other than academic performance tables through which information about school quality is communicate to parents and prospective parents, and head teachers and governors are held accountable.

Students not having a school meal may bring a packed lunch from home. These lunches 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, the audit study by Evans et al, (2018) found 89% of packed lunches to exceed this level, averaging 624 calories, and only 1% of packed lunches meeting food school standards in terms of energy and nutrients.⁵ Our prior is therefore that, other things equal, we would expect a reduction in children's bodyweight outcomes as a result of the UIFSM policy.

The means-tested FSM system remains in place for children who are in their fourth year of school or beyond. However, since September 2014 all infants (comprising the first three years in school, i.e. children aged 4-7) in about 26,000 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. The policy's stated aims are to improve children's educational attainment; to help families with the cost of living; and to ensure children have access to a

⁵ Moreover, one-third of packed lunches surveyed contained a sweet snack, processed savoury snack *and* sweetened drink.

healthy meal a day and develop long-term healthy eating habits (Department for Education, 2013; Cabinet Office, 2013). The policy was announced in September 2013. *Capital funding* for necessary enhancements to kitchen and dining facilities, totalling £150m, was allocated to Local Education Authorities (LEAs) in December 2013, with a further opportunity for schools to bid, through their LEA, for a share of a further £15m in October 2014; and a further £10m allocated (£8.5m through LEAs, the rest directly to schools) ahead of the 2015-16 academic year. *Revenue funding* of £2.30 per universal infant free school meal served (£437 per year) is provided to schools, calculated based on take-up on a census day in January each academic year. 7/12ths of a projected allocation is provided in July of the preceding year, and the remaining balance in July of the current academic year. This figure of £2.30 per meal has remained constant in nominal terms since the policy's introduction.

The then-government's case for UIFSM was largely based on evaluations carried out of earlier pilot schemes for universal FSM entitlement in three local education authorities (LEAs) in 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. However, the authors caution that it was not clear that the positive outcomes of the pilot (such as were found) would be repeated in the roll out of free school meals to all infant pupils across the country. This is because the pilot took place in two relatively deprived local authorities and the benefits might be lower in more affluent areas, and because the pilot included a host of supporting activities around awareness and encouragement of take-up. These are not included in the UIFSM policy, so the impact of offering universal free meals may be dampened. Conversely however, it is also possible that by implementing a universal policy that parents know their child will be entitled to for three years, parents and children will be more likely to see participation as the social norm. If a habit is formed, parents may be less likely to see UIFSM as a marginal input that is a substitute for their own effort in providing healthy food during the rest of the day (see examples of substitution of effort in De Fraja et al, 2010).

3. Data

The National Child Measurement Programme has collected data on the heights and weights of children in all 16,000 primary schools in England each academic year since the 2005/06 school year. 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 of the NCMP 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 Local Education Authorities (LEAs). (Later in our analysis we will use this legislation change both as a placebo and to evaluate any change in measurement quality).

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.⁶

Each visit entailed 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, 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. Schools and Local Authorities 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 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'. This reports the standard deviations above or below the British 1990 growth reference charts mean, adjusted by age in months and sex 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 normal weight as our main

⁶ 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 local authority trends, can be found at NHS Digital, 2018b).

outcomes.⁷ Our data also include the percentage of children who are female and who are of black ethnicity, but not any cross-tabulations of these characteristics.

We additionally supplied publicly available data on the characteristics of each school to NHS Digital, who linked these to the NCMP data before releasing the extract. Those characteristics we use in our analysis are the school's involvement in a pilot scheme for universal or extended Free School Meals that year; 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 take-up of by FSM-eligible students across the whole school for all years⁸ all converted into quintiles (across school-year points) to maintain anonymity of the schools in question. Our data does not enable us to weight schools in our analysis in proportion to their size. Using data on school size and its variation across primary schools in England we 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 data or for which the timing of the visit by half-term cannot be established, but include schools that closed or first opened during this 11 year period. In our final analysis sample we use only measurements taken in academic year ending 2009 and onwards, once the enhanced school food standards were fully in force, leaving us with 17,776 different schools and 154,169 visits.

In Figure 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 2008-2018. Figure 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 (the relative drop between 2008 and 2010 is large, but small on the scale of this graph), 13% overweight and just under 10% obese (so 23% 'at least overweight'). The remainder are 'healthy weight', not shown due to the different scale. There is no discernible jump in any of these outcomes for the academic year-ending-2015 when UIFSM was introduced, or subsequently. The secondary axis in Figure 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 substantial drop in mean BMI for the first year of UIFSM, but a subsequent rise.

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 September and late July.⁹ 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

⁷ Underweight has a very low prevalence in the population of 1-2% and overweight mostly mirrors healthy weight prevalence, since this is the main discrete margin affected by underlying changes in the distribution of BMI.

⁸ About 16% of school students have been eligible for free meals based on a means test throughout our period of observation. This means that 84% of students were newly eligible under the UIFSM policy.

⁹ There are 5 'inset days' usually for teaching training, and usually timed at the start or end of a holiday.

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 where 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 local authority (LA) and can vary especially around the Easter holidays. Because we do not have an LA 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 drop all the school weeks for which we cannot be sure whether the visits made should be assigned to the preceding or the subsequent half-term.

Figure 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 overall children tend to become lighter (for their height) over the course of the academic year, suggesting that the school (versus home) environment is relatively beneficial for bodyweight outcomes. Moreover, while BMI was higher for the cohorts that benefited from UIFSM in the first half term block, 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. Figure 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. 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. Identification strategy

The UIFSM programme was introduced simultaneously across the whole of England in September 2014. Because the NCMP is only conducted in England 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, and this allows us to compare children who were exposed to the policy for different durations at the time they were weighed and measured with children who were not exposed to the policy but weighed and measured at the same time of the year. 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 progressively larger in a school visited, say, in the spring and summer.

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

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

where \bar{Y}_{st} is the mean of the outcome recorded in school s in year t , $HTERM_{st}$ is the half-term of NCMP visit in school s in school year t and can take a value from 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. The intention-to-treat effect for each half-term block in this equation is given by $\tau_u + \tau_h$. We estimate equation (1) using linear models on our school-level data.

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 below.

Identifying assumptions

In order for our difference-in-difference regression to yield causal estimates of the impact of the UIFSM policy on bodyweight outcomes we need to make two identifying assumptions. These are (1) that conditional on the controls included in our model, bodyweight outcomes would have evolved in the same way over the school year in the post-UIFSM as pre-UIFSM years, had the policy not been introduced (parallel trend assumption), and (2) that the timing of NCMP visits to schools did not change between the pre-and post-UIFSM period in a way that was related to any unobserved factors affecting bodyweight outcomes. We discuss the steps we take to defend these assumptions in turn.

The parallel trend assumption could be violated if there were other policies introduced during the period of our analysis, which had an effect on bodyweight outcomes that are not otherwise accounted for. As discussed in the introduction, there were 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 local authorities over the years

preceding UIFSM. We capture and control for these using six dummy variable categories, also interacted with half-term block, in all our regression specifications.

Another potential policy is the pupil premium, extra funding made available to schools for each student eligible for free lunches plus a small number of other pupil groups (e.g. children adopted from care) which could potentially affect children's bodyweight. The funding per 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 premium available to schools as the average 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, allowing this to have differential effects pre-and post-UIFSM.

