Education Endowment Foundation

YOUTH ENDOWMENT FUND

BITUP: Updating parents on number of school days missed, a two-armed cluster randomised trial

Evaluation report

October 2025

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verian

This project is part of a joint funding round with the Youth Endowment Fund (YEF). The YEF and the Education Endowment Foundation (EEF) are partnering to find, fund, and evaluate programmes and practices in England and Wales that could keep children safe from involvement in violence and/or improve academic attainment, by increasing school presence.

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Verian is one of the largest providers of social research to the UK Government and was formerly known as Kantar Public (including at the start of this research project, until November 2023). Verian's multidisciplinary team of researchers generate high quality evidence to support policymakers, regulators, delivery teams, and the public to inform decision-making and improve outcomes.

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Executive summary

The project

The BITUP intervention aims to reduce pupil absences by sending parents/carers a text message at the start of each term about the number of days their child has been absent in the prior six- to eight-week term. This trial targeted pupils aged 11–16 (Years 7–11) in secondary schools in England. If a pupil had attended for <95% of the sessions in a term, then they were eligible for the intervention, which consisted of a text message being sent to their parent/carer at the start of the following term (which aims to leverage a 'fresh start effect'). The hypothesis is that while parents may be aware of individual absences, they underestimate the days their child has been absent. The messages were designed by the Behavioural Insights Team (BIT), inspired by academic research conducted in the United States, and were sent to parents/carers by schools. The trial was co-funded by the Education Endowment Foundation (EEF) and the Youth Endowment Fund (YEF) in 'A Safe, Positive Place to Learn' funding round.

The intervention was a two-arm cluster randomised controlled efficacy trial. The trial used family-level randomisation within schools, meaning siblings attending the same school were allocated to the same trial arm (treatment or control). In each of the first five terms of the 2023–2024 school year, eligibility for receiving text messages was assessed, with text messages being sent to eligible pupils at the beginning of Terms 2–6. A holistic implementation and process evaluation (IPE) accompanied the impact evaluation, including baseline and endline surveys of schools, an endline survey of parents/carers, and interviews with parents/carers, school personnel, pupils, and the delivery team.

A total of 108 schools were randomised into the trial, including 87,909 families of 104,029 pupils, of which there were 30,162 pupils who were eligible for free school meals (FSM). Of the pupils randomised 77,540 (74.5%) were White, 9,235 (8.9%) were Asian or Asian British, 5,947 (5.7%) were Black, Black British, Caribbean, or African, 27 (0.3%) were Gypsy, Roma, and Traveller, 6,639 (6.4%), were identified as having mixed or multiple ethnicity, and 2,277 (2.2%) were of other minority ethnicity. During the trial, 71,916 pupils from 63,295 families were eligible for the intervention in at least one term.

Table 1: Key conclusions

Key conclusions	
1.	Eligible pupils (<95% attendance) in the intervention group were on average absent for 0.21 fewer days in total across five terms (Terms 2–6), compared to eligible pupils (<95% attendance) in the control group. This is a 0.10% percentage-point increase in overall attendance during Terms 2–6. This finding has a high security rating.
2.	Eligible pupils in the intervention group had on average 0.26 fewer days of authorised absences across five terms (Terms 2–6) compared to eligible pupils in the control group. The trial found no impact on unauthorised absence rates.
3.	Eligible FSM pupils, Year 8 pupils, and female pupils in the intervention group were on average absent for 0.51, 0.61, and 0.35 fewer days in total over five terms relative to their counterparts in the control group. These results have a lower security rating as they are based on a smaller number of pupils.
4.	There was no conclusive evidence that receiving a text message improved parent knowledge about their child's absence rate or changed their perceived importance of attendance.
5.	School staff showed support for the intervention, with many schools intending to continue using the intervention in the next academic year, albeit with some adaptations (e.g. targeting the text messages specifically at pupils whose attendance fell just below the 95% benchmark). Although schools generally saw the intervention as low cost, this was influenced by staff capacity and access to text message software.

EEF security rating

These findings have a high security rating. This was an efficacy trial, which tested whether the intervention worked under developer-led conditions in a number of schools. The trial was a well-designed and well-powered two-armed cluster randomised controlled trial. The pupils in the intervention arm were similar to those in the comparison arm in terms of prior attendance and pupil characteristics. Around 12.59% of the pupils who started the trial were not included in the final analysis because of school dropout, non-consent to process pupil data, and uncertainty about pupil eligibility for the programme due to incomplete data.

Additional findings

Eligible pupils in the intervention group were on average absent for 0.21 fewer days in total compared to eligible pupils in the control group. This was an increase in attendance of 15 school days per 100 Years 7–11 pupils for Terms 2–6, or a 0.10% percentage-point increase in the attendance rate.[1] However, as with any study, there is statistical uncertainty regarding this impact, and the results of the study are consistent with the intervention having a higher positive impact (0.45 days) or a negative impact (–0.02 days). Eligible pupils in the intervention group had on average 0.26 fewer days of authorised absences in total across five terms, compared to eligible control pupils. The range of likely results all show a reduction in authorised absences, suggesting a greater degree of statistical confidence in this finding compared to the impact on overall absences. The evaluation found no conclusive evidence of impact on unauthorised absence rates.

Prior evaluations have found a larger positive impact on attendance of similar communication interventions (BIT and Ceibal, 2024; Rogers and Feller, 2018). Potential reasons for the difference may include the mode of delivery used in other interventions, which used letters rather than text messages, containing more information about the importance of attendance, tips, and practical tools.

Schools delivered the programme as intended. Schools used different communication platforms or Management Information Systems to send text messages, and some used different formats due to technological limitations. While schools were recommended to exclude pupils for whom the school felt it would be inappropriate to send a text message due to factors such as bereavement, medical conditions, anxiety, autism, and Special Educational Needs and Disabilities (SEND), or a serious one-off illness outside of their control, there were some schools that still sent text messages to these pupils/families. In these instances, the intervention was perceived as less effective by parents/carers, pupils, and schools.

While all schools viewed the intervention as cost-effective, those with limited technological resources found it labour-intensive. There was no evidence to indicate the intervention freed up staff time for other responsibilities or for further attendance support over the trial period. It is possible that over time sending text messages could become quicker and easier as staff become more familiar with the process, although this likely depends on school characteristics, systems, and processes.

Parents broadly welcomed the communication of the number of days their child was absent, compared to schools expressing the absence rate as a percentage. Evidence from the IPE suggests that the text message did not on average improve parents'/carers' knowledge of their child's attendance record, as many reported being acutely aware of the importance of attendance. However, the IPE found some evidence that the messages prompted parents/carers to have conversations with their child or with school staff, to keep track of their child's attendance going forward, or to be a bit less lenient when their child asked to stay home.

Cost

The average cost of the BITUP intervention for one school was around £381.81, or £1.11 per pupil per year, when averaged over three years. We anticipate the cost per pupil per year would be £0.29 if schools implement without BIT's support (which is the planned delivery model).

Impact

Table 2: Summary of impact on primary outcome(s)

Outcome / group	Effect size (95% confidence interval)	Estimated no. of days absent relative to control group (95% confidence interval)	EEF security rating	No. of pupils Total (intervention; control)	P-value	EEF cost rating
Absence rate (across Terms 2–6)	-0.01 (-0.02, 0.00)	-0.21 days (-0.45, 0.02)	🔒🔒🔒🔒🔒	71,916 (36,037; 35,879)	0.073	£ £ £ £ £
Absence rate for FSM pupils (across Terms 2-6)	-0.02 (-0.04, 0.00)	-0.51 days (-1.01 , 0.00)	N/A	22,314 (11,429; 11,412)	0.05	N/A

[1] The intervention can only improve the attendance of pupils who became eligible to receive it, which was 79% of the pupils in the trial. That means that the average difference in days attended per pupil (over all Years 7–11 pupils in the schools) was 0.15 days over Terms 2–6. In the trial, there were on average 150.69 attendable days in Terms 2–6.

Introduction

Background

Poor attendance at school is associated with poor academic attainment across all educational stages, as well as poor socio-emotional outcomes, increased delinquency, and worse labour-market outcomes in adulthood (Atwood and Kroll, 2014; Dräger, Klein, and Sosu, 2024; Gottfried, 2014; Klein and Sosu, 2024; Rocque *et al.,* 2017). A study conducted by the Department for Education (DfE) found that, of secondary school pupils who missed five or less than ten days of school, 52.8% achieved five or more GCSEs A* to C or equivalent including GCSE English and GCSE mathematics, whereas this figure was only 35.6% for pupils who missed 15 ≤ 20 days (DfE, 2016a). Another DfE study showed that every extra day of school missed is associated with a lower chance of achieving five or more GCSEs at grades A* to C or equivalent including English and mathematics (DfE, 2016b).

It is important that schools and policymakers find effective ways of reducing pupil absence, especially given the increase in absences post-COVID. During the 2022–2023 academic year, 9.2% of sessions in state-funded mainstream secondary schools in England were missed due to absence, up from 5.2% in 2018–2019 (Benyon, 2024). UK school attendance data is reported by 'session', splitting school attendance between the morning and afternoon session of each school day. The percentage of 5-to 15-year-old (compulsory school age) pupils in state-funded primary, secondary, and special schools in England who were persistently absent (meaning that they missed 10% or more of possible sessions) also increased, going from 10.9% of pupils in Autumn Term 2018–2019 to 19.4% of pupils in Autumn Term 2023–2024 (DfE, 2024).

Some groups have notably higher absence rates. Looking at data from the Autumn Term 2023–2024 for state-funded primary, secondary, and special schools, where there was an average overall absence rate of 6.7% (DfE, 2024):

- **Pupils from low-income households.** The overall absence rate for pupils eligible for free school meals (FSM) was 10.2%, almost twice that for non-FSM eligible pupils (5.4%); the trend is similar for persistent absence rates, with 32.1% of FSM-eligible pupils being persistently absent compared to 14.5% for non-FSM eligible pupils.
- **Pupils with Special Educational Needs and Disabilities (SEND).** Pupils receiving SEND support had an overall absence rate of 9.6%, while those who had an education, health, and care plan (increased support for those with more complex needs) had an overall rate of absence of 12.3%.
- **Pupils of Gypsy, Roma, or Traveller ethnicity.** Travellers of Irish Heritage and Gypsy or Roma pupils had the highest overall rates of absence of any ethnic group, at 20.9% and 16.8%, respectively.
- **Pupils in older year groups.** Overall absence rates increased with the year groups from Year 7 (5.8%) to Year 11 (9.6%).

If pupils feel unsafe at school, then that may lead to absences in these groups. For instance, low attendance of Gypsy, Roma, and Traveller pupils may be mainly due to negative school experiences, including high levels of racism and bullying at school (House of Commons Education Committee, 2023). In an online survey of 7,574 children aged 13 to 17 years, 20% said they had skipped school in the past 12 months because they felt unsafe, with this proportion being higher for Black (24%) and mixed ethnic background (25%) children; and for children receiving FSM (37%) and who live in a deprived area (25%) (YEF, 2023).

Specifically, for SEND pupils, higher rates of absence may be due to unmet special education needs and lack of resources to support them; but pupils with SEND are also more prone to illness and have more medical appointments, meaning higher levels of absence may be unavoidable (Hose of Commons Education Committee, 2023).

The University College London, Institute of Education suggested that the cost-of-living crisis, children's worsening mental health, and changes in parental attitudes were key factors driving the rise in school absence rates since the pandemic (Macmillan and Anders, 2024). In polling done by the Centre for Social Justice, 28% of parents agreed that the pandemic had shown it is not essential for children to attend school every day, and 13% of parents gave holidays as a reason for why children missed school (The Centre for Social Justice Foundation, 2024). Families could also be more likely to withdraw children due to minor illnesses or viruses to avoid them infecting others (House of Commons Education Committee, 2023).

In a review of existing research on interventions that aim to improve pupil attendance, the Education Endowment Foundation (EEF) identified parental communication approaches and targeted parental engagement interventions as having promise (EEF, 2022a). Following this review, the EEF and the Youth Endowment Fund (YEF) partnered on the funding round 'A Safe, Positive Place to Learn', commissioning a number of research projects to find out, which approaches are most effective in enabling pupils at the highest risk of absenteeism and/or exclusion from school to attend, positively

engage with, and remain in school/college, in order to improve their attainment and to reduce the likelihood of them becoming involved in violence. Note that where we refer to 'parents'/carers' in this report we mean those with a primary responsibility for the care of each child in our study.

Evidence from the United States (US) shows that high-school pupils' attendance can be improved by increasing the quantity and quality of school–family communication (Rogers and Feller, 2018). The intervention, a letter informing parents of pupils of the number of days of school their child had missed so far this year, was based on evidence showing that parents may be aware of individual absences but that they underestimate the total number of days their child has been absent. A pilot survey conducted in advance of the US study found that parents of high absence pupils in the school district underestimated their child(ren)'s absences by a factor of almost two (9.6 estimated absences vs 17.8 actual absences) (Rogers and Feller, 2018).

Based on this evidence from the US, the Behavioural Insights Team (BIT) developed the BITUP intervention, which involves parents/carers of pupils whose attendance was <95% in the previous (half) term (five to eight-week period) receiving text messages at the start of the next term informing them of the number of days their child had been absent from school the previous term.[2]

The BITUP intervention was implemented in a pilot randomised controlled trial (RCT) with family-level randomisation across 22 schools in Bristol starting in January 2020. This pilot trial did not find a significant effect of the BITUP intervention on its primary outcome measure, pupils' attendance rate, but it is likely that the impact of COVID-19 school closures in March 2020 influenced the result. In addition, exploratory analyses suggested that the intervention increased the proportion of pupils with good attendance records (>95%) by four percentage points (from 59.5% to 63.3%) (BIT, 2020).

In 2022, the EEF and the YEF commissioned Verian to conduct an efficacy trial of the BITUP intervention, which we describe in this report. The evaluation investigated the impact of the intervention on the absence rates of secondary school pupils aged 11–16 (Years 7–11) who have an attendance rate below 95% (or absence rate above 5%). Table 3 shows the number of school days per term that would need to be missed for attendance rates to fall below 95% (or absence rates to rise above 5%), for different term lengths. The impact evaluation involved a two-armed cluster randomised trial spanning five academic terms in the intervention period across 105 schools. Like the pilot, the trial used family-level randomisation to maximise power, although different data was used to identify siblings (the efficacy study used school sibling markers, while the pilot study matched on last names). Testing this intervention through a large-scale RCT in the field allowed us to observe the impact of the intervention as it was delivered at a national scale in a diverse range of schools across England. The RCT methodology allows us to establish robust estimates of the causal impact of the intervention on attendance.

Table 3: How many days of school a pupil would need to miss for attendance to fall below 95%, for different term lengths

Term length	Approximate no. of school days per term	No. of school days that would need to be missed for attendance to fall below 95% (absence rate to rise above 5%)
Five-week term	25	One and a half days or more
Six-week term	30	two days or more
Seven-week term	35	two days or more
Eight-week term	40	More than two days

A holistic implementation and process evaluation (IPE) accompanied the impact evaluation. This examined how the text messages were created and delivered, how parents received and responded to them, and how the pupils in question reacted to any increase in their parents' attention to the issue of school attendance. The IPE assessed how closely the assumptions made in the intervention's Theory of Change were borne out in practice. It also identified specific factors that may have influenced the intervention's outcomes to assess the extent to which they had an impact. Through this work the IPE provides learnings for any potential scaled-up delivery.

Alongside the IPE, we also conducted a cost evaluation. This reports the costs of implementing the BITUP intervention at schools (both per school and per pupil), providing the evidence needed for schools to decide whether they would like to adopt and implement the BITUP approach with the aim of reducing pupil absence.

[2] While schools differ in their terminology between 'half-term' and 'term', for the purposes of this project and throughout this report we use 'term' to refer to the five to eight-week period between school breaks.

Intervention

The BITUP intervention aimed to reduce pupil absence in secondary schools by improving parental awareness of their child(ren)'s attendance and their understanding of the impact of absenteeism on educational outcomes. The intervention was delivered through schools' usual text messaging procedures, usually involving a member of the administration team or an attendance officer sending text messages via the school's existing Management Information System (e.g. Arbor, SIMS [School Information Management System]).

Who benefits

The intervention aimed to reduce absence rates among pupils in Years 7–11 whose attendance was <95% (absence rate was >5%) in the previous term. The Theory of Change describes how the intervention could benefit the parents of those pupils by empowering them to support their child(ren); and the schools taking part in the trial insofar as they benefit from greater attendance.

What

The intervention consisted of a text message sent to the phone number held by the school for the primary parent/carer of each pupil. Schools were asked to send text messages to parents/carers of eligible pupils—with <95% attendance (>5% absence) in the previous term—at the start of the new term. The text message was personalised: each parent/carer was told the number of days that their child was absent. If a family had more than one child eligible to receive a text message, then they received one text message per eligible child.

As this intervention was delivered to and through parents/carers, the programme included text messages in ten languages in addition to English. These languages were chosen as they were the ten most common languages spoken in England other than English at the time of the intervention:

- Arabic
- Bengali
- Gujarati
- Polish
- Portuguese
- Punjabi
- Romanian
- Spanish
- Ukrainian
- Urdu

Providing the text messages in multiple languages reduced the risk that parents who use English as an Additional Language (EAL) were unable to engage with the intervention. Schools were provided with the messages in English and all the other ten languages, and were given the opportunity to contact parents in a non-English language.

Number of messages

Schools could send up to five text messages for each pupil as part of this trial (one each for Terms 2–6), depending on the termly attendance of the pupil.

When were messages sent

The intervention was delivered during the second term through the sixth term (five- to eight-week periods) of the 2023–2024 school year. The intervention text messages were shared by BIT with schools during the first two weeks of each term in each school, with the request that the schools send those text messages to parents/carers as soon as possible.

How often

Messages were sent by schools at the start of each term.

Content of messages

The two core components of the intervention were:

- **Simplification.** Text messages contained the **number of days** the pupil was absent the previous term, rather than the pupil's attendance rate expressed as a percentage (i.e. 'Tom missed five days of school' and not 'Tom's attendance rate was 90%'). Evidence from Rogers and Feller (2018) in the US and exploratory post hoc analysis

from the BITUP pilot in the UK found that informing parents/carers of the number of days of school their child had missed improved attendance. Further, qualitative interviews conducted when developing the intervention for the BITUP pilot suggested that, when attendance was expressed as a percentage (as UK schools often do), some parents/carers found it difficult to understand that there was a problem with their child's attendance (BIT, 2020).

- **Fresh start effect.** Text messages were sent to parents/carers **at the start of each term** and emphasise that the start of a new term is a 'fresh start'. Evidence shows that temporal landmarks or 'fresh start' events, such as a new year, can motivate aspirational behaviour, as they give people mental separation from past failures and help them focus on 'big picture' goals, rather than everyday issues (Dai, Milkman, & Riis, 2014). The start of the new half-term provides an opportunity to harness the 'fresh start' effect to improve pupils' attendance.

Who provided

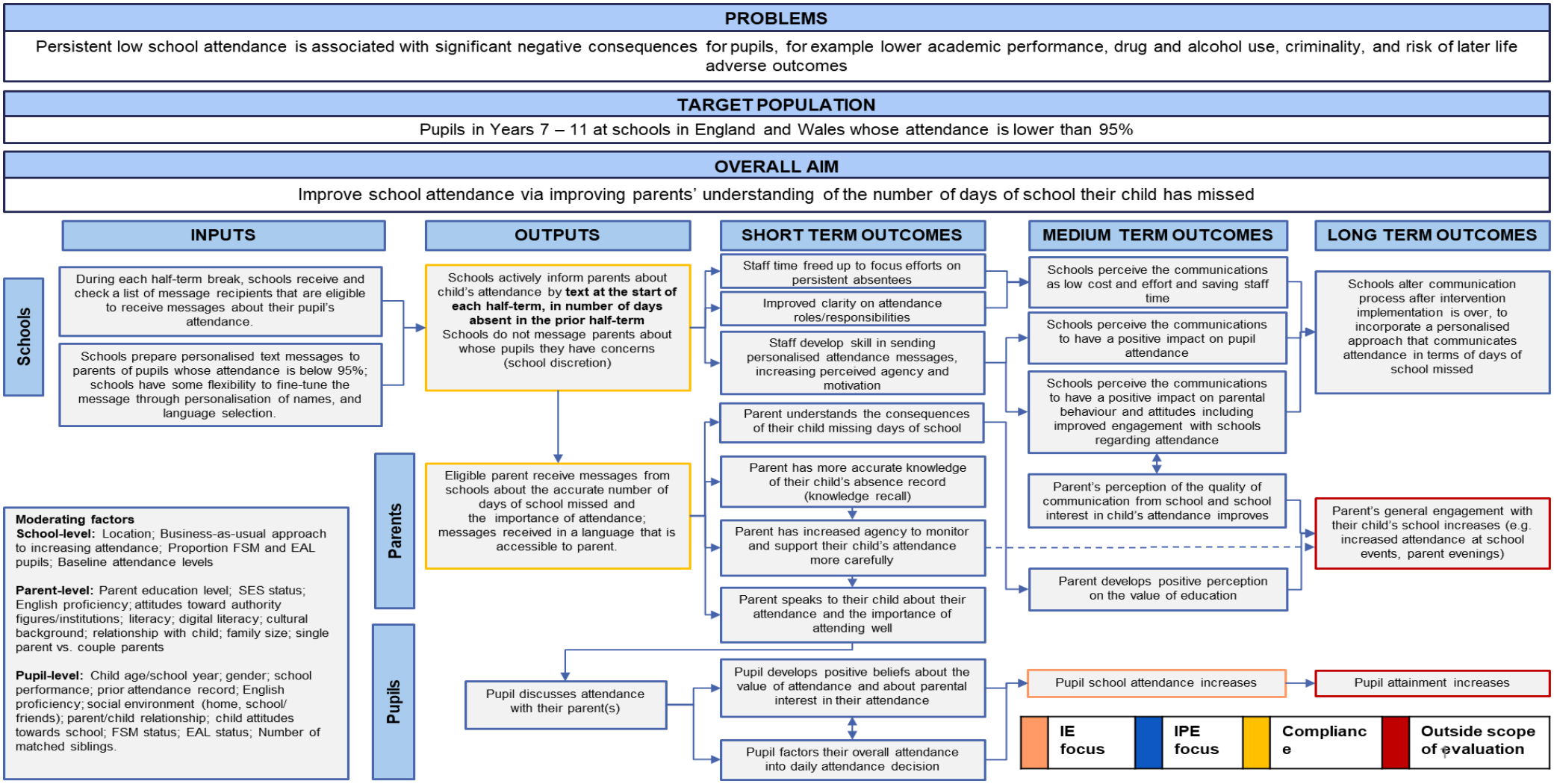
BIT were the intervention providers and supplied the message for the texts to the schools (including the translations). School staff then sent the text messages to parents/carers; the delivery team told schools that they estimated sending the text messages would take approximately four to six hours for each batch (i.e. four to six hours each term).

Tailoring

The intervention was personalised (tailored) in the sense that each parent/carer contacted was told the number of days that their child was absent and was only sent the text message if their child had attendance <95% in the previous term. The text messages also aimed to include the name of the parent/carer and of the pupil the text message referred to. The text messages were pre-filled in English to include the pupil's name and the number of days they were absent. For the translated text messages, which schools could select if they knew the recipient's primary language was one on the available list of translations, schools needed to add these personalised details manually. The schools were asked to send the text messages to the parents/carers specified on the lists provided by BIT. Finally, schools could opt to not send text messages to any families who they believed the text messages were not appropriate for. This was made explicit to participating schools in the emails they received from the delivery team each term: 'You can choose not to send the message to any parents, where you do not feel it would be appropriate to do so. For example, if a pupil has been seriously unwell, or suffered a family bereavement'.

The Theory of Change for this intervention is shown in Figure 1.

Figure 1: Theory of Change for the BITUP intervention



Evaluation objectives

This evaluation combined an impact evaluation, which sought to evaluate the impact of the intervention on pupil absence rates, and an IPE, which sought to understand how the intervention was implemented and how this process could help explain the impact of the intervention as well as how the intervention could be improved.

The objectives of the evaluation were set out in the Evaluation Protocol (Cornel *et al.,* 2023) and the subsequent Statistical Analysis Plan (Cornel *et al.,* 2024). These were both published by the EEF, and can be found on the EEF BITUP project page. The list of objectives of this evaluation are set out below.

Impact evaluation

Primary outcome

The impact evaluation assessed the effect of the intervention on the outcomes of pupils who were eligible for it. Pupils became eligible for the intervention if their attendance was <95% in any one of Terms 1–5 of the academic year. We compared the absence rates of those whose parents/carers were sent a text message in the intervention arm with those who had attendance <95% in at least one of Terms 1–5 (and therefore, would have been eligible for the intervention) in the control arm. The impact evaluation sought to answer the following primary research questions (RQs):

- **RQ1:** What was the impact of the text messaging intervention on absence rates for pupils aged 11–16 (Years 7–11) in treatment families who were eligible to receive the intervention compared to pupils aged 11–16 (Years 7–11) in control families who were eligible for the intervention, over the whole intervention period?

Given the focus of the funding round on improving outcomes for disadvantaged pupils and the fact that absence rates vary by EAL status,[3] which is a moderating factor in the Theory of Change, we examined the following subgroup RQs:

- **RQ2:** What was the impact of the text messaging intervention on absence rates **for FSM eligible (in the last six years)** pupils aged 11–16 (Years 7–11) who were eligible to receive the intervention in the treatment group, **compared to FSM eligible (in the last six years)** pupils aged 11–16 (Years 7–11) who were eligible to receive the intervention in the control arm?
- **RQ3:** What was the impact of the text messaging intervention on absence rates **for EAL pupils** aged 11–16 (Years 7–11) who were eligible to receive the intervention in the treatment group, **compared to EAL pupils** aged 11–16 (Years 7–11) who were eligible to receive the intervention in the control arm?

