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Does the provision of universal free school meals improve school attendance?

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ABSTRACT

We examine the effects of universal free school meal (UFSM) policies on school attendance and health-related absences. 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 school-level exposure to the policy and find that, in spite of a large increase in take-up rates, attendance did not improve. Using an alternative exposure measure that includes those pupils switching from paid to free school meals, we find small positive and negative effects on attendance and health-related absences respectively. These findings would suggest the presence of a channel whereby financial savings by families encourage attendance, but these effects are too small to be considered economically meaningful.

1. Introduction

Free school meals (henceforth, FSM) programmes have received considerable attention following the disruption to their provision caused by Covid-19 school closures. This affected approximately 300 million children worldwide, including 1.6 million in the United Kingdom. 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 a household's financial circumstances, to school-wide or even school system-wide universal provision. For example, 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 pupils. Around the same time, UK nations 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 the first three years of primary school, in September 2014, followed by Scotland in January 2015.[2]

A broad literature documents the beneficial effects of school meal programmes, which are linked to gains in academic performance (Belot & James, 2011; Chakraborty & Jayaraman, 2019; Gordanier, Ozturk, Williams, & Zhan, 2020; Holford & Rabe, 2020; Ruffini, 2021; Schwartz & Rothbart, 2020). In contexts where not all eligible pupils take up free school meals, universal provision is aimed at achieving multiple desirable pupil outcomes, such as raising educational attainment, improving social skills and behaviour, and providing a healthy diet. Insofar as free school meals lead to a healthier diet for children – and especially children who would not have access to healthy meals through their home diets – this can translate into improved school outcomes through a number of channels. Better nutrition can improve cognitive development (Sorhaindo & Feinstein, 2006), increase attainment (Alderman, Hoddinott, & Kinsey, 2006; Glewwe, Jacoby, & King, 2001; Victora et al., 2008; Winicki & Jemison, 2003), reduce the risk of illnesses (and thus school absences), and reduce the incidence of violent and anti-social behaviour (Benton, 2007). Moreover, since FSM take-up can be associated with stigma and consequently victimisation when means-tested, such adverse effects may be mitigated by making the provision universal.

Nonetheless, FSM implementation continues to be a controversial topic, both among policy-makers and researchers. Whilst some argue

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

[2] In England these are Reception Year, Year 1 and Year 2, whereas in Scotland these are referred to as Primary 1, Primary 2 and Primary 3.

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that FSM programmes do not improve children's health (Corcoran, Elbel, & Schwartz, 2016) and can even raise the risk of unhealthy body weight (Abouk & Adams, 2022; Polonsky et al., 2019; Schanzenbach, 2009), the core of the debate relates to the trade-off between the cost of universal implementation, and its effectiveness when provision is means-tested. In fact, the stigma attached to FSM status might explain imperfect take-up rates observed under means-tested provision. In such a case, FSM expansion can raise take-up not just among previously ineligible pupils but also among previously eligible ones (Corcoran et al., 2016; Holford, 2015; Leos-Urbel, Schwartz, Weinstein, & Corcoran, 2013; Ruffini, 2021).

Emerging evidence on the effects of universal provision also points to benefits in terms of higher academic performance (Holford & Rabe, 2020; Schwartz & Rothbart, 2020) and improved body weight outcomes (Schwartz & Rothbart, 2020; Holford & Rabe, 2022), as well as improvements in labour market outcomes (Bütikofer, Mølland, & Salvanes, 2018; Lundborg, Rooth, & Alex-Petersen, 2021) and household finances and nutrition (Bhattacharya, Currie, & Haider, 2006; Handbury & Moshary, 2021; Marcus & Yewell, 2021; Ozturk, Pekgun, & Ruffini, 2021).[3] A small literature focuses on the effects of universal free school meal policies on pupil behaviour in a U.S. (Cuadros-Meñaca, Thomsen, & Nayga, 2021; Gordon & Ruffini, 2021) or South Korean (Altindag, Baek, Lee, & Merkle, 2020) context. However, there is still little evidence of the effects of these policies on school attendance and absenteeism. These are strong correlates of the 'Big Five' personality traits, and thus of non-cognitive skills, which have been proven to be powerful predictors of adult life outcomes (see Chetty et al., 2011; Heckman & Rubinstein, 2001; Lindqvist & Vestman, 2011).[4] For example, absenteeism is linked to worse academic attainment (Aucejo & Romano, 2016; Gottfried, 2009, 2011; Klein, Sosu, & Dare, 2022; Liu, Lee, & Gershenson, 2021). We therefore believe this outcome is particularly important, especially in light of the recent school attendance crisis experienced in the UK and worldwide, and deserves further investigation.[5]

To fill this gap in the literature we look at the effects of Scotland's Universal Free School Meals (UFSM) programme on attendance and 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 years of primary school, regardless of their families' financial circumstances.

For our analysis, we use data on the universe of Scottish primary schools from the 2006/07 school year until 2016/17. We observe the share of pupils registered for free meals, alongside take-up rates. In our setup, all primary schools in Scotland 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, Lewis, and Postel (2018). We leverage different levels of exposure to the policy, as determined by variation in the fraction of the school population taking school meals prior to the change in policy. We find that schools with a higher fraction of non-school meal takers (higher exposure to the policy) experienced an increase in uptake of about 12 percentage points more than their lower exposure counterparts.

We match school-level take-up data to attendance and absence records, alongside a rich set of school characteristics. Our data on school meal take-up is unique in that we can distinguish the take-up of free and paid school meals, before and after the policy roll-out. We present two main results. First, despite the significant increase in take-up, we find that UFSM had only a minimal impact on attendance and health-related absences. In particular, a 10 percentage points increase in free school meal take-up translates into less than one school day gained. These results are precisely estimated. While the effects are slightly attenuated for schools in urban and more deprived areas, the overall null effect remains for all subgroups (based on school size, and resources) that we investigate. Second, we decompose our overall treatment effect across two key pupil groups: those taking school meals for the first time due to UFSM ('first-timers') and those who used to take paid meals but now get them for free ('switchers'). We find a small positive effect on attendance, along with a small reduction in health-related absences, for the latter group, suggesting a financial channel (school meals becoming less costly) is at play. Nonetheless, these effects are too small to be economically important.

Our study provides two main contributions to the literature and in turn the policy debate. The first contribution relies on the outcome of interest, which has so far been understudied in relation to UFSM policies, despite its importance.

There are three main reasons why UFSM provision can improve school attendance. First, a nutritionally balanced diet reduces the risk of illnesses. Second, the financial savings resulting from free provision raises the opportunity cost of keeping children out of school. Third, universal provision might make school more pleasant, by reducing the stigma typically associated with the take-up of means-tested benefits.

To date, there is limited and mixed evidence on the effect of FSM programmes on absenteeism, with most studies pointing towards small or even null effects (Corcoran et al., 2016; Cuadros-Meñaca, Thomsen, & Nayga, 2022; Gordanier et al., 2020; Holford & Rabe, 2020) and there is even less evidence on the effects of universal provision. Our work closely relates to Holford and Rabe (2020), who examine the effect of UIFSM on school absence in England, in that we investigate a similar policy context and roll-out. However, our work contains at least one important departure. We leverage a rich survey of school meal take-up at the school level, which enables us to separate the take-up of free and paid school meals, before and after the policy roll-out. This allows us to break down the effect of the policy by the type of meal takers, i.e. whether taking school meals for the first time or switching from paid (or home-packed) to free school meals. We show that there is considerable heterogeneity in take-up and treatment effects across these sub-groups, and distinguishing between them forms a crucial part of our analysis of UFSM policies. Holford and Rabe (2020) find that the effect of UFSM on attendance is concentrated among pupils who were previously eligible for free school meals – perhaps through higher take-up due to lower stigma. We find that another potential channel through which free school meals improve attendance is financial savings for families. Furthermore, unlike most previous studies, which cannot distinguish absences by type, we are able to observe the reason for each absence, which allows us to assess on health-related absenteeism.

A second important contribution is that ours is among the first studies to examine the effects of UFSM provision within the Scottish context, which is characterised by high rates of childhood obesity and a resulting strong policy focus on school meals and food standards.[6] Under these circumstances, universal provision of school meals could also

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

[4] Agreeableness, conscientiousness, neuroticism are associated with absences, tardiness and anti-social behaviour (see for example, Barbaranelli, Caprara, Rabasca, & Pastorelli, 2003; Carneiro, Crawford, & Goodman, 2007; Duckworth, Peterson, Matthews, & Kelly, 2007; Jackson, 2018; John, Caspi, Robins, Moffitt, & Stouthamer-Loeber, 1994; Lleras, 2008).