Moreover, we control for the percentage of children measured at each visit who were girls and who were of black ethnicity, since these groups are expected to exhibit lower and higher prevalence of overweight or obesity than boys and non-black pupils respectively, and school level outcomes will therefore be sensitive to changes in proportions of these variables. (This is despite sex directly being accounted for in defining the threshold measures. Ethnicity is not.) 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. (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.

Because all schools received the UIFSM treatment from academic year-ending 2015 onwards, we cannot perform a conventional test of the assumption of parallel trends between a treated and control group in the pre-treatment period. Instead, the relevant test is for parallel trends over time between 'low-dose' schools, visited in the first half-term of the school year, and 'high-dose' schools, visited later in the school year.

We know from Figures 2 and 3 that children in schools visited later in the school year tended to have better bodyweight outcomes even in the pre-treatment period. We would be concerned for our identification strategy if we were also to see a systematic relative improvement in bodyweight outcomes for 'high-dose' schools over 'low-dose' schools beginning 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.

Formally, for the pre-treatment period we run a school fixed-effect regression of bodyweight outcomes on an exhaustive set of half-term by year interactions:

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

For each year t and half-terms s the difference in coefficients ($\theta_{st} - \theta_{1t}$) provides a measure of the conditional improvement in observed bodyweight outcomes since the start of the school year. In Table 1 we present estimates of the difference-in-difference ($\theta_{6t} - \theta_{1t}$) - ($\theta_{6,2014} - \theta_{1,2014}$), representing the effect of one academic year's exposure to the school environment in year t , relative to the final year before UIFSM was introduced. For only one outcome in one academic year do we ever observe a statistically significant (and only at 10%) deviation from a constant school environment effect over this period. The magnitude of changes in some individual point estimates between years does underline the importance of identifying our treatment effect from multiple years of post-treatment data, and testing the sensitivity of our results to each specific post-treatment year, but there is no support for a systematic trend in the effect of the school environment beginning prior to 2015. While the BMI z-score estimate does become smaller (more negative) for the last two consecutive years, the change is statistically and quantitatively insignificant. The likelihood that this is down to chance is supported by the trends in healthy weight and obesity point estimates switching directions over this period, while staying close to zero the long-run mean.

Our second identifying assumption relates to the timing of measurement. Bodyweight outcomes will be representative of the children in England for each half term only if the timing of visits by NCMP is random across schools. In particular, we may expect a bias from our estimation if any pattern in the timing of visits changed between pre- and post-UIFSM introduction. For example, if NCMP had the habit of visiting schools with the smallest concern over obesity first in each academic year but changed this to late visits in recent UIFSM years, our results would overstate the beneficial effect of the policy. According to the organisation implementing the NCMP, 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.¹⁰

Table 2 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.

To investigate whether these shifts in timing were accompanied by shifts in school student composition we conduct the following check using the observable student characteristics available to us. We run similar regressions to equation (1) above, but using school-level child

¹⁰ Source: Email by NCMP programme support manager dated 2 April 2019.

characteristics as outcomes. The coefficients on the half-term x UIFSM interactions will tell us whether schools with the characteristic in question were more likely to be visited in a particular half-term block post than pre UIFSM. Table 3 displays the results for three characteristics: the percentage of students of black ethnicity, the percentage who are girls, and the quintile of the proportion of students eligible for free lunches (entering linearly). We see in column (1) that schools visited in the first half-term of the school year have a higher proportion of black students after UIFSM introduction than before. However, the effect is very small at 0.3 percentage points, or 9% of a within-school standard deviation. We see in column (3) that schools visited in the middle of the school year are more likely to be in higher quintiles of Free School Meals registration after UIFSM than before. Again, the effect size is very small, equivalent to a movement of less than 2.5% of a quintile, 0.5 percentiles, or 4% of a within-school standard deviation. Nevertheless this exercise underlines the importance of controlling for the FSM registration quintile in the analysis. The proportion of girls in the measured cohort is never significantly (or by a quantitatively important magnitude) predicted by the timing of the visit.

To ensure our identifying assumption holds by accounting for these small shifts in timing by black composition, and the possibility that metabolic differences by ethnicity and sex make the body weights of black students and girls differentially responsive to the duration of being in school and to UIFSM, we therefore control for interactions of both these variables with the half-term block. We demean both the proportion black and proportion girls within schools, so as to leave the interpretation of coefficients on our main ($HTERM_{st} \times UIFSM_t$) terms unchanged.

5. 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 equation (1). For each bodyweight outcome, column (1) shows results when controlling for free meals pilot schemes, column (2) adds demographic characteristics and time trends, and column (3) subsequently adds interactions of demographic characteristics with time trends and half-term blocks. We can see that the estimated coefficients are relatively stable across these different specifications and none of the differences we find are statistically significant. As expected, in the first half-term of the school year when there has been little exposure to UIFSM, there is 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, they reach a new steady state fairly quickly. This is not surprising since the overall effect sizes are small. The estimated effects show that children eligible for UIFSM are between 0.75 and 1.2 percentage points more likely to be a healthy weight (relative to a pre-policy average of 76%), between 0.6 and 0.9 percentage points less likely to be obese (relative to a pre-policy average of 9.4%); and have a BMI between 2.8 and 4.3% 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 2.8% standard deviation reduction in BMI z-score corresponds to about 43g of absolute weight change for boys and 50g for girls of this age.¹¹

Compared to other bodyweight reduction interventions that have been trialled in the UK, the impact of the UIFSM policy is large. 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).¹² Similarly, a physical activity program in Scotland comprising 3x30 minutes 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 minutes 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 benefits of at most 20% the size of UIFSM (Chesham et al., 2018).

Figure 4 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.

¹¹ 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 $15.5\text{kg}/\text{m}^2$ (Cole et al, 1995). This implies standard deviations $\sigma_{boy} = 0.076 \times 15.5 = 1.178\text{kg}/\text{m}^2$ and $\sigma_{girl} = 0.0925 \times 15.5 = 1.43375\text{kg}/\text{m}^2$. At heights of 113.1cm for boys and 111.8cm for girls, this means a one-standard deviation change in BMI corresponds to the following change in weight, $\Delta W_{boy} = 1.178 \times 1.131^2 = 1.507\text{kg}$ and $\Delta W_{girl} = 1.43375 \times 1.118^2 = 1.792\text{kg}$. 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 15g for boys and 18g for girls.

¹² 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 program. 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).

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 not eligible children who would not have taken up a paid-for meal and are taking it up because the policy makes it free. Finally, they can result from an improvement of household finances as a result of reduced food expenditure that positively affected children's weight status.

We first describe how take-up changed among eligible children in the years before and after UIFSM introduction.¹³ The data are somewhat noisy as 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. Figure 5 shows how take-up developed for eligible children between academic years 2006/2007 and 2016/2017, separately by the proportion of children eligible for free meals in the school, measured in quintiles. 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. Stigma effects could be lower in schools where many children take free meals, but this is not reflected in differential take-up rates. The figure shows that take-up was around 85% in the pre policy years and rose by around 2 percentage points to 87% in the first year UIFSM was introduced and remained stable in the next two years. Again, the small increase in take-up does not seem to differ by the proportion of children eligible for free meals, again suggesting that stigma effects were not driving the changes in take-up.

Figure 6 compares take-up between children eligible and not eligible for free meals for the same time-period. There are no consistent data on take-up for not eligible children in the pre policy years, but there have been different surveys and local authority-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 in academic year 2014/15 around 85% of children were eating them – an increase of more than 50 percentage points. All families were able to purchase a school meal before these became free, so it seems that the cost of around £2.30 per meal was a sufficient disincentive for a high proportion of families. In summary, the UIFSM policy resulted in small proportions of previously eligible

¹³ 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.

children to start eating school meals, and to very large increases in the proportions of not eligible children having meals.

