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Does the Provision of Universal Free School Meals Improve School Attendance and Behaviour?*

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Abstract

The importance of universal free school meals (UFSM) provision has been the subject of significant debate over the past decade. In this study we examine the effect of UFSM policies on school attendance, health-related absence and students' misbehaviour. We leverage UFSM implementation in Scotland where all pupils in the first three grades of primary schools became automatically entitled to claim free meals, regardless of their households' financial circumstances. We estimate a difference-in-differences model with variation in treatment intensity and find, in spite of a large increase in uptakes, that attendance and school discipline have not improved significantly. These estimates are close to zero and precisely estimated. We also show that effect heterogeneity does not explain the null effect.

Keywords: Attendance, Behaviour, School Meals, Welfare

JEL Codes: J13, I18, I28, H51, H52

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1 Introduction

Free school meals (henceforth, FSM) have recently been at the centre of a heated debate following disruption to their provision caused by Covid-19 school closures. This affected approximately 300 million children around the world and 1.6 million in the UK. For most of these children, school meals represent a crucial dietary component.[1] In recent years, many high-income countries have moved from means-tested programmes, in which eligibility is contingent on households' financial circumstances, to school-wide or even school system-wide universal provision. For instance, in 2014 the US launched the Community Eligibility Provision (CEP), which subsidises schools with at least 40% of pupils eligible for FSM to extend the provision to all their students. Around the same time, nations of the United Kingdom implemented similar policies, following a series of pilot schemes. England launched the Universal Infant Free School Meals (UIFSM) programme for all primary school children in year 1 and 2 in September 2014, followed by Scotland in January 2015, where eligibility was extended to pupils in the first three years of primary school.

The motivation for these policies are deep-rooted. In Sorhaindo and Feinstein (2006)'s review the authors identify four channels explaining the link between a healthy diet and school outcomes. First, deficiency of nutrients, such as iron, could affect cognitive development in children.[2] Second, a healthy diet can reduce the risk of illnesses and thus school absences. The third and fourth channels are behaviour and school life. Evidence from the medical literature suggest that a healthy diet should include nutrients which can reduce the risk of violent and anti-social behaviour as well as hyperactivity (Benton, 2007). In addition, FSM uptake can be associated with stigma and consequently victimization, both of which may be mitigated by making provision universal.

A broad literature documents the beneficial effects of school meal programmes (whether lunch or breakfast) on a variety of outcomes, and finds that these are indeed linked to gains in academic performance (Belot and James, 2011; Chakraborty and Jayaraman, 2019; Schwartz and Rothbart, 2020; Gordanier et al., 2020; Ruffini, 2021), reduction in body-mass index (Holford and Rabe, 2022), improvements in labour market outcomes (Bütikofer et al., 2018; Lundborg et al., 2021), but also households' finances

[1] See the contribution to the Economics Observatory, Link: https://www.economicsobservatory.com/how-coronavirus-affecting-provision-free-school-meals

[2] There is extensive evidence in the economic literature on the positive effect of nutrition on academic attainment (see e.g. Glewwe et al., 2001; Winicki and Jemison, 2003; Alderman et al., 2006; Victora et al., 2008).

and nutrition (Bhattacharya et al., 2006; Handbury and Moshary, 2021; Marcus and Yewell, 2021; Ozturk et al., 2021).[3]

Nonetheless, FSM implementation continues to be a debated topic, both among policy-makers and academics. Whilst some argue that FSM programmes do not improve children's health (Corcoran et al., 2016) and can even raise the risk of unhealthy body weight (Schanzenbach, 2009; Polonsky et al., 2019; Abouk and Adams, 2022) the core of the debate relates to the trade-off between the cost of their universal implementation, and their effectiveness when they are means-tested. In fact, stigma attached to FSM status might be an explanation for the imperfect uptake rates observed even for means-tested provision. In such a case, FSM expansion can raise take-up not just among previously ineligible students but also among previously eligible students (Leos-Urbel et al., 2013; Holford, 2015; Corcoran et al., 2016; Ruffini, 2021).

In spite of the emphasis that has been put on the link between FSM and academic achievement, the evidence remains rather mixed and mostly confined to short-term outcomes. Existing studies largely overlooked the effect that school meals programmes have on students' outcomes which are more likely to shape their cognitive and non-cognitive abilities in the long-run. To fill this gap in the literature we look at the effect of Scotland's Universal Free School Meals (UFSM) programme on an array of student behaviours, such as attendance, detentions as well as health-related absences. The intervention extended the eligibility for free lunches to *all* pupils in the first three (P1, P2 and P3) of the seven grades of primary school, regardless of their families' financial circumstances.

For our analysis we use a panel of primary schools over more than ten years. We observe the percentage of pupils registered for free meals, alongside take-up rates, both among previously and newly eligible pupils. In our setup all primary schools in the nation were 'treated' at once, thus we implement a difference-in-differences estimation strategy with variation in the intensity of treatment in the same fashion as, for example, Card (1992) and Clemens et al. (2018). In particular, we leverage different levels of exposure to the policy, as determined by variation in the fraction of the school population taking FSM prior to the change in policy. We find that schools with a higher fraction of non-FSM takers (high exposure to the policy) experienced an increase in uptake of about 10 percentage points more than their lower exposure counterparts. We matched these schools to their attendance and absence records, alongside a rich set of

[3]In addition, see Cohen et al. (2021) for a systematic review of the literature on the link between universal free school meals and student performance, attendance, diet quality and body mass index.

school characteristics.

Our study provides two main contribution to the literature and in turn the policy debate. First, Scotland is a nation with a high prevalence of childhood obesity. In 2016, approximately 29% of children in Scotland were at risk of becoming overweight, and about 14% were at risk of obesity (Public Health Scotland, 2021).[4] These figures are in line with the rest of the UK, and with countries like Cyprus, Greece, Italy and Spain, where between 18% and 21% of boys are obese, but far above Denmark, France, Ireland, Latvia and Norway where these figures amount to up to 9% (Organization et al., 2018). In fact, this policy is the culmination of a wider range of interventions and reforms aimed at regulating food standards in schools as well as investing schools with the role of health educators, on the same basis as mathematics and languages. Parnham et al. (2022) find that UFSM led to some improvement in pupils' diets.

Second, the outcomes we are interested in are strong correlates of the 'Big Five' personality traits and their related traits, and thus of non-cognitive skills, which have been proven to be powerful predictors of adult life outcomes (see Heckman and Rubinstein, 2001; Chetty et al., 2011; Lindqvist and Vestman, 2011) even if not captured by test-scores (Jackson, 2018).[5] For instance, absenteeism is linked to worse academic attainment (Gottfried, 2009, 2011; Aucejo and Romano, 2016; Gershenson et al., 2017; Liu et al., 2021; Klein et al., 2022).

Alongside better nutrition, UFSM can lead to reduced absenteeism as parents are incentivised to send their kids to school more often than they would absent the policy, or by reducing the risk of stigma associated with FSM uptake, thereby making school a more attractive prospect for pupils. To date, there is limited and mixed evidence on the effect of these programmes on absenteeism, with most studies pointing toward small or even null effects (Corcoran et al., 2016; Gordanier et al., 2020; Cuadros-Meñaca et al., 2022; Holford and Rabe, 2020). On the other hand, Belot and James (2011) find a 14% decrease in authorised absences, which are mostly driven by illnesses, in response to an increase in school meals quality. Therefore, unlike most of the previous studies which cannot distinguish absences by type, we are able to observe the reason of each absence, and focus on health-related absenteeism, and unlike Belot and James

[4]The likelihood of being at risk of obesity is based on the classification of obesity risk categories for children by Public Health Scotland. This classification is determined by children's BMI values as compared to reference values based on the expected 'healthy' BMI for each age and gender group. Each children's health status is then based on the probability of having a BMI lower (or higher) than the reference value. Since this comparison includes a probabilistic component, the health category is framed as a risk ('at risk of obesity' for children), as opposed to the discrete category of 'obese' for adults. More detail on these classifications can be found in Public Health Scotland (2021).

[5]Agreeableness, conscientiousness, neuroticism are associated with absences, tardiness and anti-social behaviour (see for example, John et al., 1994; Barbaranelli et al., 2003; Carneiro et al., 2007; Duckworth et al., 2007; Lleras, 2008; Jackson, 2018).

(2011), we do this in an universal FSM scenario.

Finally, our study relates to school misbehaviour, the importance of which has been studied extensively in relation to mental health outcomes (see e.g. Bowes et al., 2015; Kim and Leventhal, 2008; Hertz et al., 2013), lower educational gains and peers' misbehaviour (Figlio, 2007; Carrell and Hoekstra, 2010; Ammermueller, 2012; Ponzo, 2013; Eriksen et al., 2014; Ahn and Trogdon, 2017) in addition to lower schooling and future earnings (Brown and Taylor, 2008; Ammermueller, 2012; Carrell et al., 2018). However, there is still little evidence on how FSM policies might mitigate these issues (Altindag et al., 2020; Gordon and Ruffini, 2021; Cuadros-Meñaca et al., 2021).

Our analysis might help to reconcile results from different studies, by bringing evidence from a new country. In particular, our work is closely related to Gordon and Ruffini (2021) and Altindag et al. (2020) but it departs from these in two significant ways. First, we are able to observe actual take-up rates. This is a substantial addition with respect to most of the literature which instead measures an 'intention-to-treat' (ITT) effect (see e.g. Belot and James, 2011; Chakraborty and Jayaraman, 2019; Gordanier et al., 2020; Gordon and Ruffini, 2021), where the predictor of interest is just eligibility for a programme. Similar to us, Altindag et al. (2020) use variation in take-up rates as surveyed on one day of the school year. By focusing on a variety of outcomes beyond infractions we are able to provide a more comprehensive exploration of the potential effects of FSM policies on non-cognitive skills.

We present three main results. First, UFSM had only minimal impact on attendance and health-related absences. In particular, a 10 percentage points increase in FSM uptake translates into less than one school day gained. These results are precisely estimated. Second, we find no evidence that the policy improved student behavior. Third, the effect for attendance and health-related absences is slightly stronger within smaller schools and those with more resources per pupil, suggesting that uptake is strongly influenced by how efficiently the policy was rolled out.

The remainder of the paper is structured as follows. Section 2 discusses the institutional background and UFSM implementation; Section 3 describes the data; Section 4 outlines the estimation strategy; Section 5 presents the results. Finally, Section 6 contains our concluding remarks.

2 Institutional Background

Over the last couple of decades, a series of reforms aimed at encouraging healthy eating habits in schools have taken place in Scotland. In particular, the launch of *Hungry for Success: A Whole School Approach to School Meals in Scotland (2003)* set national nutritional standards for school lunches. These endeavours have been subsequently formalised within the Schools (Health Promotion and Nutrition) Scotland Act (2007), which set out the responsibilities and duties of schools and local authorities in terms of health education and the administration of schools meals, and the Nutritional Requirements for Food and Drink in Schools (Scotland) Regulations (2008), which aligned the nutritional standards of all food and drink in schools with the Scottish Government's dietary goals for the population.

The first experience of FSM universality dates back to the school year 2007/08, when UFSM was piloted among P1-P3 pupils in five local authorities, namely East Ayrshire, Fife, Glasgow, Scottish Borders and West Dunbartonshire. These regions were chosen to cover a wide portion of the nation and also based on deprivation (MacLardie et al., 2008). As Holford (2015) documents, the trial was announced in the Summer 2007 to be launched in the following October, setting March 2008 as the initial deadline. It was subsequently extended until June, meaning the trial would ultimately cover the entire academic year. Prior to the launch of the trial, FSM entitlement was means-tested.[6] Thus, pupils eligible under the national criteria were able to take free school meals only upon registration, which is completed by parents by providing proof of eligibility to their local authority (Holford, 2015). During the pilot trial, on the other hand, all pupils were eligible and automatically registered for free school meals regardless of their household's financial circumstances.

Starting in August 2010, local authorities launched a series of local initiatives aimed at increasing eligibility among P1-P3 pupils. The goal was to promote healthy eating by stimulating take-up among pupils who would not otherwise be entitled. A 2011 report by the Scottish Government shows that free school meals registration increased by 10.3%, whereas the overall take-up (free or paid-for meals) increased only slightly.[7]

There are some important distinctions to be made at this point. Prior to UFSM implementation, eligi-

[6]See https://www.gov.scot/publications/school-healthy-living-survey-statistics-2020/pages/4/, alongside Section 6 for more details.

[7]For more details, see https://www.webarchive.org.uk/wayback/archive/20180518073257mp_/http://www.gov.scot/Resource/Doc/920/0119410.pdf

bility alone, whether due to households financial circumstances or local initiatives, did not automatically entitle students to claim a free school meal from the school canteen. This was still contingent on registration. Similarly, registration did not guarantee that students were indeed taking free school meals every day.

The launch of Better Eating, Better Learning (2014) set out the government intention to make healthy eating habits a pillar of education in Scotland. It was paired with the introduction of the new national 'Curriculum for Excellence', which includes Health and Wellbeing as one of the eight curricular areas, alongside, for instance, mathematics, languages and sciences. Starting from January 2015, all P1-P3 pupils in Scotland would become eligible and were automatically registered for free school meals. The policy, which entailed £70.5 million funding from the Scottish Government to local authorities over the following two years, was estimated to provide households with financial savings of approximately £380 per child per year while also provide nutritional benefit to children (Beaton et al. (2014) and McAdams (2016)) A clearly declared goal of the policy was to reduce health inequalities. According to official statistics, the number of FSM registrations in primary schools increased by 135,408 compared to the previous year, to a total of 213,199 pupils. This corresponds to 55.3% of the primary school population, compared with 20.6% in 2014, and is nearly entirely attributable to the change in policy. In fact, this roughly corresponds to the number of FSM-unregistered P1-P3 pupils the year before the policy change.[8] In terms of uptake, the fraction of pupils taking a meal (free or paid for) increased from 53.2% to 64.6% in 2015, and to 78.9% among P1-P3 pupils. In addition, in 2016 approximately 66% of primary pupils were taking school meals, with the P1-P3 fraction increasing to 81.7%. On the other hand, the share of P4-P7 taking school meals remained fairly stable around 53% (McAdams, 2016). The policy seemed to have achieved at least some of the initial goals. For instance, a qualitative evaluation (Ford et al., 2015) found that parents identified three main benefits: financial savings, time savings from not having to pack lunches, and school meals being healthier.

[8] National Statistics (2015). If we assume that the fraction of FSM-registered pupils is roughly unchanged between P1-P3 and P4-P7, a back of the envelope calculation based on the formula $P(FSM) = P(FSM|P1-P3) \times P(P1-P3) + P(FSM) = P(FSM|P4-P7) \times P(P4-P7)$, where in school year 2013/2014 about 45% of the primary school population was in P1-P3 and 20.6% of the school population was FSM-registered, suggests that nearly 21% of P1-P3 pupils were FSM-registered in 2014. This means $169,485 \times (1-.21) \approx 133,893$ is the number of P1-P3 pupils who were not FSM-registered one year before the policy.