[5] See for example, 'The Guardian view on the rise in school absences: a crisis made in government', The Guardian, 24-09-2023; 'School attendance in Scotland drops to record low', TES, 12-12-2023; 'Why School Absences Have 'Exploded' Almost Everywhere', 29-03-2024, The New York Times.

[6] 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). 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% (World Health Organization, 2018).

benefit pupils from higher-income backgrounds whose 'counterfactual' home meals may also be less healthy (Holford & Rabe, 2022). Indeed, UFSM provision in Scotland is likely to have led to healthier diets for pupils (Parnham et al., 2022). Yet, despite these quality improvements and a substantial increase in take-up, our findings suggest that UFSM provision has no meaningful impact on attendance. Even in specifications where we include (or specifically focus on) those switching from paid to free school meals, where we observe positive effects on attendance and negative ones on health-related absences, these effects are too small to be considered economically important. The overall null effect is observed in urban and rural contexts alike and in schools with varying levels of resources, implying that policy effectiveness is not dependent on the school – or area-specific context.

The remainder of the paper is structured as follows. Section 2 discusses the institutional and policy background of UFSM implementation; Section 3 describes the data; Section 4 outlines our identification strategy, while Section 5 presents our results. Section 6 concludes.

2. Background

2.1. Policy context

For most pupils in Scotland, free school meals (FSM) eligibility is means-tested. Typically, children eligible for free school lunches are those whose parents or carers receive either of the following: (a) Income Support, Income-based Job Seekers Allowance or any income-related element of Employment and Support Allowance; (b) support under Part VI of the Immigration and Asylum Act 1999; (c) Child Tax Credit, do not receive Working Tax Credit and had an annual income below an annually assessed threshold; (d) both Child Tax Credit and Working Tax Credit; or (e) Universal Credit benefits (Scottish Government, 2023).[7]

Before 2007, all FSM provision in Scotland was means-tested.[8] Pupils in Scotland typically start first grade (P1) in the year they turn five. During the school year 2007/08, UFSM provision was piloted among P1–P3 pupils in five local authorities, namely East Ayrshire, Fife, Glasgow, Scottish Borders and West Dunbartonshire.[9] As Holford (2015) documents, the trial was announced in the summer of 2007 to be launched in the following October, setting March 2008 as the initial deadline. It was subsequently extended until June, meaning the trial ultimately covered the entire academic year of 2007/08.

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.[10] 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 (Scottish Government, 2011).

UFSM was finally implemented in Scotland's primary schools in January 2015, when all P1–P3 pupils in the country became eligible and were automatically registered for free school meals.[11] The policy, which was associated with £70.5 million in 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 providing nutritional benefits to children (Beaton, Craig, Katikireddi, Jepson, & Williams, 2014; McAdams, 2016). A stated objective of the policy was to reduce health inequalities. According to official statistics, upon the implementation of UFSM, 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.[12] 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 school pupils were taking school meals, with the P1–P3 fraction increasing to 81.7%. On the other hand, the share of P4–P7 pupils taking school meals remained fairly stable at around 53% (McAdams, 2016). The policy seemed to have achieved at least some of the initial goals. For example, a qualitative evaluation (Ford, Eadie, & Stead, 2015) found that parents identified three main benefits: financial savings, time savings from not having to pack lunches, and school meals being healthier.

2.2. UFSM provision and school attendance

While improving school attendance was not a stated primary goal of the policy, it was acknowledged as a potential secondary effect (Ford et al., 2015, 2016). Generally, UFSM provision may improve attendance via three different channels. First, as school meals are nutritionally richer than shop-bought or home-packed meals (Beaton et al., 2014; Holford & Rabe, 2022), UFSM can improve children's immune systems, leading to fewer health-related absences. This can be referred to as the *health channel*. Second, as UFSM provision saves families approximately £380 per child per year, it incentivises parents to send their children to school more often than they would absent the policy — this is the *financial channel*. Third, by reducing the risk of stigma typically associated with FSM take-up, universal provision can make school a more pleasant experience for pupils, who might therefore be more likely to attend school. This we call the *social channel*. Which channel dominates will depend on the effect UFSM provision has on take-up. If the policy increases take-up among first-time takers, we might expect

[7] See Appendix B for more details.

[8] Even before universal provision, there was increased policy focus on pupils' diets and school meal standards in Scottish schools. Over the last couple of decades, a series of reforms aimed at encouraging healthy eating habits in schools have taken place in the country. 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.

[9] These regions were chosen to cover a wide portion of the country and also based on deprivation (MacLardie, Martin, Murray, & Sewel, 2008).

[10] Prior to UFSM implementation, eligibility alone, whether due to households' financial circumstances or local initiatives, did not automatically entitle pupils to claim a free school meal from the school canteen. This was still contingent on registration. Similarly, registration did not guarantee that pupils were indeed taking free school meals every day.

[11] UFSM implementation was part of a wider government programme targeting pupils' health outcomes. The launch of Better Eating, Better Learning (2014) set out the intention of the government 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.

[12] Scottish Government (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 the 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.

a positive effect on attendance to occur through the health channel, as more pupils will switch from shop-bought or home-packed meals to healthier (free) school meals. At the same time, pupils who were previously paying for their school meals will now take them for free as a result of the policy. Assuming the nutritional content of the meals does not change, the financial channel will likely dominate in this case. Finally, the social channel will be most effective among those pupils who did not take free school meals prior to the policy implementation despite being eligible, through a reduction in the probability of suffering stigma.

A crucial factor of the potential mechanism at play is the nutritional content of the school meals. As mentioned in the previous section, school's food standards were regulated years before UFSM roll-out, with the launch of the Schools (Health Promotion and Nutrition) Scotland Act (2007) and the Nutritional Requirements for Food and Drink in Schools (Scotland) Regulations (2008). Therefore, changing the nutritional content of school meals was not part of UFSM provision. Chambers et al. (2016) conducted a qualitative evaluation of the policy by interviewing school and local authority members of the staff.[13] To accommodate the increasing demand for school meals, many schools introduced packed lunch options, which were still widely perceived as better-quality alternatives to home-packed (or shop-bought) lunches. There were concerns that pupils were not eating enough healthy foods, however this was not driven by the UFSM roll-out *per se*. In fact, it was noted that food choice patterns remained largely unchanged following UFSM provision (Chambers et al., 2016). Therefore, it is unlikely that children (or their parents, on their behalf) switched to different options once they no longer had to take cost into account. A small number of schools perceived a decrease in the quality of the ingredients, i.e. frozen or processed foods, and one school observed a reduction in the size of the portions to minimise waste. Parents, on the other hand, appeared satisfied with the nutritional content of the school meals, and did not perceive any change (Ford et al., 2015). Overall, this is not suggestive of a systematic change in food quality post-implementation, but we cannot completely rule out that the increased scale might have had a small detrimental effect on food quality. If that was the case, it would work against the health channel.

3. Data

3.1. Healthy Living Survey

The main data for this project come from the School Meal Survey, renamed the Healthy Living Survey (HLS) in 2012. The survey takes place in February every year. For every school, it collects the following information: (i) the number of pupils on the school roll; (ii) the number of pupils present on the day of the survey; (iii) the number of pupils registered for FSM; (iv) the number of pupils present and registered for FSM; (v) the number of pupils present and who took a school meal, whether free or paid-for; (vi) the number of pupils present and who took a FSM. Additional information is collected for a subset of waves only. For example, until 2009 the survey also reported 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.[14]

Therefore, for each wave it is possible to calculate free school meal, paid-for school meal and overall school meal take-up rates. Following Holford (2015), we calculate the overall take-up in the following way:

$$\underbrace{\frac{\#School\ Meal-Takers}{School\ Population}}_{\text{Overall Uptake, } U_s} = \overbrace{\frac{\#FSM-Takers}{School\ Population}}^{u_s^f} + \underbrace{\frac{\#PSM-Takers}{School\ Population}}_{u_s^p} \tag{1}$$

whereby the overall take-up rate for school s (U_s) is the sum of free school meal (u_s^f) and paid school meal (u_s^p) take-up rates.[15] Fig. 1 shows the overall take-up rate (left panel), defined as the % of pupils present on the survey day who took a school meal, whether free or paid for. The right panel breaks down overall take-up in free meals (solid line) and paid-for meals (dashed line), and adds the fraction of pupils eating a home-packed lunch (dotted line). The latter has decreased by approximately 10 percentage points, which is equivalent to the increase in overall take-up recorded following the policy (left-hand panel). Essentially, these are the pupils newly taking (free) school meals. The increase in free school meal take-up rates (circa 26 percentage points) is driven to a slightly greater extent by 'switchers' (decline paid school meals) than it is by 'first-timers' (decline home-packed lunches). Table A1 reports descriptive statistics for the various types of take-up.