We also hypothesised that the impact of the intervention could vary depending on the pupil's gender or school year, given the evidence that attendance varies by age and the possibility that gender moderates the impact of the intervention in the Theory of Change (because of gendered differences in relationships with parents or likelihood of listening to parents). For this reason, we included two further RQs:

- **RQ4:** Did the impact of the text messaging intervention on absence rates **vary by gender** between pupils aged 11–16 (Years 7–11) who were eligible to receive the intervention in the treatment arm, compared to pupils aged 11–16 (Years 7–11) who were eligible to receive the intervention in the control arm?
- **RQ5**: Did the impact of the text messaging intervention on absence rates **vary by year group** between pupils aged 11–16 (Years 7–11) who were eligible to receive the intervention in the treatment arm, compared to pupils aged 11–16 (Years 7–11) who were eligible to receive the intervention in the control arm?

The impact of the intervention could vary by initial absence rate, so we investigated a sixth RQ:

- **RQ6:** Did the impact of the text messaging intervention on absence rates **vary by level of absence in the first term of the school year** between pupils aged 11–16 (Years 7–11) who were eligible to receive the intervention in the treatment arm, compared to pupils aged 11–16 (Years 7–11) who were eligible to receive the intervention in the control arm?

Secondary outcomes

[3] See: https://explore-education-statistics.service.gov.uk/find-statistics/pupil-absence-in-schools-in-england/2023-24

As secondary outcomes, we also examined the mechanism (type of absence rate) by which the intervention influenced absence rates:

- **SRQ1:** What was the impact of the text messaging intervention on **unauthorised absence** rates for pupils aged 11–16 (Years 7–11) in treatment families who were eligible to receive the intervention compared to pupils aged 11–16 (Years 7–11) in control families who would have been eligible for the intervention over the whole intervention period?
- **SRQ2:** What was the impact of the text messaging intervention on **authorised absence** rates for pupils aged 11–16 (Years 7–11) in treatment families who were eligible to receive the intervention compared to pupils aged 11–16 (Years 7–11) in control families who would have been eligible for the intervention over the whole intervention period?

The capacity for an intervention to maintain its effects over time is important when assessing the overall impact of rolling the intervention out more widely. However, across contexts little is known about whether the effects of behavioural interventions persist over time, and there is some evidence that effects of behavioural interventions (including text messages) can diminish over time (Bouton, 2014; Hagger and Weed, 2019; Hecht, Priniski, and Harackiewicz, 2019; Willcox, Dobson, and Whittaker, 2019). One hypothesis we held was that, across the trial period, the intervention would have a larger effect on attendance the first time that it was received (i.e. a novelty effect).

We expected that the more effective the intervention was, the less likely a treated pupil would be to become eligible for the intervention in subsequent terms. Nevertheless, we expected some individuals would be sent multiple text messages during the trial. Therefore, we also conducted an additional analysis to investigate the impact of receiving the treatment for the first time on absence rates in the subsequent term. This answered the research question:

- What was the impact of the text messaging intervention on absence rates for pupils aged 11–16 (Years 7–11) in treatment families **in the first term they were eligible to receive the intervention** compared to pupils aged 11–16 (Years 7–11) in control families in the first term they would have been eligible for the intervention?

There is evidence that absenteeism can vary with the time of year, with a winter peak in absenteeism, possibly due to higher incidence of sickness (Zerbini *et al.,* 2019). As a robustness check to examine seasonal trends, we extended our primary model to examine how the intervention effect changes over intervention terms. This answered the following research question:

- Did the impact of the text messaging intervention on absence rates vary between the intervention terms for secondary school pupils aged 11–16 (Years 7–11) in treatment families eligible to receive the intervention compared to secondary school pupils aged 11–16 (Years 7–11) in control families who would have been eligible for the intervention?

As dosage is a feature of this intervention (parents may be sent multiple text messages across the year) we conducted a dosage analysis, albeit one that has significant caveats. Our ability to conduct a meaningful dosage evaluation for this intervention was severely limited because the effectiveness of the intervention interacts with eligibility during multiple terms. If the intervention was effective, this would reduce the number of text messages families get on average. The data is hard to interpret because, if families were sent more than one dose of the treatment, that could mean the intervention did not have an impact, but it could also be the case that the intervention had an effect but not one that was great enough to raise the attendance of children from that family to at least 95%. Examining the effects of dosage meant specifically looking for the effects of dosage among the subsample of treated families for whom receiving the intervention the first time did not increase the attendance rate to at least 95% in all subsequent terms. If the intervention has a positive impact, we expected the group receiving multiple doses of the messaging to be smaller than that of families receiving one dose. This discrepancy is greater the more effective the intervention is.

Given the reasons outlined above, we anticipated that measuring the impact of the intervention's dosage might not be possible because not all pupils in the sample are eligible for the intervention multiple times (and we therefore, have a smaller sample of pupils at each level of dosage, defined as the number of terms a text message was sent about an individual pupil e.g. having been sent two text messages, three text messages, etc.). Despite this, we included a dosage analysis aimed at answering the following research question:

- Does the impact of the text messaging intervention on absence rates among secondary school pupils aged 11–16 (Years 7–11) differ by dosage, compared to a control group of pupils who would otherwise have been eligible for the intervention?

We were also interested in understanding what the impact of the intervention was on those who received it. Therefore, we ran a compliance analysis to examine the impact on those who were treated. We constructed a compliance indicator of whether the text had been sent to a parent/carer within the first two weeks of term (for the first term post-randomisation only). Whether the text message was sent is a proxy for the parents/carers receiving the text message, as we could not collect data to confirm if text messages were actually received (and seen) by parents/carers. We used this proxy to examine the impact of the intervention on those who were treated. For more information about the compliance analysis, see the section 'Statistical analysis' below.

Finally, we tried to understand the extent to which the intervention impacted on the accuracy of parents'/carers' perceptions of their child's absence. The parent/carer survey, which was sent to parents/carers of pupils who are or would have been eligible to receive the intervention (control and treatment) asked parents/carers how many days they thought their child had been absent from school since the beginning of the school year (based on the approach taken by Rogers and Feller, 2018). This provided an indicative answer to the research question:

- What was the impact of the text messaging intervention on the accuracy of parental knowledge about their child's absence?

IPE

The research questions for the IPE were developed around the following dimensions set out in the EEF IPE guidance (EEF, 2022b)[4]:

- **Usual practice, Programme differentiation:**
  - What is usual practice (e.g. frequency, style) in communication about absence between schools and parents/carers?
  - How different are the text messages from usual communications about attendance?
- **Fidelity (quality):**
  - To what extent was the BITUP intervention implemented as intended?
  - Were school staff able to send out the text messages as planned to the pupils who became eligible in the treatment group?
  - Which school staff members were involved in delivering the intervention?
  - Who in the schools sent out the text messages?
  - Were there any barriers to implementing the text messages?
- **Fidelity (adaptation):**
  - To what extent, and how, did school staff adapt the basic intervention content, and why?
  - Which languages did the school send the text messages in, and how did they decide on which language to use with which family?
  - Did schools use other channels than text message to communicate with parents/carers who did not have phones?
- **Fidelity (reach):**
  - How many text messages were sent out?
  - How did the number of text messages sent out relate to total absence levels?
  - To what extent, and how, did staff decide which parents/carers to contact, or not contact?
- **Fidelity (spillover):**
  - To what extent, if any, were parents/carers and pupils in the control arm reached by, heard about, or influenced by the intervention?
- **Responsiveness:**
  - To what extent did parents/carers remember receiving the text message(s)?
  - To what extent did parents/carers pay attention to/engage with the intervention?
  - Did parents/carers respond to receiving a text message(s), if so, how and why?
  - What did parents/carers think of the text message?

[4] The IPE research questions were adapted slightly from those in the trial protocol to facilitate clear reporting. All pre-registered research questions are covered in the analysis in this report.

- **Perceived impact (parents/carers and pupils' perspectives):**
  - How did parents/carers perceive the impact of the intervention on attendance?
  - How did pupils perceive the impact of the intervention on attendance?
  - Did the text message have any other impacts on parents/carers or pupils both positive or negative?
- **Perceived impact (school perspective):**
  - How did schools perceive the impact of the intervention on parents/carers?
  - How did schools perceive the impact of the intervention on pupils?
  - What did schools think about the intervention?
  - How did schools perceive the impact of the intervention on their internal processes?
  - Did parents/carers, pupils, and schools think the design and delivery of text messages could have been improved to have a bigger impact on attendance (tone, content, mode, frequency, reason for receiving it, timing)?
- **Mediators:**
  - How did the intervention impact pupil absence via the parent/carer through the mechanisms set out in the Theory of Change?
  - How did the intervention impact pupil absence via the pupil through the mechanisms set out in the Theory of Change?
- **Context/moderators:**
  - What factors influenced the impact of the intervention on parents/carers (i.e. who did it work for and under what circumstances)? What role did parents'/carers' perceptions of the importance of attendance (e.g. on educational outcomes), their perceptions of the school and the importance of education, and their behaviours relating to their child's attendance, and other contextual and demographic factors play?

Ethics and trial registration

Ethical approval for this study was obtained from Verian's Research Ethics Committee in September 2023, which reviewed the study design to confirm compliance with ethical standards. Parents/carers of all pupils (in Years 7–11 in September 2023) enrolled in schools that signed up to take part in the study and gave the delivery and evaluation team access to pupil data via Wonde, were offered the opportunity to opt out of data collection prior to the study's launch. Parents/carers could opt their child(ren) out of data collection for the purpose of the study—pupils did not have the opportunity to do so without parental involvement.

The trial was preregistered with the Open Science Framework (OSF) on 05 July 2023 and the trial Evaluation Protocol (Cornel *et al.,* 2023) was uploaded on 02 October 2023 here. The OSF registration of the project, and the trial Evaluation Protocol (Cornel *et al.,* 2023) as uploaded, can be found here.

Data protection

To support their respective activities as evaluators and delivery team, Verian and BIT were each data controllers in relation to personal data (involving pupil and parent/carer data) for this project. Each organisation collected data separately, ensuring that each organisation only accessed personal data necessary for the purposes of the evaluation (Verian) or the delivery of the intervention (BIT). Each organisation took reasonable steps to protect personal information and followed procedures designed to minimise unauthorised access, alteration, loss, or disclosure of personal information.

BIT and Verian dealt with and shared personal data in accordance with a data sharing agreement between the two organisations. Each organisation also established a data sharing agreement with the participating schools, and with the EEF. These agreements set out the purposes for which data could be processed and shared.

Data was accessed by a limited number of researchers and advisors in Verian's evaluation team working on this project. We ensured that those who have permanent or regular access to personal data, or that were involved in the processing of personal data, were trained and informed of their rights and responsibilities when processing personal data. We provided such access on a need-to-know basis and had processes in place to remove that access once it was no longer required.

This project was compliant with the UK General Data Protection Regulation (GDPR), including Articles 6 and 9 (GDPR, 2016), and the Data Protection Act 2018. For all information collected via schools about parents/carers, pupils, and school staff Verian relied on the lawful basis of 'legitimate interests'. We processed sensitive category data on the lawful basis of 'legitimate interests', and per Article 9 of the UK GDPR and Article 89 (GDPR, 2016). Specifically, we collected ethnicity data, SEND data, and public care status data to monitor differential impacts of the intervention and to support equity,

diversity, and inclusion during our research as well as in any further roll-out or scale up of the intervention. All data, including the sensitive category data, was subject to appropriate safeguards for the rights and freedoms of the data subject in accordance with the UK GDPR.

For all information collected via schools about school procedures and operations, and from participants and parents/carers via surveys and interviews, Verian relied on the lawful basis of 'consent'. The Privacy Notices used and published during this project were shared with relevant data subjects (parents/carers, school staff), and each informed the data subject of their rights relating to their personal data (see Memorandum of Understanding [MoU], information for schools and parents/carers, and Privacy Notice in Further Appendices document). Parents/carers could withdraw from the evaluation by emailing the delivery team at any time.

For the purpose of research and archiving, Verian will share data from the impact evaluation with the DfE, the EEF's archive manager, the Office for National Statistics (ONS), and potentially other research teams. At the end of the research project, this data will be submitted to the ONS Secure Research Service (SRS) in the EEF data archive (this is managed by FFT Education). This will include data only identifiable to the DfE and no information will be archived that could be used to directly identify individual pupils. Further matching to the National Pupil Database (NPD) and other administrative data may take place during later research. All data will be securely deleted from Verian's network one year after the end of the project. More information about the EEF data archive can be found here.

Project data governance materials

As part of the recruitment process, schools received the following materials, which set out the project requirements and data governance arrangements:

- **The School Information Sheet**. This provided information about the project for schools, including the benefits of participation and the project and evaluation requirements. It also provided initial information about the project's data governance arrangements.
- **Project MoU.** This described the roles and responsibilities of each party in the project, including relating to data governance. Schools' signing of the MoU indicated their participation in the project and the evaluation.
- **The Verian project Privacy Notice covering data provided by schools.** This described Verian's data processing of administrative data provided by schools for the evaluation and set out the data subjects' rights.
- **The Verian project Privacy Notice covering data provided by individuals during surveys and/or interviews.** This described Verian's data processing of participant response data for the evaluation and set out the data subjects' rights.
- **The BIT project Privacy Notice.** This described BIT's data processing to develop the intervention and deliver it to schools and set out the data subjects' rights.
- **The Parent Information Sheet and Withdrawal Form.** This information sheet was provided to schools for schools to distribute to all parents/carers of their Years 7–11 pupils. It provided information about the project, contained links to the Privacy Notices, and provided a withdrawal form for parents/carers who wished to withdraw from the evaluation.

All recruitment documents can be found in the Further Appendices document.

Project team

BIT developed the intervention, provided valuable insight and learnings from their initial pilot, reviewed and inputted into project documentation and reporting, and provided support of the evaluation throughout the project. The team was led by Lal Chadeesingh, who was supported by Callum O'Mahony, Iori Thomas, and for the first part of the project by Julia Ryle-Hodges.

The evaluation team at Verian was led by Natalie Gold, Head of Trials, who oversaw all aspects of the evaluation. Pieter Cornel managed the delivery of the evaluation. Sarah Bowen supported the delivery of the evaluation, impact evaluation, and IPE. The statistical analysis for the impact evaluation was designed by Debbie Blair (until December 2023) and overseen by Michael Ratajczak. Shi Zhuo ran the primary analysis models and prepared the data and dataset for the repository. Oriol Bosch supported the data analysis. Rupert Riddle, Varvara Kuz, and Emily Bartlett provided further support to the impact evaluation. The process evaluation was designed by Ben Toombs. Penny Stothard was the day-to-day lead, with supervision from Louise Skowron and support from Olivia Sexton (fieldwork and cost analysis), Helen Doran (survey), Maine Charlton (recruitment), and Avindri Chandraharan (fieldwork).

Methods

Trial design

We used a two-armed cluster randomised controlled trial to evaluate the impact of the BITUP text message intervention on pupil attendance. Families were randomly allocated into either the treatment or a business as usual control arm, meaning that all schools in the trial had pupils in both treatment and control arms. For this trial, families were defined as groups of siblings attending the same school based on school sibling marker data. For pragmatic reasons, we randomised all families in the schools before the trial began, but only those pupils whose attendance fell below 95% in at least one of Terms 1–5 became eligible to participate. The trial design is summarised in Table 4.

The primary outcome measure was pupil absence rate. Pupil absence rate was calculated as the number of sessions marked as an authorised or unauthorised absence out of the total number of possible sessions for attendance. In the downloaded data, the sessions can be marked as attended, absent, or void (since the dataset included weekends, bank holidays, in-service training [INSET] days, etc.). We define possible attendable sessions at the pupil level, as sessions marked as attended or absent. Each school day is made up of two sessions, a morning and afternoon session, when attendance is recorded. Our secondary outcome measures were pupil authorised absence rate and pupil unauthorised absence rate.

There were no changes to the original trial design as outlined in the trial Evaluation Protocol (Cornel *et al.,* 2023) and the Statistical Analysis Plan (Cornel *et al.,* 2024), which can be found on the EEF BITUP project page here.

Table 4: Trial design

Trial design, including no. of arms		Two-armed cluster randomised controlled trial
Unit of randomisation		Family (siblings enrolled in the same school)
Stratification variable (s) (if applicable)		FSM status (dichotomous) EAL status (dichotomous)
Primary outcome	Variable	Pupil absence rate
	Measure (instrument, scale, source)	Pupil attendance record in Terms 2–6 Continuous measure between 0 and 1 Sourced from Wonde linked to the school Management Information System
Secondary outcomes	Variables	Unauthorised pupil absence rate Authorised pupil absence rate
	Measures (instrument, scale, source)	Pupil attendance record in Terms 2–6 Continuous measure between 0 and 1 Sourced from Wonde linked to the school Management Information System

Participant selection

The trial focused on pupils in Key Stages 3 and 4 (or Years 7–11) in England. Therefore, the inclusion criteria for schools were being a secondary school, state-funded, and located in England. Participating schools were required to be connected to Wonde, a secure data sharing platform, to allow the evaluation and delivery teams to access pupil data for randomisation, intervention delivery, and analysis. Existing connection to Wonde was not an inclusion criterion, and schools not already connected were onboarded to Wonde by the delivery team in preparation for the trial. However, use of a compatible Management Information System was an inclusion criterion (e.g. SIMS, Arbor, Bromcom, RM integris, Scholarpack REST).[5]

Schools were recruited into the trial by the delivery team. The delivery team led school recruitment from March 2023 to July 2023, drawing on established relationships with school networks.

[5] Around 91% of schools connected to Wonde at the beginning of 2023 used a compatible Management Information System.

In October 2023, the evaluation team randomised families into the treatment or control arm within schools. Following randomisation, the delivery team had the responsibility of drawing attendance data each term, identifying the pupils in treatment arm families who had become eligible for the intervention at the beginning of the next term and calculating the number of days of school missed to include in the intervention message. The delivery team then used this information to generate personalised text messages for all eligible pupils and sent these to schools in a spreadsheet. Schools then delivered the intervention to eligible pupils in treatment arm families, by sending the text messages provided by BIT to eligible parents/carers at the beginning of the new term.

Outcome measures

Primary outcome: Pupil absence rate

The primary outcome measure was pupil absence rate. Absence rate was defined as the number of sessions marked as an unauthorised or authorised absence out of the total number of attendable sessions in the academic year. A session refers to half a school day as attendance is taken twice per day, once in the morning (a.m. session) and once in the afternoon (p.m. session). The primary outcome measure was calculated using attendance data in Terms 2–6 (inclusive). The first round of intervention text messages were sent out at the beginning of Term 2, so Term 1 attendance was not included in our outcome measure.

Outcomes were measured using attendance data for each pupil accessed at the end of the 2023–2024 school year via Wonde. This included the number of sessions that could have been attended (attendable sessions), were attended (present sessions), and were not attended (authorised and unauthorised absence sessions) in each term. Wonde integrates with the Management Information System in each school, which is used to record and manage all school data and allows schools to easily share 'live' data with partners (e.g. timetabling programmes, or EdTech apps). Attendance data recorded through Wonde is the same data that schools upload to the NPD. This means the data collected on the Wonde platform is highly accurate and aligned with school records.

Table 5 summarises the present, authorised absence, and unauthorised absence attendance session codes used to calculate our outcome measure. Absences are authorised if they are permitted by the school, including those where a satisfactory explanation is provided (e.g. pupil illness), and are unauthorised when taken without school permission (e.g. family holidays not agreed with the school).

Some changes were made to the coding of attendance sessions by the delivery and evaluation teams during the year of the evaluation, to determine eligibility for the intervention and the primary and secondary outcome measures:

- Sessions marked as 'Late (after registers closed)' were treated as an attended session (Present) not an unattended session (Unauthorised absence). This meant that sessions marked as 'Late (after registers closed)' did not reduce attendance rates used to determine eligibility for the wave of interventions (text messages), did not count towards the days of school missed included in the text messages, and did not increase our measure of pupil absence rate.[6]
- Sessions marked as 'Study leave' for pupils in Years 10 and 11 in Terms 5 and 6 were treated as an attended session (Present). This meant that 'Study leave' sessions in Term 5 did not reduce attendance rates used to determine eligibility for the intervention in Term 6 for pupils in Years 10 and 11, did not count towards the days of school missed included in the text messages, and did not increase our outcome measure of pupil absence rate. All other sessions marked as 'Study leave' were treated as an unattended session (Authorised absence).[7]

[6] The delivery team made the same changes to session codes to determine eligibility for the intervention based on feedback from the schools during the trial. For consistency, the evaluation team also implemented these session code changes to define the outcome variables.

[7] Across six schools, 14 pupils in Years 7, 8, and 9 had one session (half of a school day) marked as 'Study leave' (one in Term 1, one in Term 2, two in Term 3, seven in Term 4, one in Term 5, and two in Term 6). There are also 647 instances where a pupil in Years 10 and 11 had one, two, or three sessions marked as 'Study leave' in Terms 2 and 3. In all these cases, the session was treated as an authorised absence.

Table 5: Attendance codes used to count 'present', 'authorised absence', 'unauthorised absence' sessions for outcome measure

Attendance type	Attendance codes
Present	• Approved education activity as pupil is attending interview • Approved education activity as pupil is attending work experience • Approved sporting activity • Attending a place, other than the school or another school at which they are a registered pupil, for educational provision arranged by the local authority • Educated off site (not Dual Registration) • Educational visit or trip • In school but not in timetabled class/designated lesson • Internal exclusion • Late (after registers closed)[a] • Late (before registers closed) • Medical/First aid room • Present • Study leave (for Years 10 and 11 pupils in Terms 5 and 6)[a] • Thrive • Truancy on site • With head of year/senior leadership team
Authorised absence	• Adapted timetable – Not Required to Attend • Anxiety/Mental Health – Formal Diagnosis Received • Anxiety/Mental Health – No Formal Diagnosis Received • Excluded • Extended family holiday (agreed) • Family holiday (agreed) • Illness (not medical/dental appointments) • Illness due to COVID-19 • Medical/dental appointments • Other authorised circumstance • Religious observance • Study leave (for Years 7–9 in all terms, and Years 10–11 in Terms 2, 3, and, 4) • Traveller absence
Unauthorised absence	• Family holiday (not agreed or days in excess) • No reason yet provided for absence • Unauthorised absence (not covered by any other code)

[a]*Indicates where an attendance code was re-coded by the evaluation and delivery team as* ***Present*** *for the purposes of the trial.*

Secondary outcomes: Pupil authorised absence rate and pupil unauthorised absence rate

Our secondary outcome measures were pupil authorised absence rate and unauthorised absence rate. Disaggregating the different absence rates allowed us to test whether the intervention affected the balance of authorised and unauthorised absences, as well as total absence rates.

Pupil unauthorised absence rate was calculated as the number of sessions marked as an unauthorised absence out of the total number of attendable sessions in the academic year (excluding the first term). Similarly, the pupil authorised absence rate was calculated as the number of sessions marked as an authorised absence out of the total number of attendable sessions in the academic year (excluding the first term). We made the same exceptions for sessions marked as 'Late (after registers closed)' and 'Study leave' for Years 10 and 11 in Terms 5 and 6 in calculating the secondary outcomes, as we did with the primary outcome.

Sample size

The minimum detectable effect size (MDES) was calculated using 'PowerUpR' in R at the protocol, randomisation, and primary outcome analysis stage. At trial protocol and randomisation, we ran sample size calculations for a two-armed, two-level cluster design with pupils nested in families. The unit of randomisation was families, and the unit of analysis was

pupils. At analysis, we ran sample size calculations for a two-armed, three-level cluster design, with pupils nested in families, and families nested in schools.
At protocol, we estimated 100 schools would need to participate in the trial to detect an MDES of 0.011 standard deviations (SDs) for the primary analysis of absence rate among Year 7–11 pupils, and 0.022 among those pupils eligible for FSM. This MDES corresponded to being able to detect 0.3–0.5% difference in attendance rate, which we aimed for given that the treatment group in the BITUP pilot had an increase in attendance of 0.3% (though this was not statistically significant). In terms of days of school missed, this equates to a difference of on average 0.44 days (1.03 days for the FSM subsample) in total over the five terms post-intervention, between the intervention and control arm.[8] To account for any potential attrition, we recommended that 115 schools be recruited for the evaluation.

The trial protocol power calculations assumed:

- a total of 895.17 Years 7–11 pupils per school, and 518.34 families per school (sets of siblings attending the same school in Years 7–11);[9]
- a total of 362.55 Years 7–11 pupils per school, in 293.04 families per school, would have attendance rates below 95% in at least one of Terms 1–5 (eligible for the intervention and included in the impact evaluation);
- 23.8% of pupils would be eligible for FSM in the last six years;[10]
- α = 0.05 (which refers to the probability of rejecting the hypothesis tested when it is true);
- intracluster correlation coefficient (ICC) for schools = 0.0598 (which refers to the variance between participants in the same school);[11]
- proportion of variance explained by covariates at the pupil level = 0.801, and at the family level = 0.5;[12] and
- baseline absence rate of 6.25% (based on the Bristol BITUP trial pilot study).