3 Data

3.1 Healthy Living Survey

The main data for this project come from the School Meal Survey, renamed Healthy Living Survey (HLS) in 2012. The survey takes place in February every year. For every school, this collects the following information: *i)* number of pupils on the school roll; *ii)* number of pupils present on the day of the survey; *iii)* number of pupils registered for FSM; *iv)* number of pupils present and registered for FSM; *v)* number of pupils present and who took a school meal, whether free or paid-for; *vi)* number of pupils present and who took a FSM. Additional information are collected for a subset of waves only. For example, until 2009 the survey would also report the number of pupils who are eligible to receive FSM under the national criteria, and until the following wave the survey would include information on whether the school: *i)* had an anonymised payment system for school meal collection; *ii)* provided pupils with fresh fruit and water; *iii)* had a breakfast club.[9]

Therefore, for each wave it is possible to calculate FSM, paid-for school meals (PSM) and overall school meals uptakes. Holford (2015) presents the overall uptake in the following way. Given school s, ρ_s represents the fraction of pupils registered for free school meals, while y_s^f and y_s^p are respectively FSM and PSM uptakes. Hence, the overall uptake can be outlined as the weighted sum of FSM and PSM uptakes, whose weights are the fractions of registered/unregistered pupils:

$$\begin{aligned}\underbrace{\frac{\#Meal-Takers}{School\ Population}}_{\text{Overall Uptake, } Y_s} = & \overbrace{\frac{\#FSM-Registered}{School\ Population}}^{\rho_s} \times \overbrace{\frac{\#FSM-takers}{\#FSM-Registered}}^{y_s^f} + \\ & + \underbrace{\frac{\#notFSM-Registered}{School\ Population}}_{1-\rho_s} \times \underbrace{\frac{\#PSM-takers}{\#notFSM-Registered}}_{y_s^p}\end{aligned} \tag{1}$$

We calculate all the elements of Equation 1 from the information provided in the HLS. For instance, of all pupils present on the survey day, y_s^f is calculated as the fraction of FSM-registered pupils who took

[9]Not surprisingly, the correlation between the fraction of pupils who are eligible and those registered is nearly 1.

a (free) school meal. In addition, we calculate the uptake of paid-for meals as a ratio whose numerator is the difference between the number of (present) pupils taking any meal minus the number of pupils specifically taking FSM, while its denominator is the number of pupils non-registered for FSM, which in our data is just the number of pupils present minus those present and registered. From here, the overall uptake can be calculated in accordance with Equation 1 or simply as the ratio between pupils taking any meal and the number of pupils present.

Figure 1 provides trends in each of the terms outlined in Equation 1. We can see how registrations start increasing after 2009, following a series of local initiatives to expand eligibility. After plateauing from 2011, we then observe a sharp hike by about 35 percentage points in 2015. Take-up of paid meals only increased by a couple of percentage points in 2015, following an upward trend started prior to 2010. It should be noted that from 2015 on, it was exclusively P4 to P7 students who were paying for school meals, as pupils in those grade were not covered by the UFSM policy. Uptakes of free school meals dropped remarkably following the introduction of their universality in 2015. This happened for two reasons. First, all P1-P3 students were automatically registered from 2015, thus expanding y_s^f's denominator. Second, the newly eligible group had a lower take-up rate than the existing eligible group.

This is also an important departure from Holford (2015)'s evaluation of the pilot. His identifying assumption relied on the fact that the policy would only affect Y_s through y_s^p due to the reduction in price, which would in turn leave y_s^f unaffected. But importantly, due to the way the pilot was implemented, the share of FSM-registered pupils was not impacted. This obviously does not apply to the UFSM set up, which automatically registered all P1-P3 pupils. This explains the apparent drop in FSM uptake. As the goal of this analysis is to examine how policy-induced variation in the fraction of school population taking FSM affected school-level behaviour, we therefore consider a slightly different measure of FSM uptakes. This can be seen after re-arranging Equation 1:

$$\underbrace{\frac{\#Meal - Takers}{School\ Population}}_{\text{Overall Uptake, } U_s} = \overbrace{\frac{\#FSM - takers}{School\ Population}}^{u_s^f} + \underbrace{\frac{\#PSM - takers}{School\ Population}}_{u_s^p} \tag{2}$$

Whereby the overall uptake is the sum of FSM and PSM takers divided by the number of pupils in school.[10]

[10]Due to the survey taking place on a single day, this is not the actual school roll but the number of pupils present on the

Equation 2 is equivalent to Equation 1 after cross-deleting registration rates. Figure 2 shows the re-defined measures of FSM take-up rate, defined as the % of pupils present on the survey day who took a free meal (solid line) and who took a paid-for meal (dashed line). Once we do not account for FSM registration rates, we observe the expected pattern. The share of pupils taking FSM - which is the focus of this work - increased by approximately 25 percentage points following the implementation of UFSM. Conversely, the fraction of those paying for their meals went down by about 14 percentage points.

It is worth reiterating that the policy targets pupils in the first three stages of primary school (P1-P3). While we cannot observe a breakdown of registrations and uptakes by stages throughout our sample period, official statistics from 2015 report this information at the national level. These are plotted in Figure 3a. The dotted black line represents the entire primary school-level FSM registration trend as in Figure 1. The solid red line disaggregates the trend for the P1-P3 group (targeted by the policy) whereas the solid black line shows the P4-P7 trend. Starting in 2015, all P1-P3 pupils become automatically FSM-registered (horizontal line) whereas P4-P7-level registrations roughly maintain the overall pre-policy trend, yet with a slight downturn. Figure 3b follows the same approach, but it plots the percentage of primary school population taking free school meals, split by stages.[11] Under the plausible assumption that uptakes and registrations did not differ substantially across grades, i.e. the fractions of FSM-registered (and/or FSM-takers) are roughly the same within P1-P3 and P4-P7 cohorts, we can see that following the change in policy, registrations and uptakes only changed significantly within the P1-P3 group.[12]

3.2 School Information

Data on school characteristics are taken from the 'school contact details' database, alongside the Scottish Pupils Census (SPC), which runs shortly after the beginning of every school year. Hence, SPC and HLS which pertain to the same school year are identified from subsequent waves, as they take place in two different calendar years. From these data sources we are able to obtain information such as: *i)* school post-

survey day. However, the latter is a good proxy for the former, with a correlation coefficient above 0.9.

[11]The official statistics only report the percentage of FSM-registered pupils who took a meal on survey day, i.e. y_s^f from Equation 1. Therefore, by multiplying this by the P1-P3 shares of FSM-registered pupils we obtain our P1-P3 uptake measure as in Figure 2. The same approach is applied to obtain P4-P7 uptakes.

[12]For example, the registration rate can be decomposed as $P(FSM) = P(FSM|P1-P3) \times P(P1-P3) + P(FSM) = P(FSM|P4-P7) \times P(P4-P7)$. As $P(P1-P3) \approx P(P4-P7)$, then $P(FSM|P1-P3)$ and $P(FSM|P4-P7)$ must not diverge excessively.

code, which can be linked to a variety of neighbourhood characteristics; *ii)* number of pupils in school; *iii)* FTE number of teachers; *iv)* fraction of pupils from ethnic minorities; *v)* breakdown of pupil numbers by stage; *iv)* average class size and fraction of composite classes. We use the Scottish Index of Multiple Deprivation (SIMD) of the school location as a proxy for school composition. SIMD is an index developed based on seven domains, i.e. income, employment, health, education, crime, housing and access to services. This information is collected at the data zone level. A data zone is a block containing between 500 to 1,000 people and currently Scotland is divided into 6,976 data zones.

3.3 Attendance and Absence Survey

Data on attendance, absences and exclusions have been consistently collected every school year until 2010/11. Thereafter, the survey took place every second year with 2018/19 being the latest wave. Appendix Table B1 provides an overview of when and how the survey was carried out. It follows the same wave structure as the SPC. For instance, wave 2010 includes attendance and absences which occurred in school year 2010/2011. HLS waves instead refer to February of the same school year, hence the calendar year following the year when the school year begins.

For a given school in a given year, the survey collects the total number of episodes of attendance, authorised absence, unauthorised absence and exclusions, broken down by reason. These refer to all stages in school. For instance, days spent in school, whether arriving on time or slightly late, days of work experience and instances of educational provision during illness all count toward attendance. Table 1 reports summary statistics for the period 2003-2018. In panels A to C we calculate the incidence of each episode within their own category. It can be seen that attendance is mainly composed of “in school” attendance (99.14%) with a residual part being mostly due to students being late and studying whilst sick.[13] From Panel B we see that about 67% of authorised absences are health-related, followed by nearly 27% of the episodes being attributed to “other” authorised absences. In addition, unauthorised absence are mostly due to holidays and ‘only’ about 30% are due to truancies.[14] Panel D shows that approximately 2.5% of all school sessions are missed due to illness and even lower shares are attributable to lateness and truancy. Moreover, exclusions are rare in primary schools (.02%). Unfortunately, we are not able to

[13]This most likely entails chronic conditions forcing pupils staying away from school.

[14]The same information is collected for secondary schools, for which we observe different patterns. For instance, truancies tend to be the main reason for unauthorised absences and episodes of work experience features more often.

observe the type of misbehaviour which lead to a disciplinary action. Rates in panels D and E are calculated in relation to the number of all possible attendances, which for each school corresponds to the roll times the total number of half-day openings.[15]

Whilst we would like to look at the effect of UFSM on each of these sub-categories individually, we face some missing data issues, primarily related to statistical disclosure control (SDC) protocols which suppress small cells based on less than 5 instances. For instance, if in wave 2014, the cell for school A for "very late" is suppressed, the entire authorised absence rate cannot be calculated for that school in that wave. This resulted in a large number of missing values within the authorised and unauthorised absences categories. For this reason we focus on three outcomes with very few instances of SDC suppression, i.e. attendance rate, health-related absences (sickness rate from panel D) and exclusion rates.

3.4 Analytical Sample

Our final sample spans from school year 2003/2004 through to 2016/2017. Within this interval, we observe school meals uptake and registration for every year. However, due to the fact that the attendance survey was collected every second year from school year 2010/2011, we experience some gaps in the outcome variables, specifically for school years 2011/2012, 2013/2014 and 2015/2016. Whilst wave 2018/2019 is in principle available to us, we decided not to include it in our analysis for two reasons. First, in contains several missing values. Second, in August 2018 Glasgow City Council extended the program to fourth graders, therefore we want to avoid an overlapping of policies. In summary, our analysis sample includes eleven time periods. Specifically, we have nine pre-treatment periods, i.e. 2003/2004, 2004/2005, 2005/2006, 2006/2007, 2007/2008, 2008/2009, 2009/2010, 2010/2011 and 2012/2013, and two post-treatment periods, namely 2014/2015 and 2016/2017. Appendix Table B1 provides more details.[16]

In addition, the secondary data used for this project also contain a series of suppressed values due to the application of statistical disclosure control. In general, any percentage whose underlying sample size is between 1 and 4 is reported as missing. In Figure A1 we compare the trends calculated with the secondary data to those obtained from the official, publicly available aggregates. The patterns are

[15]These are about 380 per school year for the vast majority of schools and local authorities.

[16]Treatment occurs half-way through the 2014/2015 school year.

virtually unchanged, despite the fact that our data seem to overstate these measures. This is mechanical, since disclosure control essentially excludes instances with very low values. Finally, we retain only those schools that can be observed in all eleven of our time periods making our panel balanced.

4 Empirical Strategy

Our research question entails a number of methodological challenges, primarily in relation to the endogeneity of school-level uptakes. First, there can be omitted variable bias induced by unobserved school-specific and time-invariant characteristics which are correlated with uptake. For instance, selection into schools might be a driver of uptake. Second, shocks in behaviour in one year might drive FSM uptake in the future, leading to simultaneity between behaviour and uptake. Comparing behaviour across schools with different levels of uptake would certainly lead to spurious correlations. To overcome these issues, we employ a difference-in-differences (DiD) model with continuous treatment (Clemens et al., 2018; Card, 1992) which is estimated using the following two-way fixed effects (TWFE) regression equation:

$$y_{sct} = \gamma(I_{t\geq 2015} \times E_{sc,2014}) + \beta' X_{sct} + \alpha_s + \lambda_t + \varepsilon_{sct} \tag{3}$$

where y_{sct} is a set of behavioural outcomes averaged for school s, in local authority (council) c and school year t. E_{sc} is our measure of exposure to the UFSM intervention. $I_{t\geq 2015}$ is a dummy variable switching to one for every wave following the policy implementation.[17] Our coefficient of interest is γ, measuring difference in behaviours across different levels of exposure to the policy. X_{sct} is a set of pre-treatment covariates, which we interact with time dummies. These include time-varying covariates such as school composition, school population and resources.[18] Since these could constitute potential outcomes of the policy we fix them to 2004, well before the change in policy. In addition, X_{sct} contains time-invariant characteristics such as religious status and whether the school is in an urban area, also interacted with time dummies. Finally, α_s and λ_t are school and year fixed effects and ε_{sct} is the idiosyncratic shock component of behaviour. Our source of identifying variation comes from the before-after comparison of treatment and control group, paired with within-school variation leveraged by α_s. Residual concerns entail

[17]These refer to the calendar years before/after the policy was implemented.

[18]We mostly employ school-level average class size as this is a good proxy of school population and resources. Its correlation with $ln(school\ population)$ and pupil-teacher ratio is .84 and .77 respectively.

school-specific time trends which might simultaneously drive uptakes and outcomes. For instance, periodic staff shortages could undermine the effectiveness of lunch deliveries but also impact students' engagement and behaviour. In this vein, in some specifications we control for school and local authority-specific time trends, i.e. $\alpha_s t$ and $\alpha_c t$. In addition, as UFSM was widely discussed in the media at least one year before the implementation, one could worry about anticipation effects. This would entail pupils switching schools on account of pre-treatment level of uptakes. We believe this is highly unlikely as the Scottish system is residence-based and implies households providing evidence of residence within the attendance area at least six months in advance of the school year start (see Rossi (2020) and Borbely et al. (2020)).

Our preferred measure of exposure is:

$$E_{sc,2014} = \frac{\#\ Non - FSM - Takers_{sc,2014}}{School\ Population_{sc,2014}}$$

whose rationale we illustrate with an example. Consider two equally sized schools, A and B. In school A 90% of the students take FSM before FSM become universal, whereas only 10% do in school B. Conditional on observables, the policy is thus likely to have a stronger effect in school B, where a larger share of the school population do not already take FSM. Put simply, our strategy compares schools where the policy induced a larger change in uptake with schools where the policy did not do so, on the account of uptake already being high prior to the policy.

Of course, we need to corroborate this with evidence that the policy led to such an increase in FSM uptake. This 'first stage' is presented in the top-left panel of Figure 4. Here we plot trends in school-level percentage of pupils taking FSM by level of exposure to the policy. We distinguish between school with high versus low exposure to the policy based on the school having a share of non-FSM-takers in school year 2013/2014 that is above or below the median. The group with high exposure to the policy is characterised by a stable trend of about 10% of FSM-takers prior the UFSM implementation, whereas the group with low exposure, where relatively more pupils were already taking FSM by 2015, had around 25 to 30% FSM-takers. Here we can see how FSM uptakes have sharply increased regardless of the pre-policy level of exposure, however, these increased by approximately 10 percentage points more within schools with high policy exposure.

Now that we have ascertained that the UFSM has indeed induced a stronger increase in uptakes for schools with fewer FSM takers before the treatment, we need to make sure these two groups are comparable over time. As stated before, a simple 'between-schools' comparison would result in inconsistent estimates of the effect of the policy. Figure 4 shows trends in outcomes for schools within the treatment and control group. While for our design to be valid we need the timing of the policy not to be associated with *changes* in outcomes – i.e. the parallel trends assumption needs to hold – a recent, yet small, literature (Jaeger et al., 2020; Kahn-Lang and Lang, 2020) has stressed the importance for treatment and control group to be similar also in *levels*. If these diverge considerably, one might wonder whether the factors driving those discrepancies might also affect *changes* in trends. We can see from Figure 4 that while the outcomes seem to follow parallel trends up to the change in policy, different exposures are associated with different *levels* of the outcomes. For instance, high exposure schools are characterised by approximately 1.5 percentage points higher attendance rate on average, as well as lower illnesses and exclusions. This is not surprising considering that, prior to the policy change, FSM eligibility was contingent to disadvantaged socio-economic status, which can therefore be related to higher absenteeism (Sosu et al., 2021), worse health outcomes (Adams et al., 2003) and anti-social behaviour (Piotrowska et al., 2015). For this reason, similarly to Jaeger et al. (2020) we present in Figure 5 outcomes' trends by sub-groups, namely quartiles of the SIMD as well as for urban and rural schools. We can observe how there are differential trends across groups, especially for exclusions and illnesses. While this is not invalidating per se, we need to keep in mind that our measure of exposure is directly related to deprivation and this might cause the parallel trends assumption not to hold. For this reason, even our most basic specifications will contain interactions between year dummies and schools' postcode SIMD score in 2004 (well before the change in policy) which proxies school composition.[19]

Furthermore, we want to rule out the possibility that divergence in outcome *levels* between high and low-exposure groups may mask differences in drivers of exposure to the policy. Figure 6 provides some evidence on this. Each point estimate is from a multiple regression model where the dependent variable is a binary indicator for whether the school was highly exposed to the policy in school year 2013/14 (above median, i.e. 83% of school population non-FSM takers) and the predictors are the variables listed on the left-hand side of the figure. The whiskers indicate 95% level confidence intervals. The analysis is conducted

[19]Primary schools' catchment areas are small enough that the school building neighbourhood is a good enough proxy of the entire catchment. In addition, the correlation between this 2004 index and the percentage of school population entitled for FSM under the national scheme in 2012 is .70.

on a cross-section of 1,630 primary schools in school year 2013/14. While we mostly include school-level variables, we also use local authority-level percentages of primary school pupils from ethnic minorities and from a non-UK white background, as well as the employment rate among people presumably too young to be the parents of primary school pupils in school year 2013/14. Results are similar whether we leverage between (Figure 6a) or within-local authority variation (Figure 6b) and suggest that none of the available school (or local authority) characteristics are significant predictors of UFSM exposure. Urban and more deprived schools seem to be significantly less exposed, i.e. they have larger pre-policy uptakes. Whilst we already control for deprivation in all of our specifications, we also show that our results are unaffected when controlling for variation in school location (urban or rural).