It is worth reiterating that the policy targets pupils in the first three years of primary school (P1–P3). While we cannot observe a breakdown of registrations and take-up for each school stage throughout our sample period, official statistics from 2015 report this information at the national level. These are plotted in Figure A1a in the Online Appendix. The dotted black line represents the entire primary school-level FSM registration trend. 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 A1b follows the same approach, but it plots the percentage of the primary school population taking free school meals, split by stages.[16] Under the plausible assumption that take-up and registrations did not differ substantially across school stages, 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 take-up rates only changed significantly within the P1–P3 group.[17]

3.2. School information

Data on school characteristics are taken from the 'school contact details' database, alongside the Scottish Pupils Census (SPC), which is conducted shortly after the beginning of every school year. Hence, the 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 postcode, which can be linked to a variety of neighbourhood characteristics; (ii) number of pupils in school; (iii) FTE number of

[13] The qualitative analysis was carried out on a sample of ten schools, split between rural, urban and 'mixed' local authorities. Interviews were conducted with head teachers, head cooks and P1–P3 teachers.

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

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

[16] The official statistics only report the percentage of FSM-registered pupils who took a meal on the survey day. Therefore, by multiplying this by the P1–P3 shares of FSM-registered pupils we obtain our P1–P3 uptake measure. The same approach is applied to obtain P4–P7 take-up rates.

[17] 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.

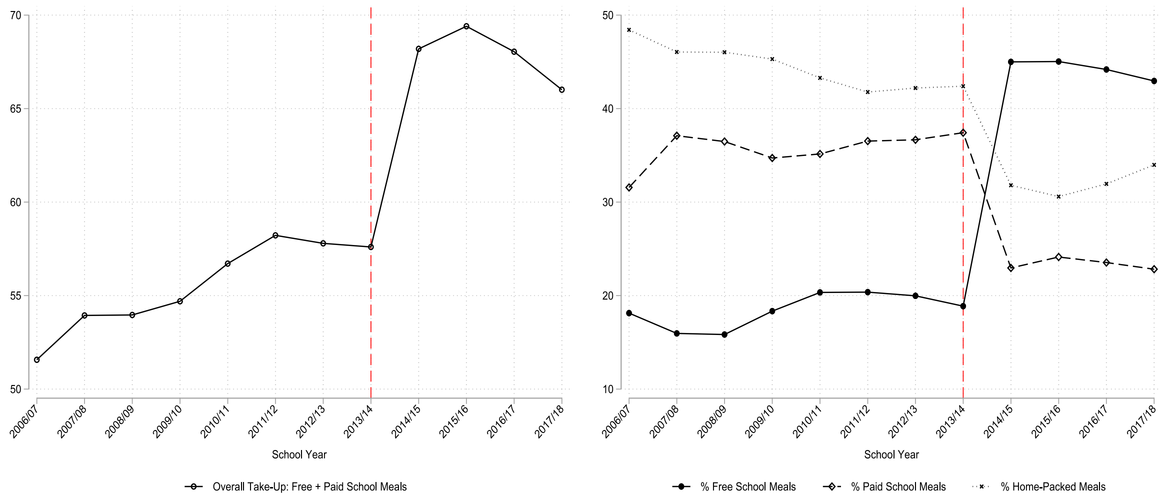

Fig. 1. Trends in take-up rates.
Notes: The above trends are calculated as raw yearly averages of the following ratios: Overall Take-Up: $\frac{\#SchoolMeal-Takers}{School\ Population}$; % Free School Meals: $\frac{\#FSM-Takers}{School\ Population}$; % Paid School Meals: $\frac{\#PSM-Takers}{School\ Population}$; % Home-Packed Meals: $\frac{\#Home-Packed}{School\ Population}$. Because the survey is run in one day, the raw counts refer to pupils present on the day of the survey.

teachers; (iv) fraction of pupils from ethnic minorities; (v) breakdown of pupil numbers by stage; (vi) average class size and fraction of composite classes. We use the 2006 Scottish Index of Multiple Deprivation (SIMD) of the school location as a proxy for school composition. SIMD is an index based on seven domains, i.e. income, employment, health, education, crime, housing and access to services. In our sample, SIMD scores range between 1.71 and 87.44, where a higher score indicates higher levels of deprivation.[18] This information is collected at the data zone level. A data zone is a block containing between 500 and 1,000 people; 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 example, wave 2010 includes attendance and absences which occurred in school year 2010/11.[19]

For a given school in a given year, the survey collects the total number of episodes of attendance, authorised absences, unauthorised absences and exclusions, broken down by reason. These refer to all stages in a 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 towards attendance. Table A2 reports summary statistics for school year 2006/07 through to 2016/17. 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 (99.09%) with a residual part being mostly due to pupils being late and studying whilst being long-term sick.[20] From Panel B we see that about 73% of authorised absences are health-related, followed by nearly 21% of the episodes being attributed to "other" authorised absences. In addition, unauthorised absences are mostly due to holidays and 'only' about 29% are due to truancies.

Rates in panels D and E are calculated in relation to the number of all possible attendances, which for each school corresponds to the number of pupils times the total number of half-day openings.[21] Panel D shows that approximately 2.7% of all school sessions are missed due to illness and even lower shares are attributable to lateness and truancy. Moreover, the average attendance rate (Panel E) is about 95%, whilst authorised and unauthorised absences account for 3.81% and 1.08% of all possible attendance respectively. Finally, exclusions are extremely rare in primary schools (circa .02%).

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 fewer 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 two outcomes with very few instances of SDC suppression, i.e. attendance rate and health-related absences (sickness rate from panel D).

3.4. Analytical sample

Our final sample is a panel including between approximately 1,700 and 2,000 primary schools, spanning from the school year 2006/07 through to 2016/17. Within this interval, we observe school meal take-up and registration for every year. However, because the attendance survey was collected every second year from the school year 2010/11, we experience some gaps in the outcome variables, specifically for school years 2011/12, 2013/14 and 2015/16. Whilst wave 2018/2019 is in principle available to us, we decided not to include it in our analysis for two reasons. First, it 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 analytical sample includes eight time periods. Specifically, we have six pre-treatment periods, namely 2006/07, 2007/08, 2008/09, 2009/10, 2010/11 and 2012/13, and two post-treatment

[18] For more information on the 2006 SIMD please see UK Data Service, 2006.
[19] As mentioned above, HLS waves refer to February of the same school year, i.e. the calendar year following the year when the school year begins.
[20] This most likely entails chronic conditions forcing pupils to stay away from school.
[21] These are about 380 per school year for the vast majority of schools and local authorities.

Table 1
Pre-treatment summary stats by exposure level.

	(1) Full Sample		(2) Low		(3) Middle		(4) High	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Overall Take-Up ($\frac{meal\ takers}{enrolment} \times 100$)	55.52	18.05	68.75	16.05	54.20	13.05	42.90	12.90
% Taking Free School Meals	18.50	14.17	23.65	17.29	18.36	12.56	13.88	9.45
% Taking Paid School Meals	35.75	16.68	43.81	19.99	35.81	13.35	28.93	11.54
% Taking Home-Packed Lunch	44.48	18.05	31.25	16.05	45.80	13.05	57.10	12.90
FSM Registration	20.60	15.48	25.75	18.59	20.43	13.98	15.88	10.74
FSM Take-Up ($\frac{FSM\ takers}{registered} \times 100$)	89.15	12.18	91.93	10.40	89.64	11.51	86.61	13.53
Paid-SM Take-Up	45.14	18.09	57.94	18.58	44.78	13.64	34.41	12.80
Urban	0.46	0.50	0.35	0.48	0.50	0.50	0.56	0.50
Religious School (1/0)	0.15	0.36	0.12	0.33	0.17	0.38	0.17	0.38
School-Average Class Size	21.06	5.13	19.32	4.93	22.11	4.11	23.12	3.85
Roll on Survey Day	177.08	126.36	123.01	102.27	195.43	117.19	231.79	130.97
(roll/capacity)*100	68.41	21.76	64.51	22.22	70.63	20.03	74.75	18.57
SIMD Score	21.57	15.01	23.72	16.71	21.94	15.09	18.00	11.03
Schools	2,001		668		677		656	

Notes: These are raw averages from the school year 2006/2007 to 2016/2017. High, Middle and Low exposure are defined based on overall exposure tertiles in school year 2013/2014.

periods, namely 2014/15 and 2016/17. Appendix Table B1 provides more details.[22]

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 A6 we compare the trends calculated with the secondary data to those obtained from the official, publicly available aggregates. The patterns are virtually unchanged. Table 1 reports descriptive statistics by the level of policy exposure.