This does not 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 Figure 7 we present the June half-term treatment effects by the school's FSM-eligibility quintile (measured in the academic year-ending 2015). In the first quintile, between 0 and 4.4% of children were FSM-eligible. In the fifth quintile at least 27%, and an average of 38%, were 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 (assuming within-group rises in take-up of 2 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). Figure 7 shows a non-linear pattern whereby for all three outcomes we find a zero treatment effect for the lowest FSM-eligibility quintile, but a significant large beneficial effect for the second quintile (between 4.4% and 8.7% FSM-eligible). Above this, there is a markedly different pattern between outcome variables. For the mean BMI z-score, treatment effects remain at over 5% of a standard deviation, larger in magnitude than 5% of a standard deviation than found for the whole sample, only losing significance due to its smaller sample size. For healthy weight, there is imprecision and no consistent pattern, although the third to fifth quintiles are all smaller in magnitude than for the second. For obesity however, the treatment effect becomes smaller in magnitude for each successive higher quintile, even changing sign for the highest 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. (2017) who found benefits from free, nutritious school lunches in Sweden for all households except the richest. School-level FSM-eligibility is negatively correlated with average household incomes (ref?). 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 result is indicative of households in the lowest quintile having either sufficient disposable income or disposable time (perhaps from a discretionarily non-working parent) and education to be able to produce appropriately balanced lunches at home, in contrast to those in higher quintiles where either an income, time or information constraint is more likely to bind. The peaking of the treatment effect in the second quintile suggests that time is likely to be the most important in this regard, indicating 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.

We can use the changes in take-up rates for a back-of-the-envelope calculation of the treatment effects on the treated. Take-up increased by roughly 40% across all children (eligible and not eligible taken together). Dividing our intention to treat estimates from Table 3 by 0.4 would indicate that among children taking school meals because of the UIFSM policy increased the likelihood to be of healthy weight by 3 percentage points (3.9%), reduced the likelihood to be obese by 1.8 percentage points (19%) and the average BMI by 10.8% of a standard deviation by the end of the first year in school among treated children. These effects are considerable.

Robustness checks

We investigate whether our estimated treatment effects are driven just by one particular year of UIFSM. We conduct this exercise in light of observing children's bodyweight outcomes broadly to deteriorate in 2016 and again in 2017, relative to 2015 (see Figure 1). It is possible that the beneficial treatment effects of UIFSM could be driven by greater efforts to increase consumption of school meals, and information campaigns perhaps resulting in dietary improvements also outside of school, in the first year of its introduction.

In columns (1-4) of Table 5 we show separate results for BMI z-score obtained by excluding one year of UIFSM exposure from the data at a time. The overall pattern from the main results is retained, though three things are noteworthy. Firstly, when we exclude 2015, the November and January half-term treatment effects are markedly smaller than for other years, though still statistically significant. Secondly, when we exclude 2018, the first half-term does show a beneficial treatment effect. This is consistent with bigger efforts being made early in the 2014/15, and to a lesser extent 2015/16 and 2016/17, academic years, to encourage take-up resulting in larger treatment effects more quickly. Third, the June treatment effect becomes progressively smaller as we exclude later years, indicating that schools have become more successful over time at *retaining* take-up of UIFSM and the benefits thereof, over the school year.

Our data does not allow us to weight schools by their size. We assess the bias that may arise from omitting to weight for school size by carrying out a simulation as follows. We use the raw, unlinked school characteristics data to predict school size as a function of the same characteristics that are available to us in our linked estimation sample, namely year, FSM-eligibility quintile, FSM take-up quintile, IDACI deprivation quintile, and whether the school was exposed to a pilot scheme for UIFSM in that year. Using this auxiliary regression, we predict the school size for the schools in our data and add error to it which we randomly draw

from the distribution of within-school and between-school error in our auxiliary regressions.¹⁴ We repeat this 10,000 times, yielding all possible distributions of school size, which we truncate at the minimum and maximum observed in the data and rounded to the nearest integer. We then re-run our difference-in-difference model (equation 1) using the predicted school size as weight, using 10,000 iterations. This yields point estimates for all possible distributions of school size. Figure 10 shows kernel density plots of the point estimates for the different half terms. Reassuringly, our main unweighted estimates, marked with a horizontal line, are very close to the most frequent estimate in all cases, sometimes marginally overstating and sometimes understating the programme's benefits relative to these simulated data. This indicates that any bias from omitting to weight by school size is very small and not systematically in one direction.

A back-of-the-envelope calculation for the overall costs and benefits of UIFSM can be conducted as follows: If exposure to UIFSM lasts three years, costing £1350 per person¹⁵, and the effects persist with 0.7 percentage point reduction in obesity and 1.2 percentage point increase in healthy weight prevalence meaning an equivalent reduction in overweight prevalence, the programme costs £191,000 per person who was not obese later in life and would have otherwise been, or £111,000 per person not overweight. It is estimated that the NHS annually spends £6.1bn on overweight and obesity-related ill-health (Public Health England, 2017). This is £377 per obese person (approximately 24% of the population, including children) or £165 per overweight-or-obese person (approximately 56% of the population including children). If such expenditure is required for every one of an expected lifespan of 80 years, the total benefit in reduced NHS costs of no longer being obese or at-least-overweight can be calculated at £30,160 or £13,200 respectively, which falls short of representing value for money. However, the overall cost to the UK economy from direct medical expenditure plus the productivity-related factors has been estimated at £60bn per year (McKinsey Global Institute, 2014, cited in Davies, 2019), or £3708 per obese person. The policy does represent value-for-money, by this metric alone, when future benefits are discounted at a rate of 1.2% per year or less.

¹⁴ We randomly draw a school-level error from the distribution of the between-school error of our auxiliary regression and a school-year error from the distribution of within-school error, and add both to the predicted school size.

¹⁵ Here 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 £175m allowed to depreciate over 10 years. The £175m figure comprises a total of £150m initially allocated through Local Education Authorities for the 2014-15 academic year; A further £15m allocated across 233 individual schools assessed as having the greatest need later that year, (when 670 schools applying for capital support but being rejected); and an additional £10m allocated, mostly through a small number of local authorities, for the 2015-16 academic year. Just over 2 million infant pupils were recorded participating in UIFSM in 2017 (Department for Education, 2017). If we allow for this equipment to be replaced after 10 years, this increases the average total cost by only £2.92 per pupil per year, to £450, or £2.37 per day.