Finally, our data structure allows us to formally test parallel trends by estimating the following event-study equation:

$$y_{sct} = \sum_{t=2003}^{2016} \gamma_t (I_t \times E_{sc}) + \beta' X_{sct} + \alpha_s + \lambda_t + \varepsilon_{sct} \tag{4}$$

Our coefficients γ_t are plotted in Figure 7, alongside their 95% confidence intervals. The horizontal axis measures the number of periods to and from the reference period (t=0), which is school year 2012/2013, just before the change in policy. The reason for this choice of reference period is due to us not observing the outcomes in 2013/2014, as the survey was not carried out. For instance, $t = -8$ is school year 2003/2004, eight periods prior to 2012/2013, once accounting for another gap in outcomes in 2011/2012. Period one is year 2014/2015, namely when UFSM is in place. Our models contain interactions between year dummies and SIMD. Standard errors are clustered at the school level. These plots confirm the finding of the parallel trends charts, i.e. higher exposure is not associated with diverging trends in outcomes prior to the policy change. The coefficients are not statistically significant at the 5% level up to four years prior to UFSM implementation. Whilst one could point out that some of the coefficients are individually significant at the 5% level, we argue that these point estimates are very small and not economically significant. For example, looking at the top-left panel, the largest lead coefficient suggests a 0.1 percentage points reduction in attendance, which in turn suggests nearly a 0% reduction. We can also observe from these charts that following the implementation of the policy, whilst the outcomes changed in the expected direction, the size of the coefficient is also very small and suggestive of a null effect. We explore this further in the next section.

5 Results

Table 2 presents estimates of γ from Equation 3. Just as in the event-study, we keep only those schools which we are able to observe every year from 2003 to 2016.[20] Every specification contains school and year fixed effects. Column (1) is our most basic specification where we additionally control for interactions between schools' SIMD score and year dummies. In column (2) alongside our proxy of school composition we control for average class size, religious status and indicator of whether the school is in a urban area, all appropriately interacted with time dummies.[21] Finally, in columns (3) and (4) we control for school and local authority-specific linear time trends.

Our preferred specification is the one in column (2), which suggests that a one percentage point increase in exposure, i.e. pre-treatment share of pupils not taking FSM, increases attendance by .011 percentage points, reduces heath-related absences by .010, that is by only about 1/100 of a percentage point, and has no impact on days missed due to disciplinary action. The first two coefficients are statistically significant at any conventional significance level, whilst we fail to reject the null hypothesis of no effect on exclusions. These results, which are consistent across specifications, confirm what we found in the previous section, i.e. null effects of the policy on the variables of interest. In particular, even a full standard deviation increase in exposure translates into less than one session missed.[22]

5.1 Robustness

Our first set of results clearly suggests that, while the policy undoubtedly increased FSM uptakes, this did not translate into improved attendance, behaviour and short-term health conditions. In this section we address a series of concerns threatening our identification strategy. First, we want to address another point raised by Kahn-Lang and Lang (2020) in relation to parallel trends when treatment and control group differ in levels. In Figure 4 and Figure 7 we ascertained that high and low exposure schools, while on different levels of the outcomes, had been trending similarly with respect to the outcomes themselves. However, as Kahn-Lang and Lang (2020) point out, it is not clear what "similarly" means in circumstances like the

[20] Table A1 and Figure A4 show estimates when using the unbalanced panel. Results do not change.

[21] Time-varying controls are fixed at the first period of our window of data.

[22] The Scottish system features a minimum of 190 days (and 380 half-day sessions) per school year. The standard deviation of our exposure measure is about 13 percentage points, so one standard deviation increase in exposure translates to $13 \times .011 = .14$ percentage points increase in attendance. These are measured in half-day sessions, so $3.8 \times .14 = .55$ sessions, which is far below a whole school day.

one at hand. For instance, parallel trends might hold in levels, meaning that treatment and control groups experience the same absolute changes, but not in a logarithmic specification as their absolute changes lead to divergent percentage changes. Figure A5 and Table A2 reports the same event study and aggregate regression exercises in Figure 7 and Table 2 but with log-transformed outcomes.[23] We can immediately notice that not only does a different functional form not alter our conclusion about parallel trends, but the regression results also point towards a null effect.

Another concern, continuing the point raised in Section 4, relates to the possibility that variables explaining the difference levels of the outcomes might have diverging trends. Table A3 assess the robustness of our results to alternative specifications. While the majority of the analysis is conducted interacting covariates with year fixed effects - as in column (2) - we also run similar exercises using interactions with a linear time trend and the post treatment dummy variable. Results are largely unchanged.

In addition, as students in the first three grades (P1, P2 and P3) were targeted by the policy, perhaps higher importance should be put on those observations where a larger fraction of the school population is enrolled in those grades around the time the policy went into effect. We therefore estimate a weighted version of Equation 3. The results are reported in Table A4. Regressions are weighted using the percentage of students in P1-P3 in 2013/2014 in each school as weights. Our results are not significantly affected by this new specification. Furthermore, we repeat the estimation exercise in Equation 3 and Equation 4 excluding from our sample: *i)* schools which took part in the FSM pilot in school year 2007/08; *ii)* schools which extended FSM eligibility among P1-P3 students based on local initiatives in 2010. The event study charts for these samples are reported in Figure A6 and Figure A7 whereas the aggregate regression results are in Table A5 and Table A6. In both cases, our conclusion does not change.

A further potential concern relates to our chosen measure of exposure, which might not generate enough variation in uptake to trigger the expected benefits in terms of attendance and behaviour. For this reason, we expand our model in Equation 3 and estimate a triple difference-in-differences (DDD) similarly to Muralidharan and Prakash (2017). Our third term of comparison is secondary schools, which were not affected by the policy. Figure A8 shows a sharper change in uptake (nearly 30 percentage points) when

[23]To accommodate this transformation in the presence of null values for health-related absences and exclusions, we took the natural log of 1 + the variables in levels, e.g. $ln(x) = ln(1 + x)$

primary schools are compared against secondary schools. Therefore, we estimate the model[24]:

$$y_{sct} = \gamma(I_{t\geq 2015} \times E_{sc,2014} \times Primary_{sc}) + \beta' X_{sct} + \alpha_s + \lambda_t + \varepsilon_{sct} \tag{5}$$

Where $Primary_{sc}$ is a dummy variable taking value 1 if a school is in the primary sector. Results are presented in Table A7. As the outcome variables follow different distributions across sectors, we estimate the models in a log-linear fashion. Similarly to the DiD results, we can see that highly exposed (primary) schools experienced an increase in attendance by approximately 0.1% conditional on school composition and characteristics. The effect, however, fades out when we control for school and local authorities' trends. Health-related absences seemed to have increased in primary schools following the UFSM implementation by up to 1%. These results, however, do not hold up in all specifications. Finally, exclusions have experienced a reduction of about 1.9%. This new set of results suggest that even when comparing highly exposed primary schools before and after the change in policy with secondary schools, the results are quite modest. In addition, the event-study in Figure A9 shows how primary schools were exhibiting positive pre-trends in attendance and health-related absence and negative ones in exclusions. Therefore, the estimates from Table A7 are at best an upper bound for the effect of FSM uptake on attendance and health-related absences and a lower bound for the effect on exclusions. This strengthens our main conclusion that there has been no effect of the policy on behavioural outcomes.

5.2 Mechanisms

One possible explanation of the null effects we have been observing so far may be related to the effectiveness with which the policy was implemented. In fact, we might expect the ease of implementation to differ across schools' characteristics. In this section we explore potential mechanisms, building on the findings of two evaluations of UFSM. McAdams (2016) found that the largest uptakes following UFSM were recorded in rural schools, as well as schools with the largest share of pupils from disadvantaged background. Conversely, urban schools experienced the lowest uptakes. Additionally, Chambers et al. (2016) pointed out that a lack of funding, staff, and school spaces worked as a barrier to policy implementation, thus affecting take-up. Based on these findings, we might expect larger effects in those contexts in which policy roll-out

[24] Triple DiD entails the inclusion of a minimum of seven terms on the right-hand side of the equation. For the sake of simplicity, we only display the main interaction.

went more smoothly.

We investigate this by stratifying our sample and looking at the heterogeneous effects of the policy across sub-groups. The sub-groups are defined as follows. Urban schools are based on the six-fold classification of the Scottish Government and include schools in large and other urban areas. We proxy staff recruitment and funding by using information on class size, school roll and pupil-teacher ratio from the academic year commencing in 2005. This is well before the treatment period and is the earliest we can observe data on teachers' numbers. We generate dummy variables for small schools and classes by using the first quartile of their 2005 distribution. These correspond respectively to schools with less than 122 pupils and whose average class size is less than 20 pupils. Similarly, a low pupil-teacher ratio is identified by the bottom quartile of the 2005 distribution, namely an average of 15 pupils per teacher or less. School internal area is collected from the 2008 School Estates Survey and is measured in square-metres. Finally, we identify a school as *'most deprived'* if it is located in a data zone which is classified as within the 25% most deprived according to the 2004 SIMD.[25] We control for school and year fixed effects in every specification, alongside interaction between year dummies and SIMD score - except, of course, in column 6. While the point estimates appear to be rather small, they suggest interesting insights.

Table 3 shows that within the most exposed schools, urban schools seem to have experienced smaller improvements than schools in rural areas or small towns. In addition, column (2) to (4) suggest that small schools and those with higher per-pupil expenditures benefited more in terms of reduced absenteeism. All coefficients are, however, again very small and not statistically significant at any conventional level, with the exception of school population. We conjecture that this is due to larger variation in number of pupils per school, relative to class size and pupil-teacher ratio.[26] However, given the large correlation shared by these three variables we can safely assume they all reflect the same dimension of school resources. Furthermore, we find no evidence that school space might have contributed to the effect of the policy, whilst highly exposed schools, and those with the highest levels of deprivation have seen an increase in absenteeism. Table 4 and Table 5 conduct the same exercise but for health-related absences and exclusions.

Results for health-related absences are similar in magnitude to those for attendance, and share the

[25]Variation in policy exposure within schools from most deprived areas is not considerably different from the one within schools from less deprived areas. Their coefficients of variation ($CV = \frac{\sigma}{\mu}$) are .18 and .12 respectively.

[26]Average school roll in 2005 is 210 with a standard deviation of 124. Average class size has a mean of 22.8 and standard deviation 4.27. Finally, PT ratio's mean is 16.7 with only 3.5 of standard deviation

same sign. In other words, these findings suggest that where uptake of the policy has been greater, i.e. rural schools and those with more resources, students' short-term health condition slightly worsened, although the effect sizes are still very small. In the most deprived schools the beneficial effects of FSM on illnesses seem to be slightly stronger. This may indicate an improvement in nutritional intakes for pupils from disadvantaged backgrounds. For exclusions, our analysis yields regression coefficients that are in line with expected reductions in misbehaviour in schools where uptakes have been higher, although coefficients are only weakly statistically significant. Taken together, our subgroup analysis fails to reveal any substantial benefits for any group. This suggests that the aggregate null effect that we find, is not masking any important sub-group effect.

6 Conclusion

The provision of universal free school meals (UFSM) has become a commonly used form of welfare policy in recent years, yet its impact is still widely debated. In this paper, we evaluate UFSM implementation in relation to an overlooked set of outcomes: school attendance, short-term health conditions and misbehaviour. We do so by focusing on the case of Scotland, where in 2015 all pupils in the first three grades of primary school became eligible to receive FSM, regardless of their household's financial circumstances. We employ a difference-in-differences (DiD) design where treatment intensity is determined by pre-policy levels of FSM uptakes. That is, the introduction of UFSM had more "bite" in schools with few pre-implementation FSM takers than in schools where FSM enrollment was high at baseline.[27] We find precisely estimated null effects on attendance and health-related absences. A 10-percentage-points increase in school population taking free school meals leads to a gain of less than one school day. We also find no evidence that the policy helps to prevent misbehaviour. Nor do the null effects mask benefits for subgroups. Rural schools, small schools, and schools with more resources see only marginally larger and economically very small benefits.

Our study has some limitations. First, the policy targeted only the first three grades of primary schools (45% of school population on average), while the outcomes are aggregated across all seven grades. Second, our outcome data allow us to only track effects for up to 3 years after policy implementation. Third,

[27]Prior to the change in policy uptakes among eligible students were on average 90%.

attendance rates - our main outcome - are very high to begin with in Scottish primary schools (95% on average) which means that there is limited room for improvements. Exclusions refer to a disciplinary procedure being issued, rather than the actual misbehaviour occurring (Altindag et al., 2020). Either way, these are rare in primary school.

With this in mind, one should be careful about concluding that UFSM are in general ineffective at encouraging attendance or improving pupils' short-term health condition. Both dimensions should be carefully considered by policymakers. However, the small effects so far found in the literature (see Corcoran et al., 2016; Gordanier et al., 2020; Cuadros-Meñaca et al., 2022; Holford and Rabe, 2020) as well as our study should raise the question of how UFSM policies are rolled out, how nutritious the meals provided are, and whether implementing such programmes on a large scale can be done whilst maintaining a high food quality (Parnham et al., 2022). One assumption of our study was that the policy did not change the nutritional content of school meals. Notably the only study finding a sizeable reduction in absenteeism is Belot and James (2011) where the authors assess the effect of a change in nutritional content, rather than just an extension in the provision of school meals. This is a key area of future research. Similarly, it would be beneficial to utilise individual pupil level data to test the effect of the more direct exposure to the policy, i.e. being enrolled in first, second or third grade on pupil level outcomes. Additionally, future work could explore how this policy has increased uptake among previously eligible students, in line with Holford (2015). Finally, as one of the goals of the policy was to reduce food insecurity among children, scholars could explore the impact of this policy on households' finances and benefit claims.

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Tables

Table 1: Summary Statistics

Panel A: Attendance	Mean	St.Dev.
In School	99.14	1.34
Late	0.85	1.34
Work Experience	0.00	0.01
Sick With Educational Provision	0.02	0.09
Panel B: Authorised Absence		
Sick w/o Educational Provision	66.85	27.58
Very Late	0.21	1.07
Authorised Holiday	3.83	4.41
Exceptional Domestic Circumstances	2.22	3.78
Other Authorised Absence	26.88	28.01
Panel C: Unauthorised Absence		
Unauthorised Holiday	55.24	27.98
Truancy	30.13	28.61
Unauthorised Exceptional Domestic Circumstances	2.33	4.85
Other Unauthorised	12.30	17.01
Panel D: Sub-categories		
Sickness Rate (% Possible Attendance)	2.50	1.24
Lateness Rate (% Possible Attendance)	0.81	1.43
Truancy Rate (% Possible Attendance)	0.45	0.75
Panel E: Aggregates		
Exclusion Rate	0.02	0.04
Attendance Rate	95.02	1.63
Authorised Absence Rate	3.90	1.24
Unauthorised Absence Rate	1.06	0.87
No. of Schools	2,335	

Notes: Panels A, B and C report for each group the average proportion of each sub-category within attendance, authorised and unauthorised absence respectively. For example, on average 99.14% of overall attendance is "in school", and about 67% of authorised absences are due to pupils being ill. In Panel E, each aggregate is calculated as % of all possible attendance. These are 2003-2018 averages.