We split our sample by tertiles of our exposure measure, which we then define as low, middle and high exposure. We observe that, when compared to low or middle exposure ones, high exposure schools have a higher (pre-policy) share of pupils taking paid school meals or home-packed lunch at school. Once free school meals are universally provided, we would expect larger effects for these two groups as the policy leads to a real change in how they access school lunches, and the costs they incur when doing so (see Section 5.3 below). High-exposure schools are also, on average, more urban, with larger student populations, and are located in less deprived areas based on SIMD scores.

4. Empirical strategy

In this section, we outline the empirical strategy used to evaluate the effects of UFSM on attendance and absences. In Section 4.1 we describe the econometric model, whereas in Section 4.2 we discuss the identifying assumption through graphical evidence, description of the policy and formal testing.

4.1. Baseline model

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

$$y_{sct} = \alpha I_{t\geq 2015} + \beta E_{sc,2014} + \gamma(I_{t\geq 2015} \times E_{sc,2014}) + \delta X_{sct} + \mu_s + \mu_t + \varepsilon_{sct} \quad (2)$$

where y_{sct} is the attendance rate (or health-related absence rate) 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.[23] Our coefficient of interest is γ, measuring differences in attendance across different levels of exposure to the policy. X_{sct} is a set of controls. These include dummy variables to control for the pilot scheme in 2007/08 and local initiatives starting in school year 2010/11, both described in Section 2; the Scottish Index of Multiple Deprivation score interacted with year dummies; an indicator for religious schools and schools in urban areas, which we interact with a time trend; time-varying levels of school-level average class size, school size and pupil-to-teacher ratio. Finally, μ_s and μ_t are school and year fixed effects and ε_{sct} is the idiosyncratic shock component. We cluster the standard errors at the school level. To account for differential trends in outcomes, in some specifications, we control for school and local authority-specific time trends, i.e. $\mu_s t$ and $\mu_c t$.

We calculate our treatment intensity measure, or overall exposure to the policy, in the following way:

$$E_{sc,2014} = \frac{\#\ Non-School\ Meal-Takers_{sc,2014}}{School\ Population_{sc,2014}} = 1 - \frac{\#School\ Meal-Takers_{sc,2014}}{School\ Population_{sc,2014}} \quad (3)$$

whose rationale we illustrate with an example. Consider two equally sized schools, A and B. In school A 90% of the pupils take school meals (free or paid-for) before UFSM, 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 does not already take school meals. Put simply, our strategy compares schools where the policy induced a larger change in take-up with schools where the policy did not do so, on account of take-up already being high prior to the policy. This measure of exposure captures changes in outcomes induced by additional children taking school meals for the first time. However, the policy also affected pupils who previously took school meals on a paid basis, and now take them for free. For this reason, we also use an alternative measure of exposure to the policy, based on

[22] Treatment occurs halfway through the 2014/15 school year.

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

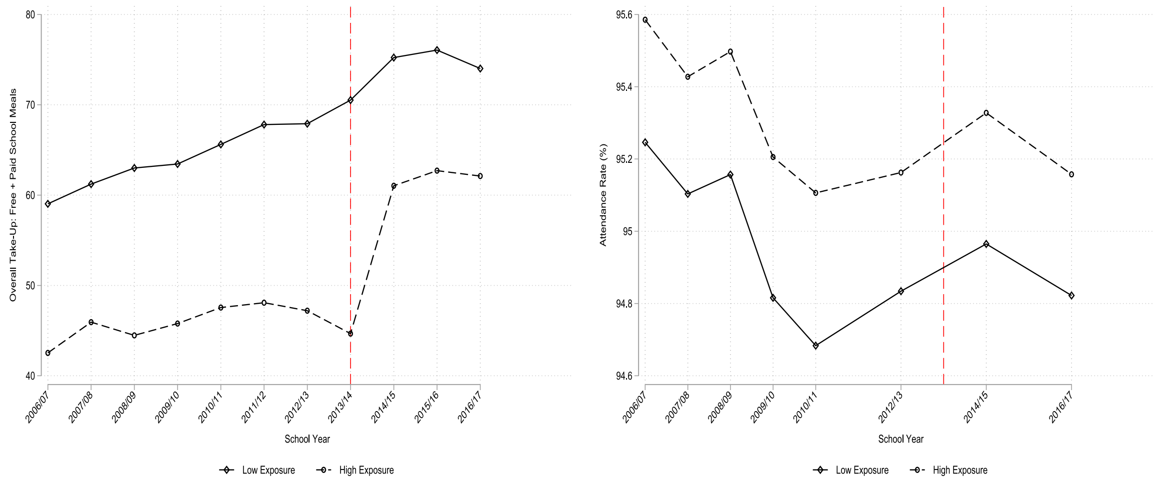

Fig. 2. Parallel trends.
Notes: The above charts are calculated as raw yearly averages of the reported variables, across levels of exposure. In particular, exposure is defined as the % of the school population not-taking any school meal (free or paid-for) in 2013, one year before the policy change. High exposure is denoted by a value above the median.

the share of the school population who did not take free school meals. This is equivalent to the measure described in Eq. (3), but relies on free school meals take-up, as opposed to the overall take-up. We explore this distinction in more detail in Section 5.3.

4.2. Parallel trends

Our source of identifying variation comes from the before/after comparison across schools with different levels of treatment, paired with within-school variation leveraged by μ_s, and after discounting for differential trends across sub-groups. 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. To visualise the change in take-up rates in response to the policy, in the left panel of Fig. 2 we plot the school-level percentage of pupils taking school meals for groups with different levels of exposure to the policy. We distinguish between schools with high versus low exposure based on the school having a share of non-school meal takers in school year 2013/2014 that is above or below the median. The high-exposure group has pre-policy take-up rates that are at least 10 percentage points below those of the low-exposure group throughout the entire pre-UFSM period. Once UFSM is introduced in 2014/15 school meals take-up sharply increases for the high-exposure group by approximately 15 percentage points and only moderately for the low-exposure group.

Most importantly, we do not see a large divergence in trends before the policy.[24] Attendance rates are on average .36 percentage points higher for the high-exposure group.[25] However, both groups show fairly similar trends until the UFSM implementation.

Differences between high- and low-exposure schools in *levels* of the outcomes are not, in principle, invalidating as these are absorbed by school fixed effects μ_s. However, factors driving discrepancies in *levels* might also affect *changes* in trends (Jaeger, Joyce, & Kaestner, 2020; Kahn-Lang & Lang, 2020). Figure A2 presents outcome trends by sub-groups, namely quintiles of the SIMD as well as for urban and rural schools. We can observe that urban schools and schools in the most deprived neighbourhoods (bottom 20% of SIMD) experienced a steeper increase in both attendance and health-related absences during the last two school years preceding UFSM roll-out, relative to the other groups. Furthermore, divergence in outcome *levels* across high and low-exposure groups may mask differences in drivers of exposure to the policy. Figure A3 provides some evidence on this. More deprived schools, those in local authorities with a higher share of pupils from ethnic minorities, and with a higher youth employment rate seem to be significantly less exposed, i.e. they have larger pre-policy take-up rates, whilst more populated and urban schools are more exposed. For these reasons, we include a rich set of time-varying control variables to account for possible shocks in the school size and staffing, but we also allow trends in outcomes to change by urban area status, religious schools, local authorities, and SIMD.