The number of pupils per school with attendance rates below 95% in at least one of Terms 1–5 was based on attendance rates observed in the Bristol BITUP trial pilot study (BIT, 2020).[13] The pilot found 40.5% of pupils per school had an attendance rate that was below 95% in at least one term during the trial period. This was considered a conservative estimate as it was based on attendance across four terms (Terms 3–6), and it is possible that pupils with an attendance rate of 95% or above over this whole period could have seen their attendance rate fall below 95% in a single term.

We reran the sample size calculations after randomisation for 108 schools. At this point, we updated our assumptions about the number of pupils and families per school in Years 7–11 and FSM eligibility:

- a total of 963.23 Years 7–11 pupils per school, and 813.97 families per school (groups of siblings attending the same school in Years 7–11);
- a total of 390.11 Years 7–11 pupils per school, in 364.60 families per school would become eligible for the intervention and included in the impact evaluation; and
- 28.9% of pupils would be eligible for FSM in the last six years.

Holding all other assumptions constant, the revised sample size calculations at randomisation yielded an MDES of 0.012, similar to the MDES reported at the protocol stage. Among pupils eligible for FSM, our revised MDES at randomisation with 108 schools was 0.022, which was the same as our initial expectation of 0.022 with 115 schools in the trial protocol.

[8] We estimated this by taking the absence rate of 11.79% in the control arm as the baseline absence rate (16.98% for the FSM subsample). Given at protocol stage, power calculations suggested we were powered to detect a minimum percentage point change of 0.28% (0.55% for FSM). This translates to a reduction in absence rate to 11.51% (16.43% for the FSM subsample). The number of days missed is calculated based on the assumption that there are 190 days in a school year, with pupils being able to attend an average of 158.33 days in Terms 2–6 (or 316.67 sessions).
[9] In 2021–2022, there were 3,401 secondary schools in England and a total of 3,044,476 Years 7–11 pupils. This estimate comes from using National Schools Pupils and Characteristics 2021/2022 (https://explore-education-statistics.service.gov.uk/find-statistics/school-pupils-and-their-characteristics/2021-22): Pupil gender and year group: 2022 (https://explore-education-statistics.service.gov.uk/find-statistics/school-pupils-and-their-characteristics/2022-23).
[10] This estimate comes using National Schools Pupils and Characteristics 2022/2023 (https://explore-education-statistics.service.gov.uk/find-statistics/school-pupils-and-their-characteristics/2022-23). We expected pupil FSM status would be highly correlated within families. Therefore, to randomise families into the treatment group by FSM status, we treated any family as FSM eligible if at least one pupil in the family was eligible for FSM.
[11] We estimated the ICC from the Bristol BITUP trial pilot study.
[12] The proportion of variance explained by covariates was estimated using data from a previous related evaluation (Mills *et al.,* 2022).
[13] We also assumed that 40.5% of FSM pupils would have an attendance rate below 95%. However, we anticipated that this is likely to be a conservative estimate as pupils eligible for free FSM have higher rates of absence than other pupils at all ages (Middlemas, 2018).

At analysis we updated our assumptions about the number of pupils, families, and schools in our primary analysis model, ICCs, and pre- and post-correlations:

- a total of 71,916 Years 7–11 pupils in 63,295 families (of which there are 22,841 FSM pupils, in 19,427 families) in 105 schools;
- school ICC = 0.032 (0.033 for FSM subsample) and family ICC = 0.506 (0.552 for FSM subsample); and
- proportion of variance explained by covariates at the pupil level = 0.377 (0.420 for FSM subsample), and at the family level = 0 (due to an absence of family-level covariates in the model).

The sample size calculations at analysis show that we are powered to detect a main effect MDES of 0.020, and an MDES of 0.035 for the FSM subgroup.

Randomisation

The randomisation was done in R using package 'randomizr'.[14] Families were randomly allocated to the treatment or control arm, to ensure that all pupils sharing a parent/carer were assigned to the same arm and therefore, mitigate against contamination between treatment and control participants. Randomisation was carried out on 19 October 2023. All families in each school were randomised at this time, as it would not have been feasible to randomise the eligible pupils in each term given the short time between the end of term (eligibility period) and the dissemination of the text messages to schools (start of next term).

To generate the random allocation sequence, the evaluation team accessed pupil data using Wonde for the first time one week before randomisation. First, school-defined sibling markers were used to identify pupils who belonged to the same family unit. This data was used to create 87,909 unique family identifications (IDs) (an average of 1.18 pupils per family) for randomisation. Following this, 87,909 families containing 104,029 pupils from 108 schools were randomly allocated to either the treatment or control arm. Randomisation was stratified by FSM eligibility and EAL status at the family level. The code used for randomisation can be found in the Appendix to the Statistical Analysis Plan (Cornel *et al.,* 2024), can be found on the EEF BITUP project page here.

One researcher in the evaluation team conducted the randomisation, then code and randomisation results were checked by a second evaluation team researcher. Independence of allocation was assured by the R command 'block_ra', which implemented a random assignment procedure in which units that are grouped into blocks defined by pre-treatment covariates are assigned using complete random assignment within block.

The same researcher who conducted the randomisation undertook the analysis. The analysis was not undertaken blinded to treatment allocation. However, analysis was done by a member of the evaluation team who did not have direct contact with schools or families in the trial, and the outcome measure (attendance rate) was objective and could not have been influenced by the delivery or evaluation team. Therefore, the risk of bias was low.

Statistical analysis

Primary analysis

The primary analysis was intention-to-treat (ITT) using multi-level modelling to reflect the clustering of pupils (Level 1) within families (Level 2) nested within schools (Level 3). This hierarchical clustering was best modelled using a multi-level model because it provided a more precise estimate of the treatment effect compared to fixed effects with clustered adjusted standard errors, meaning we had higher power to detect an effect. This relied on the assumption that the predictors were uncorrelated with the random effects. The design of our trial and the quality of our data provided reasonable assurance of this assumption.[15] Given that we had one primary research question and two arms we did not adjust for multiple hypotheses testing.

[14] See: https://cran.r-project.org/web/packages/randomizr/randomizr.pdf.

[15] Randomisation is designed to ensure that treatment allocation is independent of both observed and unobserved baseline characteristics (the probability of this increases with sample size). This means that any random effects should be uncorrelated with the predictors in our model, including treatment assignment and other covariates. The fact that we have very few missing values for primary and secondary outcomes strengthens this assumption, as it reduces the potential for bias due to missing data. Additionally, balanced baseline characteristics across treatment arms further support the idea that the predictors are uncorrelated with the random effects.

We investigated the average impact of the intervention over Terms 2–6 by comparing the attendance of pupils in the treatment group who were eligible to receive the intervention against pupils in the control arm who would have been eligible to receive the intervention, had they been in the treatment group. Pupils were eligible to receive the intervention if their attendance rate was below 95% in at least one of Terms 1–5. We used the following primary model:

$$Y_{ijk} = \beta_0 + \gamma_k + u_{jk} + \beta_1 Prior_{ijk} + \beta_2 Intervention_{jk} + \beta_3 FSM_{ijk} + \beta_4 EAL_{ijk} + \varepsilon_{ijk},$$

Where:

- Y_{ijk} was the absence rate of the i^{th} pupil ($i = 1, \dots, n,$ where n is the number of participating pupils) in the j^{th} family ($j = 1, \dots, m$, where m is the number of participating families) in k^{th} school ($k = 1, \dots, l$, where l is the number of participating schools)
- $Prior_{ijk}$ was the absence rate in Term 1 for the i^{th} pupil in j^{th} family in k^{th} school.
- $Intervention_{jk}$ was an indicator variable for intervention allocation for the j^{th} family in k^{th} school (0 = control, 1 = intervention).
- γ_k was the deviation of school $k's$ mean z-score from the grand mean ($\gamma_k \sim N(0, \sigma_1^2)$).
- u_{jk} was the deviation of j^{th} family within a particular k^{th} school ($u_{jk} \sim N(0, \sigma_2^2)$).
- ε_{ijk} was the residual error term for the i^{th} pupil in j^{th} family in k^{th} school ($\varepsilon_{ijk} \sim N(0, \sigma_3^2)$).

The multi-level nature of this equation (u_{jk}) accounted for the randomisation within school. Note that in this model and in the models that follow we did not transform or scale variables.

We included controls for our pupil-level stratification variables, FSM (using FSM_EVER_6, coded in Wonde as extended_details.data.free_school_meals_6) and EAL (using the variable coded in Wonde as extended_details.data.english_as_additional_language). FSM was a binary variable with two levels: ever eligible for FSM in the last six years; or not. EAL was a categorical variable with three levels: EAL; non-EAL; and not known. The EAL data did not allow us to assess if the alternative language was one of the languages that BIT had translated the text messages into.

Secondary analysis

Our two secondary outcomes (authorised and unauthorised absence rate) were analysed using the same approach as for the primary outcome. The specification of these models was the same, but with the coefficient for the absence rate in the first term referring to either the authorised or unauthorised absence rate, respectively.

Analysis in the presence of non-compliance

We defined compliance at the pupil level. We do not have information on receipt of texts, so compliance was determined by the schools' actions of sending texts. Given the intervention is considered to have two key components—including the number of days missed in the previous term in the message and being sent to parents/carers within the first two weeks of term (to harness the fresh start effect)—our measure of compliance would ideally include both components. However, we did not have access to the text messages sent by schools, so our compliance analysis relies on self-reported data from schools about the number of text messages sent and who they were sent to.

To capture compliance data, the delivery team sent schools a monitoring form at the beginning of each term in Terms 2–6 that they participated in the trial, up to five forms in total. Each form contained the list of pupils in the intervention arm eligible for the intervention that term, including: the number of days of school they had missed last term; and a personalised text message to send to their parent/carer. Schools were instructed to update the monitoring form with:

- the date the text messages were sent out;
- for each pupil, whether a text was, or was not, sent; and
- the language the text message was sent in.

Schools were instructed to send the updated monitoring form to the evaluation or delivery team by the end of the same term.

With this compliance data, we constructed two binary compliance indicators:

- Primary indicator (sent the text):
    a. Compliance = 0 if a pupil's family was not sent a text message in a term they were eligible.

   b. Compliance = 1 if a pupil's family was sent a text message in a term they were eligible.
- Secondary indicator (timeliness):
   a. Compliance = 0 if the text message was sent more than two weeks after the start of term.
   b. Compliance = 1 if the text message was sent within the first two weeks of term.

'Within the first two weeks of term' was selected by the delivery and evaluation team as a sensible threshold to capture whether the intervention was sent at the 'beginning of term' or not, and hence whether the 'fresh start effect' had been harnessed.

Measures were taken by the delivery team to reduce the administrative burden on schools of completing and returning the monitoring forms, including sending the forms with pre-filled fields and allowing schools to return the form to only one of the delivery or evaluation teams. In response to the low number of schools returning their monitoring forms, in Term 3 the evaluation team, the EEF, and the YEF agreed to introduce a financial incentive of £30 per form returned.

To estimate the Complier Average Causal Effect (CACE), we took an instrumental variable approach using two-stage least squares (2SLS) using the primary outcome only. This analysis was conducted using absence rates from the first term a pupil became eligible to receive the intervention as the outcome.

To ensure the CACE estimator was comparable to the ITT estimator, we generated and compared the ITT estimator using absence rates from the first term a pupil became eligible to receive the intervention as the outcome. We did not compare the CACE estimator to the primary analysis (using pupil absence rates in Terms 2–6 as the outcome measure) due to the treatment-inherent nature of this: if the treatment is effective at reducing pupil absence rates (improves pupil attendance to 95% or higher), then the likelihood of receiving a text message in a subsequent term will go down.

Missing data analysis

We ran cross-tabulations of the proportions of missing values on all characteristics used in the analysis (at the pupil, family, and school levels) to assess missingness. This allowed us to identify whether missingness was a school-level issue and map out any outliers.

Subgroup analyses

The impact of the intervention on our primary outcome was investigated for five characteristics of interest. These were: FSM (RQ2), EAL (RQ3), gender (RQ4), year group (RQ5), and absence rate in Term 1 **(**RQ6**)**. We did not look at ethnic minority subgroups, because the sample sizes were too low to draw meaningful conclusions (e.g. Gypsy, Roma, and Traveller accounted for less than 1% of the sample). Specifically, such low sample sizes would be likely to yield imprecise and potentially misleading estimates. We note, however, that FSM and EAL capture substantial, though not exhaustive, portions of the variance that is correlated with ethnicity.

FSM and gender are binary indicators, with FSM = 1 indicating the pupil is eligible for FSM (or has been eligible for FSM in the last six years), and gender indicating if the pupil is male or female. There are three levels for EAL—either a pupil is identified by the school as speaking EAL, not speaking EAL (Non-EAL), or the data is missing (Not Known). For year group, we have five year groups taking part in the intervention—Years 7–11. Initial absence rate, absence rate in Term 1, is treated as a continuous measure.

For each subgroup (FSM (RQ2), EAL (RQ3), and gender (RQ4)), we ran a model with an interaction term:

$$Y_{ijk} = \beta_0 + \gamma_k + u_{jk} + \beta_1 Prior_{ijk} + \beta_2 Intervention_{jk} + \beta_3 FSM_{ijk} + \beta_4 EAL_{ijk} + \beta_5 (Intervention_{jk}.X_{ijk}) + \varepsilon_{ijk},$$

where $X_{ijk}$is equal to FSM, EAL, or gender at the pupil level, respectively. For the gender subgroup analysis, gender is also added as a fixed effect to the model. This model examines the effects of the intervention by each subgroup (FSM, EAL, and gender), as well as determining whether there are significant differences between the subgroups.

For the year group subgroup analysis (RQ5), we ran a model with four interaction terms:

$$\begin{aligned} Y_{ijk} = \beta_0 + \gamma_k + u_{jk} + \beta_1 Prior_{ijk} + \beta_2 Intervention_{jk} + \beta_3 FSM_{ijk} + \beta_4 EAL_{ijk} + \beta_5 \big(Intervention_{jk}.Yr8_{ijk}\big) \\ + \beta_6 \big(Intervention_{jk}.Yr9_{ijk}\big) + \beta_7 \big(Intervention_{jk}.Yr10_{ijk}\big) + \beta_8 \big(Intervention_{jk}.Yr11_{ijk}\big) + \beta_9 Yr8_{ijk} \\ + \beta_{10} Yr9_{ijk} + \beta_{11} Yr10_{ijk} + \beta_{12} Yr11_{ijk} + \varepsilon_{ijk}. \end{aligned}$$

Year 7 was designated as the reference category. This meant that the coefficients for the interaction between the intervention and each year group (Years 8–11) were interpreted in relation to the baseline established by Year 7. The reference category, Year 7, was not explicitly included in the model as its effect was accounted for in the intercept (β_0).

For initial absence rate in Term 1 (RQ6), the model was specified as follows:

$$Y_{ijk} = \beta_0 + \gamma_k + u_{jk} + \beta_1 Prior_{ijk} + \beta_2 Intervention_{jk} + \beta_3 FSM_{ijk} + \beta_4 EAL_{ijk} + \beta_5 (Intervention_{jk}. Prior_{ijk}) + \varepsilon_{ijk}.$$

Where $Prior_{ijk}$ was a continuous variable equal to the absence rate in Term 1 at the pupil level.

In line with the EEF analysis guidance (EEF, 2022c), we also ran a split sample analysis for each subgroup, containing only pupils from that subgroup.

Additional analyses and robustness checks

First time of receiving a text message

To investigate the impact of receiving the treatment for the first time on absence rates in the subsequent term, we used the following model:

$$Y_{ijk} = \beta_0 + \gamma_k + u_{jk} + \beta_1 Prior_{ijk} + \beta_2 Intervention_{jk} + \beta_3 FSM_{ijk} + \beta_4 EAL_{ijk} + \varepsilon_{ijk},$$

where Y_{ijk} was the absence rate of the i^{th} pupil ($i = 1, \dots, n$, where n was the number of participating pupils) in the j^{th} family ($j = 1, \dots, m$, where m was the number of participating families) in the k^{th} school ($k = 1, \dots, l$, where l was the number of participating schools), in the first term a pupil was (or would be, in the case of control families) eligible to receive the intervention. The rest of the terms were the same as in the primary analysis model.

Depending on the term

As a robustness check to examine seasonal trends, we extended our primary model to examine how the intervention effect changes over intervention terms:

$$Y_{tijk} = \beta_0 + \gamma_k + u_{jk} + \tau_{ijk} + \beta_1 Prior_{ijk} + \beta_2 Intervention_{jkt} + \beta_3 FSM_{ijk} + \beta_4 EAL_{ijk} + \beta_5 Term_t + \beta_6 Intervention * Term_{tjk} + \varepsilon_{tijk},$$

where $Term_t$ was a categorical variable indicating the t^{th} term in the 2023–2024 school year ($t = 2, \dots, 6$), $Intervention * Term_{tjk}$ was an interaction term indicating allocation for the j^{th} family in the k^{th} school in the t^{th} Term (0 = control, 1 = intervention), and τ_{ijk} was a random effect for each pupil, capturing the individual-specific variation that was consistent across different terms.

School-level factors

As schools vary across observable and unobservable factors (including ones that may impact attendance levels, such as the approaches they take to improving attendance) it is reasonable to assume that eligibility for the intervention and the effectiveness of the intervention may vary across schools. As a robustness check to control for this, we added a school-level variable for the proportion of pupils of that school who became eligible to receive a text message:

$$Y_{ijk} = \beta_0 + \gamma_k + u_{jk} + \beta_1 Prior_{ijk} + \beta_2 Intervention_{jk} + \beta_3 FSM_{ijk} + \beta_4 EAL_{ijk} + \beta_5 SPPE_k + \varepsilon_{ijk},$$

where $SPPE_k$ specified the proportion of pupils in k^{th} school who became eligible to receive a text message.

Dosage

We used the following model to investigate dosage:

$$Y_{ijk} = \beta_0 + \gamma_k + u_{jk} + \beta_1 Prior_{ijk} + \beta_2 Intervention_{jk} + \beta_3 FSM_{ijk} + \beta_4 EAL_{ijk} + \beta_5 Dose_{ijk} + \beta_6 (Intervention_{jk}. Dose_{ijk}) + \varepsilon_{ijk},$$

where $Dose_{ijk}$ is a categorical variable indicating the number of doses sent to parents/carers about each pupil (e.g. two doses, three doses, four doses, etc.), with one dose as baseline.

Estimation of effect sizes

An adaptation of Hedges' g (Hedges, 2007) was used to estimate the effect size of the intervention. This approach follows previous efficacy trials by the EEF involving measures of reading attainment, comprehension, and fluency (Dimova and Illie, 2021). Specifically:

$$g = \frac{(\hat{Y}_T - \hat{Y}_C)_{adjusted}}{\sqrt{\sigma_s^2 + \sigma_f^2 + \sigma_{error}^2}}$$

where $(\hat{Y}_T - \hat{Y}_C)_{adjusted}$ is the mean difference between treatment and control arms adjusted for baseline attendance rate and $\sqrt{\sigma_s^2 + \sigma_f^2 + \sigma_{error}^2}$ is an estimate of the population SD (school-level σ_s^2, family-level σ_f^2, and individual-level σ_{error}^2 variance).

CIs for the effect size were calculated using:

$$\hat{g} - c_{\frac{\alpha}{2}}\sqrt{\gamma_{\hat{g}}} \leq \hat{g} \leq \hat{g} + c_{\frac{\alpha}{2}}\sqrt{\gamma_{\hat{g}}}$$

where $\hat{g}$ is the estimated effect size, and $\sqrt{\gamma_{\hat{g}}}$ is the estimated standard error, and $c_{\frac{\alpha}{2}}$ is the 100(1- $\alpha/2$) percentage point of the standard normal distribution.

Estimation of ICC

We report the post-test ICCs at family level and school level using empty hierarchical linear models from the primary outcome analysis model (with no predictors) as follows:

$$Y_{ijk} = \beta_0 + \gamma_k + u_{jk} + \varepsilon_{ijk},$$

where Y_{ijk} was the absence rate of the i^{th} pupil, in the j^{th} family, in k^{th} school, β_0 is the fixed intercept, γ_k was a school-level random effect ($\gamma_k \sim N(0, \sigma_s^2)$), u_{jk} was a family-level random effect clustered within schools ($u_{jk} \sim N(0, \sigma_f^2)$), and ε_{ijk} was the residual error term ($\varepsilon_{ijk} \sim N(0, \sigma_{error}^2)$). The ICC estimate was recovered as follows for families and schools:

$$ICC_{family} = \frac{\sigma_f^2}{\sigma_1^2 + \sigma_f^2 + \sigma_{error}^2}$$

$$ICC_{school} = \frac{\sigma_s^2}{\sigma_s^2 + \sigma_f^2 + \sigma_{error}^2}$$

Longitudinal analysis

No longitudinal analysis or follow-up with schools or families was planned.

IPE

Research methods

Table 6 and Table 7 outline the IPE research methods, sample designs, and analysis approaches used to answer the IPE research questions.

Table 6: IPE methods overview (simple)

Sample	Schools					Parents/carers			Pupils		Delivery team
Method and timing	Monitoring data in Terms 2–6	Survey		Interviews		Survey	Interviews		Interviews		Interviews
		Baseline	Endline	Baseline	Endline	Endline	Midline	Endline	Midline	Endline	Endline
Usual practice, Programme differentiation		✓		✓	✓						
Fidelity (quality)	✓		✓		✓						✓
Fidelity (adaptation)	✓		✓		✓						
Fidelity (reach)	✓		✓		✓						
Fidelity (spillover)	✓					✓	✓	✓	✓	✓	
Responsiveness						✓	✓	✓			
Perceived impact				✓	✓	✓	✓	✓	✓	✓	
Mediators					✓	✓	✓	✓	✓	✓	
Context/moderators					✓		✓	✓	✓	✓	
Cost		✓	✓	✓	✓						✓

Table 7: IPE methods overview (detailed)

IPE dimension	Method	Sample	Timing	Sample description	Data analysis methods
Usual practice, Programme differentiation	Survey	School	Baseline	90x baseline survey responses by an administrator or senior leader responsible for the attendance intervention, at each school	Cross-sectional descriptive statistics
	Interviews	School	Baseline and endline	10x baseline interviews with school staff (administrators, attendance officers, or senior leader responsible for attendance) 9x endline interviews with school staff (administrators, attendance officers, or senior leader responsible for attendance)	Framework analysis using a deductive-inductive approach
Fidelity (quality)	Monitoring data	School	Terms 2–6	Data collected from schools every term in the intervention period (one observation per school in each of Terms 2–6)	Cross-sectional descriptive statistics
	Survey	School	Endline	42x administrator or senior leader responsible for the attendance intervention, at each school (one response per school)	Cross-sectional descriptive statistics
	Interviews	School	Endline	10x endline interviews with school staff (administrators, attendance officers, or senior leader responsible for attendance)	Framework analysis using a deductive-inductive approach
	Interviews	Delivery team	Endline	2x interviews with BIT staff working on the project	Framework analysis using a deductive-inductive approach
Fidelity (adaptation, reach)	Monitoring data	School	Terms 2–6	Data collected from schools every term in the intervention period (one observation per school in each of Terms 2–6)	Cross-sectional descriptive statistics
	Survey	School	Endline	42x administrator or senior leader responsible for the attendance intervention, at each school (one response per school)	Cross-sectional descriptive statistics
	Interviews	School	Endline	9x endline interviews with school staff (administrators, attendance officers, or senior leader responsible for attendance)	Framework analysis using a deductive-inductive approach
Fidelity (spillover)	Monitoring data	School	Terms 2–6	Data collected from schools every term in the intervention period (one observation per school in each of Terms 2–6)	Cross-sectional descriptive statistics
	Survey	Parent/carer	Endline	781x parents/carers in the control and treatment group whose child(ren)'s attendance was <95% in at least one term eligibility for text messages was assessed (Terms 1–5)	Cross-sectional descriptive statistics
	Interviews	Parent/carer	Midline and endline	45x midline/endline interviews with parents/carers	Framework analysis using a deductive-inductive approach
	Interviews	Pupil	Midline and endline	45x midline/endline interviews with pupils	Framework analysis using a deductive-inductive approach

IPE dimension	Method	Sample	Timing	Sample description	Data analysis methods
Responsiveness	Survey	Parent/carer	Endline	781x parents/carers in the control and treatment group whose child(ren)'s attendance was <95% in at least one term eligibility for text messages was assessed (Terms 1–5)	Cross-sectional descriptive statistics
	Interviews	Parent/carer	Midline and endline	45x midline/endline interviews with parents/carers	Framework analysis using a deductive-inductive approach
Perceived impact	Survey	Parent/carer	Endline	781x parents/carers in the control and treatment group whose child(ren)'s attendance was <95% in at least one term eligibility for text messages was assessed (Terms 1–5)	Cross-sectional descriptive statistics
	Interviews	Parent/carer	Midline and endline	45x midline/endline interviews with parents/carers	Framework analysis using a deductive-inductive approach
	Interviews	Pupil	Midline and endline	45x midline/endline interviews with pupils	Framework analysis using a deductive-inductive approach
	Interviews	School	Baseline and endline	10x baseline interviews with school staff (administrators, attendance officers, or senior leader responsible for attendance) 9x endline interviews with school staff (administrators, attendance officers, or senior leader responsible for attendance)	Framework analysis using a deductive-inductive approach
Mediators	Survey	Parent/carer	Endline	781x parents/carers in the control and treatment group whose child(ren)'s attendance was <95% in at least one term eligibility for text messages was assessed (Terms 1–5)	Cross-sectional descriptive statistics
	Interviews	Parent/carer	Midline and endline	45x midline/endline interviews with parents/carers	Framework analysis using a deductive-inductive approach
	Interviews	Pupil	Midline and endline	45x midline/endline interviews with pupils	Framework analysis using a deductive-inductive approach
	Interviews	School	Endline	9x endline interviews with school staff (administrators, attendance officers, or senior leader responsible for attendance)	Framework analysis using a deductive-inductive approach
Context / moderators	Interviews	Parent/carer	Midline and endline	45x midline/endline interviews with parents/carers	Framework analysis using a deductive-inductive approach
	Interviews	Pupil	Midline and endline	45x midline/endline interviews with pupils	Framework analysis using a deductive-inductive approach
	Interviews	School	Endline	9x endline interviews with school staff (administrators, attendance officers, or senior leader responsible for attendance)	Framework analysis using a deductive-inductive approach
Cost	Survey	School	Baseline and endline	90x baseline survey responses by an administrator or senior leader responsible for the attendance intervention, at each school	Cross-sectional descriptive statistics

IPE dimension	Method	Sample	Timing	Sample description	Data analysis methods
				39x endline survey responses by an administrator or senior leader responsible for the attendance intervention, at each school	
	Interviews	School	Baseline and endline	10x baseline interviews with school staff (administrators, attendance officers, or senior leader responsible for attendance) 9x endline interviews with school staff (administrators, attendance officers, or senior leader responsible for attendance)	Framework analysis using a deductive-inductive approach
	Interviews	Delivery team	Endline	2x interviews with BIT staff working on the project	Framework analysis using a deductive-inductive approach

Data collection

Qualitative research

Qualitative research was conducted with all groups involved in this trial (school administrative staff, parents/carers, pupils, and the delivery team) at three stages during the intervention: baseline in September 2023–October 2023; midline in February 2024–March 2024, and endline in June 2024–September 2024 (see also Table 8).