Table 2: Main Results

Panel A: Attendance	(1)	(2)	(3)	(4)
Post × Exposure	0.010***	0.011***	0.002	0.010***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	17,766	17,766	17,766	17,766
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	95.05	95.05	95.05	95.05
SD Dep. Var.	1.57	1.57	1.57	1.57
R-squared	0.757	0.762	0.713	0.766
Panel B: Illnesses	(1)	(2)	(3)	(4)
Post × Exposure	-0.009***	-0.010***	-0.003	-0.009***
	(0.003)	(0.003)	(0.003)	(0.002)
Observations	17,781	17,781	17,781	17,781
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	2.57	2.57	2.57	2.57
SD Dep. Var.	1.54	1.54	1.54	1.54
R-squared	0.585	0.587	0.443	0.667
Panel C: Exclusions	(1)	(2)	(3)	(4)
Post × Exposure	0.001	0.001	-0.000	0.001
	(0.001)	(0.001)	(0.002)	(0.001)
Observations	17,308	17,308	17,308	17,308
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	0.19	0.19	0.19	0.19
SD Dep. Var.	0.48	0.48	0.48	0.48
R-squared	0.300	0.303	0.198	0.306
Baseline Controls		✓	✓	✓
School FE	✓	✓	✓	✓
School Trends			✓	
Local Authories Trends				✓

Notes: Coefficients are obtained by estimating γ from Equation 3. Outcomes are calculated in % of all possible half-day openings. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) score and school average class size in 2004, both interacted with year dummies, alongside indicators for urban and religious all appropriately interacted with year dummies. Data span from school year 2003/2004 through to 2016/2017. Outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. Standard errors (in parentheses) are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Heterogeneity - Attendance

Dependent Variable: Attendance	(1)	(2)	(3)	(4)	(5)	(6)
Post × Exposure × Urban	-0.012***					
	(0.004)					
Post × Exposure × Small School		0.014***				
		(0.005)				
Post × Exposure × Small Classes			0.009*			
			(0.005)			
Post × Exposure × Low PT-ratio				0.004		
				(0.005)		
Post × Exposure × ln(Internal Area)					-0.003	
					(0.003)	
Post × Exposure × Most Deprived						-0.020***
						(0.005)
Observations	17,766	17,766	17,766	17,766	17,766	17,766
No. of Schools	1630	1630	1630	1630	1630	1630
Mean Dep. Var.	95.05	95.05	95.05	95.05	95.05	95.05
SD Dep. Var.	1.57	1.57	1.57	1.57	1.57	1.57
R-squared	0.761	0.761	0.761	0.761	0.759	0.758

Notes: Outcomes are calculated in % of all possible half-day openings. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) in 2004 score interacted with year dummies, alongside indicators for urban, religious, below-median school population indicators, all appropriately interacted with year dummies. Standard errors (in parentheses) are clustered at the school level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Heterogeneity - Health-related Absences

Dependent Variable: Illnesses	(1)	(2)	(3)	(4)	(5)	(6)
Post × Exposure × Urban	-0.014***					
	(0.005)					
Post × Exposure × Small School		0.002				
		(0.005)				
Post × Exposure × Small Classes			0.011**			
			(0.005)			
Post × Exposure × Low PT-ratio				0.006		
				(0.005)		
Post × Exposure × ln(Internal Area)					0.003	
					(0.003)	
Post × Exposure × Most Deprived						-0.013**
						(0.006)
Observations	17,781	17,781	17,781	17,781	17,781	17,781
No. of Schools	1630	1630	1630	1630	1630	1630
Mean Dep. Var.	2.57	2.57	2.57	2.57	2.57	2.57
SD Dep. Var.	1.54	1.54	1.54	1.54	1.54	1.54
R-squared	0.586	0.585	0.585	0.585	0.585	0.582

Notes: Outcomes are calculated in % of all possible half-day openings. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) in 2004 score interacted with year dummies, alongside indicators for urban, religious, below-median school population indicators, all appropriately interacted with year dummies. Standard errors (in parentheses) are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

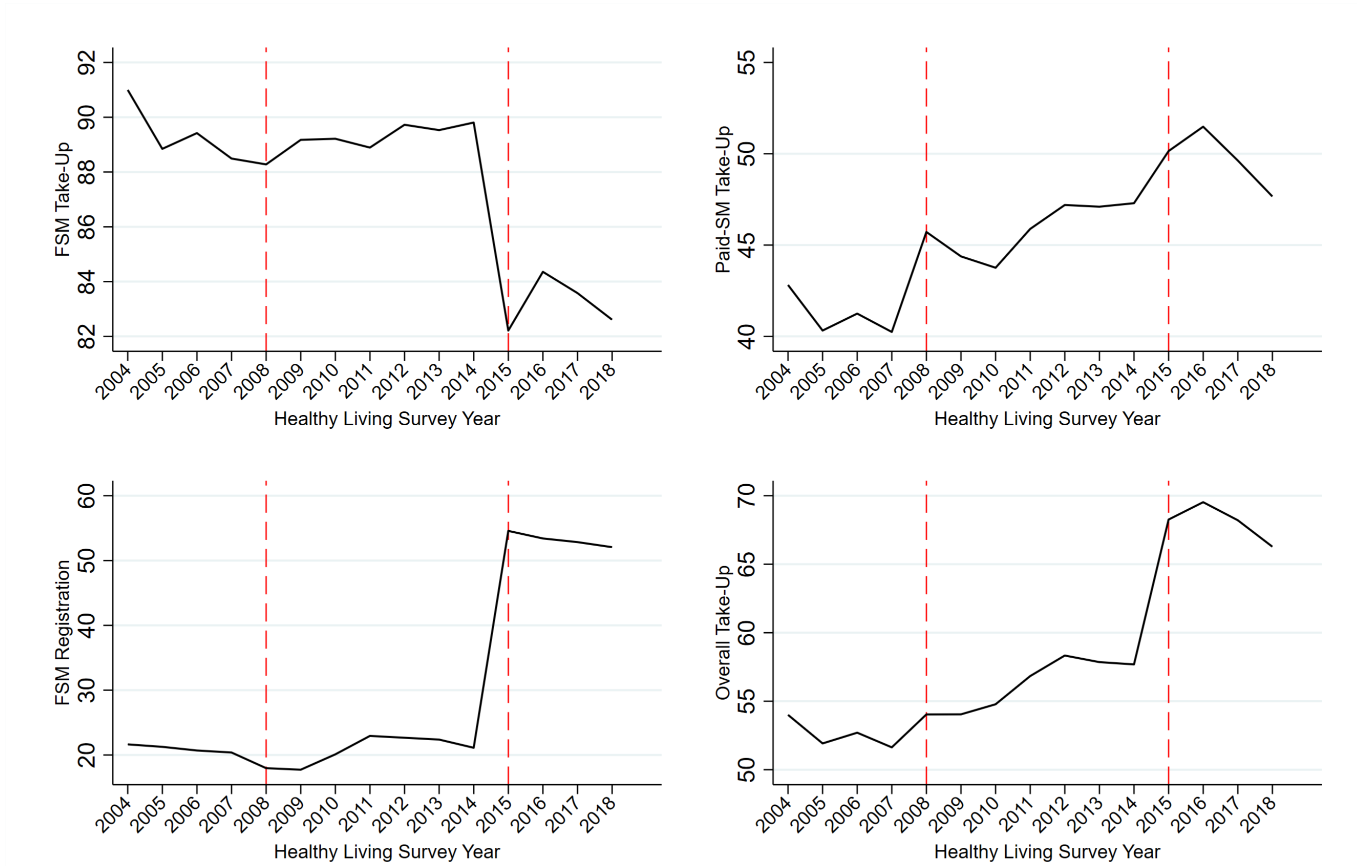
Table 5: Heterogeneity - Exclusions

Dependent Variable: Exclusions	(1)	(2)	(3)	(4)	(5)	(6)
Post × Exposure × Urban	0.005**					
	(0.002)					
Post × Exposure × Small School		-0.003				
		(0.003)				
Post × Exposure × Small Classes			-0.002			
			(0.002)			
Post × Exposure × Low PT-ratio				-0.001		
				(0.002)		
Post × Exposure × ln(Internal Area)					0.002*	
					(0.001)	
Post × Exposure × Most Deprived						0.005*
						(0.002)
Observations	17,308	17,308	17,308	17,308	17,119	17,308
No. of Schools	1630	1630	1630	1630	1630	1630
Mean Dep. Var.	0.19	0.19	0.19	0.19	0.19	0.19
SD Dep. Var.	0.48	0.48	0.48	0.48	0.48	0.48
R-squared	0.300	0.300	0.300	0.300	0.585	0.582

Notes: Outcomes are calculated in % of all possible half-day openings. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) in 2004 score interacted with year dummies, alongside indicators for urban, religious, below-median school population indicators, all appropriately interacted with year dummies. Standard errors (in parentheses) are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

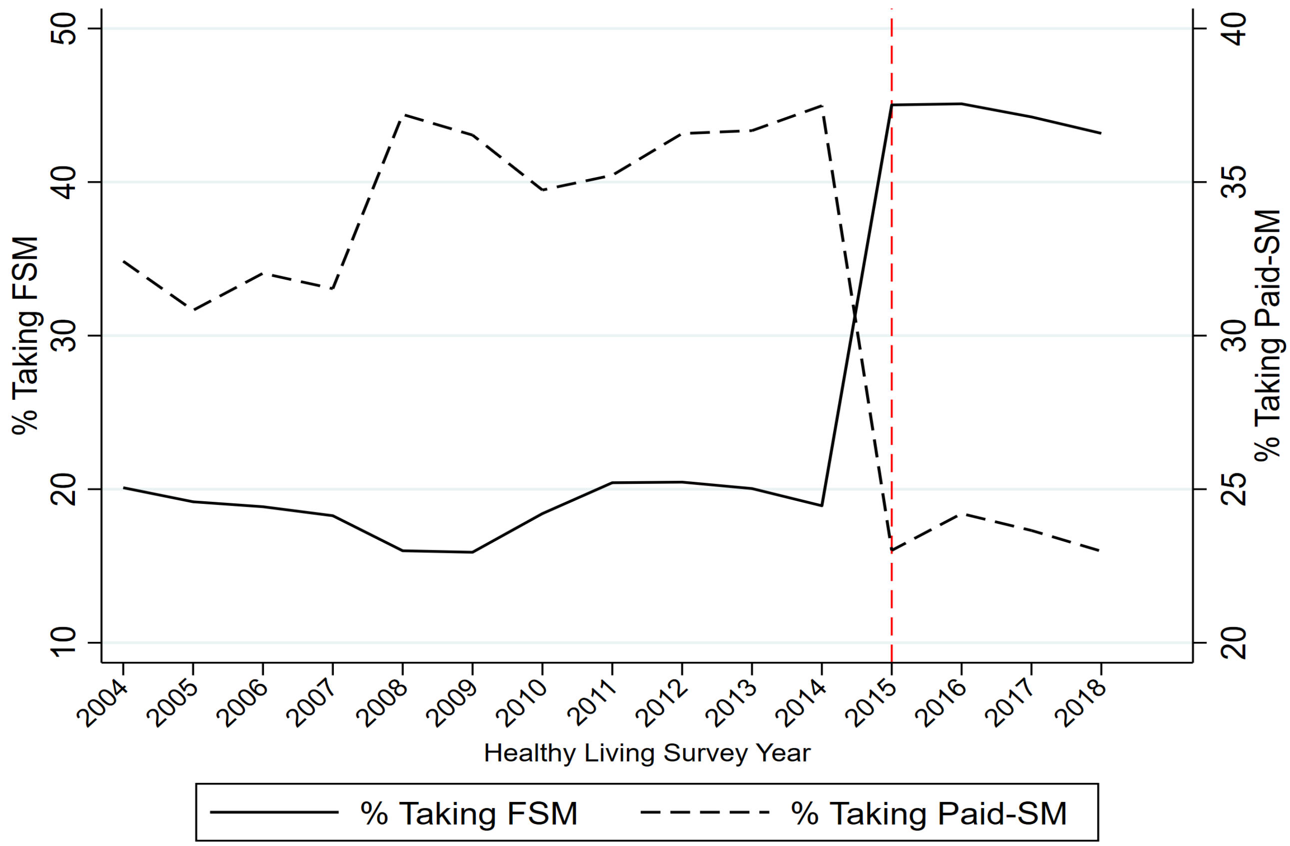
Figures

Figure 1: Trends



Note: The above charts are calculated as raw yearly-averages of the following ratios: FSM-Take-up: $\frac{\#FSM-Takers}{\#FSM-Registered}$; Paid-SM Take-Up: $\frac{\#PSM-Takers}{\#non-FSM-Registered}$; FSM Registration: $\frac{\#FSM-Registered}{School\ Population}$; Overall Take-Up: $\frac{\#Meal-Takers}{School\ Population}$. Because the survey is run in one day, the raw counts refer to pupils present on the day of the survey.

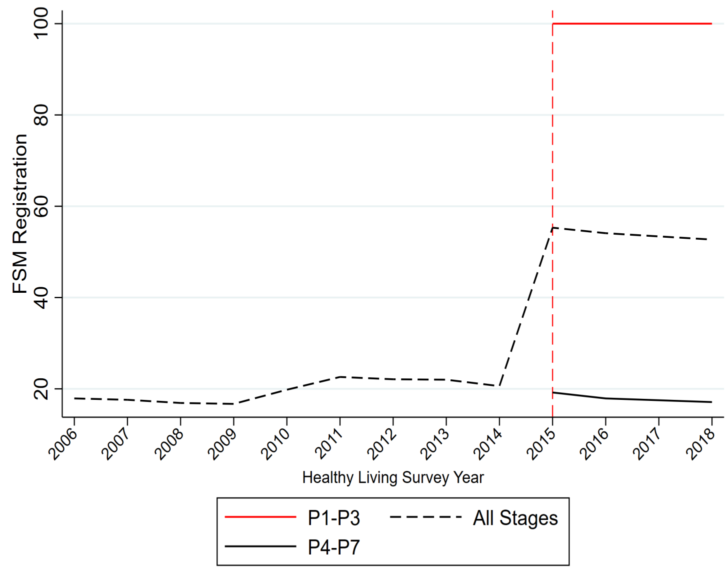
Figure 2: FSM vs PSM Uptakes



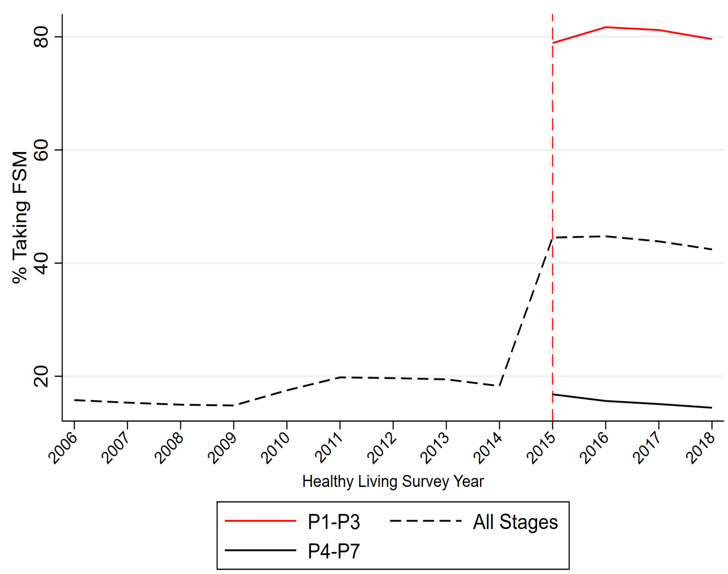
Note: The above trends are calculated as raw yearly-averages of the following ratios: % Taking FSM: $\frac{\#FSM-Takers}{School\ Population}$; % Taking Paid-SM: $\frac{\#PSM-Takers}{School\ Population}$. Because the survey is run in one day, the raw counts refer to pupils present on the day of the survey.