We can also test parallel trends formally by estimating the following event-study equation:

$$y_{sct} = \alpha I_{t\geq 2015} + \beta E_{sc,2014} + \sum_{t=2003}^{2016} \gamma_t (I_{t\geq 2015} \times E_{sc,2014}) + \delta X_{sct} + \mu_s + \mu_t + \epsilon_{sct} \tag{4}$$

Like in Eq. (2), y_{sct} is the attendance rate (or health-related absence rate) for school s, in local authority (council) c and school year t. Our coefficients γ_t are plotted in Fig. 3, alongside their 95% confidence intervals. Our models contain interactions between year dummies and SIMD. Standard errors are clustered at the school level.

The event study confirms the findings from the figures plotting parallel trends, i.e. that 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 six years prior to UFSM implementation. We can also observe from these figures that following the implementation of the policy, the point estimates are small and not statistically significant, suggestive of a null effect. We explore this further in the next section.

Finally, 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 the pre-treatment level of take-up. We believe this is highly unlikely as Scotland follows a residence-based school assignment system and relies on households providing evidence of residence within the attendance area at least six months in advance of the school year start (see Borbely, Gehrsitz, McIntyre, Rossi, & Roy, 2023; Rossi, 2020). Nevertheless, we formally test for the presence of anticipation effects in Figure A4. This is an event study where we use school year 2012/13 as the treatment year, i.e. the first year with observed outcomes before UFSM

[24] The divergence in take-up is likely mechanical due to the way the measure is defined. We experiment with variations of the exposure measures.

[25] This is not surprising considering that the overall school meals take-up is related to socio-economic disadvantage, which is associated with higher absenteeism (Sosu, Dare, Goodfellow, & Klein, 2021).

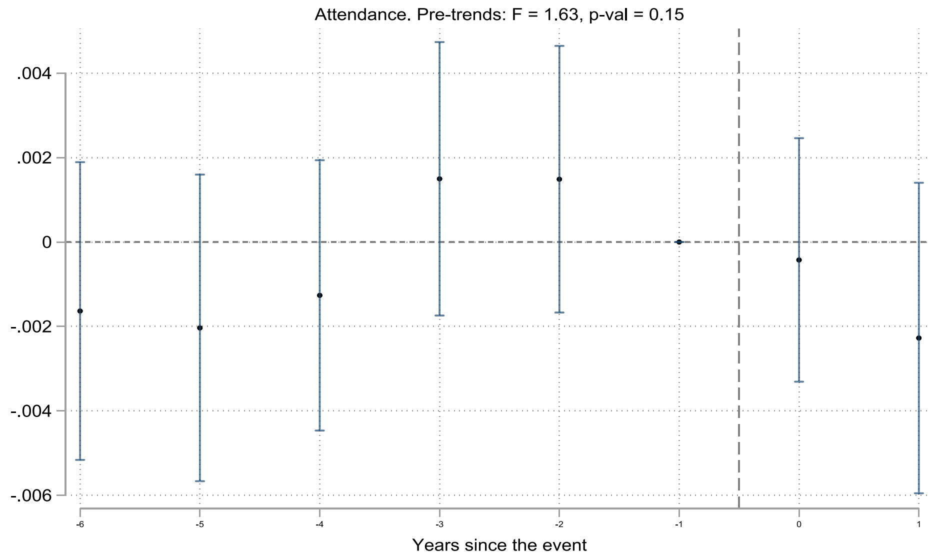

Fig. 3. Event study.
Notes: Coefficients are obtained by estimating γ_t from Eq. (4). Each coefficient can be interpreted as the difference in outcome for that period relative to the reference period (t=-1), which is the school year 2012/2013, the earliest we observe the outcomes before the policy change. Time is defined as the number of school years to and from the UFSM implementation, which is 0 – school year 2014/2015. Exposure is defined as the % of the school population not taking any school meal (free or paid-for) in 2013/2014, one year before the policy change and outcomes are in levels. Data span from the school year 2006/2007 through to 2016/2017 and outcomes are not available for years 2011/2012, 2013/2014 and 2015/2016. The estimated regression includes two indicators for 'Pilot schemes'. The first switches to 1 for all schools in local authorities that took part in the 2007/2008 pilot, from the school year of the pilot onward, whereas the second switches to 1 for the schools that adopted local initiatives to increase FSM take-up, starting from school year 2010/2011 onward. Controls include the Scottish Index of Multiple Deprivation score interacted with year dummies; an indicator for religious schools and one for urban schools, which we interact with a linear time trend; time-varying levels of school-level average class size, school size and pupil-to-teacher ratio. Whiskers are 95% confidence intervals. Standard errors are clustered at the school level.

implementation in 2014/15. There is no evidence of anticipation effects as all 'post-treatment' point estimates are small and not significant.

5. Results

5.1. Baseline results

Tables 2 and 3 report estimates from Eq. (2), using take-up and attendance rates as outcomes, respectively. Columns 1 to 4 in Table 2 use the overall school meal take-up rate (free and paid) as an outcome variable. We can see from columns 1 to 4 that a one percentage point increase in overall exposure is associated with an increase in overall take-up between 0.141 and 0.257 percentage points. Looking at the most conservative estimates, this is approximately equivalent to 3 percentage points higher take-up for a 26 percentage points increase in exposure, which is roughly the difference between the average lower-exposure (below median) and higher-exposure schools (high-median). In general, it is clear from our results that UFSM implementation had a positive effect on overall take-up rates.

Moving to the results on attendance in Table 3, we observe that higher overall exposure is not associated with a change in attendance or health-related absences (top panel). On the other hand, when using free school meal exposure we observe statistically significant effects across the board. A one percentage point increase in free school meal exposure leads to a 0.006 percentage point increase in attendance and a 0.004 to 0.014 percentage points decrease in health-related absences. These results are robust to the inclusion of our full set of control variables as well as local authority linear trends, however, they change substantially after the inclusion of school-specific linear trends. For instance, the effect of higher free school meal exposure on attendance turns negative, while the effect on health-related absences goes from -.014 to .001. In either case, neither are statistically significant. It is worth mentioning that these effects are very small. For example, looking at Column 6, at the bottom panel, of Table 3, a .014 percentage points decrease in health-related absence is equivalent to half of a session missed, even for a full standard deviation increase in exposure.[26] In addition, attendance and health-related absence rates use the same denominator, i.e. all possible attendance in a school year. The fact attendance has increased less than proportionally compared to health-related absences –which are only one of the reasons for school absences– means that some other type of absence must have increased as well.

Whilst the above estimates shed some light on the effect of the policy on take-up rates and attendance, they do not provide sufficient insight into which group of pupils is driving the (small) effect. For instance, using the overall exposure carries the advantage of estimating the impact the policy had on pupils who started eating (free) school meals for the first time, but on the other hand, ignores any effect resulting from switching from paid to free school meals. Equally, using free school meals exposure prevents us from distinguishing the effects on those taking school meals for the first time due to the policy. Section 5.3 elaborates on these distinctions and related channels.

5.2. Heterogeneity

One possible explanation for the observed null effects 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 with different characteristics. In this section we explore potential heterogeneous effects, building on the findings of two evaluations of UFSM. McAdams (2016) found that the largest increase in take-up following UFSM was recorded in rural schools, as well as schools with the biggest share of pupils from disadvantaged backgrounds.

[26] 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 × -.014 = .182 percentage points decrease in health-related absences. These are measured in half-day sessions, so 3.8 ×.182 = .69 sessions, which is slightly more than half of a session — hence slightly more than a quarter of a full school day.

Table 2
Baseline results - Take-up.

	Overall Take-up			
	(1)	(2)	(3)	(4)
Post ×Overall (Free + Paid) Exposure	0.147***	0.155***	0.257***	0.141***
	(0.018)	(0.018)	(0.022)	(0.019)
N	15,631	15,626	15,626	15,626
Schools	2,001	2,001	2,001	2,001
Mean Dep Var	57.84	57.84	57.84	57.84
SD Dep Var	17.87	17.87	17.87	17.87
Pilot schemes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
School linear trend	No	No	Yes	No
Local Authority linear trend	No	No	No	Yes

Notes: Coefficients are obtained by estimating γ from Eq. (2). Outcome is calculated as the percentage of the school population taking any type of school meal, whether free or paid for. All specifications include the Scottish Index of Multiple Deprivation score interacted with year dummies. In 'Pilot schemes' we include two dummy variables. The first switches to 1 for all schools in local authorities that took part in the 2007/2008 pilot, from the school year of the pilot onward, whereas the second switches to 1 for the schools that adopted local initiatives to increase FSM take-up, starting from school year 2010/2011 onward. Controls include an indicator for religious schools and one for urban schools, which we interact with a linear time trend; time-varying levels of school-level average class size, school size and pupil-to-teacher ratio. The exposure measure is calculated as the fraction of pupils not taking school meals in school year 2013/14. Data span from the school year 2006/2007 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3
Baseline results - Attendance and health-related absences.