6. Conclusion

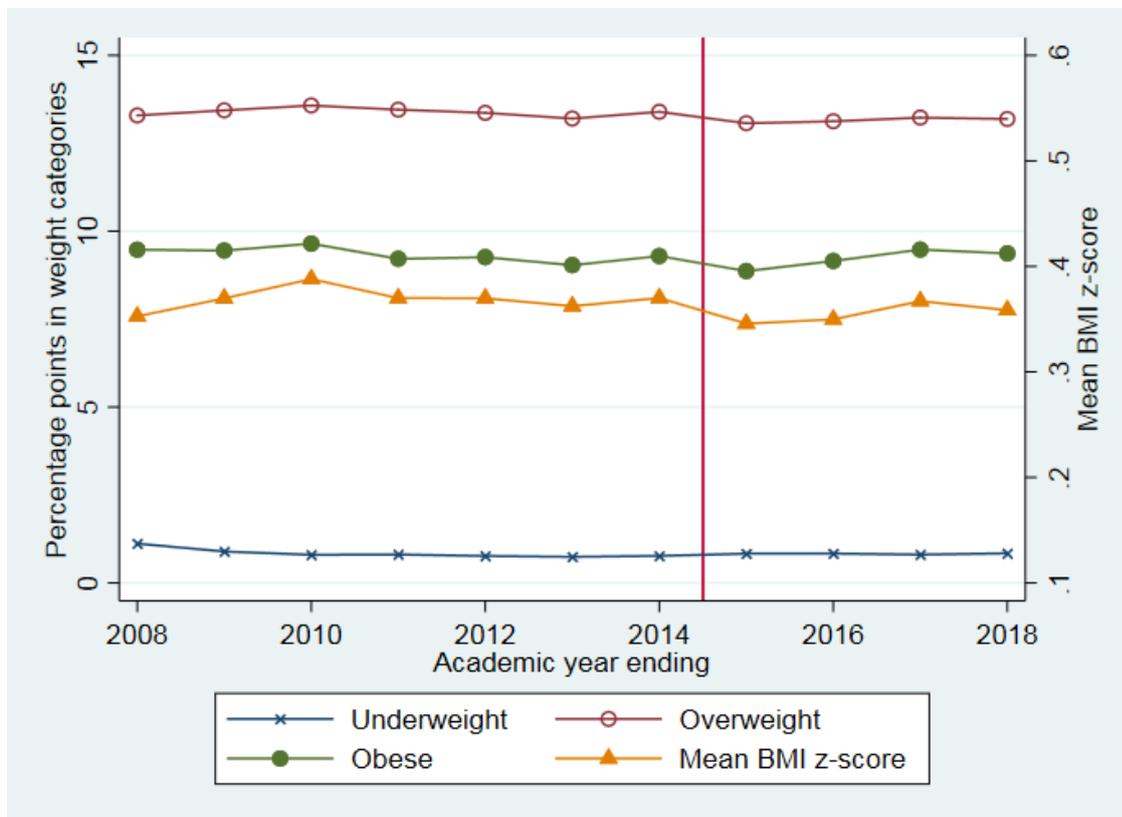
In this paper we have evaluated the effect of cumulative exposure to Universal Infant Free School Meals (UIFS) over the course of the first year of school of children in England on bodyweight outcomes. We find evidence that after the first 6-7 weeks of the school year, those exposed to UIFS have significantly better bodyweight outcomes than they otherwise would, in terms of being more likely to be healthy weight (1.2 percentage point by the end of the school year), less likely to be obese (0.7 percentage points) and have a lower BMI (4.3% 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 a universal programme, with rigorous nutritional requirements.

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 richest and poorest 20% do not benefit – presumably because their counterfactual meal similar in terms of calories provided or because the positive effects lead to worse nutrition at other times of the day.

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, UIFS appears to be cost-effective if evaluated on its bodyweight benefits alone. Further work is ongoing to evaluate other possible effects on health (measured via school absences) and school attainment.

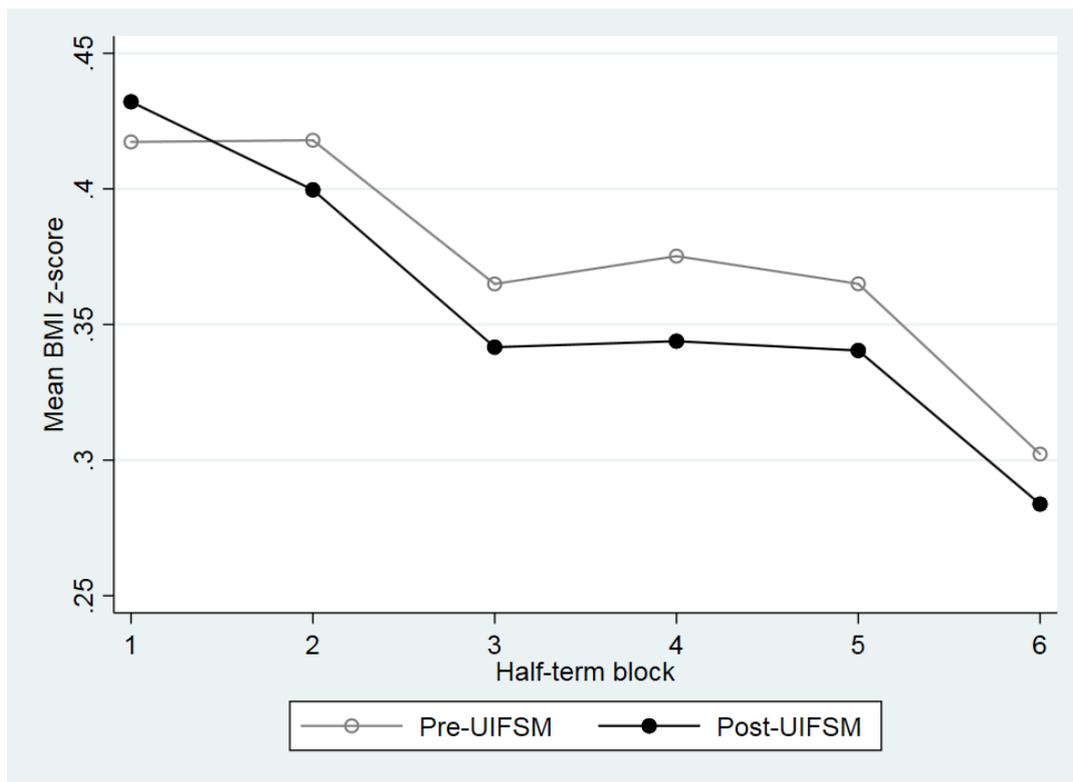
Figures and Tables:

Figure 1: Trends in mean school bodyweight outcomes across academic years



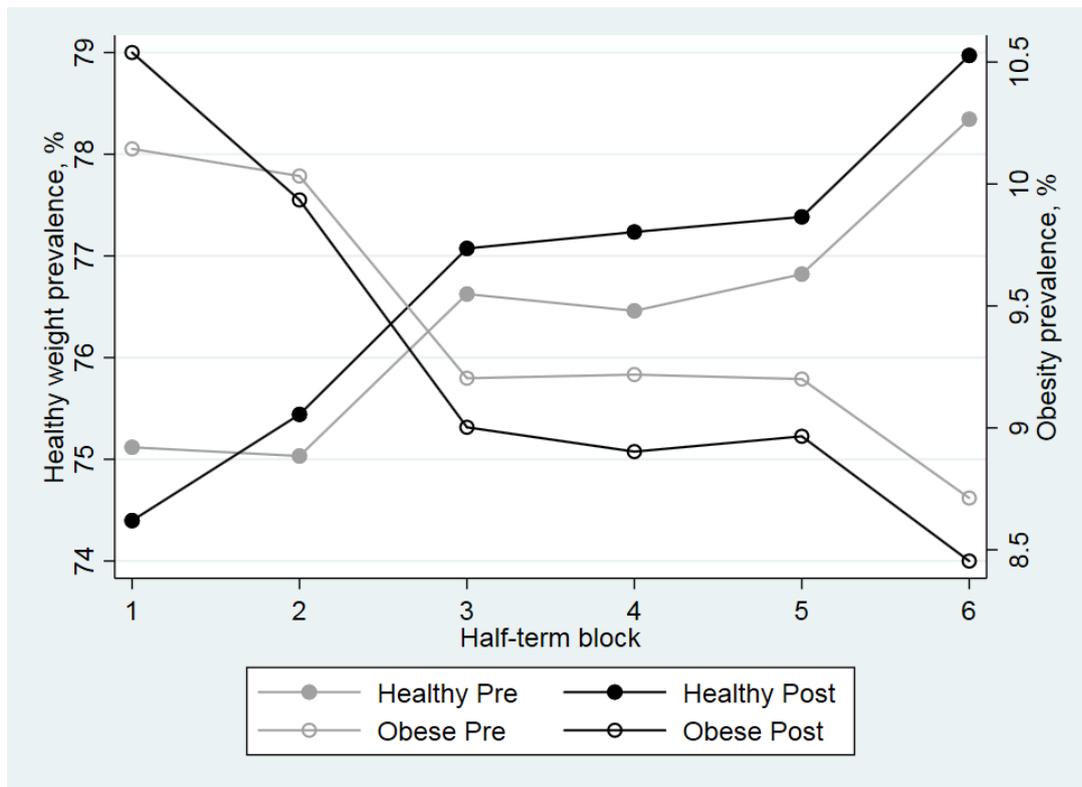
Note: Data source: National Child Measurement Programme. Unweighted means of proportions in bodyweight categories and of mean BMI z-score (accounting for age and sex of children measured) for each academic year with no additional controls.

Figure 2: Trends in BMI z-score over the school year, and pre- and post-UIFSM



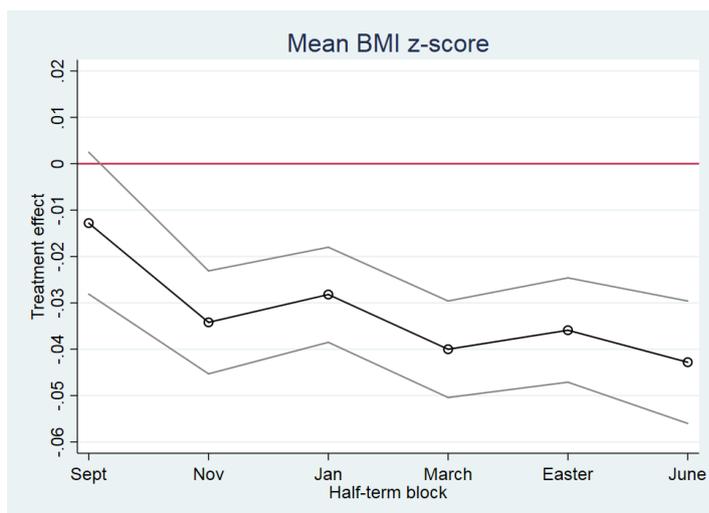
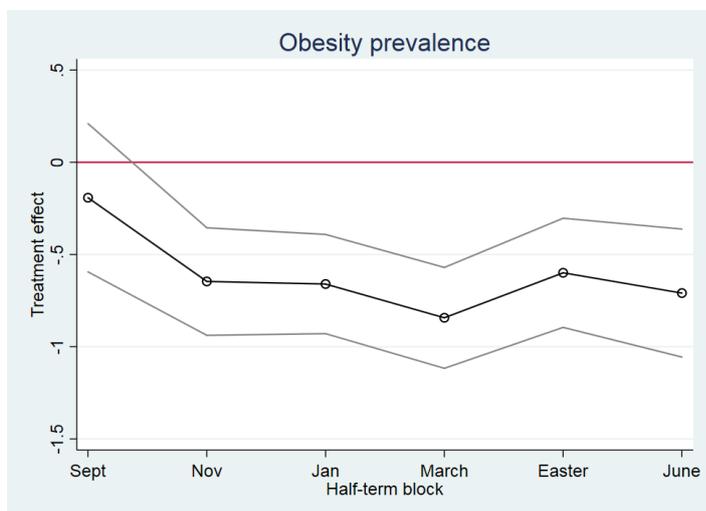
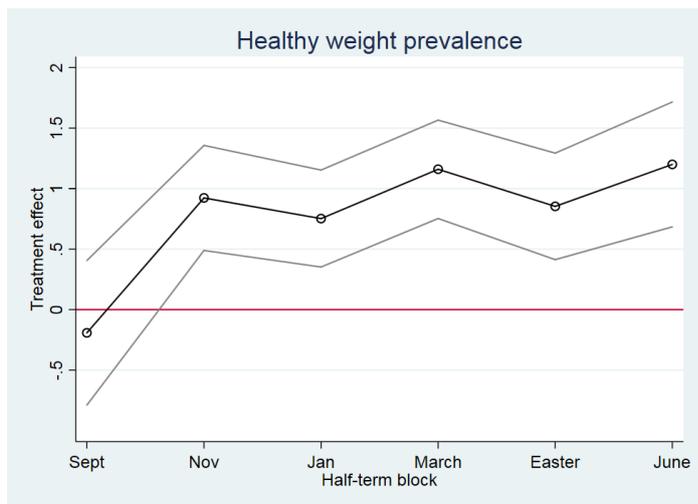
Note: Data source: National Child Measurement Programme. Unweighted means of within-school mean BMI z-score (accounting for age and sex of children measured) for schools measured in each half-term block, by pre- (academic years ending 2009-2004) and post- (academic years ending 2015-2018) UIFSM

Figure 3: Trends in healthy weight and obesity over the school year, and pre- and post-UIFSM



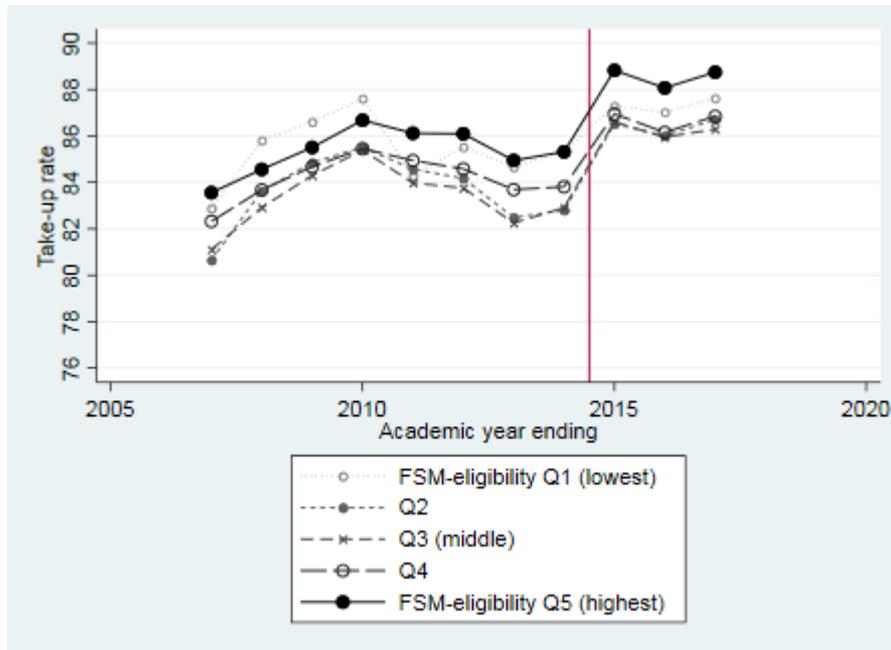
Note: Data source: National Child Measurement Programme. Unweighted means of within-school proportions healthy weight and obese (accounting for age and sex of children measured) for schools measured in each half-term block, by pre- (academic years ending 2009-2004) and post- (academic years ending 2015-2018) UIFSM