Figure 3: FSM Registrations and Uptakes - by Stages

(a) FSM Registration

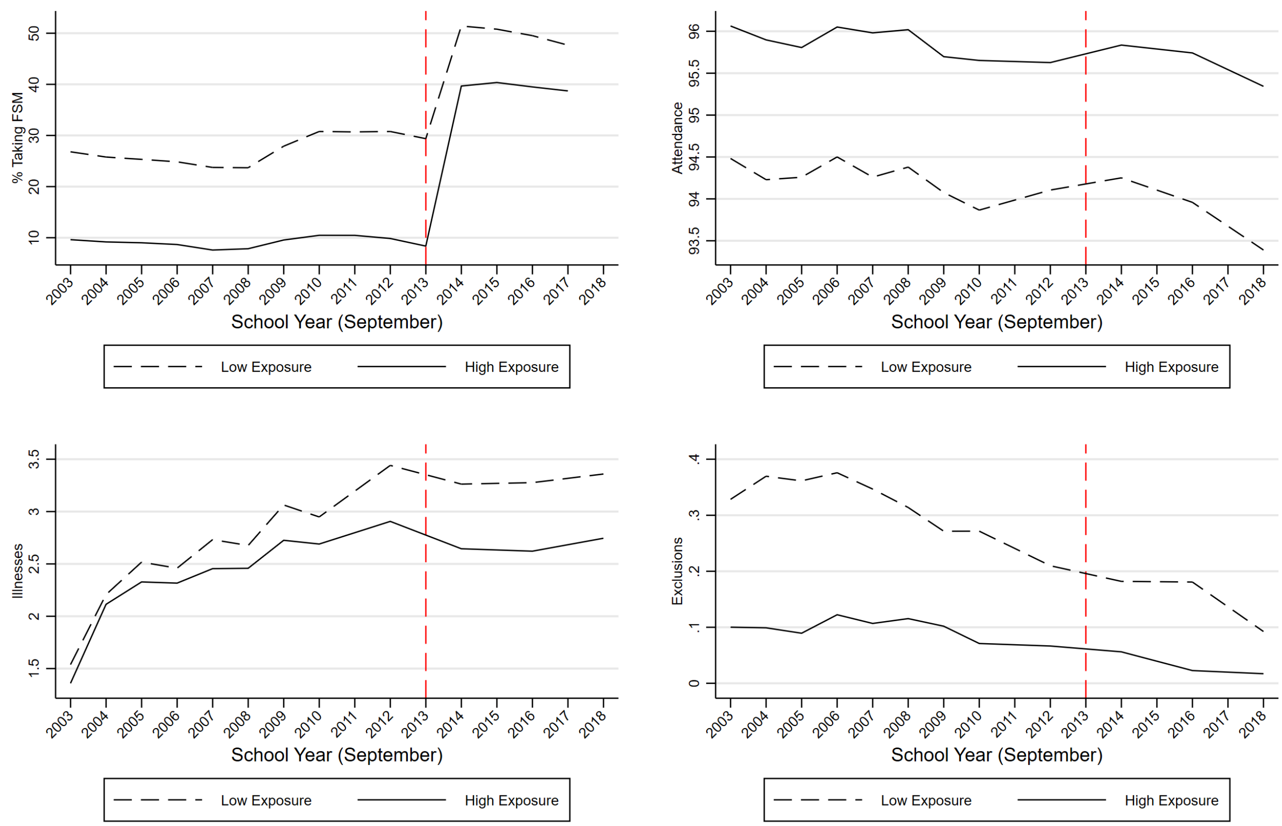


(b) FSM Uptakes



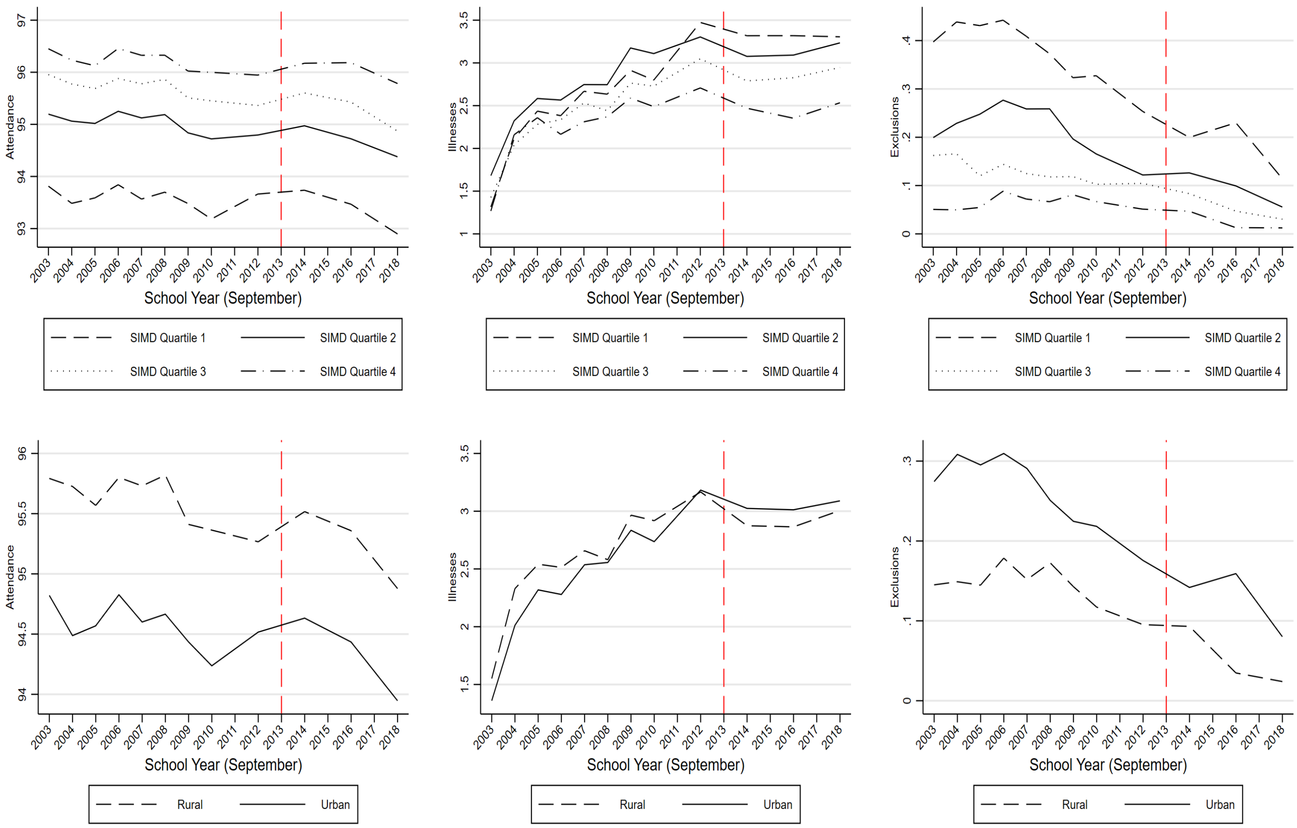
Note: The above charts are calculated as raw yearly-averages of the following ratios: FSM Registration: $\frac{\#FSM{-}Registered}{School\ Population}$; % Taking FSM: $\frac{\#FSM{-}Takers}{School\ Population}$. Because the survey is run in one day, the raw counts refer to pupils present on the day of the survey. The dotted black line represents the entire primary school-level trend. The solid red line dis-aggregates the trend for the P1-P3 group (targeted by the policy) whereas the solid black line shows the P4-P7 trend. The breakdown of registration and take-up is only available, at the national level, starting from HLS wave 2015.

Figure 4: Parallel Trends



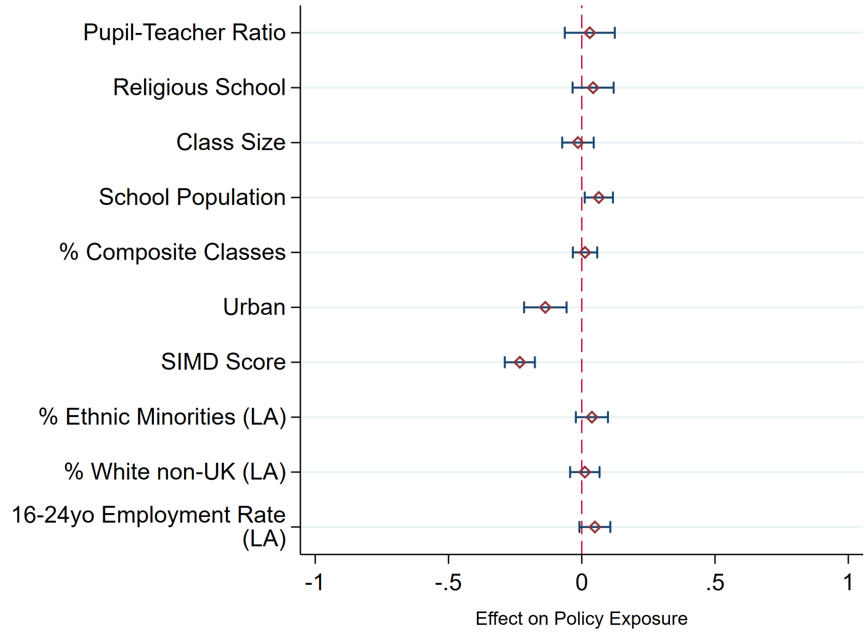
Note: The above charts are calculated as raw yearly-averages of the reported variables, across levels of exposure. In particular, exposure is defined as the % of school population not-taking FSM in 2013, one year before the policy change. A high exposure is denoted by a value above the median.

Figure 5: Trends Within Sub-Groups

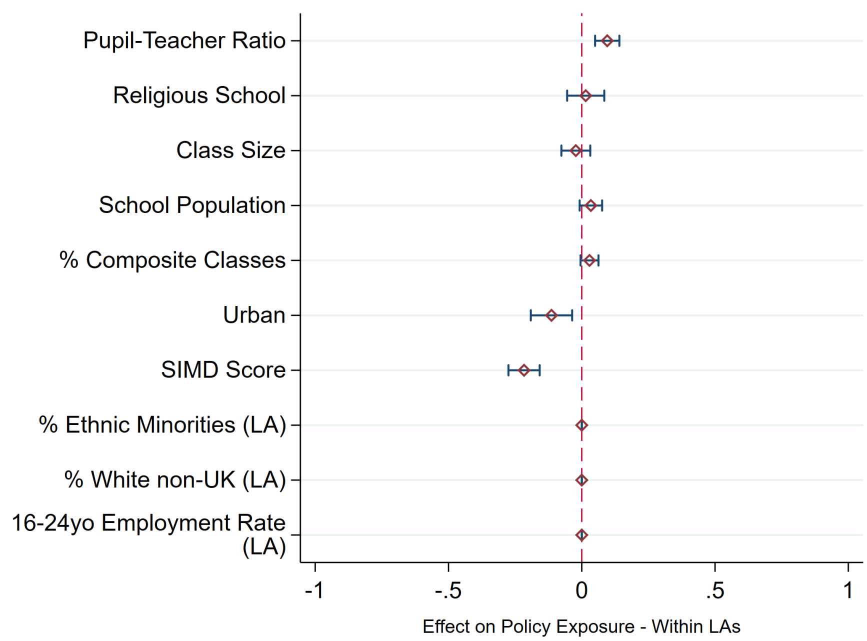


Note: The above charts are calculated as raw yearly-averages of the reported variables, across Scottish Index of Multiple Deprivation quartiles and whether the schools are in urban vs small town or rural area.

Figure 6: Balancing Tests



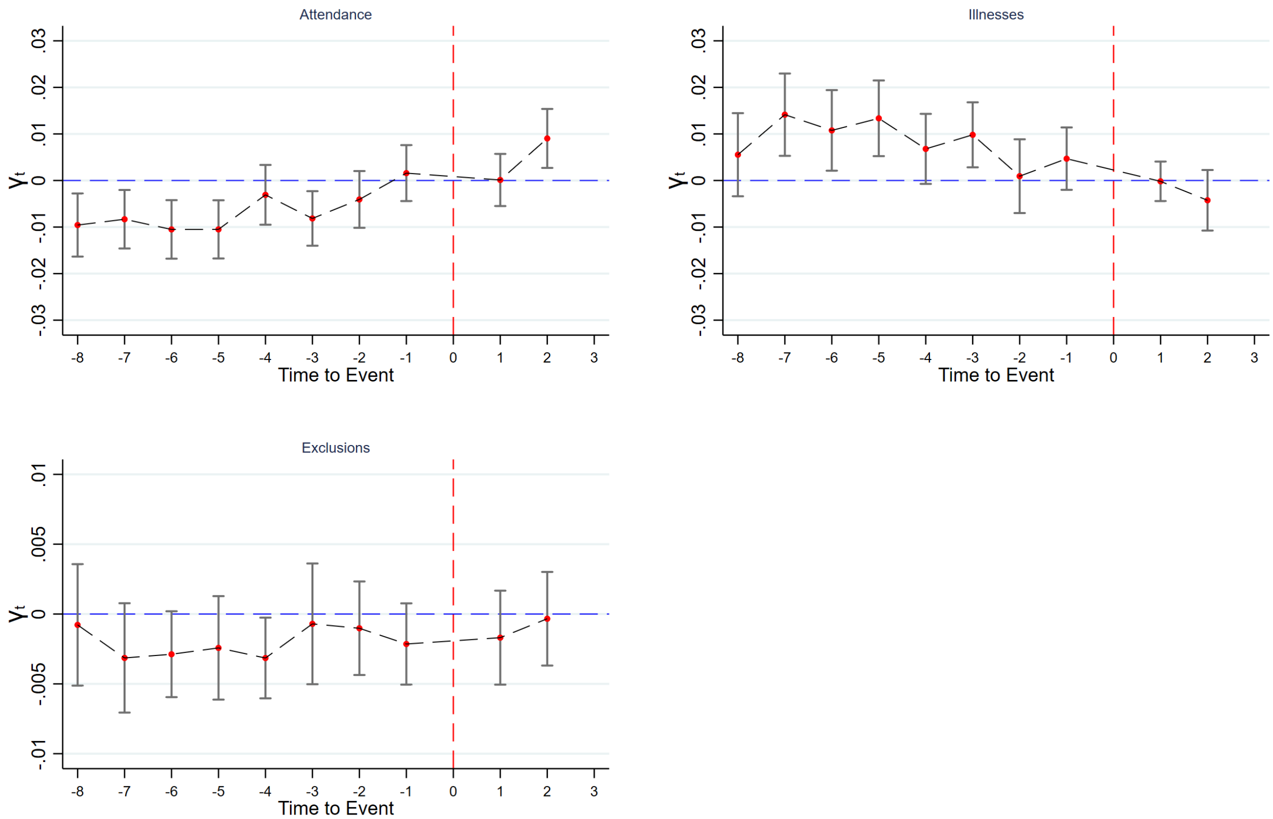
(a) w/o LA Fixed Effects



(b) w LA Fixed Effects

Note: Each coefficient results from a multiple regression of the sort $HighExposure_s = \beta_0 + \beta_1 x_{1,s} + \beta_2 x_{2,s} + ... + \beta_k x_{k,s} + \varepsilon_s$ whereby the dependent variable is a binary indicator for whether the school was highly exposed to the policy in school year 2013/14 (above median, i.e. 83% of school population non-FSM takers) and the predictors are the variables listed on the left-hand side of the figure. The whiskers indicate 95% level confidence intervals. The analysis is run on a cross-section of 1,630 school in school year 2013/14.