	Attendance				Health-Related Absences			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post ×Overall (Free + Paid) Exposure	−0.000	−0.001	−0.003	−0.001	−0.002	−0.003*	−0.002	0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
N	15,576	15,560	15,560	15,560	15,595	15,579	15,579	15,579
Schools	2,001	2,001	2,001	2,001	2,001	2,001	2,001	2,001
Mean Dep Var	95.13	95.13	95.13	95.13	2.75	2.75	2.75	2.75
SD Dep Var	1.59	1.59	1.59	1.59	1.41	1.41	1.41	1.41
Post ×Free School Meals Exposure	0.006**	0.006**	−0.003	0.006**	−0.013***	−0.014***	0.001	−0.004**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
N	13,340	13,327	13,327	13,327	13,359	13,346	13,346	13,346
Schools	1,709	1,709	1,709	1,709	1,709	1,709	1,709	1,709
Mean Dep Var	95.00	95.00	95.00	95.00	2.80	2.80	2.80	2.80
SD Dep Var	1.59	1.59	1.59	1.59	1.43	1.43	1.43	1.43
Pilot schemes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
School linear trend	No	No	Yes	No	No	No	Yes	No
Local Authority linear trend	No	No	No	Yes	No	No	No	Yes

Notes: Coefficients are obtained by estimating γ from Eq. (2). Outcomes are calculated in % of half-day openings. All specifications include the Scottish Index of Multiple Deprivation score interacted with year dummies. In 'Pilot schemes' we include two dummy variables. The first switches to 1 for all schools in local authorities that took part in the 2007/2008 pilot, from the school year of the pilot onward, whereas the second switches to 1 for the schools that adopted local initiatives to increase FSM take-up, starting from school year 2010/2011 onward. Controls include an indicator for religious schools and one for urban schools, which we interact with a linear time trend; time-varying levels of school-level average class size, school size and pupil-to-teacher ratio. The exposure measure is calculated as the fraction of pupils not taking school meals in school year 2013/2014. Data span from the school year 2006/2007 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Conversely, urban schools experienced smaller increases in take-up. Additionally, Chambers et al. (2016) pointed out that a lack of funding, staff, and school space worked as a barrier to policy implementation, thus affecting take-up. Based on these findings, we might expect larger effects in those contexts where the policy roll-out went more smoothly.

We investigate this by stratifying our sample and looking at potential heterogeneous effects of the policy across sub-groups, based on whether the school is in an urban area, levels of resources and deprivation of the area the school is located in. We proxy resources and funding by using information on class size, school roll and pupil–teacher ratio. We generate dummy variables for small schools and classes by using the first quintile of their distribution from the academic year commencing in 2014. These correspond to schools with fewer than 50 pupils, and to those whose average class size is less than 17 pupils, respectively. Similarly, a low pupil–teacher ratio (high resources) is identified by the bottom quintile, namely 12 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 20% most deprived according to the 2006 SIMD. Like in column 1 of Table 3, we control for school and year fixed effects in every specification, along with interaction terms between year dummies and SIMD score, and control for pilot schemes.

Table 4 uses overall exposure to the policy as the measure of treatment, and shows no evidence of heterogeneous effects across different types of schools and/or areas. All coefficients are, again, very small and not statistically significant at any conventional level. On the other hand, when using free school meals exposure in Table 5 we find a positive and statistically significant effect on attendance among rural and small town schools, and those in least deprived

Table 4
Heterogeneity - Overall take-up exposure.

Attendance	Urban vs Small Town & Rural		Small vs Large Schools		Small vs Large Classes		High vs Low Resources		Small vs Large Spaces		More vs Less deprived areas	
Post ×Exposure	−0.002	0.000	0.003	−0.001	0.004	−0.001	0.005	−0.002	0.002	−0.000	−0.007*	0.001
	(0.002)	(0.002)	(0.005)	(0.001)	(0.004)	(0.001)	(0.005)	(0.001)	(0.004)	(0.002)	(0.004)	(0.002)
N	7,352	8,224	2,776	12,747	2,776	12,747	2,782	12,741	2,729	12,451	2,694	12,835
Schools	940	1,061	365	1,627	364	1,628	366	1,626	357	1,576	345	1,648
Mean Dep Var	94.58	95.63	95.77	94.99	95.55	95.04	95.30	95.10	95.88	95.00	93.52	95.47
SD Dep Var	1.69	1.30	1.51	1.57	1.61	1.57	1.74	1.55	1.45	1.55	1.60	1.36
Health-Related Absences												
Post ×Exposure	−0.003	−0.002	−0.001	−0.002	−0.002	−0.001	−0.004	−0.001	−0.001	−0.003	−0.004	−0.001
	(0.003)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.005)	(0.002)
N	7,356	8,239	2,772	12,770	2,770	12,772	2,774	12,768	2,731	12,466	2,690	12,858
Schools	940	1,061	365	1,627	364	1,628	366	1,626	357	1,576	345	1,648
Mean Dep Var	2.77	2.72	2.48	2.80	2.53	2.79	2.63	2.77	2.45	2.80	2.95	2.70
SD Dep Var	1.59	1.22	1.34	1.42	1.47	1.39	1.54	1.38	1.28	1.43	1.89	1.28
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Outcomes are calculated in % of all possible half-day openings. All regressions include the Scottish Index of Multiple Deprivation (SIMD) interacted with year dummies, alongside two indicators for 'Pilot schemes'. The first switches to 1 for all schools in local authorities that took part in the 2007/08 pilot, from the school year of the pilot onward, whereas the second switches to 1 for the schools that adopted local initiatives to increase FSM take-up, starting from school year 2010/2011 onward. 'Small Schools' are those whose population is in the bottom 20% in September 2014, 'Small Classes' are those whose size is in the bottom 20% in 2014, 'High Resources' identifies those schools whose pupil–teacher ratio is at the bottom 20% in 2014, 'Small Spaces' is based on bottom 20% of gross internal area, 'Deprived Areas' identifies schools in data zones at the bottom 20% of the Scottish Index of Multiple Deprivation. Data span from school year 2006/2007 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5
Heterogeneity - Free school meal exposure.

Attendance	Urban vs Small Town & Rural		Small vs Large Schools		Small vs Large Classes		High vs Low Resources		Small vs Large Spaces		More vs Less deprived areas	
Post ×Exposure	−0.000	0.013***	0.011	0.003	0.013*	0.003	0.011*	0.004	0.015*	0.003	−0.006	0.008***
	(0.003)	(0.004)	(0.007)	(0.003)	(0.007)	(0.003)	(0.007)	(0.003)	(0.008)	(0.003)	(0.005)	(0.003)
N	7,215	6,125	1,434	11,870	1,564	11,740	1,703	11,601	1,453	11,547	2,694	10,616
Schools	922	787	188	1,515	204	1,499	222	1,481	190	1,460	345	1,359
Mean Dep Var	94.55	95.53	95.79	94.91	95.42	94.94	95.08	94.99	95.86	94.92	93.52	95.38
SD Dep Var	1.69	1.26	1.49	1.57	1.64	1.57	1.80	1.55	1.44	1.55	1.60	1.34
Health-Related Absences												
Post ×Exposure	−0.018***	−0.006*	0.000	−0.016***	−0.008	−0.015***	−0.005	−0.016***	−0.004	−0.016***	−0.013**	−0.014***
	(0.004)	(0.004)	(0.006)	(0.003)	(0.006)	(0.003)	(0.005)	(0.003)	(0.006)	(0.003)	(0.006)	(0.003)
N	7,219	6,140	1,429	11,894	1,559	11,764	1,694	11,629	1,454	11,563	2,690	10,639
Schools	922	787	188	1,515	204	1,499	222	1,481	190	1,460	345	1,359
Mean Dep Var	2.78	2.82	2.49	2.83	2.61	2.82	2.75	2.80	2.49	2.82	2.95	2.75
SD Dep Var	1.60	1.21	1.37	1.44	1.54	1.42	1.61	1.41	1.30	1.45	1.89	1.29
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Outcomes are calculated in % of all possible half-day openings. All regressions include the Scottish Index of Multiple Deprivation (SIMD) interacted with year dummies, alongside two indicators for 'Pilot schemes'. The first switches to 1 for all schools in local authorities that took part in the 2007/2008 pilot, from the school year of the pilot onward, whereas the second switches to 1 for the schools that adopted local initiatives to increase FSM take-up, starting from school year 2010/2011 onward. 'Small Schools' are those whose population is in the bottom 20% in September 2014, 'Small Classes' are those whose size is in the bottom 20% in 2014, 'High Resources' identifies those schools whose pupil–teacher ratio is at the bottom 20% in 2014, 'Small Spaces' is based on bottom 20% of gross internal area, 'Deprived Areas' identifies schools in data zones at the bottom 20% of the Scottish Index of Multiple Deprivation. Data span from school year 2006/2007 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