Table 8: IPE interview design

Interview sample	Baseline interviews (September 2023 – October 2023)	Midline interviews (February 2024 – March 2024)	Endline interviews (June 2024 – September 2024)
School staff	10	–	9
Parents/carers	–	9	36
Pupils	–	9	36
Delivery team (BIT project team members)	–	–	2

The delivery team selected a sample of ten schools in the trial to participate in the IPE. The schools were selected based on characteristics directly linked to attendance, including school baseline absence levels and pupil eligibility for FSM i.e. schools with a higher-than-average number of pupils eligible for FSMs. Schools with a range of attendance levels were selected (overall absence levels for 2021–2022 of the initial case study schools ranged from 5.5%–20.1%). Variation in school size and location was also considered.

Interviews with the school staff took place at the two timepoints using semi-structured topic guides to steer the interviews and offer flexibility to embrace new topics. Baseline interviews were held with these ten schools during Term 1. Endline interviews with staff in five of these schools were conducted in Term 6 (June 2024–July 2024). Staff in the remaining five schools failed to respond to multiple requests via email and telephone for endline interviews. Therefore, four new schools were selected based on engagement with other aspects of the evaluation, including prompt submission of monitoring forms and completion of the school survey. Interviews with these staff from these schools took place at the start of the next school year (September 2024).

In total, 19 interviews were conducted with 14 schools: five schools at baseline and endline; five at baseline only; and four at endline only. One to one or paired in-depth online interviews were conducted with a range of school staff with responsibility for attendance and included individuals from senior leadership teams, for example, principals, vice principals and heads of pastoral care; and individuals with day-to-day responsibilities such as updating registers, contacting parents/carers, and processing absence penalty notices, for example, attendance officers and administrative staff.

At baseline, interviewed staff were asked about usual practice around attendance communications and staff's perceptions of this (IPE dimension: Usual practice), and their initial thoughts about how the intervention would work within the context of their existing attendance monitoring and support processes (IPE dimension: Perceived impact). At endline, we sought to understand how text messages were delivered (IPE dimension: Fidelity—quality, adaptation, reach); and to investigate short-term outcomes such as perceived changes in parents'/carers' and pupils' attitudes and behaviours, and reasons for these (IPE dimensions: Responsiveness, Perceived impact, and Contexts/moderators), and any changes to usual practice (IPE dimension: Programme differentiation).

Interviews were conducted with a total of 90 different parents/carers and pupils at midline between January 2024 and March 2024 (18 interviews) and endline between July 2024 and August 2024 (72 interviews), with different families interviewed at midline and endline. For both interview points, we purposively recruited parents/carers via the ten initial schools based on discussions with school staff to identify eligible pupils with a range of characteristics. Absenteeism for the qualitative research sampling was defined as the number of text messages a parent/carer had been sent by the school by the time of the interview. We interviewed parents/carers and pupils across a range of pupil absenteeism levels (about a third had received one text message, a quarter two text messages, and the rest three or more text messages). Once parents/carers

gave verbal consent to be sent further information about the research, Verian contacted them to discuss and schedule an interview and sent a link via text message to an online Participant Information Sheet (PIS) and consent form. At endline, parents/carers were also recruited via the parent/carer survey and were sampled purposively using data accessed via Wonde. Of those who agreed to be contacted, consent to participate was sent to parents/carers directly from Verian by text message or email depending on parents'/carers' communications preference. Verian also promoted the research in its endline monitoring communication with all schools and included a link to the PIS for schools to share with parents/carers. Pupils were recruited via their parents/carers to ensure that consent was granted; the interview with the parent/carer was used to reassure them about the content of the proposed interview with their child and about safeguarding procedures, and, if the pupil interview took place directly after the parent/carer interview, to ask assent from the pupil before starting their interview. Parent/carer and pupil interviews took place online, usually within the home setting, and with the pupil interview taking place immediately after the parent/carer interview. Parent/carer interviews were one hour in duration (though this was shortened to help recruitment for endline interviews); pupil interviews lasted 20 minutes. Some pupil interviews were conducted within the school setting with a staff member in close proximity; this was viewed as more convenient to these families. Our sample framework considered parents/carers who received one or multiple messages and pupils in receipt of FSMs and with EAL. Schools were made aware that simultaneous translation was available to facilitate interviews with non- or low-English speakers; however, this provision was not requested.

We also considered pupils' gender, ethnicity, and year group, and whether the pupil had a sibling at the same school; however, we adopted a pragmatic approach to sampling based on the completed consent forms submitted by parents/carers. We recruited through an opt-in model via the case study schools as an intermediary and (for endline only) directly with parents/carers via the school survey. The number of parents consenting to be contacted was low in relation to the desired sample size, meaning we were less able to purposely select participants. In most situations, this fell out naturally in the patterns of characteristics, for example, gender and number of siblings and text messages received; however, for ethnicity this was not the case and sampling was therefore, more impacted. We provided quotas to case study schools in an attempt to achieve higher participation from pupils of different ethnicities, but this failed to produce the numbers to fulfil our quotas.

Recruitment via the ten initial schools was challenging: schools had full responsibility for identifying, contacting, and gaining verbal consent from parents/carers to be contacted for interview; and it was often difficult for Verian to make contact with parents/carers to request written consent and schedule interviews. Schools also reported that parents/carers were apathetic about attending a one-hour interview, in addition to a 20-minute interview with their child. In response to these challenges, the research team made a number of adjustments. For example, we widened recruitment to include all schools, rather than limiting recruitment to the ten initial schools; we reduced the duration of the parent/carer interview from one hour to 30–45 minutes; and we increased the participant incentive from £30 to £75. We also worked with schools to facilitate the pupil interview in the school setting, if this were seen as helpful to families; and we assumed a more active role in our communications with parents/carers by telephoning them, rather than relying on emails or text messages, which were frequently ignored.

A further deviation from the trial protocol was that parents/carers with children in the control arm were removed from the interview sample. This decision was taken to limit potential spillover effects, for example, prompting parents/carers to discuss text messages among themselves while the trial was still live, and to focus interview resources on individuals who had received the intervention (rather than those who had not) and who would be able to provide insights about the impact and perceptions of the text messages. On reflection, it was also felt that the parents/carers in the control arm would have little to say if their awareness of the text message intervention was low. To minimise the impact of this deviation, we asked parents/carers in the interviews whether they had discussed receiving their text message(s) with anyone, including other parents/carers at the same school, to get a one-sided view on spillover effects; the endline parent/carer survey, which reached a larger number of parents/carers than the qualitative interviews, included questions to identify any spillover effects.

We conducted midline interviews at the end of Term 3 (February 2024–March 2024) to allow for any insights to be built into the endline survey. Endline interviews were conducted during Term 6 (June 2024–August 2024). We excluded pupils who participated—or whose parent/carer participated—in the qualitative research at midline and endline from the impact evaluation.

In addition, we interviewed two members of the delivery team (BIT) at endline to understand whether the intervention was delivered as planned, and how they worked with schools and the influence this may have had on delivery of the intervention (IPE dimension: Fidelity–quality).

Survey research

Surveys were implemented at baseline and endline to collect additional information from schools and parents/carers, as shown in Table 9.

All schools were invited to complete an online survey at baseline to gather information on usual practice regarding attendance communications (IPE dimension: Programme differentiation). The survey included questions to assess, which staff members are responsible for monitoring attendance and communicating with parents/carers about pupil attendance, how often the school monitors pupil attendance, if the school already sends communications to parents/carers about their child's attendance, which mode of communication is used, when and how frequently are communications sent out, and what information about attendance is included in parent/carer communications. The baseline school survey also collected information about whether they use any automated text messaging software, and the costs associated with this to support the cost evaluation.

At endline, schools were invited to complete two online surveys: one focused on implementation of the intervention (IPE dimensions: Fidelity–quality, adaptation, and reach); and any divergence from business as usual (IPE dimension: Programme differentiation). A second endline survey focused on the time and costs associated with delivering the intervention to support the cost evaluation.

Table 9: IPE survey design

Survey sample	Baseline responses (September 2023 – October 2023)	Endline responses – implementation survey (June 2024 – July 2024)
Schools (staff members)	90	42
Parents/carers	–	781

At endline, parents/carers were invited to participate in a short online survey to quantify key aspects of the qualitative findings. Parents/carers in the control and treatment arms were invited to participate if their child(ren)'s attendance fell below 95% in at least one term where eligibility for text messages was assessed (Terms 1–5). The survey was designed to identify spillovers between families who received text messages in the treatment group and families (in the treatment and control arms) who did not receive the intervention but who were eligible to receive it (IPE dimension: Fidelity—spillover), assess parental beliefs about their child's attendance (e.g. absolute and relative accuracy of the number of days their child is absent, and parent/carer beliefs about their ability to influence child absence), and support the exploration of factors that may have impacted the effect of the intervention on parents/carers (IPE dimensions: Context/Moderators and Mediators). The endline parent/carer survey also explored how parents/carers responded to receiving the intervention (IPE dimension: Responsiveness) and views of the intervention and its effectiveness (IPE dimension: Perceived impact).

We sent each school two endline parent/carer survey links, one for parents/carers in the treatment group and the other for parents/carers in the control. To ensure that the school sent the correct survey link to the correct parents/carers, we sent each school two lists of pupils whose attendance had fallen below 95% in at least one term between Terms 1 and Terms 5, one for those in the control arm and one for those in the treatment group. Parents/carers were invited by schools directly to complete the survey, using the same channel schools had used to send the intervention text messages. We offered a £5 voucher as an incentive to parents/carers for completing the survey. We set a maximum quota of 32 parents/carers per school to complete the survey (16 completes in the control and 16 in the treatment group). However, in some cases this quota was exceeded slightly due to parents/carers completing the survey at the same time. The survey was offered in English, Arabic, Bengali, Gujarati, Polish, Portuguese, Punjabi, Romanian, Spanish, Ukrainian, and Urdu.

Monitoring data on intervention delivery

During the intervention period, schools provided us with information about which families were sent a text message, the language the text messages were sent in, and the date the text messages were sent out. This information was collected for five terms (Terms 2–6) in a CSV (comma-separated values) file called a 'monitoring form'. The monitoring form provides data on the date text messages were sent out (IPE dimension: Fidelity—quality), the language schools used to send the text messages (IPE dimension: Fidelity—adaptation), and how many text messages were sent out and held back (IPE dimension: Fidelity—reach).

The form was developed by the delivery team based on feedback from schools during formative work completed in Spring Term 2023 and Summer Term 2023. During this formative work it was decided that, as schools use different management

information and text messaging systems, it would not be feasible or practical to ask schools to share segments of this data with the evaluation team (e.g. which text messages were sent, to whom, and when).

To minimise the administrative burden on schools, school staff were asked to update the CSV file sent out by the delivery team at the beginning of each term. This file contained the list of pupils eligible to receive a text message that term, a personalised text message for each eligible pupil containing the number of days of school missed in English, and a template text message in ten other languages, and the number of days of school missed that would be included in the text message. The form was refined based on feedback from school administration staff in the baseline interviews.

Schools were instructed to send the completed forms back to the delivery and/or evaluation team every term, although not all schools did this. Therefore, the sample of monitoring form data should not be considered representative of the schools who took part (for more information on monitoring form data see the discussion in 'Outcomes and analysis' section below under the subheading 'Analysis in the presence of non-compliance').

Analysis

A two-stage approach was taken to analysing the qualitative and quantitative IPE strands. First, the interview, survey, and monitoring form data were analysed separately. The qualitative data was analysed using a framework analysis method. This approach is flexible and permits a mixed deductive-inductive approach to analyse the results of the semi-structured interviews with schools, parents/carers, and pupils. Themes were identified deductively from our Theory of Change and inductively from the accounts of schools, parents/carers, and pupils. Interview data was then discussed as a team to agree themes, and standardise and strengthen the analytical process, seeking to minimise the risk of potential researcher bias.

Survey data was analysed descriptively using SPSS (SPPS Inc., Chicago, IL, USA) and R. The results were also analysed using an analysis framework to draw out insights from the school and parents'/carers' surveys under the relevant IPE dimensions. The monitoring forms that schools returned each term were combined into a single panel dataset in R, summarising the number of text messages schools sent, the number of text messages schools decided not to send, and the number of text messages sent in each language for each school in each term. The data was reported descriptively, and as we did not receive forms from all schools in each term, the results are reported with appropriate caveats.

Second, brainstorm sessions were used to draw together insights from the qualitative and quantitative data. This allowed us to identify common themes and assess the apparent strength of our findings against each IPE dimension.

Additionally, we carried out a case study analysis using qualitative data (in this context a case being a school unit). This was somewhat affected by some case study schools not engaging in the evaluation post-baseline interview; however, baseline interview questions were asked of newly engaged schools at endline to understand their context. A within-case analysis was conducted to understand at a holistic level, in each given school, how the intervention compared with usual communication, how it was implemented in the school, and how parents/carers and pupils responded. A between-case analysis was also conducted to identify overall patterns, differences, and commonalities between schools, to inform wider understanding of the perceived impact of the intervention, and what factors may have influenced this.

Costs

Information about the time and costs associated with delivering the intervention was collected from baseline and endline surveys and interviews with school staff, and endline interviews with the delivery team. This included personnel cost of assessing intervention eligibility (i.e. attendance rates each term), sending text messages, training, and responding to parents/carers who respond to the intervention, and costs of text messaging and data platforms. Costs associated exclusively with participating in this trial (e.g. personnel time reporting to Verian about texts sent) were excluded from the cost analysis.

A cost per pupil per year of this intervention was calculated in accordance with the EEF cost evaluation guidance (EEF, 2023). We also report the total cost per school to implement the intervention for three consecutive years and the cost per pupil per school year, of those expected to benefit from the intervention, namely, those across the treatment and control arms who were eligible to receive the intervention.

Timeline

Table 10: Timeline

Dates	Activity	Staff responsible/leading
March 2023 – September 2023	Recruitment of schools	BIT
July 2023	Access to school data on Wonde requested	Schools/Verian/BIT
September 2023	Parent/carer information sheets and parent/carer consent forms sent out by schools and returned ahead of accessing pupil data for the first time for randomisation	Schools/BIT
September 2023 – October 2023	IPE: School baseline survey and staff interviews	Verian
19 October 2023	Randomisation of settings	Verian
October 2023	First wave of BITUP intervention text messages sent by schools	Schools/BIT
January 2024	Second wave of BITUP intervention text messages sent by schools	Schools/BIT
January 2024 – March 2024	IPE: Midline interviews with pupils and parents/carers	Verian
February 2024	Third wave of BITUP intervention text messages sent by schools	Schools/BIT
April 2024	Fourth wave of BITUP intervention text messages sent by schools	Schools/BIT
June 2024	Fifth wave of BITUP intervention text messages sent by schools	Schools/BIT
June 2024 – September 2024	IPE: Endline school and parents/carers survey, and staff, pupil, parent/carers, and BIT interviews	Verian
July 2024	Attendance data accessed at the end of the school year for impact evaluation analysis	Verian
July 2024 – December 2024	Analysis and report writing	Verian
December 2024	Submission of draft report to the EEF	Verian
October 2025	Final report by the EEF published	Verian

Impact evaluation

Participant flow including losses and exclusions

The target number for recruitment was 115 schools (to allow up to 15 schools to drop out before the end of the trial and still be sufficiently powered to detect a difference in average absence rate of eligible pupils between the two intervention arms). Recruitment was carried out on a first come, first served basis. In July 2024, the delivery team approached 980 schools and recruited 115, with a waiting list of 27 schools.

At the start of the 2023–2024 school year (September 2023) the delivery team instructed participating schools to share information about the evaluation and a consent form with parents/carers. Parents/carers were given until the end of September 2023 to return the form and withdraw their child from the evaluation if they so wished.[16] Schools were then required to manually update permissions to allow the delivery and evaluation teams to access pupil data through Wonde, removing access to pupils whose parents/carers had withdrawn. Schools may have withdrawn access to pupil data via Wonde for other reasons, both before and after randomisation. However, we do not have information on how many pupils were affected or the reasons for the withdrawal of access. This is a separate issue from pupils whom schools did not want to send the intervention but who remained part of the evaluation.

Eight schools with places in the trial dropped out during the first weeks of the new school year.[17] The delivery team filled seven of these places in the trial with schools on the waiting list, leaving us with 114 schools in the trial at the end of September 2023. In the first half of October 2023, a further four schools dropped out due to RAAC (Reinforced Autoclaved Aerated Concrete) in their school buildings. The affected schools felt that, due to closure, they would either be unable to deliver the intervention or it would not be appropriate to do so (e.g. schools felt it would be insensitive to highlight days of school missed in a year when pupils had been unable to attend due to school closures). The deadline for completing manual pupil withdrawals in Wonde was 13 October 2023. Two schools missed this deadline and so were excluded from the trial as the delivery team could not access their data at randomisation.

At randomisation, there were 108 schools in the trial. During the trial, three schools withdrew from the evaluation. Another three schools stopped sending out the text messages part-way through the school year (primarily because of the resource requirements of sending text messages) but agreed to stay in the evaluation. This left 105 schools in the impact evaluation. For the full participant flow diagram, see Figure 2 below.

[16] Wonde requires schools to manually withdraw pupils from sharing permissions with the delivery and evaluation team so that their data cannot be accessed by either party. This was necessary to ensure that withdrawn pupils were not included in the list of pupils eligible for the text message intervention sent to schools at the beginning of each subsequent term, and that withdrawn pupils were not included in the randomisation and analysis stages.

[17] Reasons for withdrawal included: schools being affected by the issues surrounding Reinforced Autoclaved Aerated Concrete (RAAC); schools deciding that they are now unhappy with the project GDPR arrangements; schools deciding they no longer want to take part; or the members of staff that signed schools up to the project had since left the school and their replacements not wishing/having the capacity to take part.

Figure 2: Participant flow diagram (two arms)

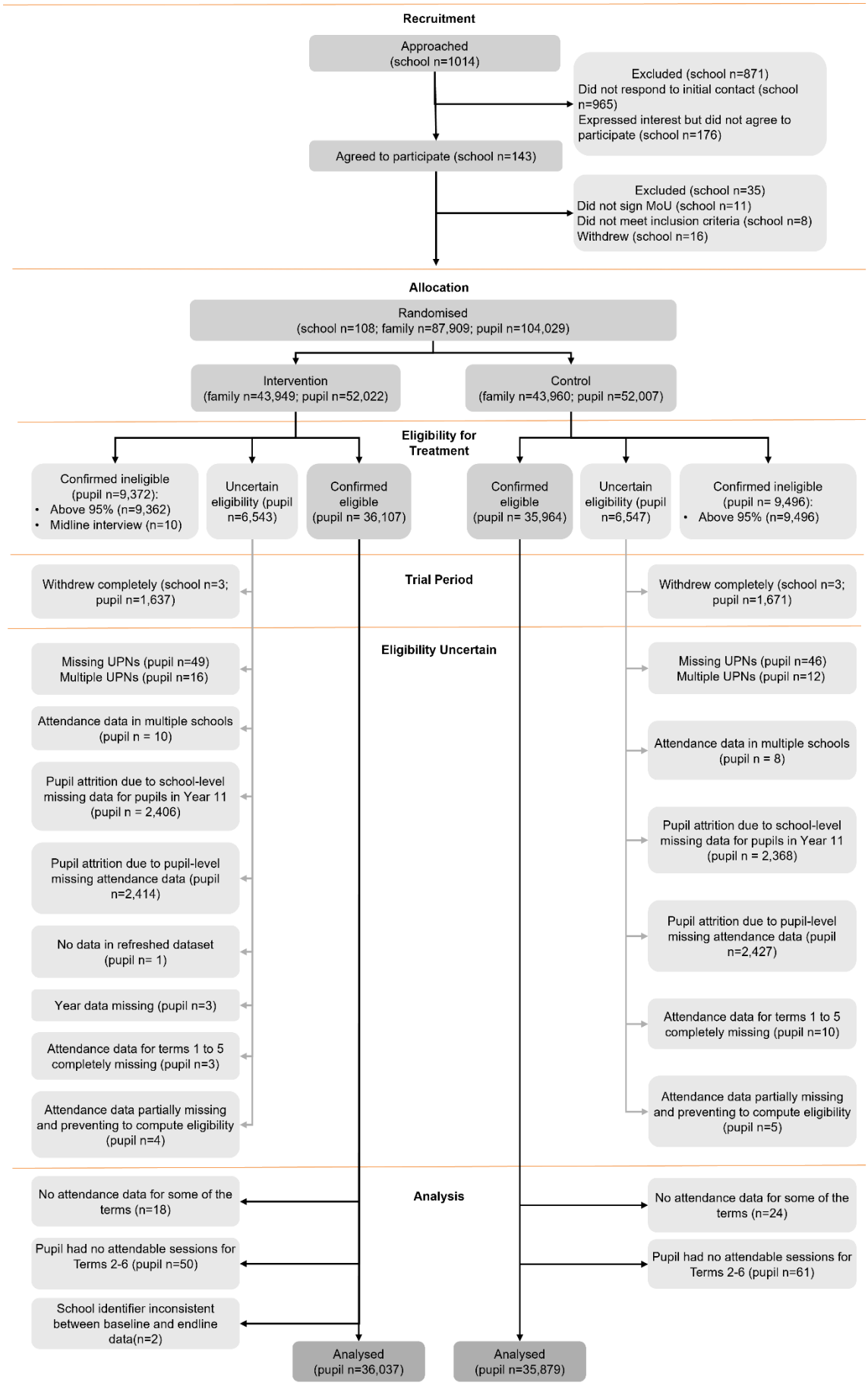


Table 11 shows the sample size calculations at each stage of the trial.

Sample size calculations at the protocol stage indicated we would need 100 schools to achieve an MDES of 0.011 for the main effect and 0.022 for the FSM subgroup. To account for attrition over the course of the trial, 115 schools were invited to participate in the trial. Our sample size calculations for the full sample and the FSM subgroup assumed:

- a 0.801 proportion of variance explained by covariates at the pupil level (Level 1) and 0.5 at the family level (Level 2);
- family-level ICC of 0.0598;
- the average number of eligible pupils per family (average cluster size) of 1.91; and
- baseline absence rate of 6.25% (based on the BIT's pilot study).

The ICC was calculated from the BITUP intervention pilot RCT in Bristol (BIT, 2020), the proportion of variance explained by covariates was estimated using pupil-level data from a previous related evaluation (Mills *et al.,* 2022), and average cluster size from ONS data (ONS, 2022) and the text message trial pilot study (BIT, 2020).

At randomisation, the sample size calculations were updated with the data from 108 schools (fewer schools and a smaller average cluster size than expected), which contained 87,909 families of 104,029 pupils, of which there were 30,162 FSM pupils. We assumed that 40.5% of pupils would become eligible, based on data from the BITUP intervention pilot RCT (BIT, 2020). At randomisation, sample size calculations suggested an achievable MDES of 0.012 and 0.022 for the main effect and FSM subgroup, respectively.

The results at analysis show we were powered to detect an MDES of 0.020 for the full sample, and 0.035 for the FSM subgroup. Taking the absence rate of 11.79% among eligible pupils (with attendance <95% in at least one of Terms 1–5) in the control arm as the baseline absence rate (16.98% for the FSM subsample), we are powered to detect a minimum percentage-point change of 0.65% (1.31% for the FSM subsample).[18] This translates to a reduction in absence rate to 11.14% among eligible pupils (15.67% for the FSM subsample).

In terms of days of school missed, this equates to a difference of on average 1.03 days for eligible pupils (2.07 days for the FSM subsample) in total over the five terms post-intervention, between the intervention and control arm. This assumes that there are 190 days in a school year, with pupils being able to attend an average of 158.33 days in Terms 2–6 (or 316.67 sessions).