Figure 7: Event Study



Note: Coefficients are obtained by estimating γ_t from Equation 4. Each coefficient can be interpreted as the difference in outcome for that period relative to the reference period, which is school year 2012/2013, the earliest we observe the outcomes before the change in policy. Time is expressed in number of periods to and from the reference period, which is 0. Period one is year 2014/2015, namely when UFSM is in force. Exposure is defined as the % of school population not-taking FSM in 2013, one year before the policy change and outcomes are in levels. Data span from school year 2003/2004 through to 2016/2017 and outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. Whiskers represent coefficients' 95% confidence interval. Standard errors are clustered at the school level.

Appendix A

Table A1: Main Results - Unbalanced Panel

Panel A: Attendance	(1)	(2)	(3)	(4)
Post × Exposure	0.009***	0.010***	0.001	0.009***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	18,346	17,995	17,995	17,995
No. of Schools	1714	1714	1714	1714
Mean Dep. Var.	95.03	95.03	95.03	95.03
SD Dep. Var.	1.58	1.58	1.58	1.58
R-squared	0.764	0.76	0.712	0.765
Panel B: Illnesses	(1)	(2)	(3)	(4)
Post × Exposure	-0.009***	-0.011***	-0.003	-0.009***
	(0.003)	(0.003)	(0.003)	(0.002)
Observations	18,365	18,010	18,010	18,010
No. of Schools	1714	1714	1714	1714
Mean Dep. Var.	2.58	2.58	2.58	2.58
SD Dep. Var.	1.54	1.54	1.54	1.54
R-squared	0.584	0.585	0.44	0.666
Panel C: Exclusions	(1)	(2)	(3)	(4)
Post × Exposure	0.001	0.000	0.000	0.001
	(0.001)	(0.001)	(0.002)	(0.001)
Observations	17,872	17,537	17,537	17,537
No. of Schools	1714	1714	1714	1714
Mean Dep. Var.	0.19	0.19	0.19	0.19
SD Dep. Var.	0.49	0.49	0.49	0.49
R-squared	0.323	0.304	0.200	0.308
Baseline Controls		✓	✓	✓
School FE	✓	✓	✓	✓
School Trends			✓	
Local Authories Trends				✓

Notes: Coefficients are obtained by estimating γ from Equation 3. Outcomes are calculated in % of all possible half-day openings. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) score and school average class size in 2004, both interacted with year dummies, alongside indicators for urban and religious all appropriately interacted with year dummies. Data span from school year 2003/2004 through to 2016/2017. Outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. This sample does not include schools which extended eligibility following local initiatives from 2010. This sample also includes schools which are not observed every year. Standard errors (in parentheses) are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Main Results (log)

	(1)	(2)	(3)	(4)
Panel A: ln(Attendance)				
Post × Exposure	0.000***	0.000***	0.000	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	17,766	17,766	17,766	17,766
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	4.55	4.55	4.55	4.55
SD Dep. Var.	0.02	0.02	0.02	0.02
R-squared	0.761	0.762	0.714	0.766
Panel B: ln(1 + Illnesses)	(1)	(2)	(3)	(4)
Post × Exposure	-0.003**	-0.003***	-0.002*	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	17,781	17,781	17,781	17,781
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	1.14	1.14	1.14	1.14
SD Dep. Var.	0.58	0.58	0.58	0.58
R-squared	0.597	0.600	0.421	0.701
Panel C: ln(1 + Exclusions)	(1)	(2)	(3)	(4)
Post × Exposure	0.001	0.000	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	17,308	17,308	17,308	17,308
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	0.13	0.13	0.13	0.13
SD Dep. Var.	0.25	0.25	0.25	0.25
R-squared	0.386	0.387	0.289	0.389
Baseline Controls		✓	✓	✓
School FE	✓	✓	✓	✓
School Trends			✓	
Local Authories Trends				✓

Notes: Coefficients are obtained by estimating γ from Equation 3. Outcomes are calculated in % of all possible half-day openings. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) score and school average class size in 2004, both interacted with year dummies, alongside indicators for urban and religious all appropriately interacted with year dummies. Data span from school year 2003/2004 through to 2016/2017. Outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. Standard errors (in parentheses) are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Table A3: Different Trends Specifications

	(1) Baseline Model	(2) Year FE ×	(3) Linear Trend ×	(4) Post ×
Panel A: Attendance				
Post × Exposure	0.010***	0.011***	0.006***	0.011***
	(0.003)	(0.003)	(0.002)	(0.003)
Observations	17,766	17,766	17,766	17,766
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	95.05	95.05	95.05	95.05
SD Dep. Var.	1.57	1.57	1.57	1.57
R-squared	0.757	0.762	0.759	0.758
Panel B: Illnesses				
	(1) Baseline Model	(2) Year FE ×	(3) Linear Trend ×	(4) Post ×
Post × Exposure	-0.009***	-0.010***	-0.013***	-0.010***
	(0.003)	(0.003)	(0.002)	(0.003)
Observations	17,781	17,781	17,781	17,781
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	2.57	2.57	2.57	2.57
SD Dep. Var.	1.54	1.54	1.54	1.54
R-squared	0.585	0.587	0.586	0.584
Panel C: Exclusions				
	(1) Baseline Model	(2) Year FE ×	(3) Linear Trend ×	(4) Post ×
Post × Exposure	0.001	0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	17,308	17,308	17,308	17,308
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	0.19	0.19	0.19	0.19
SD Dep. Var.	0.48	0.48	0.48	0.48
R-squared	0.300	0.303	0.300	0.297
Baseline Controls		✓	✓	✓
School FE	✓	✓	✓	✓
School Trends				
Local Authories Trends				

Notes: Coefficients are obtained by estimating γ from Equation 3. Outcomes are calculated in % of all possible half-day openings. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) score and school average class size in 2004, both interacted with year dummies, alongside indicators for urban and religious all appropriately interacted with year dummies. Data span from school year 2003/2004 through to 2016/2017. Outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. Standard errors (in parentheses) are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Table A4: Main Results P1-P3 Weighted

	(1)	(2)	(3)	(4)
Panel A: Attendance				
Post × Exposure	0.008***	0.009***	0.002	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	15,662	15,662	15,662	15,662
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	95.05	95.05	95.05	95.05
SD Dep. Var.	1.57	1.57	1.57	1.57
R-squared	0.796	0.797	0.752	0.801
Panel B: Illnesses	(1)	(2)	(3)	(4)
Post × Exposure	-0.011***	-0.013***	-0.005	-0.009***
	(0.004)	(0.004)	(0.004)	(0.003)
Observations	15,681	15,681	15,681	15,681
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	2.57	2.57	2.57	2.57
SD Dep. Var.	1.54	1.54	1.54	1.54
R-squared	0.604	0.606	0.456	0.687
Panel C: Exclusions	(1)	(2)	(3)	(4)
Post × Exposure	0.003**	0.002*	0.001	0.003*
	(0.001)	(0.001)	(0.002)	(0.001)
Observations	15,177	15,177	15,177	15,177
No. of Schools	1630	1630	1630	1630
Mean Dep. Var.	0.19	0.19	0.19	0.19
SD Dep. Var.	0.48	0.48	0.48	0.48
R-squared	0.342	0.346	0.238	0.350
Baseline Controls		✓	✓	✓
School FE	✓	✓	✓	✓
School Trends			✓	
Local Authories Trends				✓

Notes: Coefficients are obtained by estimating γ from Equation 3. Outcomes are calculated in % of all possible half-day openings. Regressions are weighted using percentage of P1-P3 students in 2013/2014 as weights. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) score and school average class size in 2004, both interacted with year dummies, alongside indicators for urban and religious all appropriately interacted with year dummies. Data span from school year 2003/2004 through to 2016/2017. Outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. Standard errors (in parentheses) are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Table A5: Main Results - No Local Initiatives

Panel A: Attendance	(1)	(2)	(3)	(4)
Post × Exposure	0.011***	0.012***	0.004	0.012***
	(0.003)	(0.003)	(0.004)	(0.003)
Observations	16,097	16,097	16,097	16,097
No. of Schools	1477	1477	1477	1477
Mean Dep. Var.	95.01	95.01	95.01	95.01
SD Dep. Var.	1.58	1.58	1.58	1.58
R-squared	0.764	0.765	0.726	0.769
Panel B: Illnesses	(1)	(2)	(3)	(4)
Post × Exposure	-0.012***	-0.012***	-0.002	-0.008***
	(0.003)	(0.003)	(0.004)	(0.003)
Observations	16,120	16,120	16,120	16,120
No. of Schools	1477	1477	1477	1477
Mean Dep. Var.	2.58	2.58	2.58	2.58
SD Dep. Var.	1.56	1.56	1.56	1.56
R-squared	0.582	0.584	0.437	0.665
Panel C: Exclusions	(1)	(2)	(3)	(4)
Post × Exposure	0.001	0.001	-0.000	0.001
	(0.001)	(0.002)	(0.002)	(0.002)
Observations	15,687	15,687	15,687	15,687
No. of Schools	1477	1477	1477	1477
Mean Dep. Var.	0.19	0.19	0.19	0.19
SD Dep. Var.	0.49	0.49	0.49	0.49
R-squared	0.309	0.312	0.206	0.316
Baseline Controls		✓	✓	✓
School FE	✓	✓	✓	✓
School Trends			✓	
Local Authories Trends				✓

Notes: Coefficients are obtained by estimating γ from Equation 3. Outcomes are calculated in % of all possible half-day openings. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) score and school average class size in 2004, both interacted with year dummies, alongside indicators for urban and religious all appropriately interacted with year dummies. Data span from school year 2003/2004 through to 2016/2017. Outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. This sample does not include schools which extended eligibility following local initiatives from 2010. Standard errors (in parentheses) are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Table A6: Main Results - No Pilot

Panel A: Attendance	(1)	(2)	(3)	(4)
Post × Exposure	0.013***	0.013***	0.004	0.012***
	(0.003)	(0.003)	(0.004)	(0.003)
Observations	13,756	13,756	13,756	13,756
No. of Schools	1263	1263	1263	1263
Mean Dep. Var.	95.24	95.24	95.24	95.24
SD Dep. Var.	1.44	1.44	1.44	1.44
R-squared	0.727	0.728	0.668	0.731
Panel B: Illnesses	(1)	(2)	(3)	(4)
Post × Exposure	-0.007**	-0.008***	0.001	-0.009***
	(0.003)	(0.003)	(0.004)	(0.003)
Observations	13,766	13,766	13,766	13,766
No. of Schools	1263	1263	1263	1263
Mean Dep. Var.	2.73	2.73	2.73	2.73
SD Dep. Var.	1.44	1.44	1.44	1.44
R-squared	0.571	0.575	0.473	0.662
Panel C: Exclusions	(1)	(2)	(3)	(4)
Post × Exposure	-0.000	-0.001	-0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	13,424	13,424	13,424	13,424
No. of Schools	1263	1263	1263	1263
Mean Dep. Var.	0.18	0.18	0.18	0.18
SD Dep. Var.	0.47	0.47	0.47	0.47
R-squared	0.267	0.271	0.173	0.275
Baseline Controls		✓	✓	✓
School FE	✓	✓	✓	✓
School Trends			✓	
Local Authories Trends				✓

Notes: Coefficients are obtained by estimating γ from Equation 3. Outcomes are calculated in % of all possible half-day openings. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) score and school average class size in 2004, both interacted with year dummies, alongside indicators for urban and religious all appropriately interacted with year dummies. Data span from school year 2003/2004 through to 2016/2017. Outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. This sample does not include schools which took part in the FSM pilot in school year 2007/08. Standard errors (in parentheses) are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Table A7: Secondary Schools Triple DiD

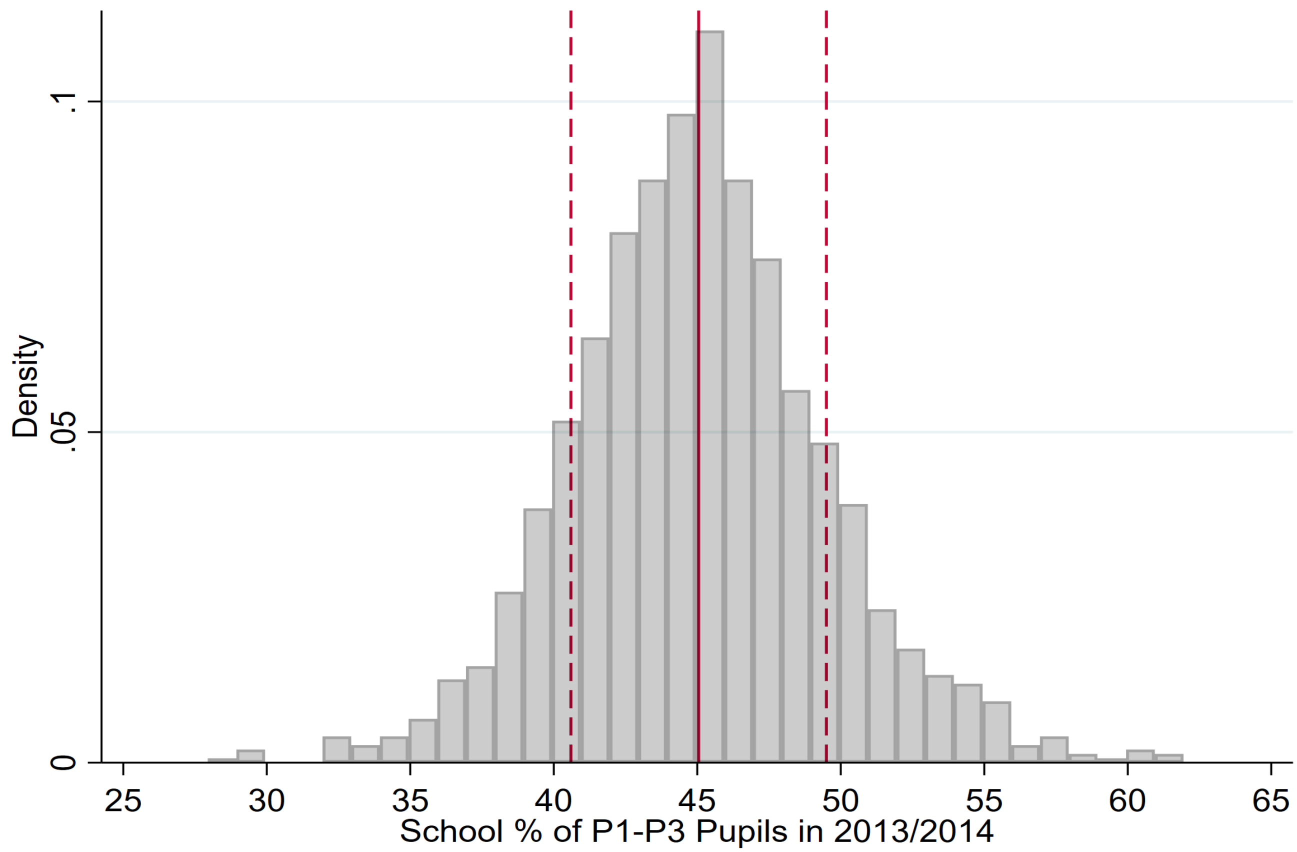
	(1)	(2)	(3)	(4)
Panel A: ln(Attendance)				
Post × Exposure × Primary	0.001***	0.001***	-0.000	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	21,209	21,209	21,209	20,547
No. of Schools	1949	1949	1949	1949
Mean Dep. Var.	4.55	4.55	4.55	4.55
SD Dep. Var.	0.03	0.03	0.03	0.03
R-squared	0.844	0.845	0.806	0.841
Panel B: ln(1 + Illnesses)	(1)	(2)	(3)	(4)
Post × Exposure × Primary	0.010**	0.009**	0.001	0.003
	(0.005)	(0.004)	(0.005)	(0.003)
Observations	21,346	21,346	21,346	20,683
No. of Schools	1949	1949	1949	1949
Mean Dep. Var.	1.19	1.19	1.19	1.19
SD Dep. Var.	0.61	0.61	0.61	0.61
R-squared	0.614	0.616	0.436	0.725
Panel C: ln(1 + Exclusions)	(1)	(2)	(3)	(4)
Post × Exposure × Primary	-0.019***	-0.019***	-0.002	-0.016***
	(0.003)	(0.003)	(0.004)	(0.003)
Observations	20,850	20,850	20,850	20,187
No. of Schools	1949	1949	1949	1949
Mean Dep. Var.	0.25	0.25	0.25	0.25
SD Dep. Var.	0.40	0.40	0.40	0.40
R-squared	0.725	0.713	0.664	0.677
Baseline Controls		✓	✓	✓
School FE	✓	✓	✓	✓
School Trends			✓	
Local Authories Trends				✓