areas. Conversely, urban and more deprived schools experience a small decrease in attendance. These results are not statistically significant at any conventional level, preventing us from discerning any differential effect across these sub-groups. However, it perhaps suggests that non-urban and less deprived areas might be driving the (small) effect we observe. For health-related absences, the effect is concentrated among large and relatively low-resource schools. We do not find evidence that deprivation might be driving the effect for health-related absences, as both coefficients are of roughly the same magnitude. However, the reduction in health-related absences seems to be concentrated in urban areas.

These results would imply that while the reduction in health-related absences is driven by urban areas and large schools, the opposite is true for the positive effect on attendance, which seems to be driven by schools in rural areas. It is worth noting however that these effects are small, and differences between subgroups are rarely statistically significant.

Taken together, our subgroup analysis using the overall policy exposure variable fails to reveal any substantial benefits (or detriments) for any group. This suggests that the aggregate null effect we find is not masking any important sub-group effects. We do find suggestive evidence of heterogeneous subgroup effects when using free school meals exposure as the treatment variable, but these effects (and differences) are too small to be meaningful.

5.3. Mechanisms

In this section, we further decompose our overall exposure measure to take into account the extent to which effects (including on take-up) are driven by different groups of pupils with different changes in school meal status due to the UFSM policy. The two main groups are (1) the *switchers*, who were getting paid school meals but after UFSM are getting them for free and (2) the *first-timers* who did not receive school meals before UFSM implementation. Not only can we break down overall take-up into measuring these two categories, but we can also use these to examine some of the channels driving the possible effects of the UFSM policy on attendance and absences.

We illustrate these channels using an example with four schools in Fig. 4. Schools A, B, C and D are equally sized schools with different, pre-policy, levels of school meal take-up, and free-to-paid school meal ratios. Schools A and B have the same levels of overall exposure to the policy, i.e. % of the school population taking any meal. For A and B, exposure is relatively low – relatively few pupils would take school meals for the first time as a result of the policy. On the other hand,

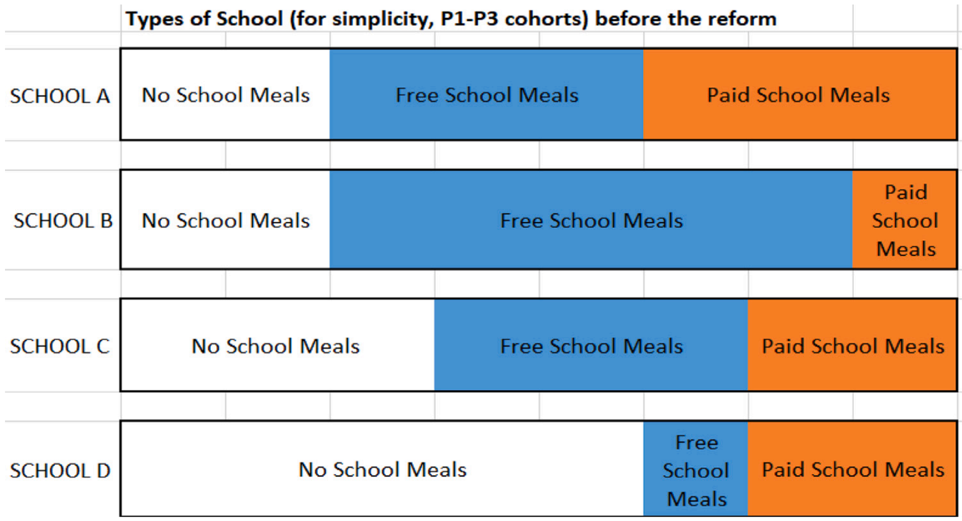

Fig. 4. Illustration of different types and levels of exposure

schools C and D are more exposed to changes in overall take-up, though to different extents, i.e. the policy is likely to increase overall take-up in School D more than in school C.

Our model in Eq. (2) compares schools of type A (or B) with schools of type C (or D). Following the UFSM roll-out, school C experiences a larger increase in 'first-timers' (white area in Fig. 4) relative to A. However, school A experiences a larger increase in 'switchers' relative to school C, as more pupils were taking paid school meals pre-UFSM (orange area in Fig. 4). It is worth mentioning that before UFSM implementation, on average, about 70% of all school meals were taken on a paid basis. Therefore, when comparing schools before and after the UFSM roll-out uniquely on their overall take-up, we automatically compare schools with different take-up compositions, and, as a result, fail to pick up changes in the number of 'first-timers' versus 'switchers'.

This distinction is important as it implies different potential mechanisms through which UFSM can lead to changes in attendance and absences. Namely, an increase in take-up can improve attendance through two main channels: the *health channel* from fewer health-related absences, if taking school meals for the first time and; the *financial channel*, due to more pupils now taking school meals for free, whether they used to take (paid) school meals, or brought a packed lunch from home. For instance, the policy will likely produce larger effects in school A than in school B, mostly through a financial channel. As per school C, where there is a higher share of first-timers, the policy's effect will likely operate mainly through the health channel.

To identify (and control for) the effects of 'switchers' and 'first-timers', along with overall exposure to the policy, we include the school-level share of those on paid school meals (before the policy) to control for (or capture) the policy's effect on those who used to take school meals on a pay basis and now take them for free (the 'first-timers'). In Table A6, we estimate variations of Eq. (2) using school meal take-up rates as an outcome variable. All specifications include the same set of controls as column 2 in Table 2.[27]

Column 1 compares free school meal take-up rates, following UFSM roll-out, across different levels of exposure types, using overall school meal exposure. The negative effect in column 1 is due to schools with low exposure (high overall take-up before UFSM) experiencing a larger increase in % of free school meal takers, relative to those with high exposure (low overall take-up before UFSM), following UFSM implementation. In other words, schools with high pre-policy take-up rates (most of which were paid school meals) have experienced larger shifts from paid to free school meals. This means that for these schools, the policy mostly worked through an increase in 'switchers', rather than in 'first-timers' (see Table 6).

In column 2 we can separately quantify the change in school free meal take-up by first-timers and switchers. The coefficient on overall exposure is equivalent to comparing schools C and D in Fig. 4. This measures the change in free school meal take-up rate, keeping 'switchers' constant. In other words, a 0.176 percentage points increase in free school meal take-up is concentrated among 'first-timers'. The coefficient on paid school meals exposure, on the other hand, compares schools with similar overall take-up, but different school meal composition. This is equivalent to comparing schools A and B in Fig. 4, before and after the reform. This coefficient measures a change in composition after the reform, whereby school A experiences a larger increase in 'switchers' relative to C, keeping 'first-timers' constant. In other words, a 0.305 percentage point increase in free school meals is attributable to 'switchers'.

Columns 3 to 6 use attendance (columns 3 and 4) and health-related absence rates (columns 5 and 6) as outcomes. Results in columns 3 and 5 suggest that a higher exposure to the policy, measured as the percentage of pupils not taking any type of school meal, did not drive a sizeable or statistically significant change in attendance or health-related absences, despite the change in take-up. As outlined earlier, while the policy increases the number of 'first-timers' in treated (high-exposure) schools, it also increases the number of 'switchers' in control (low-exposure) schools. This could lead to a simultaneous, positive effect on attendance –although this is occurring through different channels– on treated and control schools alike, thus preventing us from discerning the effect of taking (free) school meals for the first time.