Even though we had three fewer schools than anticipated at randomisation, our power increased at analysis because the proportion of pupils whose attendance fell below 95% at least once in Terms 1–5 was higher than we had assumed at randomisation. Over the course of the trial 79% of pupils in the sample became eligible for the intervention (attendance fell below 95% in at least one of Terms 1–5). Therefore, the number of pupils and families was higher than previously expected, with 71,916 pupils (22,841 FSM-eligible pupils) nested within 63,295 families (19,427 FSM-eligible families).

We also updated our assumptions about pupil-level proportion of variance explained by covariates (lower than previously assumed), family-level proportion of variance explained by covariates (0 due to the absence of family-level covariates in the model) and the family-level ICC (higher than previously assumed). The sample size calculations at analysis also specified the school-level ICC (0.03 for the full sample and for the FSM subgroup).

[18] The calculation of these absence rates involves calculating the baseline standard deviation for each proportion using the binomial formula $\sigma = \sqrt{p(1-p)}$, and multiplying the MDES by σ.

Table 11: MDES at different stages

		Protocol		Randomisation		Analysis	
		Overall	FSM	Overall	FSM	Overall	FSM
MDES (95% CIs)		0.011 (0.003, 0.019)	0.022 (0.007, 0.038)	0.012 (0.004, 0.020)	0.022 (0.007, 0.037)	0.020 (0.006, 0.033)	0.035 (0.011, 0.060)
Minimum detectable difference in proportions		0.28	0.55	0.30	0.55	0.65	1.31
Proportion of variance explained by covariates	Level 1 (pupil)	0.801	0.801	0.801	0.801	0.377	0.42
	Level 2 (family)	0.5	0.5	0.5	0.5	0[a]	0[a]
ICCs	Level 2 (family)	0.0598	0.0598	0.0598	0.0598	0.506	0.552
	Level 3 (school)	–	–	–	–	0.032	0.033
Alpha		0.05	0.05	0.05	0.05	0.05	0.05
Power		0.8	0.8	0.8	0.8	0.8	0.8
One-sided or two-sided?		2	2	2	2	2	2
No. of schools		115	115	108	108	105	105
Average cluster size (in sample)		1.73	1.73	1.18	1.18	1.17	1.18
No. of families	Intervention	29,804	7,093	43,949	12,731	38,850	11,001
	Control	29,805	7,094	43,960	12,724	38,902	11,008
	Total	**59,609**	**14,187**	**87,909**	**25,455**	**77,752**	**22,009**
No. of pupils	Intervention	51,472	12,250	52,022	15,101	45,420	12,838
	Control	51,473	12,251	52,007	15,061	45,410	12,785
	Total	**102,945**	**24,501**	**104,029**	**30,162**	**90,830**	**25,623**
Average cluster size (in sample with attendance <95% in at least one of Terms 1–5)		1.91	1.91	1.27	1.30	1.14	1.156
No. of families (with at least one child with attendance <95% in at least one of Terms 1–5)	Intervention	16,850	4,010	19,688	5,762	31,674	9,749
	Control	16,850	4,011	19,689	5,745	31,621	9,678
	Total	**33,700**	**8,021**	**39,377**	**11,507**	**63,295**	**19,427**
No. of pupils (with attendance <95% in at least one of Terms 1–5)	Intervention	20,846	4,961	21,069	6,116	36,037	11,429
	Control	20,846	4,961	21,063	6,100	35,879	11,412
	Total	**41,693**	**9,923**	**42,132**	**12,216**	**71,916**	**22,841**

[a] *Level 2 pre- and post-correlation (families) is 0 at analysis due to the absence of family-level covariates in the model.*

Attrition

Table 12: Attrition between sample as randomised and sample as analysed (primary outcome)

			Intervention	Control	Total
No. of pupils		Randomised	52,022	52,007	104,029
		In schools who continued to send data	50,385	50,336	100,721
		Analysed	36,037	35,879	71,916
		Percentage analysed (from randomised)	69.3%	69.0%	69.1%
Ineligible to be treated	Pupil was never eligible for the intervention (attendance rate was >95% in all Terms 1–5)	Number	9,362	9,496	18,858
		Percentage (from randomised)	18.0%	18.3%	18.1%
Attrition from sample	Missing Unique Pupil Numbers (UPNs) or multiple UPNs	Number	65	68	133
		Percentage (from randomised)	0.1%	0.1%	0.1%
	School-level attrition	Number	1,637	1,671	3,308
		Percentage (from randomised)	3.1%	3.2%	3.2%
	Pupil attrition due to school-level missing data for pupils in Year 11	Number	2,406	2,368	4,774
		Percentage (from randomised)	4.6%	4.6%	4.6%
	Pupil attrition due to pupil-level missing data (at endline data stage)	Number	2,414	2,427	4,841
		Percentage (from randomised)	4.6%	4.7%	4.7%
	Pupil attrition due to data inconsistencies	Number	21	23	44
		Percentage (from randomised)	0.0%	0.0%	0.0%
Ineligible to be analysed	No attendance data for some of the terms	Number	18	24	42
		Percentage (from randomised)	0.0%	0.0%	0.0%
	Pupil was interviewed for the IPE	Number	10	0	10
		Percentage (from randomised)	0.0%	0.0%	0.0%
	Pupil had no attendable sessions for Terms 2–6	Number	50	61	111
		Percentage (from randomised)	0.1%	0.1%	0.1%
	Inconsistent school	Number	2	0	2
		Percentage (from randomised)	0.0%	0.0%	0.0%

Among the 104,029 pupils randomised into the trial, 72,071 pupils were confirmed as eligible at any point of the intervention. Eligibility could not be determined for 13,100 pupils and as a result the attrition rate for the trial was 12.59% (see Figure 2 and Table 12). This trial has the slightly unusual feature that pupils were randomised before we knew whether they would be eligible for the intervention, so the attrition (post randomisation) includes pupils whose eligibility could not be confirmed because of incomplete data, as well as any who withdrew or had missing data.

The total pupil sample reduced by 30.9% between randomisation and analysis, by an equal proportion in the intervention and control arm (for a breakdown of reasons why pupils did not appear in the final dataset, see Table 12). The main reason for this was pupils not becoming eligible for the intervention, and so not being included in the ITT analysis: 18,858 (18.1% of randomised pupils) pupils had an attendance rate that did not drop below 95% in any of Terms 1–5.

There were three sources of attrition over the trial period, leading to an attrition rate of 12.59% overall. One was schools withdrawing from the evaluation. This accounted for the attrition of 3,308 pupils (~3.2% of the randomised sample). A second source of attrition was school-level missing data for 4,774 pupils in Year 11 from 29 schools (~4.6% of the randomised sample) in the endline data pulled from Wonde at the end of the school year. This was all Year 11s in those 29 schools; since we have no data for these pupils, it is not possible to assess whether they would have been eligible.[19] Finally, there were 5,018 pupils (4.8% of the randomised sample[20]) with either missing data at the endline (n=4,841; ~4.7%), or UPN missing or appearing in multiple schools (n=133; ~0.1%) in the trial, or other data inconsistencies (n=44; ~0.04%). Possible reasons for this include pupils moving schools or the schools withdrawing the evaluation team's access to pupil data in Wonde during the trial (e.g. following a pupil being withdrawn from the evaluation).

In addition, ten pupils in the intervention arm were excluded because they took part in midline qualitative interviews. A further 111 pupils had zero attendable sessions. Specifically, every record for Terms 2–6 was coded VOID rather than 'present', 'authorised absence', or 'unauthorised absence', indicating there were no sessions during which the pupil was expected to be in school. Because the calculation of absence rates includes number of attendable sessions in the denominator, a denominator of 0 renders the absence rate as undefined, so the pupils could not be included in the analysis. Most of them (n=93) had zero attendable sessions for the entire school year, while the remaining few had attendable sessions only in Term 1, with an attendance rate below 95% (n=18). An additional 42 pupils only had partial attendance data available, with data missing for entire terms, and were therefore, excluded. Finally, two pupils were dropped because their school identifier differed between the randomisation data file and the endline Wonde extract.

The proportion of pupils in the analysis by ethnicity group and gender was similar compared to the proportions at randomisation (see Table 13 and Table 14). The percentage of pupils who have FSM and/or EAL status was also lower for the analysed pupils compared to the whole-randomised sample. See Table 13 for a demographic breakdown of these pupils by treatment group, alongside the breakdown for the whole sample at randomisation.

Table 13: Pupil attrition due to missing data, duplicate UPNs, or data inconsistences, by characteristics

Pupil level (categorical)	Pupil attrition due to missing data or UPN appearing in multiple schools in the trial		Randomised	
	Control (N=2,561)	Intervention (N=2,551)	Control (N=52,007)	Intervention (N=52,022)
	Count (%)	Count (%)	Count (%)	Count (%)
FSM status				
FSM	1,166 (45.5%)	1,122 (44.0%)	15,061 (29.0%)	15,101 (29.0%)
Non-FSM	1,395 (54.5%)	1,429 (56.0%)	36,946 (71.0%)	36,921 (71.0%)
EAL status				
EAL	454 (17.7%)	424 (16.6%)	8,388 (16.1%)	8,421 (16.2%)

[19] The reason for this missingness is not clear, but one hypothesis is that it is connected to the fact that data was requested after the school year had ended. There is the possibility that Year 11 leavers were removed from the system as being 'actively enrolled' and therefore, the Application Programming Interface (API) did not return their data.

[20] The use of all pupils randomised as the base follows the EEF guidance (EEF, 2019), see: https://d2tic4wvo1iusb.cloudfront.net/documents/evaluation/peer-review-process/Classifying_the_security_of_EEF_findings_2019.pdf

Non-EAL	1,535 (59.9%)	1,572 (61.6%)	29,654 (57.0%)	29,665 (57.0%)
Not known	572 (22.3%)	555 (21.8%)	13,965 (26.9%)	13,936 (26.8%)
Gender				
Female	1,316 (51.4%)	1,269 (49.7%)	24,692 (47.5%)	24,690 (47.5%)
Male	1,245 (48.6%)	1,282 (50.3%)	27,314 (52.5%)	27,332 (52.5%)
Missing	0 (0%)	0 (0%)	1 (0.0%)	0 (0%)
SEND status				
Non-SEND	1,792 (70.0%)	1,794 (70.3%)	40,110 (77.1%)	39,901 (76.7%)
SEND	769 (30.0%)	757 (29.7%)	11,897 (22.9%)	12,121 (23.3%)
Ethnicity				
White	1,878 (73.3%)	1,860 (72.9%)	38,694 (74.4%)	38,846 (74.7%)
Asian or Asian British	162 (6.3%)	154 (6.0%)	4,628 (8.9%)	4,607 (8.9%)
Black, Black British, Caribbean, or African	144 (5.6%)	144 (5.6%)	2,960 (5.7%)	2,987 (5.7%)
Gypsy, Roma, and Traveller	17 (0.7%)	24 (0.9%)	134 (0.3%)	143 (0.3%)
Mixed or multiple ethnicity	162 (6.3%)	167 (6.5%)	3,390 (6.5%)	3,249 (6.2%)
Other minority ethnicity	67 (2.6%)	79 (3.1%)	1,136 (2.2%)	1,141 (2.2%)
Missing/N/A	131 (5.1%)	123 (4.8%)	1,065 (2%)	1,049 (2%)
Age				
12	308 (12.0%)	309 (12.1%)	7,075 (13.6%)	7,084 (13.6%)
13	496 (19.4%)	479 (18.8%)	10,395 (20.0%)	10,458 (20.1%)
14	554 (21.6%)	538 (21.1%)	10,531 (20.2%)	10,491 (20.2%)
15	446 (17.4%)	496 (19.4%)	10,224 (19.7%)	10,206 (19.6%)
16	542 (21.2%)	548 (21.5%)	10,296 (19.8%)	10,306 (19.8%)
17 or older	215 (8.4%)	180 (7.0%)	3,486 (6.7%)	3,476 (6.6%)
Data entry error	0 (0.0%)	1 (0.0%)	0 (0.0%)	1 (0.0%)
Year group				
7	468 (18.3%)	486 (19.1%)	10,561 (20.3%)	10,710 (20.6%)
8	533 (20.8%)	492 (19.3%)	10,540 (20.3%)	10,505 (20.2%)
9	515 (20.1%)	539 (21.1%)	10,407 (20.0%)	10,264 (19.7%)
10	451 (17.6%)	479 (18.8%)	10,267 (19.7%)	10,163 (19.5%)
11	594 (23.2%)	555 (21.8%)	10,232 (19.7%)	10,380 (20.0%)

Table 14: Pupil ethnicity and gender characteristics by intervention arm at randomisation, eligibility (after attrition) and analysis (after attrition and ineligibility exclusions)

		Randomised	Eligible	Analysed	Randomised but not eligible (n)	Eligible but not analysed (n)	Randomised but not eligible (%)	Eligible but not analysed (%)
Asian or Asian British	Intervention	4,607	3,210	3,207	-1,397	-3	30%	0%
	Control	4,628	3,283	3,278	-1,345	-5	29%	0%
Black, Black British, Caribbean, or African	Intervention	2,987	1,649	1,646	-1,338	-3	45%	0%
	Control	2,960	1,630	1,627	-1,330	-3	45%	0%
Mixed or multiple ethnicity	Intervention	3,249	2,258	2,251	-991	-7	31%	0%
	Control	3,390	2,392	2,388	-998	-4	30%	0%
Gypsy, Roma, and Traveller	Intervention	143	115	115	-28	0	20%	0%
	Control	134	104	104	-30	0	22%	0%
White	Intervention	38,846	27,551	27,494	-11,295	-57	29%	0%
	Control	38,694	27,219	27,149	-11,475	-70	30%	0%
Other minority ethnicity	Intervention	1,141	748	748	-393	0	34%	0%
	Control	1,136	745	743	-391	-2	35%	0%
Male	Intervention	27,332	18,857	18,820	-8,475	-37	31%	0%
	Control	27,314	18,822	18,781	-8,492	-41	31%	0%
Female	Intervention	24,690	17,250	17,217	-7,440	-33	30%	0%
	Control	24,692	17,142	17,098	-7,550	-44	31%	0%

Pupil and school characteristics

Table 15 shows the baseline characteristics of 108 schools in the trial at randomisation and the three schools who withdrew from the trial or stopped implementing the intervention part-way through.

Table 15: Baseline characteristics of schools as randomised

School level (categorical)	National-level mean Count (%)	Randomised (N=108 schools)		Withdrew (N=3 schools)	
		n/N (missing)	Count (%)	n/N (missing)	Count (%)
Type of school					
Academy	2,770 (80.4%)	108/108 (0)	85 (78.7%)	3/3 (0)	3 (100%)
Local authority maintained	674 (19.6%)	108/108 (0)	23 (21.3%)	3/3 (0)	0 (0%)
In urban area	2,948 (85.6%)	108/108 (0)	95 (88.0%)	3/3 (0)	2 (66.7%)
Ofsted (Office for Standards in Education, Children's Services and Skills) rating					
Outstanding / Good	2,733 (77.6%)	101/108 (7)	81 (75.0%)	3/3 (0)	3 (100%)
Requires improvement / Serious weaknesses / Special measures	509 (14.5%)	101/108 (7)	20 (18.5%)	3/3 (0)	0 (0%)
School level (continuous)	National-level mean Mean (SD)	n/N (missing)	Mean (SD)	n/N (missing)	Mean (SD)
% of pupils achieving 9–5 passes in GCSE English and maths in 2022	45.3% (24)	103/108 (5)	45.1% (13.6)	2/3 (1)	45.5% (3.54)
No. of pupils	1,054 (413)	108/108 (0)	1,085 (388)	3/3 (0)	1,192 (450)
% FSM	27.6% (14.34)	108/108 (0)	30.8% (13.6)	3/3 (0)	24.5% (4.0)
% EAL	17.6% (18.3)	108/108 (0)	16.1% (17.5)	3/3 (0)	11.6% (10.0)
% SEND	13.0% (5.6)	108/108 (0)	13.8% (5.6)	3/3 (0)	13.4% (9.8)
Average attendance rate (2022–2023)	90.9% (2.6)	108/108 (0)	89.7% (2.7)	3/3 (0)	91% (1.7)

Compared to national figures, the following types of schools were slightly overrepresented at randomisation: local authority maintained schools; schools located in urban areas; and schools rated as 'Requiring improvement/Inadequate/Serious weakness'. In contrast, academies and schools rated as 'Outstanding/Good' were slightly underrepresented in the sample at randomisation. Trial schools were also slightly larger compared to the national average in terms of pupil numbers, with a slightly higher proportion of FSM pupils and a slightly lower proportion of EAL pupils compared to the national average. At randomisation, trial schools were comparable to national figures in terms of the proportion of SEND pupils and average school performance in terms of the proportion of pupils achieving 9–5 passes in GCSE English and maths in 2022.

Three schools withdrew from the trial and so were not included in the analysis. Since each school in the trial had families assigned to both the intervention and the control arm, this did not affect the school-level characteristics of the intervention and control arms, which remained balanced.

Pupil characteristics were balanced between the control and treatment group (see Table 16). FSM and EAL pupils were evenly distributed between the control and treatment group at the family level, as these characteristics were included as stratifying variables in the randomisation.

Table 16: Baseline characteristics of families and pupils as randomised

Family level (categorical)	Intervention		Control	
	n/N (missing)	Count (%)	n/N (missing)	Count (%)
FSM categorisation				
FSM	12,731/43,949 (0)	12,731 (29.0%)	12,724/43,960 (0)	12,724 (28.9%)
Non-FSM	31,218/43,949 (0)	31,218 (71.0%)	31,236/43,960 (0)	31,236 (71.1%)
EAL categorisation				
EAL	7,416/43,949 (0)	7,416 (16.9%)	7,432/43,960 (0)	7,432 (16.9%)
Non-EAL	36,533/43,949 (0)	36,533 (83.1%)	36,528/43,960 (0)	36,528 (83.1%)
No. of siblings				
1	36,439/43,949 (0)	36,439 (82.9%)	36,448/43,960 (0)	36,448 (82.9%)
2	6,984/43,949 (0)	6,984 (15.9%)	7,000/43,960 (0)	7,000 (15.9%)
3	493/43,949 (0)	493 (1.1%)	490/43,960 (0)	490 (1.1%)
4 or more	33/43,949 (0)	33 (0.1%)	22/43,960 (0)	22 (0.0%)
Pupil level (categorical)	n/N (missing)	Count (%)	n/N (missing)	Count (%)
FSM status				
FSM	15,101/52,022 (0)	15,101 (29.0%)	15,061/52,007 (0)	15,061 (29.0%)
Non-FSM	36,921/52,022 (0)	36,921 (71.0%)	36,946/52,007 (0)	36,946 (71.0%)
EAL status				
EAL	8,421/52,022 (0)	8,421 (16.2%)	8,388/52,007 (0)	8,388 (16.2%)
Non-EAL	29,665/52,022 (0)	29,665 (57.0%)	29,654/52,007 (0)	29,654 (57.0%)
Not known	13,936/52,022 (0)	13,936 (26.8%)	13,965/52,007 (0)	13,965 (26.9%)
Gender				
Female	24,690/52,022 (0)	24,690 (47.5%)	24,692/52,007 (0)	24,692 (47.5%)
Male	27,332/52,022 (0)	27,332 (52.5%)	27,314/52,007 (0)	27,314 (52.5%)
SEND status				
Non-SEND	39,901/52,022 (0)	39,901 (76.7%)	40,110/52,007 (0)	40,110 (77.1%)
SEND	12,121/52,022 (0)	12,121 (23.3%)	11,897/52,007 (0)	11,897 (22.9%)
Ethnicity				
White	38,846/50,957 (1,065)	38,846 (76.2%)	38,694/50,958 (1,049)	38,694 (75.9%)
Asian or Asian British	4,607/50,957 (1,065)	4,607 (9.0%)	4,628/50,958 (1,049)	4,628 (9.1%)
Black, Black British, Caribbean, or African	2,987/50,957 (1,065)	2,987 (5.9%)	2,960/50,958 (1,049)	2,960 (5.8%)

Gypsy, Roma, and Traveller	143/50,957 (1,065)	143 (0.3%)	134/50,958 (1,049)	134 (0.3%)
Mixed or multiple ethnicity	3,249/50,957 (1,065)	3,249 (6.4%)	3,390/50,958 (1,049)	3,390 (6.7%)
Other minority ethnicity	1,141/50,957 (1,065)	1,141 (2.2%)	1,136/50,958 (1,049)	1,136 (2.2%)
Age				
12	7,084/52,023 (1)	7,084 (13.6%)	7,075/52,007 (0)	7,075 (13.6%)
13	10,395/52,023 (1)	10,395 (20.1%)	10,395/52,007 (0)	10,395 (20.0%)
14	10,531/52,023 (1)	10,531 (20.2%)	10,531/52,007 (0)	10,531 (20.2%)
15	10,224/52,023 (1)	10,224 (19.6%)	10,224/52,007 (0)	10,224 (19.7%)
16	10,296/52,023 (1)	10,296 (19.8%)	10,296/52,007 (0)	10,296 (19.8%)
17 or older	3,476/52,023 (1)	3476 (6.7%)	3,486/52,007 (0)	3,486 (6.7%)
Year group				
7	10,710/52,022 (0)	10,710 (20.6%)	10,561/52,007 (0)	10,561 (20.3%)
8	10,505/52,022 (0)	10,505 (20.2%)	10,540/52,007 (0)	10,540 (20.3%)
9	10,264/52,022 (0)	10,264 (19.7%)	10,407/52,007 (0)	10,407 (20.0%)
10	10,163/52,022 (0)	10,163 (19.5%)	10,267/52,007 (0)	10,267 (19.7%)
11	10,380/52,022 (0)	10,380 (20.0%)	10,232/52,007 (0)	10,232 (19.7%)

Outcomes and analysis

In this section we summarise the results of the impact evaluation including the ITT, compliance, missing data, and subgroup analyses.

Primary analysis

Participants in the intervention arm (who had attendance <95% in at least one of Terms 1–5 and were therefore eligible for the intervention) were absent for 11.51% of the attendable sessions across Terms 2–6, compared to an absence rate in the control arm (among those who were eligible) of 11.79%. This gives a standardised effect size of close to zero and a CI that crosses zero (g = -0.0093, 95% CI: -0.0194, 0.0009) in our pre-specified primary analysis, see Table 17. The coefficient in the model shows that adjusted mean absence rate was 0.14 percentage points lower for those in the intervention arm who were eligible for the intervention, compared to those in the control arm who were eligible—again with a CI crossing zero (95% CI: -0.29, 0.01); this translates into a mean reduction of approximately 0.21 days of absence in total across five terms among the eligible pupils. These observed changes are smaller than the minimum percentage-point changes we are powered to detect with the full sample.

The distribution of the primary outcome was skewed to the right, with a long tail (see Figure 3), suggesting that some of the assumptions of the pre-specified multi-level linear model might not hold very well. The errors were also heteroskedastic. Therefore, to check the robustness of the results of the primary model, we ran two alternative multi-level models: a negative binomial model looking at the counts of absent sessions, with the log of total number of attendable sessions as a regressor for each eligible pupil; and a binomial model looking at the number of present and absent sessions across all attendable sessions for each eligible pupil.

Altering the regression specification did not meaningfully alter the point estimates, but it did change the CIs. The negative binomial model estimated that the total number of absent sessions was reduced by 1.41% among pupils eligible for the intervention in the intervention arm relative to those who were eligible in the control arm (95% CI: -2.49, -0.31); this translates into a reduction of approximately 0.24 days of absence in total across five terms among eligible pupils, which is similar to the estimate given by the primary model. The binomial model estimated the odds of an eligible pupil having an absent session in the intervention arm to be 0.983 times the odds in the control arm (95% CI: 0.969, 0.996), indicating a 0.14 percentage points decrease in the absence rate among eligible pupils across Terms 2–6 in the intervention arm.

Table 17: Primary analysis

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	36,037 (2,500)	11.51 (11.36, 11.66)	35,879 (2,518)	11.79 (11.63, 11.94)	71,916 (36,037; 35,879)	-0.0093 (-0.0194, 0.0009)	0.073

Data source: Wonde.