Notes: Coefficients are obtained by estimating γ from Equation 5. Outcomes are calculated in % of all possible half-day openings and are in logarithmic form. Exclusion rate is reported per 1,000 students and refer to the number of half-days missed on the account of temporary exclusion. Baseline controls include Scottish Index of Multiple Deprivation (SIMD) score and school average class size in 2004, both interacted with year dummies, alongside indicators for urban and religious all appropriately interacted with year dummies. Data span from school year 2003/2004 through to 2016/2017. Outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. Standard errors (in parentheses) are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Figure A1: Trends

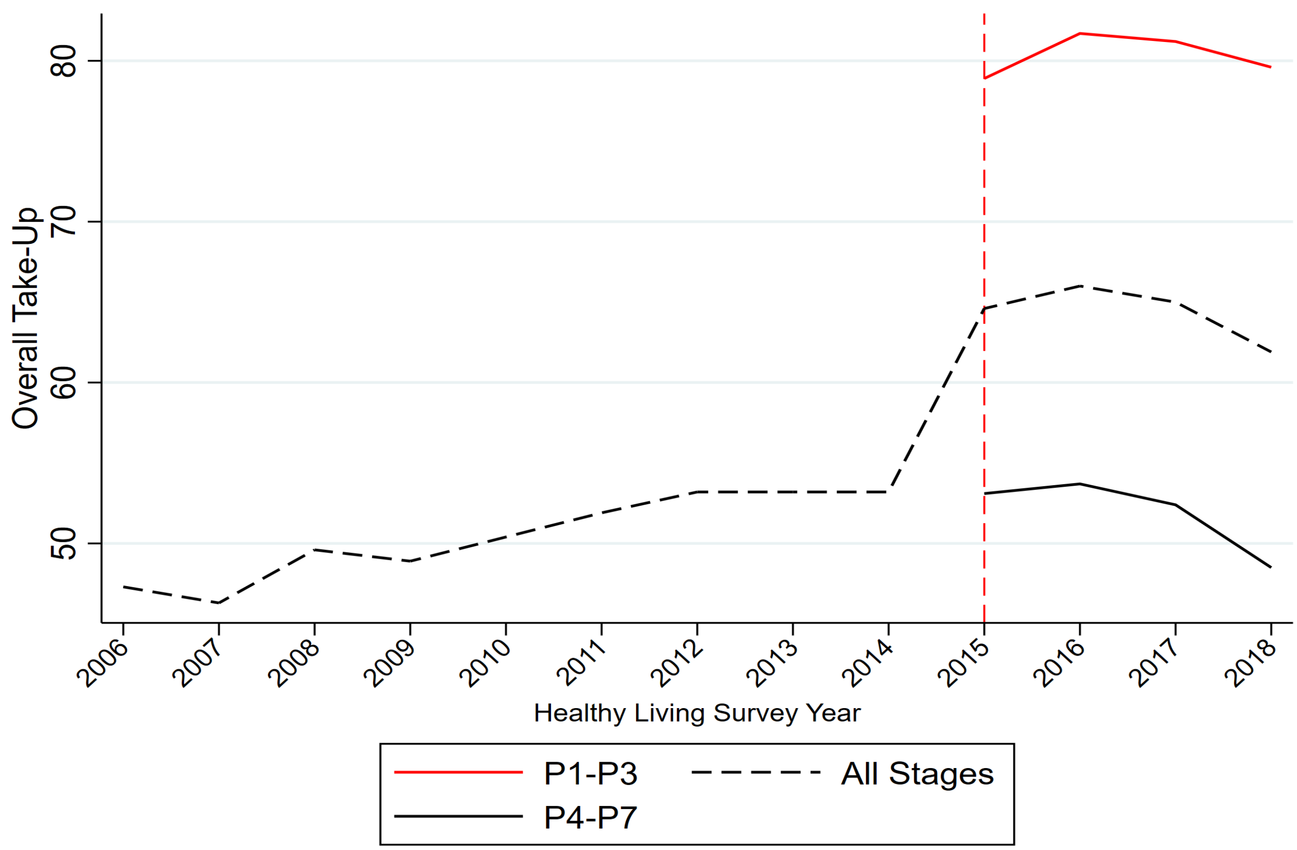
Notes: Our sample seems to follow the same trends (except for FSM uptake around 2008) of the official statistics released within SG reports. However, there it seems to be a systematic overstatement of all the variables. We suspect this is due to statistical disclosure control which suppresses values between 0 and 5. This way, the variable in question will necessarily have a large mean as the smallest values are removed from the sample.

Figure A2: P1-P3 distribution in School Year 2013/2014



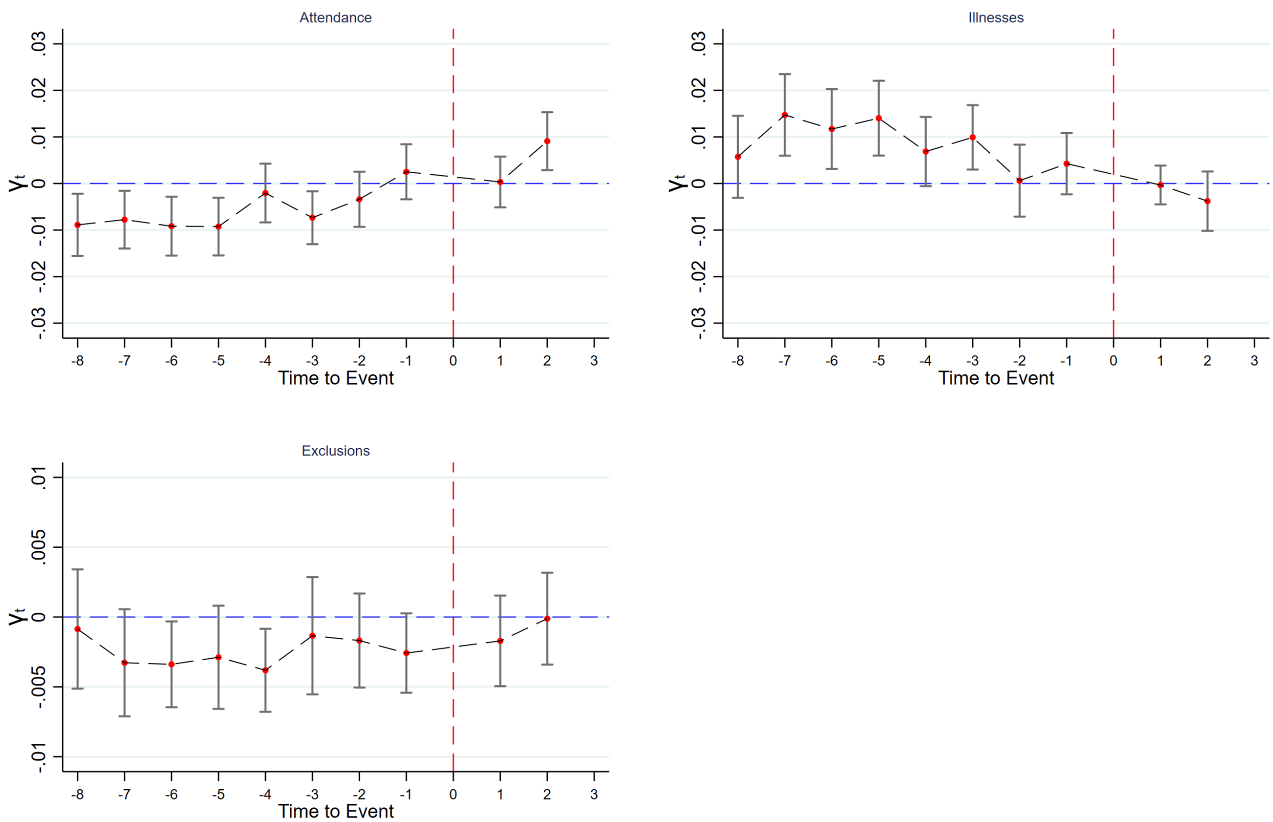
Notes: This is the distribution of P1-P3 pupils in school year 2013/2014, the one immediately before UFSM implementation. The solid red line is the average, whereas the dashed lines identify the mean +/- one standard deviation. Each bin correspond to one percentage point.

Figure A3: FSM vs PSM Uptakes



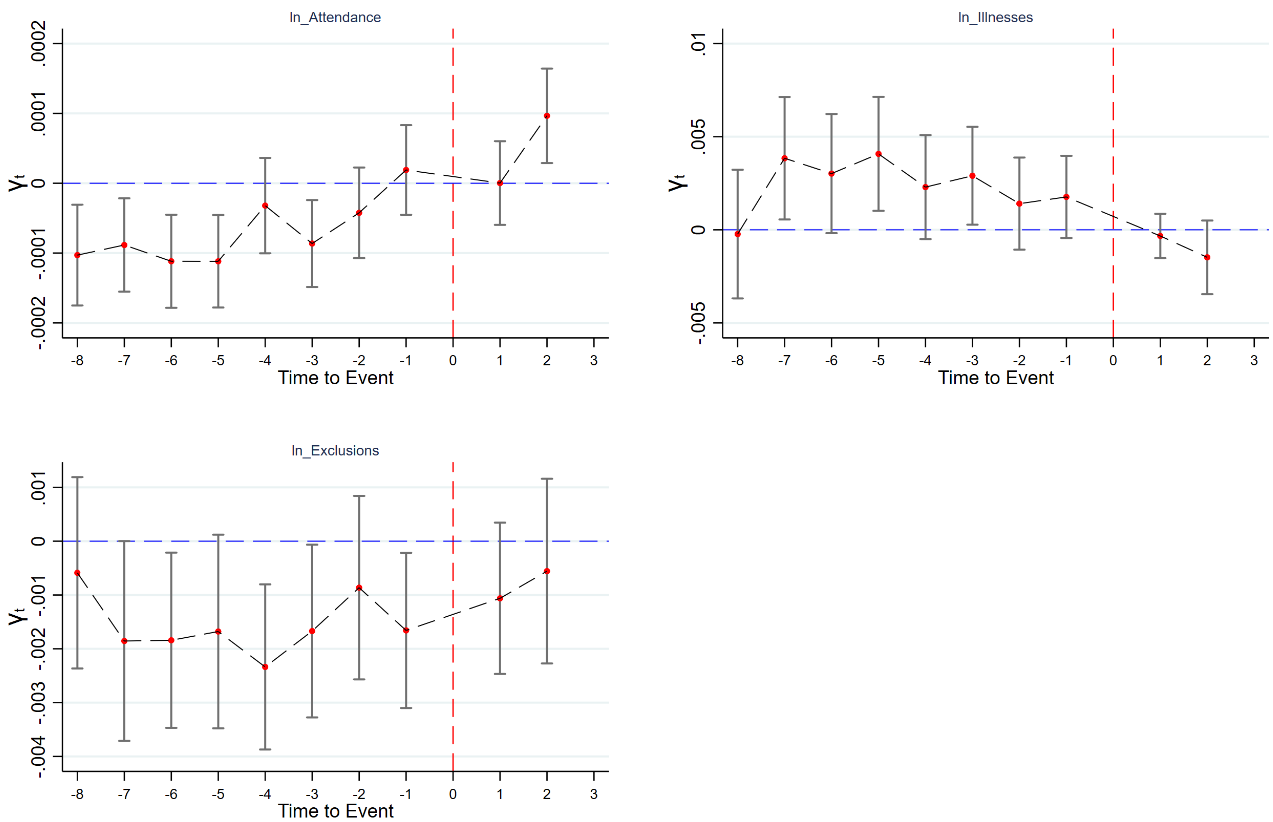
Note: The above chart is calculated as raw yearly-averages of the overall school meal take-up, i.e. $\frac{\#Meal-Takers}{School\ Population}$. Because the survey is run in one day, the raw counts refer to pupils present on the day of the survey. The dotted black line represents the entire primary school-level trend. The solid red line dis-aggregates the trend for the P1-P3 group (targeted by the policy) whereas the solid black line shows the P4-P7 trend. The breakdown of registration and take-up is only available, at the national level, starting from HLS wave 2015.

Figure A4: Event Study - Unbalanced Panel



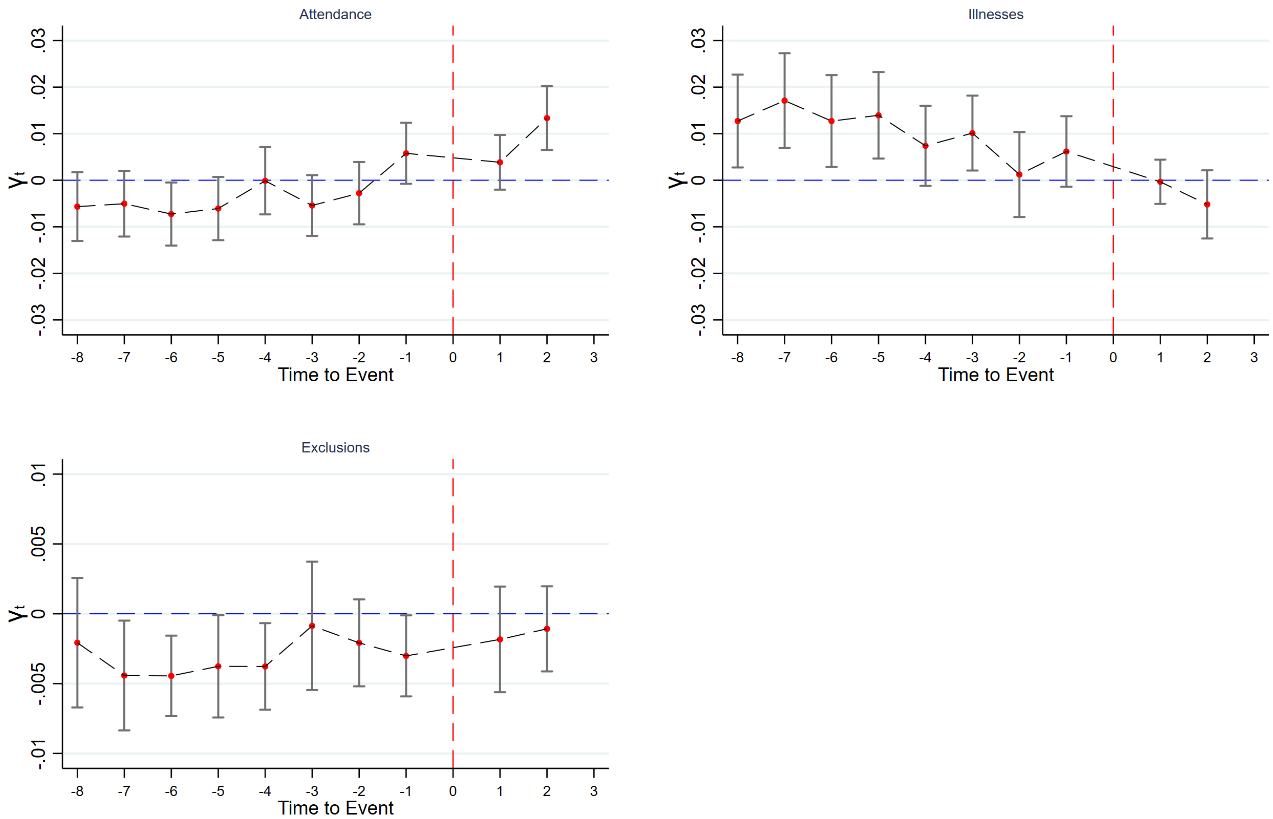
Notes: Coefficients are obtained by estimating γ_t from Equation 4. Each coefficient can be interpreted as the difference in outcome for that period relative to the reference period, which is school year 2012/2013, the earliest we observe the outcomes before the change in policy. Time is expressed in number of periods to and from the reference period, which is 0. Period one is year 2014/2015, namely when UFSM is in force. Exposure is defined as the % of school population not-taking FSM in 2013, one year before the policy change and outcomes are in levels. Data span from school year 2003/2004 through to 2016/2017 and outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. This sample also includes schools which are not observed every year. Whiskers represent coefficients' 95% confidence interval. Standard errors are clustered at the school level.