Figure 4: Treatment effects of UIFSM by half-term block



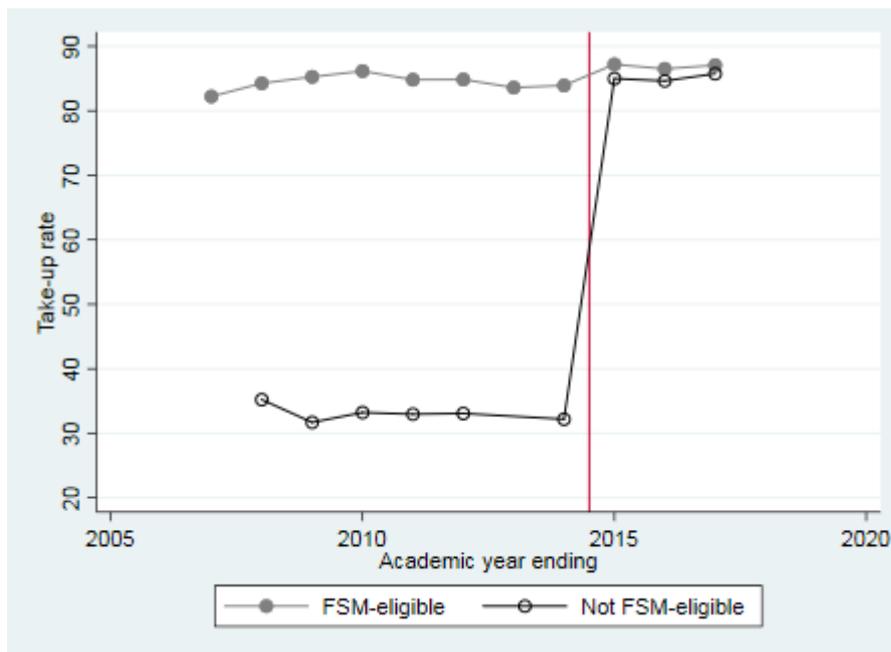
Note: Data source: National Child Measurement Programme. Estimated treatment effect of exposure to UIFSM (academic years ending 2015-2018, relative to pre-UIFSM period 2009-2014). Derived from school fixed effect regression controlling for exposure to UIFSM pilot schemes, pupil premium exposure, proportion 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.

Figure 5: Take-up of school meals among free meal eligible children, by school-level eligibility quintiles



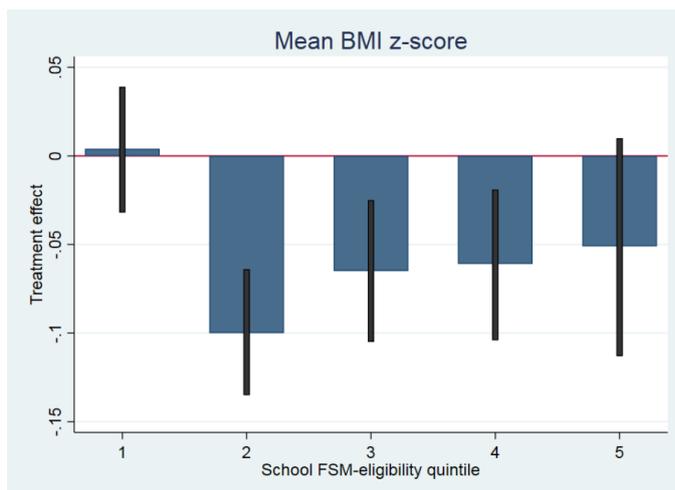
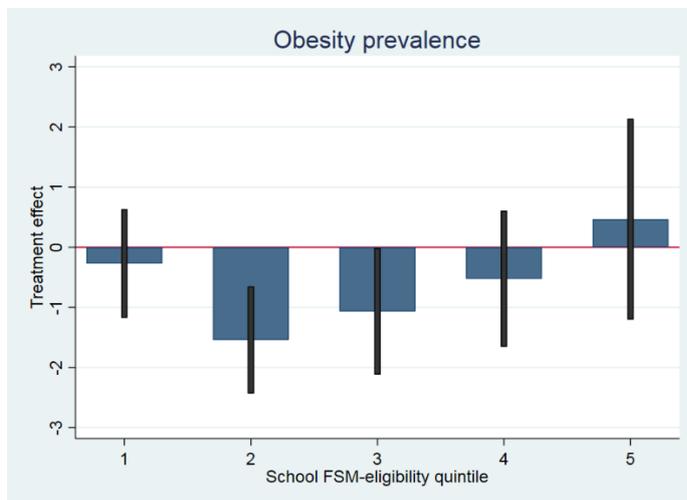
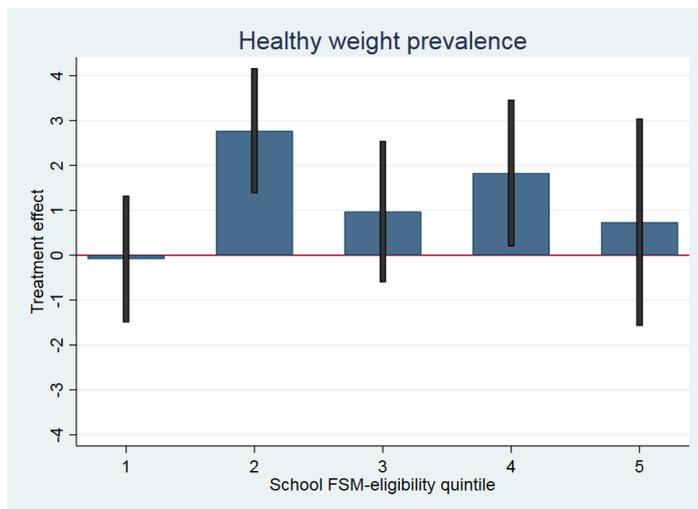
Note: Sources: Academic years ending 2007-2014: School level 'Schools, pupils and their characteristics' data issued by Department for Education, with take-up rates weighted by the number of FSM-eligible primary school aged children. 2015-2017: Spring School Census, with take-up rate equal to the proportion 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.

Figure 6: Take-up of school meals among free meal eligible and not eligible children



Note: Sources: FSM-eligible series 2007-2014 derived from 'Schools, pupils and their characteristics' and 2015-2017 from Spring School Census, See note on Figure 6. Not FSM-eligible series: 2008-2010: 'National Indicators' from the Department for Communities and Local Government; 2011- 2012: School Food Trust take-up surveys; 2014: Department for Education take-up survey; Combining these figures for overall take-up by primary-age children at the Local Education Authority 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. 2015-2017 derived from Spring School Census, with take-up rate equal to the proportion of all not-FSM-eligible infant-age pupils taking a school lunch.