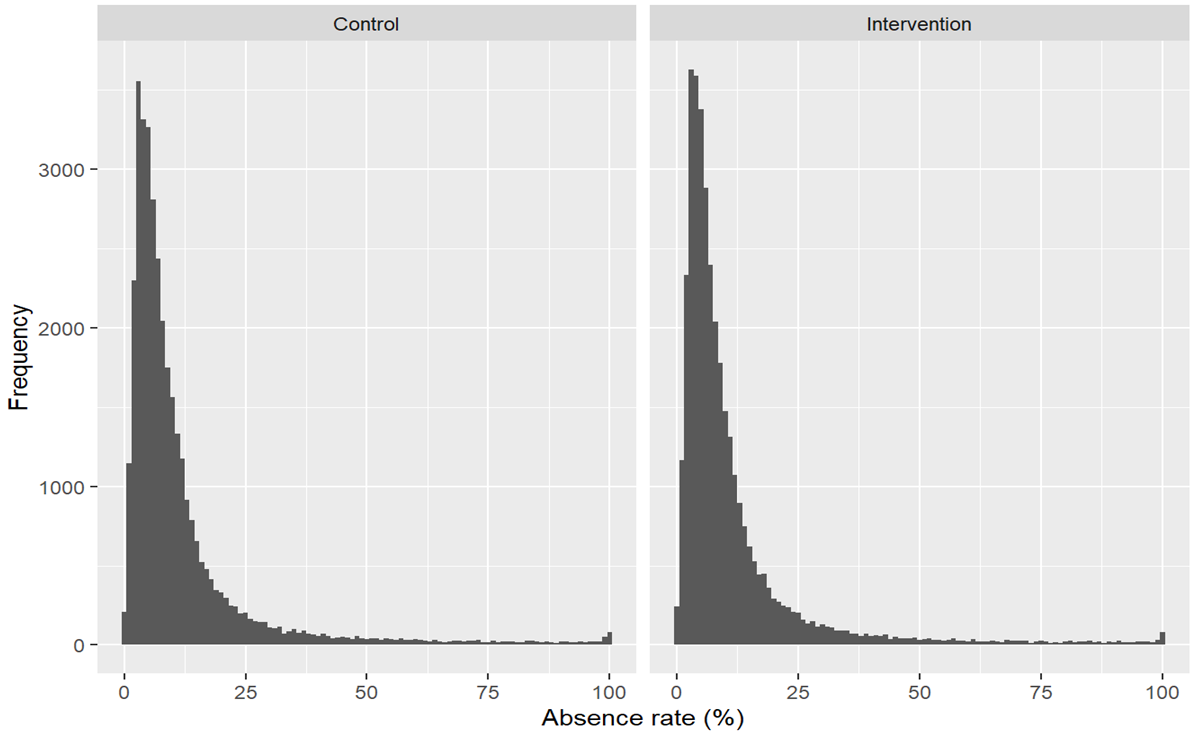


Figure 3: Distribution of absence rate (%) across Terms 2–6 (primary outcome), by trial arm

Secondary analysis

The average authorised absence rate across Terms 2–6 was 6.61% among eligible pupils in the intervention arm (SRQ1), compared to 6.80% in the control arm. This gives a standardised effect size of -0.0208 (95% CI: -0.0332, -0.0083), see Table 18. The coefficient in the pre-specified analysis model shows that the adjusted mean authorised absence rate was 0.17 percentage points lower among eligible pupils in the intervention arm compared to the control arm (95% CI: -0.28, -0.07)—this translates into a reduction of approximately 0.26 days of authorised absence in total across five terms, for pupils whose attendance fell below 95% in at least one of Terms 1–5.

The average unauthorised absence rate across Terms 2–6 was 4.90% among eligible pupils in the intervention arm (SRQ2), compared to 4.98% in the control arm. The pre-specified analysis model indicates a standardised effect size of close to zero and a CI that crosses zero (g = 0.0012, 95% CI: -0.0093, 0.0118), see Table 18. The distribution of the two secondary outcomes by arm can be found in Appendix C Figure C1 and Appendix C Figure C2.

Table 18: Secondary analyses

	Unadjusted means	Effect size

	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Authorised absence rate (across Terms 2–6)	36,037	6.61 (6.52, 6.69)	35,879	6.80 (6.72, 6.89)	71,916 (36,037; 35,879)	-0.0208 (-0.0332, -0.0083)	0.001
Unauthorised absence rate (across Terms 2–6)	36,037	4.90 (4.78, 5.02)	35,879	4.98 (4.86, 5.11)	71,916 (36,037; 35,879)	0.0012 (-0.0093, 0.0118)	0.817

Data source: Wonde.

Analysis in the presence of non-compliance

Primary indicator (sending the text)

Compliance data was available for around half of the 105 schools included in the analysis for each of Terms 2–6 (47, 57, 60, 53, 53 schools for each of Terms 2–6, respectively).[21] For the primary compliance analysis, we included the pupils in our main analysis sample who belonged to the subset of schools that we had compliance data for in each term.[22] Those schools who supplied compliance data indicated that they sent a message in 81.7% of the 48,665 cases where pupils in the treatment group were eligible for a term; while in 12.1% of such cases, schools indicated that they did not send a text message. Schools were less likely to send text messages to eligible pupils in later terms, especially in Term 6, see Table 19.

Table 19: Distribution of the primary compliance indicator (receiving the text) for all terms where a pupil in the intervention arm was eligible for

Whether a text message was sent to the pupil in the term	Term 2 (N=7,720)	Term 3 (N=11,105)	Term 4 (N=10,985)	Term 5 (N=9,326)	Term 6 (N=9,529)	Overall (N=48,665)
Yes	6,888 (89.2%)	9,327 (84.0%)	9,131 (83.1%)	7,636 (81.9%)	6,772 (71.1%)	39,754 (81.7%)
No	505 (6.5%)	817 (7.4%)	1,291 (11.8%)	1,358 (14.6%)	1,900 (19.9%)	5,871 (12.1%)
Not clear from the data	0 (0%)	0 (0%)	0 (0%)	0 (0%)	2 (0.0%)	2 (0.0%)
Missing	327 (4.2%)	961 (8.7%)	563 (5.1%)	332 (3.6%)	855 (9.0%)	3,038 (6.2%)

Data source: Returned school monitoring forms.

The focus of the compliance analysis was the first term that the pupils were eligible to receive a text message; when we restrict the data to that term, from schools who sent compliance data, then the overall compliance rate increased to 86.6%. This suggests schools were less likely to withhold sending text messages the first time a pupil became eligible for the intervention, and/or as most of the pupils became eligible in the earlier terms, schools were less likely to withhold sending text messages in the earlier terms of the trial (see Table 20).

[21] There were 77 schools that shared the compliance data for at least one term, with 26 schools sharing data for all five terms.
[22] This included 22,723 of 36,037 pupils in the treatment group, and 22,561 of 35,879 pupils in the control group.

Table 20: Distribution of the primary compliance indicator (receiving the text) for the first term a pupil in the intervention arm was eligible for the intervention

Whether a text message was sent to the pupil in the term	Term 2 (N=7,720)	Term 3 (N=4,960)	Term 4 (N=2,339)	Term 5 (N=1,368)	Term 6 (N=1,032)	Overall (N=17,419)
Yes	6,888 (89.2%)	4,293 (86.6%)	2,002 (85.6%)	1,138 (83.2%)	771 (74.7%)	15,092 (86.6%)
No	505 (6.5%)	212 (4.3%)	212 (9.1%)	186 (13.6%)	193 (18.7%)	1,308 (7.5%)
Missing	327 (4.2%)	455 (9.2%)	125 (5.3%)	44 (3.2%)	68 (6.6%)	1,019 (5.9%)

Data source: Returned school monitoring forms.

The final sample for the primary compliance analysis included 16,393 pupils in the intervention arm, after excluding the 1,019 pupils with missing values for the primary compliance indicator (whether a text was sent to a pupil in the intervention arm for all terms they were eligible to receive it) and seven pupils for whom the absence rate for the first eligible term was missing due to zero attendable sessions in the term (all sessions were marked as 'VOID'). The compliance rate was 92.0% in the intervention arm. The sample also included 17,088 pupils in the control arm, after excluding 11 pupils with a missing absence rate for the first eligible term. We did not identify any pupils in the compliance data who belonged to the control arm, suggesting that none of the control pupils was sent a message by the schools. We note that the compliance data is based on information provided by schools in the returned monitoring forms and that we cannot know for sure whether schools sent the text messages, who they sent text messages to, and what information the text messages contained. Overall, we included 33,481 pupils from 77 schools in the primary compliance analysis.

The indicator variable for intervention allocation was a strong instrument for the endogenous primary compliance indicator, with a correlation of 0.92 and a F-value of 201,840 in the first stage of the 2SLS analysis. The full model results for the first stage can be found in Appendix D Table D1. The 2SLS analysis estimated that the intervention reduced the absence rate in the first eligible term by 0.59 percentage points on average among the compliers (CACE) who had a text message sent to them by their school and were in a school that returned compliance data (95% bootstrap percentile CI: -1.05, -0.45). This translates into a mean reduction of approximately 0.19 days of absence per term for pupils who were sent a text message in the term that they became eligible for the intervention. In comparison, the ITT analysis of the primary compliance analysis sample estimated that the intervention reduced the absence rate in the first eligible term by 0.38 percentage points (95% CI: -0.62, -0.15), see Appendix D Table D2.

The effect size estimated by the ITT analysis among this sample was larger than that among the main analysis sample (g = -0.0243, 95% CI: -0.0394, -0.0092), see Table 17 for the effect size for the main analysis sample. This suggests that there might be some selection bias in terms of which schools we had compliance data for, and the results of the compliance analysis might provide an estimated effect that is larger than it would have been without missing data. Indeed, compliance data was only available for around 50% of schools in the trial in each term. Because these schools both returned monitoring forms and appeared to have delivered the intervention as intended, the estimated complier-average effect is expected to be larger than the full-sample ITT effect. In other words, the subset of schools that provided compliance data showed a larger effect than the full set of schools under the primary analysis, meaning that the former might systematically differ in a way that boosts the measured impact of the intervention. Some schools that did not return compliance forms may have failed to implement the intervention, which would attenuate the ITT estimate. Therefore, the compliance analysis likely represents an upper-bound estimate under full delivery. Taken together, the ITT effect reflects real-world roll-out, including non-delivery in some settings, whereas the complier-average estimate shows what could be achieved under full adherence.

Secondary indicator (timeliness)

Schools indicated that they sent the text messages within two weeks of the term start in 172 of the 270 cases where we had access to the compliance data for a school in a term, while in 12 cases, schools reported sending the text messages after the first two weeks. The date was missing for 33 cases and another 53 cases had a reported date of sending texts that was earlier than the date that term started, which we treated as missing as well, see Table 21.[23]

[23] We extracted the date schools reported to send the text messages from returned monitoring forms. If schools did not record the date in the dedicated date input cell, we treated it as missing. The date input cell was pre-filled in Terms 2–5 but not in Term 6. This explained why we saw a larger number of missing cases in Term 6. In 32/33 of the cases where the date in date input cell was earlier than the term start date, the schools seemed to just leave the pre-filled date, so we were not sure when the schools actually sent out the text message;

Table 21: Distribution of the secondary compliance indicator (timeliness) for the schools with compliance data available in each term

Whether a school sent the text messages within the first two weeks of the term	Term 2 (N=47)	Term 3 (N=57)	Term 4 (N=60)	Term 5 (N=53)	Term 6 (N=53)	Overall (N=270)
Yes – Within the first two weeks	35 (74.5%)	30 (52.6%)	46 (76.7%)	48 (90.6%)	13 (24.5%)	172 (63.7%)
No – After the first two weeks	4 (8.5%)	3 (5.3%)	3 (5.0%)	2 (3.8%)	0 (0%)	12 (4.4%)
Not clear from the data – Earlier than term start date	7 (14.9%)	18 (31.6%)	7 (11.7%)	1 (1.9%)	0 (0%)	33 (12.2%)
Missing	1 (2.1%)	6 (10.5%)	4 (6.7%)	2 (3.8%)	40 (75.5%)	53 (19.6%)

Data source: Returned school monitoring forms.

We excluded pupils in both the intervention and control arms from this secondary compliance analysis if the secondary indicator (whether schools sent the text messages within two weeks of the start of the term) was missing for their schools in their first eligible terms. The final sample for the secondary compliance analysis included 11,797 pupils in the intervention arm and 12,446 pupils in the control arm from 75 schools. The compliance rate was 83.0% in the intervention arm, lower than the compliance rate for the primary indicator, see Table 22.[24]

Table 22: Distribution of the secondary compliance indicator (timeliness) for the first term a pupil in the intervention arm was eligible for

Whether a text message was sent to the pupil within the first two weeks in the term	Term 2 (N=6,076)	Term 3 (N=2,434)	Term 4 (N=1,772)	Term 5 (N=1,281)	Term 6 (N=234)	Overall (N=11,797)
Yes	4,992 (82.2%)	2,127 (87.4%)	1,421 (80.2%)	1,083 (84.5%)	168 (71.8%)	9,791 (83.0%)
No	1,084 (17.8%)	307 (12.6%)	351 (19.8%)	198 (15.5%)	66 (28.2%)	2,006 (17.0%)

Data source: Returned school monitoring forms.

The ITT effect size of the intervention on the absence rate of eligible pupils in the first term they became eligible in the secondary compliance analysis sample was similar to that of the primary compliance analysis sample (g = -0.0248; 95% CI: -0.0425, -0.0071). The correlation between the randomised intervention allocation and the secondary compliance indicator was 0.85, with a F-value of 69,704 in the first stage of the 2SLS analysis. The full model results for the first stage can be found in Appendix D Table D3. The 2SLS analysis estimated that the intervention reduced the absence rate in the first eligible term by 0.76 percentage points on average among the compliers (CACE) who had a text message sent to them by school in the first two weeks of a term (95% bootstrap percentile CI: -1.30, -0.28). This was larger than the 0.40 percentage-point decrease estimated by the ITT analysis of the same sample (95% CI: -0.68, -0.11), see Appendix D Table D4, and larger than the CACE for the primary compliance indicator as well.

Non-compliance when pupils received the intervention despite not being eligible for it

The compliance analyses in the subsections above examined whether a text message was sent to a pupil (in a timely way) when a pupil was eligible. We also checked a different kind of non-compliance, which was when a non-eligible pupil appeared in the compliance data. There were 24,674 pupils from 77 schools in the compliance data, of which 24,638 existed in the randomisation sample. All of the 24,638 pupils belonged to the intervention arm. Out of the 52,427 cases where a pupil appeared in the compliance data for a term, in 46,107 (87.9%) of the cases the pupils were identified as eligible in our attendance data, while in 753 (1.4%) of the cases, the pupils were identified as ineligible in the term.[25] The attendance data was missing for the remaining 5,567 (10.6%) cases. In 562 (74.6%) out of the 753 cases where a pupil was identified as ineligible in our attendance data, the schools indicated that they sent a text message to the pupil.

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and in the one remaining case, we were not sure why the sent date was earlier than the start of the term. Therefore, we treated all 33 cases as missing. Furthermore, for Terms 2–5, it is possible that some schools did not update the pre-filled date, which happened to be within the first two weeks of the term start date, making the compliance indicator unreliable.

[24] N.B. The compliance rate for the secondary indicator was calculated with a smaller sample due to missing secondary indicator data.

[25] Possible reasons for ineligible pupils being included in the compliance data include: the evaluation and delivery teams accessing and extracting Wonde data separately and at different times and so getting different versions of the same attendance record; and the delivery and evaluation teams treating some attendance codes differently (such as 'educated off-site'). This could have led to some pupils having a lower number of absent sessions in our calculation compared to BIT's, resulting in some pupils in the compliance data being identified as ineligible in our attendance data.

Missing data analysis

A total of 155 eligible pupils were excluded from the analyses. Of these, the outcome was undefined at either baseline or endline for 111 pupils (50 intervention, 61 control) in the primary and secondary analyses. This was due to two reasons: i) pupils having zero attendable sessions across Terms 2–6 and consequently having missing primary and secondary outcomes (7 intervention, 11 control); and ii) pupils having zero attendable sessions in Term 1 and consequently having missing baseline measures (43 intervention, 50 control). A further 42 pupils had partial attendance data. While they had enough data to determine their eligibility, they had one or more terms with no attendance data whatsoever. Finally, two pupils had inconsistent information about the school they attended across the data used at randomisation and that pulled from Wonde.

These 155 pupils belonged to 70 schools in total (47 schools for pupils in the intervention, and 48 schools for pupils in the control) and 152 families (68 intervention, 84 control).[26] Distribution of baseline characteristics of these pupils can be found in Appendix E Table E1. As we had pupil-level missingness less than 5% in our primary and secondary outcomes, we conducted complete-case analyses.

For the 4.8% of pupil-level missing data, the demographic characteristics at randomisation are in Table 13. Since this was less than the threshold of 5% specified in the Statistical Analysis Plan (Cornel *et al.,* 2024), we did not proceed further with investigating the characteristics associated with missingness.

Subgroup analyses

Pupils eligible for FSM

Pupils who were eligible for the intervention who were also eligible for FSM had a higher absence rate on average (16.33% intervention, 16.98% control) compared to pupils who were eligible for the intervention but not eligible for FSM (9.27% intervention, 9.36% control). The model interacting FSM status with trial arm indicated that the intervention led to a 0.34 percentage-point decrease in absence rate among pupils who became eligible for the intervention and who were also eligible for FSM (95% CI: -0.61, -0.07), and the decrease was 0.29 percentage points smaller among pupils who were eligible for the intervention but not eligible for FSM compared to pupils eligible for FSM, albeit with a CI that includes zero (95% CI: -0.03, 0.62) (see Appendix F Table F1).

The split-sample models also show a larger effect among the FSM-eligible pupils than non-FSM eligible pupils (given that their attendance rates made them eligible for the intervention), see Table 23 and Table 24. The split-sample model indicated that FSM pupils in the intervention arm who were eligible for the intervention were on average absent 0.51 fewer days over five terms than those in the control arm who were eligible for the intervention, equivalent to a 0.65 percentage-point reduction. Although this difference is statistically significant ($p = 0.0495$), it should be interpreted with caution because statistical significance alone does not guarantee that the estimate is reliable. The trial was powered to detect a minimum change of 1.31 percentage points in the FSM subgroup. Detecting an effect only half that size had low statistical power meaning that: i) the study had a small chance of detecting a true effect of that size, so obtaining a p-value lower than 0.05 could be a chance finding (i.e. higher risk of a false positive); and ii) estimates that do reach significance in low-powered settings are often imprecise and upward biased (i.e. likely to overstate the magnitude of the effect). In practical terms, the CI is wide enough that the true effect could be much smaller or larger, so the finding should be regarded as suggestive rather than conclusive. Consequently, replication with a larger FSM sample is recommended before drawing firm conclusions.

[26] The numbers in parentheses do not sum to 70 because some schools have eligible pupils missing in both the control and intervention groups. There are 43 schools in total with at least one pupil missing, 23 schools in total with some pupils in the intervention missing, and 31 schools in total with some pupils in the control missing.

Table 23: Subgroup analysis – pupils eligible for FSM

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	11,429	16.33 (15.99, 16.67)	11,412	16.98 (16.63, 17.33)	22,314 (11,429; 11,412)	-0.0177 (-0.0354, 0.000)	0.0495

Data source: Wonde.

Table 24: Subgroup analysis – pupils not eligible for FSM

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	24,608	9.27 (9.12, 9.42)	24,467	9.36 (9.21, 9.51)	49,075 (24,608; 24,467)	-0.0039 (-0.0170, 0.0092)	0.559

Data source: Wonde.

EAL pupils

EAL pupils who were eligible for the intervention had a lower absence rate on average (9.68% intervention, 9.84% control) compared to non-EAL pupils who were eligible (12.19% intervention, 12.47% control). The model interacting EAL status with trial arm indicated that the intervention led to a 0.30 percentage points decrease in absence rate among EAL pupils who were eligible for it (95% CI: -0.68, 0.07), while the decrease was 0.19 percentage points smaller among non-EAL pupils who were eligible, albeit with a CI that includes zero (95% CI: -0.23, 0.61) (see Appendix F Table F2). The split-sample models also show a larger effect among the EAL pupils, see Table 25 and Table 26. The split-sample models indicated that EAL pupils in the intervention arm who were eligible for the intervention were on average absent 0.42 days less in total across five terms than those in the control arm who were eligible.

Table 25: Subgroup analysis – EAL pupils

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	5,665	9.68 (9.40, 9.96)	5,705	9.84 (9.56, 10.11)	11,370 (5,665; 5,705)	-0.0267 (-0.0563, 0.0029)	0.077

Data source: Wonde.

Table 26: Subgroup analysis – non-EAL pupils

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention;	Hedges' g (95% CI)	P-value

					control)		
Absence rate (across Terms 2–6)	22,059	12.19 (11.98, 12.40)	22,061	12.47 (12.26, 12.68)	44,120 (22,059; 22,061)	-0.0076 (-0.0201, 0.0050)	0.237

Data source: Wonde.

Gender

Eligible female pupils had a higher absence rate on average (11.99% intervention, 12.29% control) compared to eligible male pupils (11.07% intervention, 11.33% control). The model interacting gender with trial arm indicated that the intervention led to a 0.22 percentage points decrease in absence rate among female pupils who were eligible (95% CI: -0.43, -0.004; female was the reference group in the model, so this is the coefficient on the variable for the intervention), while the decrease was 0.15 percentage points smaller among male pupils who were eligible, albeit with a CI that includes zero (95% CI: -0.14, 0.44; this is the coefficient of the interaction term, which indicates whether there is a difference between genders, and the direction suggests that the effect is smaller for eligible males than for eligible females, but the CI includes zero) (see Appendix F Table F3). The split-sample models also show a larger effect among the eligible female pupils, see Table 27 and Table 28. The results of the split-sample analysis suggest female pupils in the intervention arm who were eligible were on average absent for 0.35 days less in total across five terms than female pupils in the control arm who were eligible.

Table 27: Subgroup analysis – female pupils

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	17,217	11.99 (11.76,12.21)	17,098	12.29 (12.05,12.52)	34,315 (17,217; 17,098)	-0.0152 (-0.0296, -0.0008)	0.038

Data source: Wonde.

Table 28: Subgroup analysis – male pupils

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	18,820	11.07 (10.87,11.28)	18,781	11.33 (11.12,11.54)	37,601 (18,820; 18,781)	-0.0033 (-0.0171, 0.0106)	0.642

Data source: Wonde.

Year group

The model interacting year groups with trial arm suggests that there might not be an effect for pupils in Year 7 who are eligible for the intervention (coefficient = 0.08, 95% CI: -0.24, 0.40), which was also the reference group. However, the interaction term suggests that the intervention led to a larger decrease in the absence rate of those who were eligible in Year 8 (coefficient = -0.47, 95% CI: -0.91, -0.02). In contrast, there was no difference in absence rate between those who were eligible in Year 7 and Year 9 (coefficient = -0.14, 95% CI: -0.58, 0.30), Year 10 (coefficient = -0.24, 95% CI: -0.68, 0.21), and Year 11 (coefficient = -0.25, 95% CI: -0.73, 0.23) (see Appendix F Table F4). Consistent with this, the split-sample models indicate a largest effect among pupils in Year 8 (g = -0.0289, 95% CI: -0.0507, -0.0070). This suggests that Year 8 pupils in the intervention arm who were eligible were on average absent for 0.61 days less in total across five terms than Year 8 pupils in the control arm who were eligible. For all of the split-sample models by year group, see Table 29, Table 30, Table 31, Table 32, and Table 33.

Table 29: Subgroup analysis – Year 7 pupils

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	7,421	9.90 (9.62,10.18)	7,280	9.77 (9.50,10.03)	14,701 (7,421; 7,280)	0.0076 (-0.0178, 0.0330)	0.559

Data source: Wonde.

Table 30: Subgroup analysis – Year 8 pupils

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	7,630	10.70 (10.40,11.00)	7,615	11.18 (10.86,11.51)	15,245 (7,630; 7,615)	-0.0289 (-0.0507, -0.0070)	0.010

Data source: Wonde.

Table 31: Subgroup analysis – Year 9 pupils

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	7,655	11.83 (11.48,12.17)	7,734	11.79 (11.46,12.12)	15,389 (7,655; 7,734)	-0.0088 (-0.0303, 0.0128)	0.424

Data source: Wonde.

Table 32: Subgroup analysis – Year 10 pupils

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	7,582	12.50 (12.13,12.87)	7,660	13.19 (12.80,13.58)	15,242 (7,582; 7,660)	-0.0043 (-0.0245, 0.0159)	0.676

Data source: Wonde.

Table 33: Subgroup analysis – Year 11 pupils

	Unadjusted means	Effect size

	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges'g (95% CI)	P-value
Absence rate (across Terms 2–6)	5,749	12.92 (12.50,13.35)	5,590	13.31 (12.87,13.74)	11,339 (5,749; 5,590)	-0.0122 (-0.0341, 0.0097)	0.274

Data source: Wonde.

Absence level in the first term

The effect of the intervention was higher for pupils with a higher level of absence in Term 1: the intervention led to an additional decrease of 0.015 percentage points in the total absence rate across Terms 2–6 when the baseline level of absence was one percentage-point higher, among pupils who were eligible for the intervention in Terms 1–5 (95% CI: -0.026, -0.005).

Additional analyses and robustness checks

First time a text message was sent

We found a larger effect of the intervention in the first term that a pupil was eligible (or would have been eligible if in the control arm) to receive the intervention (g = -0.0137, 95% CI: -0.0242, -0.0033), compared to the primary outcome (g = -0.0093, 95% CI: -0.0194, 0.0009), which looked at the absence rate across Terms 2–6, see Table 34. The model suggests that the intervention led to a decrease of 0.23 percentage points in the adjusted mean absence rate in the first eligible term (95% CI: -0.40, -0.05)—this translates into a reduction of approximately 0.07 days of absence in a term.[27]

Table 34: Additional analysis – first receipt of a text message

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate in the first term eligible (or would have been eligible) for the intervention	36,013 (24)	10.88 (10.72,11.05)	35,844 (35)	11.26 (11.09,11.44)	71,857 (36,013, 35,844)	-0.0137 (-0.0242, -0.0033)	0.010

Data source: Wonde.

First time a text message was sent and onwards

We conducted further analysis looking at the effect of the intervention from the first term pupils were eligible (or would have been eligible if in the control arm) for the intervention until the end of the intervention period, given that the additional analysis for the receipt of a text suggested it had a larger effect the first time it was sent than in the primary analysis (across all of Terms 2–6). The result of this further analysis did not differ much from that of the primary analysis (g = -0.0093, 95% CI: -0.0198, 0.0013), see Table 35. This could be because most (50.4%) of the pupils became eligible for the intervention in Term 2.

Table 35: Additional analysis – first receipt of a text message and onwards

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean	n	Mean	Total n	Hedges' g	P-value

[27] Calculated using the mean number of attendable days per term, which was 30.14.