Finally, in Columns 4 and 6 we control for pre-policy paid school meals rate. Conditional on paid school meals exposure, and therefore on the number of 'switchers', an increase in overall take-up is associated with a decrease in health-related absences by .015 percentage points, but not in overall attendance, among 'first-timers'. For attendance, the coefficient is less than half the size (.005 percentage points) and not statistically significant at any conventional level. Similarly, we find the health-related absences have decreased among 'switchers' by .015 percentage points, but this does not translate into a 1-to −1 change in overall attendance, which only increases by 0.008 percentage points. Both coefficients are statistically significant at the 1% level.

[27] Arguably this is a more appropriate outcome than overall take-up, as we can distinguish between the effect driven by 'switchers' from the effect of 'first-timers' on free school meal take-up. Changes in overall take-up only capture first-timers – keeping the number of school meal payers constant– as they do not record switchers from paid to free school meals.

Table 6
Results - Take-up, attendance and absences - By school meal taker composition.

	Free School Meal Take-up		Attendance		Health-Related Absences	
	(1)	(2)	(3)	(4)	(5)	(6)
Post ×Overall (Free + Paid) Exposure	−0.080***	0.176***	−0.001	0.005*	−0.003*	−0.015***
	(0.015)	(0.023)	(0.002)	(0.003)	(0.001)	(0.003)
Post ×Paid School Meals Exposure		0.305***		0.008***		−0.015***
		(0.025)		(0.003)		(0.003)
N	13,403	12,182	15,560	13,239	15,579	13,259
Schools	1,978	1,688	2,001	1,697	2,001	1,697
Mean Dep Var	25.44	25.70	95.13	95.00	2.75	2.80
SD Dep Var	17.64	17.43	1.59	1.58	1.41	1.43

Notes: Attendance and health-related absences are calculated as the percentage of half-day openings. Free school meals take-up is calculated as the school level percentage of pupils taking free school meals. All specifications include two indicators for 'Pilot schemes'. The first switches to 1 for all schools in local authorities that took part in the 2007/2008 pilot, from the school year of the pilot onward, whereas the second switches to 1 for the schools that adopted local initiatives to increase FSM take-up, starting from school year 2010/2011 onward. All specifications include the Scottish Index of Multiple Deprivation score interacted with year dummies; an indicator for religious schools and one for urban schools, which we interact with a linear time trend; time-varying levels of school-level average class size, school size and pupil-to-teacher ratio. The overall exposure measure is calculated as the fraction of pupils not taking school meals in school year 2013/2014, whereas paid school meals exposure measure is calculated as the percentage of pupils taking paid school meals in school year 2013/2014. Data span from the school year 2006/2007 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Whilst these results align with the presence of the *financial channel* increasing attendance among 'switchers', it does not seem to align with the presence of a *health channel*. If this was the case, the reduction in health-related absences would be concentrated among 'first-timers' only, and would not apply to 'switchers' who were previously taking school meals, but paying for them. This is provided that the nutritional content of the meals did not change following UFSM implementation, as we previously discussed. To date, there is no evidence that school meals became more or less healthy following UFSM implementation. If anything, the increase in scale might have forced caterers to employ fewer fresh products, with negative effects on the overall quality of the meals. One possibility, however, is that the introduction of school-packed lunches (see Section 2.2) might have been more attractive to 'switchers', who were previously opting for less healthy options available in the menus, or leaving vegetables and fruit lying uneaten on their trays. This might explain the reduction in health-related absences among switchers and first-timers alike.

Overall the results support the presence of *financial channel* increasing attendance among 'switchers', through financial savings for families. This is an additional channel to the one proposed by Holford and Rabe (2020), who find that the effect of UFSM on attendance is driven by pupils who were previously eligible for free school meals, suggesting that a more pleasant lunch environment and/or absence of stigma might increase take-up within this group.

It is also worth reiterating that these effects are very small, and translate in less than one day of school gained on average, for a full standard deviation increase in exposure. In addition, the net increase in overall attendance is smaller than the decrease in health-related absences, meaning that another type of absence might have been increasing over this period.

5.4. Robustness

Our first set of results clearly suggests that, while the policy undoubtedly increased free school meal take-up, it did not translate into improved attendance and short-term health conditions. In this section, we address a series of concerns surrounding our identification strategy.

One concern is that while we are estimating a single treatment effect in Eq. (2) there is only partial treatment in the school year 2014/15, potentially offsetting an overall treatment effect. To mitigate this, we re-estimate our baseline specification, but this time our exposure measures are interacted by separate time dummies for the 2014 and 2016 school years, respectively. Our results are summarised in Table A3. The overall null effect remains, and while there are some small effects (mostly for free school meal exposure) that seem to be largely driven by the 2016/17 school-year, these estimates do not differ significantly from those presented in Table 3.

Moreover, as pupils 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 Eq. (2). The results are reported in columns 3 and 4 of Table A4 and Table A5. Regressions are weighted using the number of pupils in P1–P3 in each school year as weights.[28] Our results do not change significantly for health-related absences, but become smaller and non-statistically significant for attendance.

We also check whether our results are sensitive to the year(s) we are using as reference periods for our exposure measures. In our baseline specification, the data for these are taken from the 2013/14 school year, which might introduce mean reversion effects (especially in pre-trends) if take-up rates converge towards these fixed values. To mitigate this concern, we use two alternative specifications of our exposure measures, one using average exposure from all pre-treatment years (2006–2013) and the other one using 'permanent exposure', obtained from regressing take-up on year and school fixed effects in the pre-treatment period and using the predicted values for the school fixed effects. The event studies using these measures are presented in Figure A5. For both measures of exposure, we notice a slight positive trend up to two years before UFSM implementation. However, only one of these coefficients is statistically significant and coefficients on all leads are virtually zeros. In addition, the F-tests fail to reject the null hypothesis that all coefficients are jointly equal to zero. Results from the aggregate regressions are summarised in columns 5 to 8 of Table A4 and Table A5. Our overall null effects remain in these specifications, and all effects on attendance are now insignificant and close to zero.

Finally, we check the sensitiveness of our results from Table 4, Table 5 and Table A6 to the inclusion of school and local authority-specific linear trends. Table A6 shows little difference relative Table 4

[28] We use analytic weights as if for each school, attendance was the mean computed from a sample whose size is given by the number of P1, P2 and P3 students.

— some of the coefficients for attendance are marginally larger in absolute value and statistically significant. Specifically, attendance dropped among large and low-resources schools. The bottom panel of Table A6, Table A7 and Table A10 show a pattern that we already observed before, when controlling for school-specific linear trends. In other words, there is an attenuation of most coefficients and each turns non-statistically significant. The only exception is column 2 in Table A10, which shows a larger effect of exposure on free school meal take-up rates, similar to what we observed in column 3 of Table 2. The inclusion of local authority-specific trends, on the other hand, seems to carry a similar attenuating effect on coefficients, but to a lesser degree. Again, Table A8 and Table A9 show that schools in urban and most deprived areas might have experienced an attenuation of the (small) effect of the policy on attendance rates. Table A11 shows that the inclusion of local authority-specific trends had hardly any impact on the coefficients measuring the impact of UFSM provision on free school meals take-up and attendance rates. However, no evidence of an effect on health-related absences among 'first-timers' remains.

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 and short-term health conditions. 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 school meal take-up. That is, the introduction of UFSM represented a greater change for schools with few pre-implementation school meal takers than in schools where school meals enrolment was high at baseline.[29] We find precisely estimated null effects on attendance and health-related absences. A 10-percentage-point increase in the school population taking school meals leads to a gain of less than a school day. When we use an alternative measure of exposure that accounts for those pupils switching from paid to free school meals, we do find positive (negative) and very small effects for attendance (health-related absences). The small effects we do find are concentrated among pupils previously taking school meals on a paid basis, which suggests a financial mechanism at play, whereby school meals meant financial savings for households. Even these effects are too small however to constitute economically significant findings.

Our study has some limitations. First, the policy targeted only the first three grades of primary schools (45% of the 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, 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 improvement.

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 found in the literature so far (see Corcoran et al., 2016; Cuadros-Meñaca et al., 2022; Gordanier et al., 2020; Holford & 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). 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. One assumption of our study was that the policy did not change the nutritional content 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 pupils, in line with Holford (2015).

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

Declaration of competing interest

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

Data availability

The data used in this study are available at https://www.gov.scot/collections/school-education-statistics/ but also from the corresponding author on reasonable request.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.econedurev.2024.102597.

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