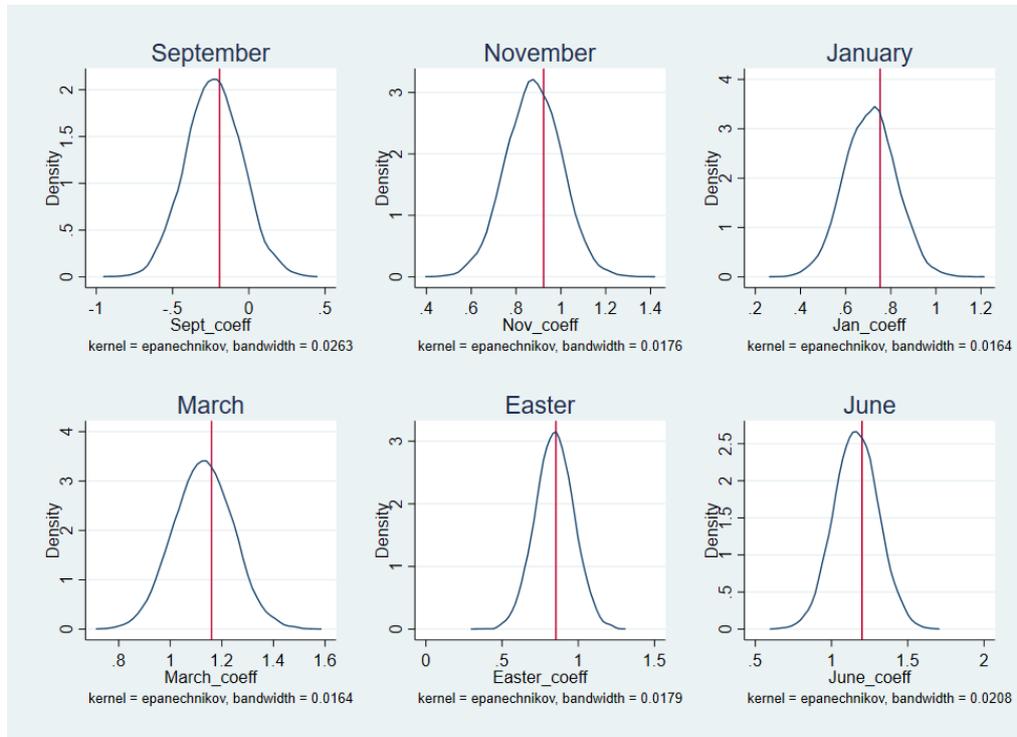
Figure 7: Treatment effects of UIFSM for June half-term block by School FSM-eligibility Quintile



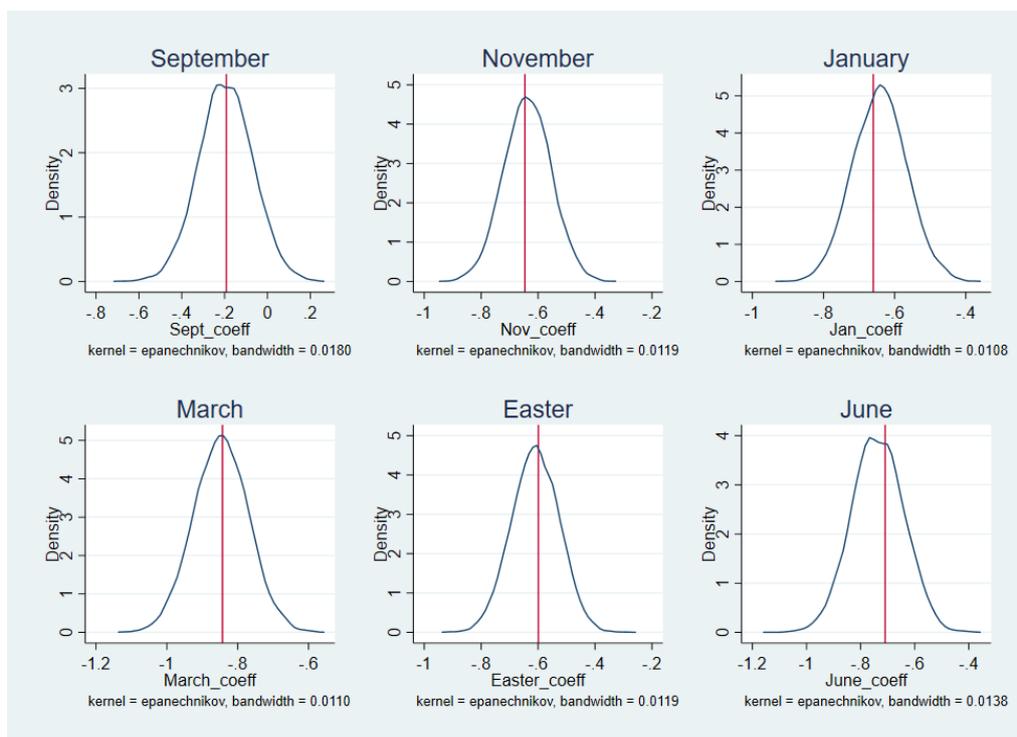
Note: Data source: National Child Measurement Programme. Estimated treatment effect of exposure to UIFSM (academic years ending 2015-2018, relative to pre-UIFSM period 2009-2014) for sixth half-term of the school year. Derived from school fixed effect regression controlling for exposure to UIFSM pilot schemes, pupil premium exposure, proportion measured black (and missing indicator), proportion measured girls, cubic year-trend interacted with IDAC1 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

Figure 8: Distribution of estimated treatment effects with observations weighted in proportion to predicted school size (10,000 simulations)