	(missing)	(95% CI)	(missing)	(95% CI)	(intervention; control)	(95% CI)	
Absence rate from the first term eligible (or would have been eligible) for the intervention onwards	36,036 (1)	11.54 (11.38, 11.70)	35,877 (2)	11.82 (11.66, 11.99)	71,913 (36,036, 35,877)	-0.0093 (-0.0198,0.0013)	0.085

Data source: Wonde.

Depending on the school term

We found some evidence that the effect of the intervention varied across school terms. Absence rate was 0.21 percentage points lower among those who were eligible in the intervention arm compared to those who were eligible in the control arm in Term 2 (95% CI: -0.41, -0.01). The interaction terms do not suggest the effects were different in Term 3 (coefficient = 0.03, 95% CI: -0.18, 0.23), Term 4 (coefficient = -0.06, 95% CI: -0.26, 0.15), Term 5 (coefficient = 0.12, 95% CI: -0.08, 0.33), and Term 6 (coefficient = 0.21, 95% CI: -0.0002, 0.4102).

School-level factors

The results of the primary analysis were robust in controlling for the school-level variable indicating the proportion of pupils who were eligible to receive a text message at least once during the trial in a school (g = -0.0093, 95% CI: -0.0198, 0.0013).

Dosage

Nearly half of the pupils were eligible for the intervention in one or two terms across Terms 2–6, with around 18% being eligible for all of the five terms, see Figure 4. We did not find strong evidence of effects differing by dosage, defined as the number of terms for which a pupil was eligible for the intervention. The model interacting the number of eligible terms with the trial arm suggests that the intervention might not have had an effect on the absence rate (across Terms 2–6) of those who were only eligible in one term (coefficient = -0.06, 95% CI: -0.31, 0.20). The negative interaction terms suggest that the intervention might have led to a decrease in the absence rate for those eligible for two, three, or four terms in the intervention arm compared to those in the control arm. However, we cannot conclude anything here as the estimates are noisy (two terms: coefficient = -0.06, 95% CI: -0.43, 0.32; three terms: coefficient = -0.16, 95% CI: -0.56, 0.25; and four terms: coefficient = -0.01, 95% CI: -0.44, 0.42). In contrast, the interaction for the highest dosage is positive and of a relatively large size (coefficient = 0.40, 95% CI: -0.005, 0.807), suggesting that the intervention did not work for those eligible for all five terms.

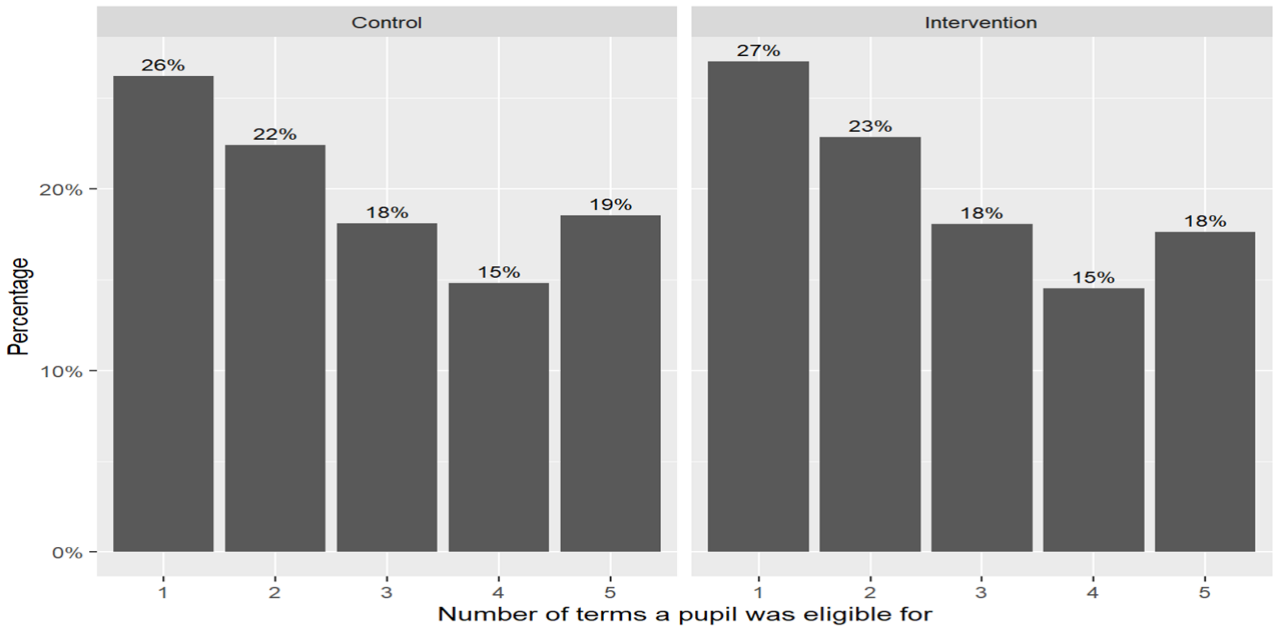

Figure 4: Distribution of number of eligible terms for a pupil, by trial arm

Excluding Year 11s

In order to investigate whether the attrition due to school-level missing data for pupils in Year 11 affected the result, we conducted an additional analysis that excluded all Year 11s; the results were nearly identical to the primary analysis model

involving all pupils. Participants in the intervention arm who were eligible were absent for 11.24% of the attendable sessions across Terms 2–6, compared to an absence rate among those who were eligible in the control arm of 11.51%. This gives a standardised effect size of close to zero and a CI that crosses zero (g = -0.0174, 95% CI: -0.0377, 0.0029), see Table 36. The coefficient in the model shows that the adjusted mean absence rate was 0.14 percentage points lower in the intervention arm compared to the control arm—again with a CI crossing zero (95% CI: -0.30, 0.02); this translates into a mean reduction of approximately 0.21 days of absence in total across five terms for those who were eligible for the intervention.

Table 36: Additional analysis – no Year 11s

	Unadjusted means				Effect size		
	Intervention		Control				
Outcome	n	Mean (95% CI)	n	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	P-value
Absence rate (across Terms 2–6)	30,288	11.24 (11.08, 11.40)	30,289	11.51 (11.34, 11.67)	60,577 (30,288; 30,289)	-0.0174 (-0.0377, 0.0029)	0.093

Estimation of effect sizes

Effect sizes (Hedges' g) are reported in Table 17 and Table 18 above. The estimates used to calculate the effect sizes for each outcome are presented in Appendix G Table G1.

Estimation of ICC

The family-level conditional ICC was 0.353 and the school-level conditional ICC was 0.015 based on the sample of 63,295 families in 105 schools in the primary analysis. The empty model with no predictors estimated the family-level unconditional ICC to be 0.506 and the school-level unconditional ICC to be 0.032.

Implementation and Process Evaluation

This section is structured according to the EEF IPE dimensions.[28] A summary of each dimension is provided first and links findings to the assumptions set out in the Theory of Change when applicable. Case studies are also provided at the end of each dimension to contextualise findings where relevant. The 14 schools that provided interview data at baseline and endline are referred to as interviewed schools.

Throughout this section we refer to surveyed schools and parents/carers as survey respondents for the school baseline and endline survey, and the parent/carer endline survey. At baseline, 90 staff members completed the school survey on behalf of their school (out of a possible 108), and at endline 42 school staff members completed the survey (out of a possible 105). At endline, 781 parents/carers from control and treatment group families completed the parent/carer survey for their own child who became eligible for the intervention at least once. The number of times parent/carer survey participants could have received the intervention (the number of times their child became eligible for the intervention) varied between one to five times, see Table 37. All survey results are presented in percentages and number of respondents.

Table 37: Distribution of endline survey respondents by the number of terms during which their child was eligible for the intervention

No. of terms parent/carer endline respondents could have received the intervention	Parent/carer endline survey respondents in the control (N=396) n (%)	Parent/carer endline survey respondents in the intervention (N=385) n (%)
One term	144 (36%)	132 (34%)
Two terms	94 (24%)	94 (24%)
Three terms	64 (16%)	62 (16%)
Four terms	46 (12%)	55 (14%)
Five terms	48 (12%)	42 (11%)

Usual practice, Programme differentiation

All interviewed schools monitored pupil attendance and delivered a variety of attendance interventions and communication strategies to improve pupil attendance. School communications to parents/carers varied depending on the context of the pupil absence with email being the most commonly used mode of school-to-parent communication. Schools used a variety of communication platforms to communicate with parents/carers. Prior to the intervention, text messages were used within half of interviewed schools; however, no schools communicated cumulative attendance data via text in usual practice. The use of text messages in a more systematic way to try to influence parents/carers and subsequently pupil behaviour across multiple terms (through building better awareness of attendance, impact of poor attendance in the previous term, and resetting and motivation in the following term) was different from business as usual.

What is usual practice (e.g. frequency, style) in communication about absence between school and parents/carers?

Usual practice in school communication about pupil absence to parents/carers varied between schools at baseline. Almost all schools regularly monitored pupil attendance and used parent/carer communications as part of attendance intervention strategies. From the baseline school survey:

- 93% (N=84) of survey respondents reported using a Management Information System to track pupil attendance.
- 92% (N=83) of survey respondents reviewed pupil attendance at least once a week to identify pupils with low attendance and 51% (N=46) reviewed this daily.
- 89% (N=80) of survey respondents sent communications to parents/carers if their child's attendance fell below a certain level.

[28] The re-structure is a slight variation from the trial protocol; however all pre-registered research questions are discussed.

Data from the school interviews and survey at baseline suggest that schools used many forms of communication with parents/carers, varying in the tone, channel, frequency, and responsiveness of communication depending on the circumstances of a pupil's absence. For generic attendance messages, such as attendance initiatives or updates on schools' attendance performance, interviewed schools tended to send parents/carers emails using their communication platforms (e.g. Bromcom, Arbor Management Information System, CPOMS [Child Protection Online Management System], Edulink, SIMS Attendance, Agilent). Data from the school baseline survey also showed that email was the most common way schools communicated with parents/carers about pupil absence across schools (83%, N=66). Survey respondents also reported communicating with parents/carers via phone (76%, N=61), face-to-face meetings (71%, N=57), and letters sent in the post (60%, N=48). Most interviewed schools either conducted calls and/or sent text messages to parents/carers on the first day of an unexpected absence in an effort to prevent subsequent absences and for safeguarding purposes. Interviewed schools perceived letters as a more formal means of communication to be used less frequently, for example, to share attendance reports on a termly or annual basis, or to give informal or formal warnings as and when attendance dropped below certain statutory thresholds. In less common and more complex attendance circumstances, such as if a parent/carer failed to respond to a morning phone call or text message from the school about their child's absence, some schools would conduct a home visit, made by a safeguarding officer or member of the senior leadership team; and for more persistent absences, schools would arrange a face-to-face meeting between the parent/carer and school. Beyond termly or annual attendance reports, communications tended to be daily calls or text messages to inform parents/carers of missed days, or a targeted intervention (usually by email or phone call) if a school was worried about a pupil. None of the interviewed schools described sending communications to parents/carers that resembled a fresh start.

How different are the text messages from usual communications about attendance?

The BITUP intervention has three core components: i) it is sent directly to parents/carers via text message; ii) it is sent out once at the beginning of Terms 2–6 and only to the parents/carers of pupils with an attendance rate lower than 95% in the previous term; and iii) it provides parents/carers with their child's attendance feedback in terms of the number of days of school missed.

Prior to the BITUP intervention, half of the interviewed schools used text messages as part of their usual practice. Around half of the school survey respondents at baseline reported already using text messages (48%, N=38) and messages via school apps or websites (50%, N=40) to communicate with parents/carers. However, in contrast to the BITUP intervention, data from school interviews suggest text messages tended to be used on an ad hoc basis to inform parents/carers immediately about their child's absence or lateness (e.g. a text message sent on the same morning of the absence/lateness) and request details of the absence.[29] None of the schools interviewed at baseline were sending out pre-scheduled text messages on a regular basis providing feedback about cumulative absences over a period of time.

At baseline, most interviewed schools tended to send termly or annual attendance updates in the form of percentages, despite schools acknowledging that parents/carers can find percentages difficult to understand or grasp the severity of absence.

> *We predominantly communicate in percentages. A lot of people don't understand them, which is what really interested me about this [intervention].* (Urban location, Large school, Large proportion of pupils eligible for FSM, Medium proportion of pupils with EAL)

This finding was supported by responses to the school baseline survey, completed by schools before they started sending the BITUP intervention. Of the 80 respondents who reported sending communication to parents/carers about their child's attendance, 86% (N=69) reported communicating attendance to parents/carers as a rate or percentage. Around 24% (N=19) reported communicating attendance to parents/carers in terms of days missed. In some cases, survey respondents reported using both a rate and the number of days (18%, N=14).

[29] All schools interviewed reported communicating morning registration absences and late arrivals to parents on the day of absence to make parents aware of and request the reason for absence. Those schools without a text message system in place would communicate via a phone call instead; these schools tended to have greater staff capacity.

Fidelity (quality)

Almost all interviewed schools sent messages out in text message format, within the timeframe and at the frequency intended, suggesting there was fidelity in the quality of the intervention delivery. In a few cases there was a slight deviation from the format used due to the capabilities of the text messaging platform used by schools and cost of sending texts. Other minor fidelity issues resulted from inconsistent use of attendance codes and schools' ability to accurately assess which families/pupils to exclude from receiving a text message. There was also fidelity to the key inputs and output of the Theory of Change (schools receiving and checking a list of eligible message recipients, preparing a personalised message to send to parents/carers, and actively informing parents/carers of their child's attendance by text at the start of each half-term in the number of days absent in the prior half-term).

To what extent was the BITUP intervention implemented as intended? Were school staff able to send out the messages as planned to the pupils who became eligible in the treatment group?

School staff who were interviewed broadly felt that their school was able to send out the text messages as planned and to the intended families. This indicates that the delivery process of sending schools a list of eligible pupils with the number of absent days in the previous term to include in the text message was successful.

The quantitative IPE data suggest that in most cases the interventions were sent within the first two weeks of the new five- to eight-week term, but this was not always done consistently. Monitoring form data indicates that schools sent text messages to parents/carers within the first two weeks of term 64% of the time (see Table 21). Additionally, results from the endline school survey found 79% (N=33) of respondents sent text messages about once every five- to eight-week term. Of these, 85% (N=28) reported they had sent the text message out at the beginning of the five- to eight-week term, the rest either indicated that the timing varied each term (6%, N=2) or that the text messages were sent at the middle or end of the term (9%, N=3). In a few cases, interviewed schools reported a delay in sending text messages on time. One school described a particularly challenging half-term before Christmas, which meant they were unable to send the text messages out until January. For another, differences in holiday periods meant text messages were sent out a week later than intended.

During the trial, issues with the ways that some schools used attendance codes and how these interacted with Wonde, in ways that were not anticipated by the developer, meant that some pupils were erroneously listed as eligible (having below 95% attendance) for the intervention when they were not. The developer and Verian both found that there are inconsistencies across—and even within—schools in how attendance codes are applied, despite DfE guidance. This may be problematic for intervention delivery or evaluations that rely on attendance data across schools.

In our study, different issues occurred in Waves 1 and 2. This was flagged to the delivery team early by schools in both terms, as schools had either noticed differences between the delivery team's information and their own data or had been contacted by parents/carers who noticed the number of days missed was incorrect (this was only in the case for Term 2, as in Term 3 the mistake was caught before any intervention text messages were sent out). In response to this, the delivery team updated how they calculated days of school missed and sent out information in smaller, staggered batches to minimise the number of schools impacted in the case of another error. The delivery team reported no issues with the information sent to schools in Terms 4–6. None of the interviewed schools recalled an incident where families were sent a text message that they should not have had. One interviewed school had two instances where absence data was incorrect. As a result, the school sent text messages to the correct recipients but with the incorrect number of days. The school reported this had distressed parents/carers receiving the text message. For another interviewed school, the encryption software made it challenging to download attendance data at times, which required support from BIT to resolve.

Almost all interviewed schools sent messages out in text message format as intended. There was, however, some variation across the schools in the way the schools interacted with the messaging platforms to deliver the texts. Some interviewed schools were able to upload personalised data and send individual messages in bulk with ease. These schools reported using the CPOMS and ParentPay platforms. In contrast, other schools used different platforms, for example, Edulink (or a version of this platform the school had purchased), that were described as having limited functionality and did not allow for the bulk upload of names and other personalised data. In these scenarios school administrative staff needed to manually send the personalised templates prepared by BIT (including the pupil's names and days of school missed) for each pupil individually.

One interviewed school sent messages out via its school app (Edulink), which meant parents/carers would be sent a notification in the app rather than an actual text message because the school judged it to be more cost-effective. The schools said that parents/carers in this school were used to receiving messages on the app. Another school was unable to confirm whether parents/carers were sent the intervention message as a text or email because this depended on the communication preferences selected by parents/carers when they signed up to the platform.

> *It would have cost hundreds of pounds to get the message to be delivered as an actual text message. So I decided it will come through as a notification on the app that they've got rather than being an actual text message vibrating on their phone.* (Urban location, Small school, Small proportion of pupils eligible for FSM, Medium proportion of pupil with EAL)

Which school staff members were involved in delivering the intervention? Who in the schools sent out the text messages?

In interviewed schools, it was most common for the messages to be sent out by an attendance officer. This finding was supported by the endline school survey results: the three most reported staff roles with primary responsibility for sending out the intervention messages were attendance officers (36%, =18), school administrators (19%, N=8), and assistant headteachers (17%, N=7).

The number of staff involved in intervention delivery varied across schools. From the endline school survey, 48% (N=20) of respondents reported that only one member of staff was involved in sending out the messages, and 52% (N=22) indicated more than one member of staff was involved. In interviewed schools, the process for approving and sending out intervention messages tended to involve more than one staff member. Attendance officers tended to be responsible for filtering the attendance data shared by the delivery team and deciding which families should and should not be sent the messages. Then, attendance leads (most commonly assistant headteachers) would review and confirm those selected to be sent the message before messages were sent out. In some cases, the review process involved wider staff such as heads of years and behavioural or pastoral leads, to ensure that pupils were appropriately selected or excluded from the message.

Were there any barriers and enablers to implementing the text messages?

Data from the IPE suggests that ease of implementing the intervention differed between schools. From the endline school survey, 45% (N=19) of respondents reported that sending the intervention messages was easy or very easy, 33% (N=14) found it neither easy nor difficult, and 21% (N=9) found it difficult or very difficult.

Interviewed schools described some challenges to implementing the intervention. For example, at the beginning of the intervention some schools recognised that their administrative staff did not have sufficient experience or skills to use the relevant platforms, which hindered initial implementation. However, over time staff were able to gain the necessary skills, as they became more familiar with the platforms and processes. These schools stated that they would have welcomed some training sessions to address these perceived skills gaps and reduce the burden on staff of having to learn how to use the platforms. Having said this, on the whole, schools found the step-by-step instructions provided by BIT comprehensive and straightforward, and if schools had any issues with downloading/accessing data these were met with timely and effective support from BIT.

Interviewed schools who used a text message platform or Management Information Systems with limited capabilities were required to manually input data before sending out messages. For example, one school using Edulink described the platform as not having the functionality to upload parents'/carers' and pupils' names and number of days missed. The process of adding this information manually was time-consuming and often led to these administrative and attendance staff having negative perceptions of the intervention as a whole. These schools also tended to be negative about the impact of the messages in relation to the effort it took to administer them. One school described how they manually removed the names of the pupils whose parents/carers had not consented to receiving messages; this process had to be repeated each half-term and was perceived as onerous and time-consuming. This process, as described by the school, does not align with the instructions provided by BIT through the project information sheet and withdrawal form to all parents. The instructions specified schools should remove BIT and Verian's access to all data on Wonde for any pupils whose parents withdrew them from the programme. This would have negated the need to manually remove messages for these pupils from the spreadsheets BIT sent out each term to schools.

The intervention was experienced as particularly burdensome by several schools that had placed significant emphasis on ensuring precise targeting of the message.

I would look at each individual case to see why they were off, then I would send the list to their Heads of Houses to see if there was any reason why they didn't feel a text should go to that family and once they gave the OK, then I would send the text out to each family. I would have to do them individually, so it was a very lengthy process. (Urban location, Large school, Small proportion of pupils eligible for FSM, Small proportion of pupils with EAL)

The more tricky part of the process is trying to personalise the response to each individual, in terms of getting the appropriate salutation—the process which we use for communication doesn't allow you to bulk upload each of these communications, you've got to do them one by one. (Urban location, Large school, Small proportion of pupils eligible for FSM, small proportion of pupils with EAL)

Case study 1: Low cost and easy to implement

Urban location, small school, large proportion of pupils eligible for FSM, and large proportion of pupils with EAL

This school was able to upload the attendance data provided by BIT to its existing Management Information System, ParentPay, with ease. Parents/carers had registered their details including their phone number, child's name, and preferred language on the ParentPay app when their child joined the school. Having this pre-registered information on the platform meant that the intervention text messages were personalised automatically, with both the parent first name, and sent in the appropriate language. The process of sending the messages to all eligible parents/carers took between three to four hours and had no direct financial impact on the school in terms of software, hardware, or training. Staff described the process as smooth and straightforward.

Case study 2: Time-consuming and onerous for staff

Urban location, small school, small proportion of pupils eligible for FSM, and small proportion of pupils with EAL

This school's existing Management Information System did not allow for bulk upload of attendance data, a feature that is only available to schools who purchase automatic reporting systems. To send messages to eligible parents/carers, staff needed to input the details of each pupil manually, which staff reported as being straightforward but time-consuming and so they judged this to be an inefficient use of their time. This meant that despite the initial enthusiasm among staff about delivering the intervention, they quickly became frustrated with the limitations of the platform and perceived their time as being the largest cost associated with delivering the intervention. Staff remained committed to the intervention nevertheless and showed interest in understanding its potential impact on attendance.

Fidelity (adaptation)

The Theory of Change assumed that schools would have flexibility to fine-tune the message through personalisation of parent/carer names. In a few cases, schools adapted the intervention message to include a generic rather than personalised salutation due to the limited capabilities of their messaging platforms, and where a need was identified schools sent translated versions of the message to EAL parents/carers.

To what extent, and how, did school staff adapt the basic intervention content, and why?

Most school survey respondents in the trial did not adapt the text message content. In the endline school survey, 74% (N=31) reported making no amendments to the text messages. Similarly, most of the interviewed schools did not make any changes to the content or wording of the text messages before sending them to parents/carers, either because they were happy with the wording or because they wanted to operate within the parameters of the trial.

Of the ten schools in the endline school survey that reported making adaptations to the intervention, adaptations included: changing the number of days the pupil had been absent from school (N=3); making changes to the pupil name (N=2);

softening the language or providing reassurance to parents/carers (N=2); changing the pupil's days of school missed to a percentage (N=1); adding a parent's first name (N=1), or shortening the message due to character limits (N=1). In the interview sample, there were also examples of adaptations relating largely to the personalisation of the text message. The text messages shared with schools already contained a standardised salutation 'Dear Parent / Carer', but schools were informed they could personalise the text messages with the name of the parent/carer. A few interviewed schools reported standardising the recipient's name because their messaging platform did not have the functionality to personalise individual text messages; these schools felt the administrative burden of personalising each text message was too high. For example, one school did not use the personalisation of 'Dear [parent/carer name]' and opted for 'Dear Family' instead. This streamlined the administrative process and eliminated the step of adding parents'/carers' names. Another school did not personalise messages at all. Their systems would not allow for parents'/carers' and pupils' names to be inputted automatically and the school's administrative team did not have the capacity to manually input these.

Another interviewed school added a personalised text message from the assistant headteacher at the top of the text to parents/carers to explain the purpose of the text message and outline the DfE's priorities in relation to improving attendance. The school believed this would soften the overall tone of the text message and would minimise potential negative responses from text recipients.

> *Parents are very sensitive to being sent a blanket message that's being sent to everyone…I checked with BITUP if it was possible to send a little personalised blurb from me, being someone parents know and trust…it is my little personalised bit at the top and then their message is underneath, so their wording has stayed exactly the same, we haven't edited that at all. We just needed to soften it a little bit.* (Urban location, Small school, Small proportion of pupils eligible for FSM, Medium proportion of pupils with EAL)

Which languages did the school send the text messages in, and how did they decide on which language to use with which family?

Nearly all interviewed schools were aware that text messages could be sent in different languages, but use of translations varied by school. In the endline school survey, 9 (21%) respondents indicated that they sent some text messages in a language other than English. Monitoring form data suggests fewer than 1% of text messages over the trial period (0.6%, N=340) were sent in a language other than English. However, it is possible that schools did not always record the text message the language was sent in within the returned monitoring forms. Interviewed schools that did not have EAL families, or were unaware of any EAL families, did not feel the need to pursue the translation option.

A few interviewed schools translated the intervention text messages. For example, one school with a small EAL cohort, which had recorded parents'/carers' communication preferences, sent text messages in a different language. They found the translated text message templates provided by the delivery team helpful as their existing in-house language support was limited to one teacher. Another school, which reported frequently communicating using different languages on its ParentPay system, sent out text messages in Bengali due to having a high proportion of EAL families within the school. One school was unaware of the opportunity to send the text messages in languages other than English, but acknowledged at the time of the interview that they had several EAL families who preferred to be sent school communications in Urdu. The availability of the text message in languages other than English prompted one school that sent all text messages in English to think more deeply about their data collection regarding language needs.