Figure A5: Event Study in Logarithmic Form



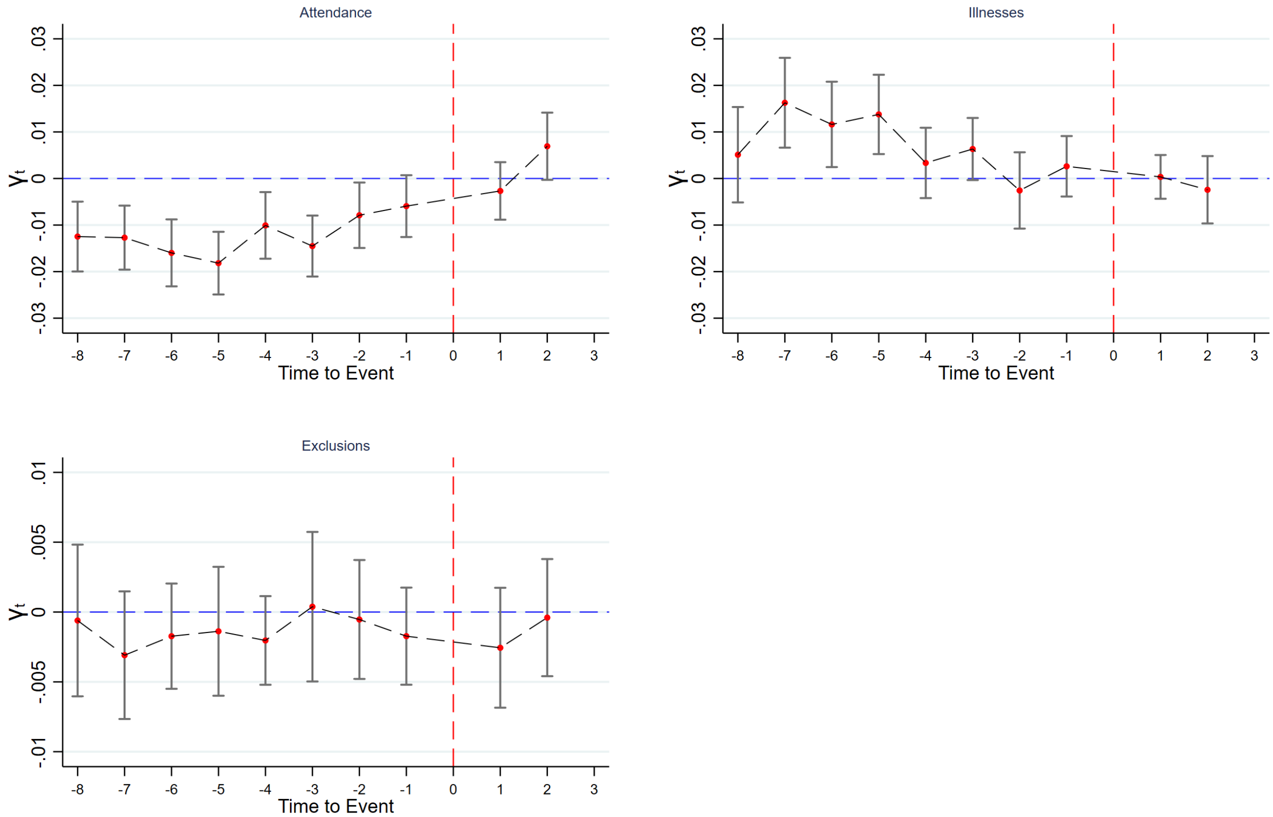
Notes: Coefficients are obtained by estimating γ_t from Equation 4. Each coefficient can be interpreted as the difference in outcome for that period relative to the reference period, which is school year 2012/2013, the earliest we observe the outcomes before the change in policy. Time is expressed in number of periods to and from the reference period, which is 0. Period one is year 2014/2015, namely when UFSM is in force. Exposure is defined as the % of school population not-taking FSM in 2013, one year before the policy change and outcomes are in log. Data span from school year 2003/2004 through to 2016/2017 and outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. Whiskers represent coefficients' 95% confidence interval. Standard errors are clustered at the school level.

Figure A6: Event Study - No Local Initiatives



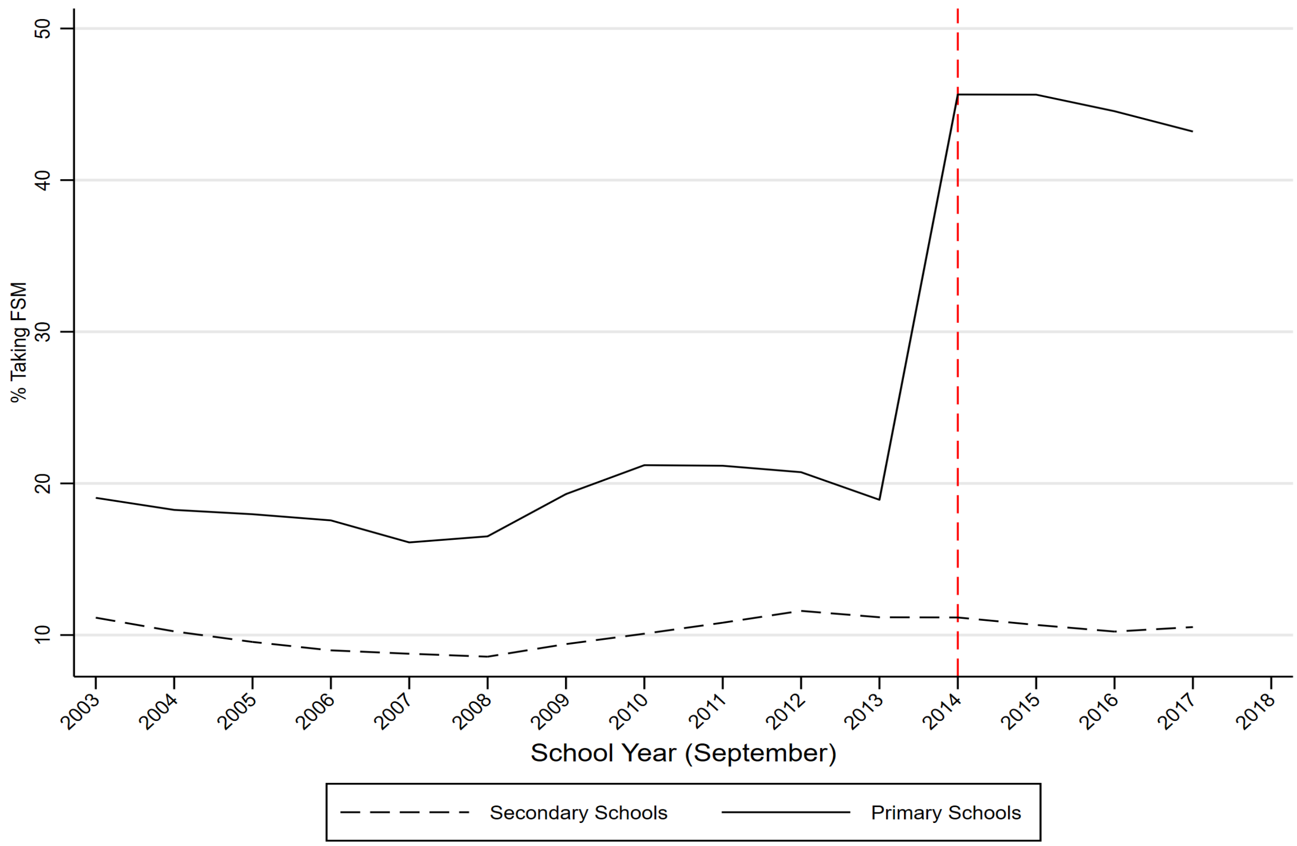
Notes: Coefficients are obtained by estimating γ_t from Equation 4. Each coefficient can be interpreted as the difference in outcome for that period relative to the reference period, which is school year 2012/2013, the earliest we observe the outcomes before the change in policy. Time is expressed in number of periods to and from the reference period, which is 0. Period one is year 2014/2015, namely when UFSM is in force. Exposure is defined as the % of school population not-taking FSM in 2013, one year before the policy change and outcomes are in levels. Data span from school year 2003/2004 through to 2016/2017 and outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. This sample does not include schools which extended eligibility following local initiatives from 2010. Whiskers represent coefficients' 95% confidence interval. Standard errors are clustered at the school level.

Figure A7: Event Study - No Pilot



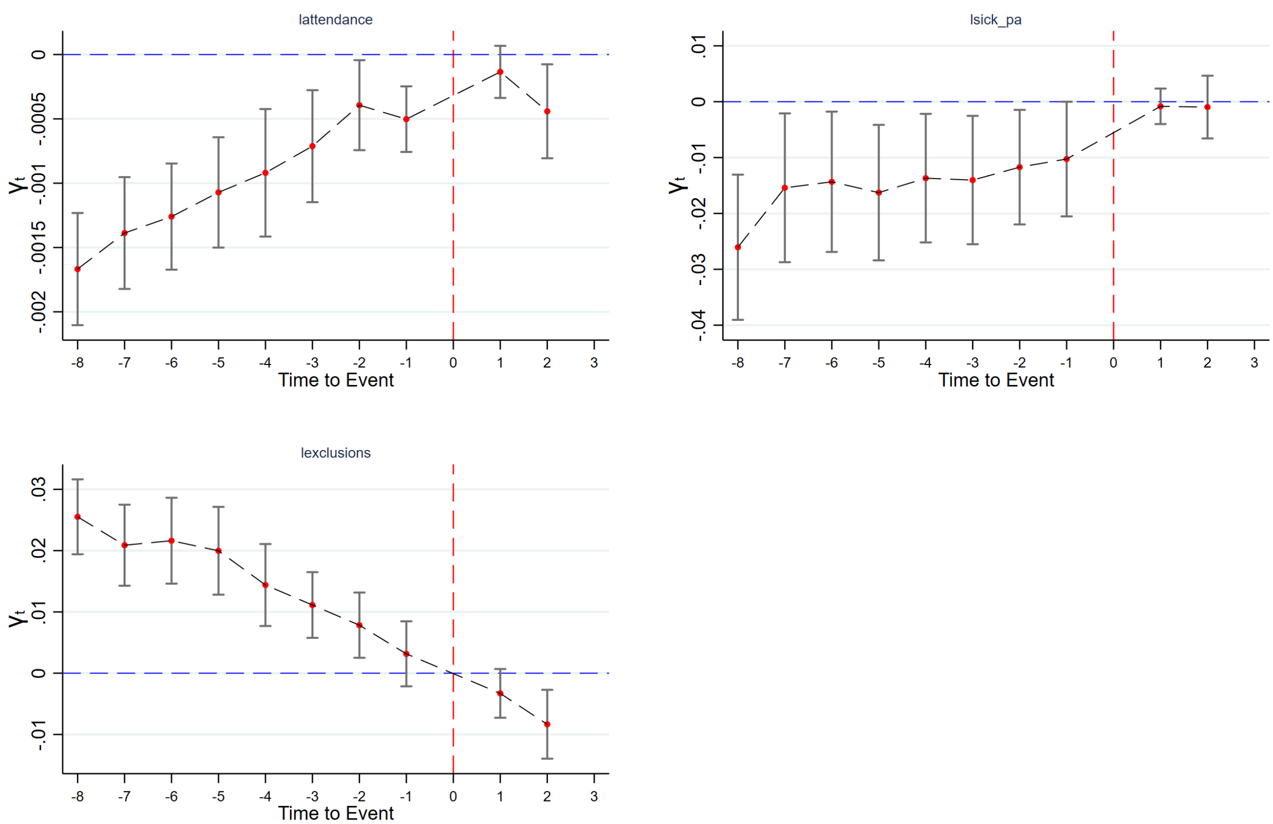
Notes: Coefficients are obtained by estimating γ_t from Equation 4. Each coefficient can be interpreted as the difference in outcome for that period relative to the reference period, which is school year 2012/2013, the earliest we observe the outcomes before the change in policy. Time is expressed in number of periods to and from the reference period, which is 0. Period one is year 2014/2015, namely when UFSM is in force. Exposure is defined as the % of school population not-taking FSM in 2013, one year before the policy change and outcomes are in levels. Data span from school year 2003/2004 through to 2016/2017 and outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. This sample does not include schools which took part in the FSM pilot in school year 2007/08. Whiskers represent coefficients' 95% confidence interval. Standard errors are clustered at the school level.

Figure A8: Uptakes Trends Primary vs Secondary Schools



Notes: The above trends are calculated as raw yearly-averages of the following ratio: % Taking FSM: $\frac{\#FSM{-}Takers}{School\ Population}$ separately for primary and secondary schools, which were not targeted by the policy.

Figure A9: Secondary Schools Triple DiD Event Study



Notes: Coefficients are obtained by estimating γ_t from Equation 4. Each coefficient can be interpreted as the difference in outcome for that period relative to the reference period, which is school year 2012/2013, the earliest we observe the outcomes before the change in policy. Time is expressed in number of periods to and from the reference period, which is 0. Period one is year 2014/2015, namely when UFSM is in force. Exposure is defined as the % of school population not-taking FSM in 2013, one year before the policy change and outcomes are in log. Data span from school year 2003/2004 through to 2016/2017 and outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. Whiskers represent coefficients' 95% confidence interval. Standard errors are clustered at the school level.

Appendix B

6.1 National Eligibility Criteria

The national criteria for eligibility to free school meals includes the following:[28]

- Pupils within families who receive Income Support, Income-based Job Seekers Allowance or any income related element of Employment and Support Allowance.
- Pupils within families who receive support under Part VI of the Immigration and Asylum Act 1999.
- Pupils whose parents or carers receive Child Tax Credit, do not receive Working Tax Credit and had an annual income (as assessed by the Inland Revenue) of below £16,105 (from April 2013).
- Pupils whose parents or carers are in receipt of both Child Tax Credit and Working Tax Credit and their income is up to £6,900 were also entitled (from August 2009).
- Pupils whose parents or carers are in receipt of Universal Credit and their monthly earned income does not exceed £610 were also entitled (from August 2017).
- Pupils in school education who receive any of these benefits in their own right are also entitled to receive free school meals.

Table B1: Sample Description

School Year	SPC Wave	HLS Wave	School Meals Data	Attendance Data	Final Sample
2003/2004	2003	2004	✓	✓	✓
2004/2005	2004	2005	✓	✓	✓
2005/2006	2005	2006	✓	✓	✓
2006/2007	2006	2007	✓	✓	✓
2007/2008	2007	2008	✓	✓	✓
2008/2009	2008	2009	✓	✓	✓
2009/2010	2009	2010	✓	✓	✓
2010/2011	2010	2011	✓	✓	✓
2011/2012	2011	2012	✓		
2012/2013	2012	2013	✓	✓	✓
2013/2014	2013	2014	✓		
2014/2015	2014	2015	✓	✓	✓
2015/2016	2015	2016	✓		
2016/2017	2016	2017	✓	✓	✓
2017/2018	2017	2018	✓		
2018/2019	2018	2019	FSM-Registration Only	✓	

Notes: Scottish Pupils Census (SPC) is run every school year in September. School Meals Survey (subsequently renamed Healthy Living Survey, HLS) is run in February of the school year beginning the previous August. After 2010/2011 school year attendance and absence survey were administered every second year. This explains the gaps.

[28]Source: School Healthy Living Survey Statistics: 2020. Scottish Government.