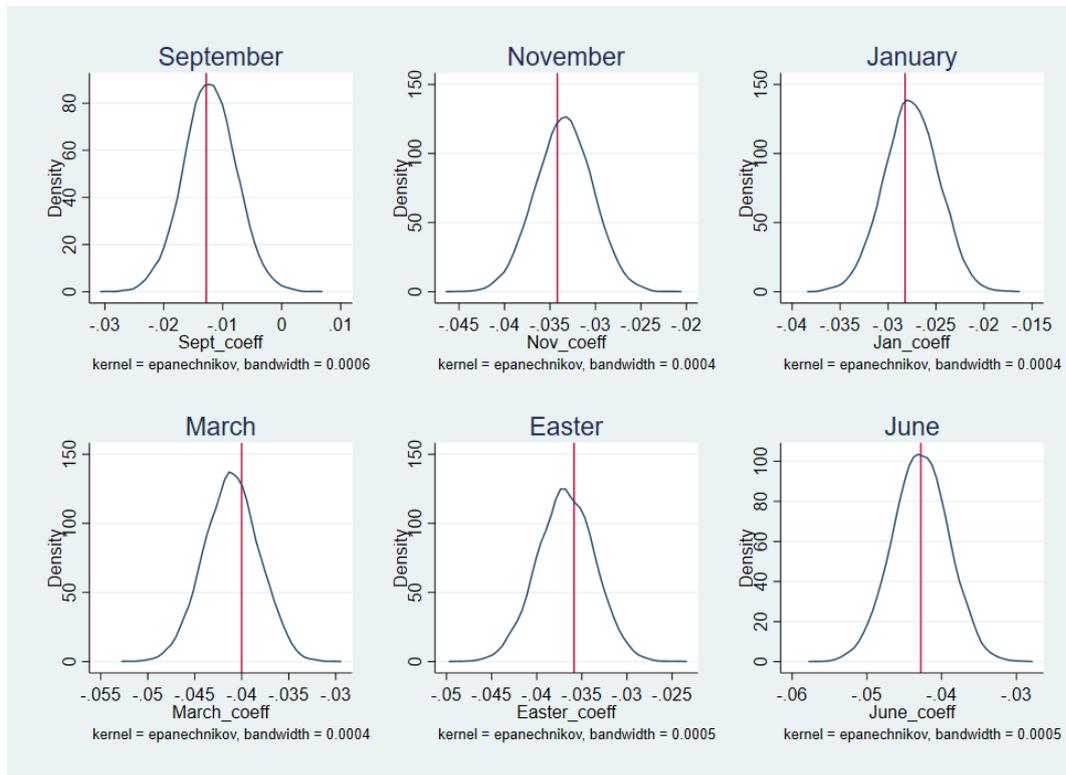
Panel A: Healthy weight prevalence



Panel B: Obesity prevalence



Panel C: BMI z-score



Note: to be completed

Table 1: Test for parallel trends in bodyweight outcomes

<i>Academic year ending</i>	BMI z-score	Healthy weight prevalence, per cent	Obesity prevalence, per cent
2009	0.001 (0.021)	0.125 (0.817)	-0.673 (0.549)
2010	-0.029 (0.020)	0.691 (0.793)	-0.646 (0.534)
2011	0.021 (0.020)	-0.327 (0.775)	0.073 (0.521)
2012	0.033 (0.021)	-0.485 (0.815)	0.911* (0.548)
2013	0.014 (0.019)	-0.414 (0.729)	-0.248 (0.491)
N observations	91,573	91,573	91,573
N schools	16,718	16,718	16,718

Notes: Source: National Child Measurement Programme. Treatment effect of school environment in 1st to 6th half-terms by academic year, pre-UIFSM period, relative to academic year-ending 2014. Treatment effects derived from exhaustive half-term block by academic year interactions in school fixed-effect regressions. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. 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, 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.

Table 2: Timing of NCMP visits by half-term block of the school year

Acad. year	Percent of visits in school year, during half-term block:						Not classifiable	N schools
	1	2	3	4	5	6		
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

Source: National Child Measurement Programme

Table 3: School characteristics by timing of NCMP visit

	School Fixed Effect Regression coefficients		
	Percent Black	Per cent girls	FSM eligibility quintile
	(1)	(2)	(3)
UIFSM	0.167*	-0.285	-0.0981***
	(0.068)	(0.183)	(0.008)
September x UIFSM	0.332**	0.425	-0.00646
	(0.117)	(0.314)	(0.014)
November x UIFSM	-0.0675	0.345	0.00920
	(0.073)	(0.198)	(0.009)
January x UIFSM	<i>Base</i>	<i>Base</i>	<i>Base</i>
March x UIFSM	0.0388	0.212	0.0209**
	(0.064)	(0.173)	(0.008)
Easter x UIFSM	-0.0599	0.211	0.0188*
	(0.074)	(0.200)	(0.009)
June x UIFSM	0.0523	-0.0904	0.00877
	(0.098)	(0.263)	(0.012)
Sample mean	3.860	48.895	2.963
Between school standard deviation	9.098	4.931	1.282
Within school standard deviation	3.667	10.420	0.489
N schools	16,876	17,127	17,085
N observations	136,300	147,975	144,374
F (Interactions are all zero)	2.58	1.09	1.98
p-value	0.0242	0.3633	0.0778

Note: Source, National Child Measurement Programme. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$, Observations with missing Percent Black or FSM eligibility quintile are excluded from these estimation samples but accounted for with missing-dummies in subsequent analysis. Derived from school fixed effect regressions with following additional controls: 6 half-term block dummies, Cubic year-trend. Standard errors in OLS regressions are clustered at the school level.

Table 4: Treatment effect of UIFSM by half term block

	Treatment effect of UIFSM by half-term block								
	Healthy weight			Obese			Mean BMI z-score		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
$\tau_u(\text{UIFSM}) + \tau_h(\text{Block} \times \text{UIFSM})$									
September	-0.515*	-0.270	-0.192	0.128	-0.148	-0.192	0.001	-0.010	-0.013
	(0.273)	(0.299)	(0.305)	(0.184)	(0.201)	(0.205)	(0.007)	(0.008)	(0.008)
November	0.625***	0.868***	0.923***	-0.375***	-0.643***	-0.646***	-0.022***	-0.032***	-0.034***
	(0.176)	(0.214)	(0.221)	(0.119)	(0.144)	(0.149)	(0.005)	(0.005)	(0.006)
January	0.466***	0.706***	0.752***	-0.389***	-0.652***	-0.660***	-0.016***	-0.026***	-0.028***
	(0.155)	(0.196)	(0.204)	(0.104)	(0.132)	(0.137)	(0.004)	(0.005)	(0.005)
March	0.894***	1.121***	1.160***	-0.577***	-0.834***	-0.843***	-0.028***	-0.038***	-0.040***
	(0.159)	(0.199)	(0.208)	(0.107)	(0.134)	(0.140)	(0.004)	(0.005)	(0.005)
Easter	0.568***	0.792***	0.853***	-0.324***	-0.577***	-0.599***	-0.023***	-0.033***	-0.036***
	(0.180)	(0.216)	(0.224)	(0.121)	(0.145)	(0.151)	(0.005)	(0.006)	(0.006)
June	0.905***	1.145***	1.200***	-0.425***	-0.686***	-0.709***	-0.030***	-0.040***	-0.043***
	(0.227)	(0.256)	(0.263)	(0.152)	(0.172)	(0.177)	(0.006)	(0.007)	(0.007)
Pilot x half-term dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cubic year trend	x	Yes	Yes	x	Yes	Yes	x	Yes	Yes
Demographics in levels	x	Yes	Yes	x	Yes	Yes	x	Yes	Yes
IDACI quintile, proportion black, and proportions girls cubic year-trends	x	x	Yes	x	x	Yes	x	x	Yes
Demeaned proportion black and proportions girls interacted with half-term block	x	x	Yes	x	x	Yes	x	x	Yes
N data points	154,169	154,169	154,169	154,169	154,169	154,169	154,169	154,169	154,169
N schools	17,776	17,776	17,776	17,776	17,776	17,776	17,776	17,776	17,776
Pre-treatment: dependent-variable mean:		76.368			9.411			0.373	
... between school standard deviation:		6.312			4.209			0.178	
... within school standard deviation:		8.689			5.689			0.224	

Notes: Source: National Child Measurement Programme. Treatment effects derived from exhaustive half-term block by academic year interactions in school fixed-effect regressions. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. Standard errors in parentheses.

Table 5: Sensitivity of mean BMI z-score treatment effects to exclusion of individual years

	(1) Excluding 2015	(2) Excluding 2016	(3) Excluding 2017	(4) Excluding 2018
September	-0.007 (0.009)	-0.007 (0.008)	-0.008 (0.008)	-0.017** (0.008)
November	-0.024*** (0.007)	-0.032*** (0.006)	-0.033*** (0.006)	-0.036*** (0.006)
January	-0.023*** (0.007)	-0.030*** (0.005)	-0.024*** (0.005)	-0.025*** (0.005)
March	-0.038*** (0.007)	-0.038*** (0.006)	-0.035*** (0.006)	-0.040*** (0.006)
Easter	-0.035*** (0.007)	-0.035*** (0.006)	-0.032*** (0.006)	-0.032*** (0.006)
June	-0.049*** (0.008)	-0.042*** (0.007)	-0.040*** (0.007)	-0.031*** (0.007)
Observations	138,626	138,462	138,456	138,536
N schools	17,773	17,773	17,769	17,563

Notes: Source: National Child Measurement Programme. Treatment effects derived from exhaustive half-term block by academic year interactions in school fixed-effect regressions. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. Standard errors in parentheses. Additional controls as in specifications (3) of Table 3

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