> *We've got a high Bengali population, so we actually sent some Bengali messages as well…I'm very big on, if we need to contact by email or anything, to try and get it in their language, it is so much easier.* (Urban location, Small school, Large proportion of pupils eligible for FSM, High proportion of pupils with EAL)

> *It has made me reflect that we have sent all our text messages in English, but I am aware that isn't the first language for many and maybe it would be more effective if we were to do collect the types of data, we don't have a list of people we would contact in additional language. That has been part of the school policy, we don't do that, although we have translators that can support if we are actually having conversations with parents*. (Urban location, Medium-sized school, Large proportion of pupils eligible for FSM, Large proportion of pupils with EAL)

Did schools use other channels than text message to communicate with parents/carers who did not have phones?

Interviewed schools did not purposefully send out text messages in an alternative mode, for example, via a phone call or letter, to parents/carers with limited digital access and who may not be able to receive a text message. Nearly all of these schools were confident that parents/carers had access to mobile phones to receive the text messages, although one school

reported being aware of the digital divide within its school community and was working on developing interventions more generally to engage parents/carers with limited access.

Fidelity (reach)

Intervention messages were sent out to the parents/carers of children whose attendance fell below 95%, according to the list of eligible pupils sent to schools by the delivery team at the beginning of each Term 2–6. All interviewed schools used their discretion on a case-by-case basis to exclude pupils from receiving a text message. This was as anticipated in the Theory of Change. Although interviewed schools said they used discretion, some parents reported receiving the text message when they thought that sending it was not appropriate.

How many text messages were sent out? How did the number of text messages sent out relate to total absence levels? To what extent, and how, did staff decide which parents/carers to contact, or not contact?

Over the course of the trial, 34,698 pupils in the intervention arm became eligible for the intervention at least once. Given 73% of these pupils became eligible for the intervention in more than one term (see 'Outcomes and analysis' section, Figure 4 above) approximately 94,726 messages could have been sent out in 102 schools across the trial. Monitoring form data from approximately 50% of schools in the trial in each term suggests that—in those schools—intervention messages were sent out in 82% of the cases where a pupil in the intervention arm became eligible for the intervention, and schools decided not to send 12% of all intervention messages. The proportion of messages withheld by schools was lowest in Term 2 (7%) and increased with each term over the course of the trial. Term 6 had the highest proportion of withheld messages, with 19.9% of messages not being sent by schools (see Table 19 above).

Most schools in the trial chose not to send text messages to some eligible pupils. This is supported by the results of the endline school survey, where 93% (N=39) of respondents chose to withhold some intervention text messages. Of those who decided not to send text messages to parents/carers, 90% (N=35) excluded pupils who had missed school due to long-term illness, 77% (N=30) due to a family bereavement, and 36% (N=14) because the parent/carer indicated that they did not want to receive the intervention. Given the high missingness of compliance data (i.e. schools' indication of whether or not they sent text messages) we are unable to assess exactly how many parents/carers fall in each of these groups.

All interviewed schools excluded specific families from receiving the messages due to individual pupil circumstances. Decisions around selection and exclusion were at the schools' discretion, decided in most cases by the school's attendance officer and reviewed by pastoral leads, heads of year, or the senior leadership team. Schools provided a range of circumstances that would warrant exclusion from a message including: authorised absences (for some schools); long-term or sudden illness; sensitive issues such as bereavement, attending a funeral, or bullying; and Year 11 pupils on study leave.[30] In situations where schools were aware of an existing safeguarding issue or where they were already working with families, schools decided it would be inappropriate and even detrimental to send a text message. This aligns with the delivery team's instructions to schools, 'You can choose not to send the message to any parents, where you do not feel it would be appropriate to do so. For example, if a pupil has been seriously unwell, or suffered a family bereavement'. (However, as we report below, parents/carers sometimes felt that they had been sent a message when it was not appropriate, including when absence was due to illness or bereavement.) It is unclear whether interviewed schools excluded specific families based on absence levels.

The process for deciding who to send text messages to and when to use discretion not to send them varied across the interviewed schools. In some schools the process was meticulous and attendance officers went through their absence data case-by-case to inform the decision to send the text message. There were also examples of schools taking a blanket approach in which, unless they were aware of very specific circumstances, for example, a child undergoing chemotherapy, they would send messages even if parents/carers had resisted or reacted negatively to them previously.

> *I can think of one family who automatically would have got the message. […] We know why she's not in. If the parents received a message talking about her attendance when they've just got off the phone to their*

[30] The delivery team did not count study leave towards days of school missed in Term 5 to determine whether Years 10–11 pupils would be eligible for the intervention in Term 6, so these pupils should not have been included on the lists. However, it is possible that some Year 11 pupils did appear on the lists of pupils eligible for the intervention during exam periods and staff decided not to send the text to their parents. Staff members may also have mentioned these pupils when thinking about those they would exclude if they continued implementing the intervention.

> *Head of House about their attendance plan, that just wouldn't make sense. It would sort of feel like the school doesn't know what the other part of the school is doing.* (Urban location, Large school, Small proportion of pupils eligible for FSM, Small proportion of pupils with EAL)
> *It's about us having the relationship with the families and using it as an additional tool to help promote positive attendance, rather than beat people with a stick…it's got to come from a 'we're on the same page' point of view.* (Urban location, Large school, Small proportion of pupils eligible for FSM, Small proportion of pupils with EAL)

Interviewed schools believed that decisions on pupil selection and exclusion criteria should be made at the schools' discretion and that any advice or guidance on this was unnecessary. Schools felt they understood their pupils best and were best placed to consider the varied circumstances of their pupils.

> *Each school is so different, they have their own policies, and whatever BITUP are trying to do, they have to make it work within every school's policy. So, I think leaving it to the school's discretion was the right thing to do.* (Urban location, Small school, Small proportion of pupils eligible for FSM, Medium proportion of pupils with EAL)

Fidelity (spillover)

Some parents/carers in the control group had heard about the text messages from their child's school or fellow parents/carers. No schools reported sending text messages to parents/carers in the control group; however nearly one-third of surveyed control parents/carers indicated receiving a school communication with the number of days absent. This suggests some confusion with the school's business as usual communications, but the potential of spillover cannot be ruled out completely.

To what extent, if any, have parents/carers and pupils in the control arm reached by, heard about, or influenced by the intervention?

None of the interviewed schools reported any incidents of families receiving a text message when they should not have; and schools believed that text messages were delivered to the families they were intended for. Schools, however, tended to download the information spreadsheet from BIT containing pupil names and the text message, and upload this to their text message platform. Schools did not know which families had been assigned to the intervention or control arm, so they could not have checked that only treatment families were sent the text messages. However, the delivery team did check that no pupils from control arm families were included in these lists. Despite there being no evidence of spillover in the qualitative interviews, the possibility of this cannot be ruled out completely.

In the parent/carer endline survey, 28% (N=110) of parent/carer respondents in the control arm indicated they had received a text message about their child's attendance specifying the number of days they were absent. We found no evidence that pupils from control arm families were included in the list of pupils shared with schools to send the interventions to. Therefore, it is possible that these parents/carers in the control arm received text messages about the number of days their pupil had missed as part of their school's normal practice, or that this was a result of acquiescence bias.[31]

In the parent/carer endline survey, 74% (N=212) of control arm respondents who had not received a text message about the number of days missed from their school were not aware that text messages were being sent to parents/carers. Out of the 74 (19%) respondents in the control arm who said they had heard about the text messages but not received them, 61% (N=45) had heard about them from their child's school, 18% (N=13) from other parents/carers, and 8% (N=6) had been shown the text messages by other parents/carers. This suggests that there may have been some spillovers between treatment and control families in terms of schools sending information about the messages to all families, and conversations with other parents/carers. However, it is possible that in some or all cases control arm respondents were thinking about other types of school-to-parent/carer communications when answering this question.

We did find some evidence that parents/carers discussed the text messages with other parents/carers in the parent/carer interviews. Most interviewed parents/carers stated that they had not spoken to other parents/carers about receiving the

[31] We know from the baseline school survey, that some schools reported communicating attendance in terms of days of school missed as part of their usual practice, see 'Usual practice, Programme differentiation' section above.

text message, which would indicate the likelihood of spillover is very low. Some parents/carers had spoken to other parents/carers at the same school about receiving a text message, some of whom had received a similar text message and some who had not: it is unclear whether the parents/carers spoken to were part of the control arm or not. Parents/carers who mentioned speaking to other parents/carers about the text messages tended to be those who were more negative or distressed about receiving them.

> *I mentioned it to a few friends who also thought it was pathetic.* (Parent of male pupil, Year 8, Received two or more texts)

> *I've talked to a number of parents who received similar sort of messages and say we almost feel like we're being shamed into sending our children to school. That's not what it should be about.* (Parent of female pupil, Year 8, Received two or more texts)

Responsiveness

The Theory of Change assumed that receiving a text message would prompt parents/carers to talk to their child about their attendance. Insights from the parent interviews and survey support this causal mechanism to some extent, where in some cases parents reported that the intervention acted as a reminder, prompted conversations, and was used by parents/carers as leverage to encourage attendance. However, the IPE results suggest that while some parents/carers took action after receiving a text message there was no universal response.

To what extent did parents/carers remember receiving the message(s)?

Interviewed parents/carers generally remembered receiving the message, although there was some limited recall of its precise content and on a few occasions the mode in which it was received. For example, parents/carers frequently recalled receiving attendance messages from their child's school through a variety of modes including text, emails, letters, and in-app messages. This suggests some potential confusion during the parent/carer interview discussions between schools' business as usual communications and the intervention text message, although a few interviewed schools reported sending messages in alternative modes because of the restrictions of their messaging platform or parent's preregistered mode preference for receiving school communications. In addition, a few interviewed parents/carers recalled messages that stated their child's absence in percentages, which did not feature in the intervention text message. This was consistent with interviewed school staff, who reported that their usual communications to parents/carers detailed attendance in percentages rather than days.

Interviewed parents/carers mostly had high recall of the message because the delivery mode was perceived as a novel way to communicate historical attendance data on a half-termly basis. A direct text message of this nature deviated from their school's usual practice and was therefore, prominent for many interviewed parents/carers. The sentiment and tone of the message (that the new half-term was a fresh start), irrespective of whether this was perceived positively or negatively by parents/carers, also supported parents'/carers' recall of the message: parents/carers felt this approach was unconventional for their school. Furthermore, parents/carers who perceived their child as generally having good overall attendance had particularly high recall because the message had been the first direct communication from the school about their child's attendance and so was memorable.

> *If it goes straight to email, you sometimes miss it but with text messages, you immediately spot it straightaway.* (Parent of female pupil, Year 9, Received one text)

To what extent did parents/carers pay attention to/engage with the intervention? Did parents/carers respond to receiving a text message(s), if so, how and why?

Most parents who responded to the survey reported taking action in response to receiving the intervention. In the endline parent/carer survey, 51% (N=195) of respondents in the intervention arm reported receiving a message from the school specifying the number of days of school that their child had missed. Of these respondents, 43% (N=84) said the message prompted them to have a conversation with their child, 29% (N=56) a conversation with their school, and 23% (N=45) monitored their child's attendance more closely. This data was collected using a multi-coded question asking parents to select all actions that they took, with 21% reporting they took no action at all.

Of the parents/carers interviewed, some took action after receiving a text message but most did not. Illness was the main cause for child absence among interviewed parents/carers, this in itself prompted parents/carers not to react to the text as the cause of absence was not within the parent's control. Some parents/carers tended not to act because they did not feel it was clear from the text message what action was required from them. Similarly, interviewed parents/carers who took a more holistic view of their child and considered their historic attendance as well as their current academic performance were less likely to take action after receiving the text message. For these parents/carers, the causal link between receiving a text message and improving understanding of the number of missed days, and the consequences of absences, was not apparent.

> *I didn't feel that I had to respond, I felt it was informative. The impression I got was this is them letting us know and keeping us informed about my child.* (Parent of female pupil, Year 7, FSM, Received two or more messages)

> *All of her grades already she's been predicted 8s and 9s and she's in [Y]ear 7. So, I don't really think being poorly for three days is going to impact her GCSEs.* (Parent of female pupil, Year 7, Received one message)

Nonetheless, interviewed parents/carers who did respond to the messages did so in three ways, using it as: a reminder; a prompt for conversations; and leverage for decision-making. Those parents/carers who perceived the message as informative occasionally used it as a reminder to pay closer attention to and keep track of their child's attendance going forward: this was sometimes in response to absences related to illness.

> *I think if you see it written down, it makes you more aware…it is a good reminder of how important attendance is, and if you see the figures written down, it'll prompt you to try make an improvement in the following month*. (Parent of female pupil, Year 10, Received one message)

Other interviewed parents/carers used the message as a prompt to start conversations about attendance with their child, within which they could reiterate the importance of attendance and inform their child about the communications they had received from the school. This was often when absences related to a wider variety of reasons, including illness, mental health, and SEND.

> *The text is a handy little thing to have in your back pocket to show the figures. I suppose what it does is that it prompts the conversation with your child to say: 'This is the text I've had today'.* (Parent of male pupil, Year 7, FSM, Received two or more messages)

In some cases, parents/carers perceived the messages as evidence they could use from school to encourage their child to attend. Leverage was rarely used but sometimes employed when children wanted to stay at home but did not have a clear cause for absence.

> *They [the child] will challenge and say why do I have to go to school…but now you've got this evidence that they [the school] are going to flag it up.* (Parent of female pupil, Year 9, FSM, Received two or more messages)

What did parents/carers think of the text message?

Results from the endline parent/carer survey suggest most parents/carers either had a positive or neutral view on the intervention messages. In the parent/carer endline survey, 61% (N=477) of respondents thought their school should continue using the intervention, 28% (N=222) did not mind either way, and only 11% (N=82) thought schools should not continue the intervention. Interviewed parents/carers also had mixed perceptions of the text message: this was frequently influenced by the circumstances and level of complexity surrounding their child's absence, and the parent's perception of their child's attendance record.

Some parents/carers, whose children had largely been absent due to illness viewed the text message either neutrally or favourably and welcomed the text message as a reminder to keep track of their child's attendance. These parents/carers perceived the text message as a means of informing them rather than attributing blame for their child's absence.

> *I think it's quite nice to know that the school is keeping track of attendance, and as a parent we can do a bit more next time.* (Parent of male pupil, Year 7, Received one text)

However, many interviewed parents/carers perceived the text messages negatively, particularly when the reasons behind their child's absence were perceived as more complex, for example, due to family bereavement, SEND, or long-term physical and mental health issues (including bullying). Schools had the option of excluding pupils for these reasons; school discretion in who and why not to send text messages was part of the intervention design. Instructions provided to schools stated: 'You can choose not to send the message to any parents, where you do not feel it would be appropriate to do so. For example, if a pupil has been seriously unwell, or suffered a family bereavement'. Sometimes parents/carers also reacted negatively if their child's absence was a one-off episode of illness that was out of their control, for example, chicken pox, particularly if they perceived their child as having consistently good attendance otherwise. In some cases, absence authorisation influenced parental responses, for instance in situations where an absence had been authorised by the school the parents/carers felt surprised to have received the text message, leading some parents/carers to contact the school to confirm the authorisation had been logged.

> *She's had very particular health issues over the last year and the school are very well aware of that, yet they choose to send this message out which is not particularly great…why do they continually send me messages about attendance, when they know the reason behind it is medical?* (Parent of female pupil, Year 8, Received two or more messages)

> *I was very annoyed, I found it deeply patronising and it's utterly ridiculous in my opinion…it's made me look like a bad parent because my child has had 6 days off school in about 10 years…it's putting me in the bag of people who don't care whether their kids go to school or not.* (Parent of male pupil, Year 9, Received two messages)

Some interviewed parents/carers felt distressed to have received the text message and perceived its tone as accusatory, blaming them for their child's absence and implying they were indifferent toward their child's attendance. These parents/carers also felt the message suggested that they were intentionally keeping their children off school, rather than the absence being a one-off necessity.

> *My initial reaction is a negative one reading that, because I'm doing the best I can and now you're picking me up when I'm genuinely fighting every day to put food on the table, get the kids to school.* (Parent of a female pupil, Year 9, FSM, Received two or more messages)

> *When the school send a message like this, they have no idea what's going on in our lives and it seems like they don't care either.* (Parent of a male pupil, Year 10, Received one message)

Interviewed parents/carers tended to feel the general tone of the message aligned with its informative nature. Some found the phrase 'fresh start' uplifting, whereas other parents/carers felt it lacked sincerity or was condescending. There were no clear trends in the type of absence and parental responses to the text message. Some single parents/carers, and parents/carers who felt very involved with their child's attendance, perceived the text message as reductive, as they believed it failed to acknowledge their efforts in trying to secure good attendance for their child. Others (including single parents/carers) welcomed the text message and felt it demonstrated the school cared about their child's attendance and future: they appreciated this extra involvement and support.

> *I think it's a really good idea. I personally found it very useful to be on top of [his] attendance at school and being aware if actually he is not where he's supposed to be.* (Parent of male pupil, Year 7, Received one message)

> *The second half of the text, it's just so patronising. It's like, 'Oh, should we have a fresh start?' That isn't helpful, I'm a grown adult, I know that he needs to be in education.* (Parent of male pupil, Year 8, Received one message)

The personalised elements of the text message, namely, the inclusion of the parents'/carers' and children's names and the number of days absent, were perceived as useful by most interviewed parents/carers, particularly those with multiple children at the same school (which helped parents/carers understand which child the message related to). As discussed above, the inclusion of the parents'/carers' name was inconsistently applied across the interviewed schools: for some parents/carers this lack of personalisation reduced the impact of the message, as they perceived the text messages as being sent out indiscriminately. Some parents/carers, however, perceived personalisation as tokenistic: they felt that if it were truly personalised it would have acknowledged the individual circumstances of their child's absence.

Parents/carers generally liked the inclusion of the number of days missed: they felt it was easier to quantify absence figures in this way, compared to the usual reporting of attendance through percentages.

> *It's more straightforward and easier to understand than trying to work out what the percentage means.* (Parent of female pupil, Year 10, Received one message)

However, a few parents/carers mentioned that it would have been helpful for the text message to include both the number of days missed and the corresponding attendance percentage, as well as an understanding of the number of days that equates to 95%. They believed that this would have provided a clearer indication of how their child was performing in relation to attendance benchmarks.

Perceived impact (parents/carers and pupils' perspectives)

Some parents/carers felt the intervention had a small, positive impact on their child's attendance. However, in many cases parents/carers felt that the intervention had no effect. Parent/carer perceptions of the intervention's effectiveness were impacted by the circumstances and context for their child's absence. A limitation of the Theory of Change was that it failed to consider the context in which an absence occurred and the impact of this on the causal mechanisms that improve attendance. Overall, interviewed pupils' reactions often mirrored the reactions of their parent/carer: pupils tended to be indifferent when their parents/carers were indifferent to the message; or frustrated when their parents/carers were frustrated. There were also no clear trends between the types of pupils or absences and the pupils' response or perceived impact of the message on attendance. The assumption, as set out in the Theory of Change, that receiving a text message would support pupils to develop positive beliefs about parent/carer interest in their attendance was only supported in some cases.

How did parents/carers perceive the impact of the intervention on attendance?

In some cases, parents/carers felt the intervention had a small but positive effect on their child's attendance by prompting them to be less lenient about allowing their child to stay home following the intervention.

> *There would have been another one day a term that she may not have gone in, had I not had the message, because I might have been a bit more lenient…I do think it [the text] does make a difference.* (Parent of female pupil, Year 9, FSM, Received two or more messages)

Similarly interviewed parents/carers who described broaching the topic of attendance proactively in conversations with their child to deter future absences (rather than reacting on the day to a possible absence occurrence) felt the text message positively impacted their child's attendance.

> *I took action with [my daughter] and said: 'This is what I've got', but it was never punitive. It was more of a fresh start conversation…I don't think we would've if I didn't get those prompts honestly because it would just be out of mind.* (Parent of female pupil, Year 7, Received two or more messages)

In cases where interviewed parents/carers perceived their child's absence as unavoidable, believing they had little control over the situation to affect the outcome, the message was not felt to affect their child's attendance. These were often cases in which the child had a one-off episode of acute illness or more complex absence circumstances such as bereavement or mental health (even though serious illness and bereavement were the two examples that BIT had given the school of when it might be appropriate not to send parents/carers a text message). In these situations, parents/carers were either frustrated to have received a text message because they perceived the absence was wholly unavoidable or upset because they were already struggling with their child's attendance and for them the text message felt inappropriate.

> *A text message is a text message. If my child's poorly, it doesn't matter how many text messages they send me…I don't think a text message is going to make much difference to that.* (Parent of female pupil, Year 7, FSM, Received two or more messages)

> *The text messages are a bit impersonal, and it wouldn't make me do anything different because I'm not choosing having my daughter not go in. We are having very strong battles every morning to get into school,*

so a message is not going to change anything other than how I feel, feeling guilty. (Parent of female pupil, Year 8, Received two or more messages)

How did pupils perceive the impact of the intervention on attendance?

Most interviewed pupils felt indifferent about receiving the text message. They were also not surprised their school had sent this type of text message: most of pupils were aware of attendance communications being sent to parents/carers. Pupils with neutral reactions stated that illness had caused their absence. In these cases, both the parents/carers and pupils were more often aware of the number of days absent and so the text messages were not perceived as providing new information. These pupils frequently felt the text message was irrelevant to their situation since they thought they could do little about being ill; the text messages were therefore, perceived as having minimal impact on preventing future absences.

It wasn't that shocking because I missed a week of school because I was sick. (Female pupil, Year 8, Received one message)

Similarly, these pupils tended to feel that the text messages should be further personalised to reflect the type of absence. Some viewed this lack of personalisation negatively and felt frustrated that the text message was being sent even if an absence was caused by acute illness. In this context, the text was frequently described by interviewed pupils as lacking a clear purpose and irrelevant to future attendance.

To me the [text] messages seem they are automatically generated, which I don't think it is great—I think they should be more personalised. They look like a template with placeholders. The school could describe better what is going on and say exactly what they want from you. (Male pupil, Year 10, FSM, Received two or more messages)

It was common for interviewed pupils to exclude themselves from the category of pupils for whom they perceived the text message was intended, for example, pupils who regularly avoid school, which led them to feel the text message would have little impact on their own attendance.

I was a bit annoyed with it because it was a bit unnecessary…it seems they think I'm skipping school or something, but it's not intentional. (Male pupil, Year 10, Received two messages)

In contrast, some interviewed pupils, who found the text helpful, felt it was a good way to keep parents/carers up to date with attendance. These pupils viewed the text message as a necessary and positive communication with parents/carers, especially for informing parents/carers of unauthorised absences: they felt the message could support parents'/carers' involvement in their child's attendance.

I feel like it's necessary to let my mum know [text message], I think it helps your parents understand if your kids are off or not. (Female pupil, Year 10, Received one message)

There was some indication that interviewed pupils' perceptions of the impact of the text on attendance was somewhat influenced by their perceptions of their school environment. Interviewed pupils who viewed pastoral care and the school environment positively tended to be slightly more receptive to the text message and feel motivated to change their behaviours.

It gives me a bit more of an understanding of how much of an effect it can have [text message], and probably I should go in a bit more…some days I'm just not feeling 100% but I think I'm able to go in, so I should make the effort. (Male pupil, Year 9, Received two or more messages)

Perceived impact (school perspective)

School perceptions of the impact of the intervention on parents/carers and pupils was mixed. Generally, the intervention was perceived as most effective for pupils whose attendance was just below the threshold and who have responsive parents/carers. The Theory of Change hypothesised that in the medium-term the interventions would be low cost and low effort to implement and save staff time. Interviews with schools suggest that schools did view the text messages as low cost to send. However, the text messages were only low effort to send in better-resourced schools, with access to more advanced messaging platforms. There was no evidence that the intervention freed up staff time to enable schools to focus on persistent absentees, nor did it improve clarity on attendance roles and responsibilities as assumed in the Theory of Change.

How did schools perceive the impact of the intervention on parents/carers?

According to the interviewed schools, there was minimal reaction from parents/carers to the text messages. Most schools reported only a minority of families reacting negatively to and complaining about receiving the text messages, although they acknowledged that this was similar to most school communications sent out. Across all schools, positive responses to the trial were also rare, with schools only recalling a handful of cases where parents/carers had reached out to the school to provide positive feedback.

> *It did upset some people when we sent a generic text like that, even though we obviously used their names —we would sometimes get messages back saying: 'Oh so can my child never be ill[?] But it was a very small percentage that even replied to them.* (Urban location, Large school, Small proportion of pupils eligible for FSM, Small proportion of pupils with EAL)

> *One of our parents emailed us back to say: 'This is much easier to understand than when you send percentages, because it is really clear that there's been 1/2 days of absence, which was really nice'.* (Urban location, Small school, Small proportion of pupils eligible for FSM, Medium proportion of pupils with EAL)

The Theory of Change assumed that schools would perceive the text message as having a positive impact on parental behaviour. However most interviewed schools reported observing few, if any, changes in parental behaviour.

> *There are still pockets of families that don't think it's that big of a deal, when attendance is between 85 and 90 that sounds like a high number, so the text message doesn't have a huge impact…they will always be the ones that are hardest to budge.* (Urban location, Large school, Small proportion of pupils eligible for FSM, Small proportion of pupils with EAL)

Schools that had not previously sent out text messages believed that parents/carers were increasingly accepting of texts as a form of communication from schools.

How did schools perceive the impact of the intervention on pupils?

The schools' perceptions of the impact of the intervention on pupils' attendance was mixed. Schools tended to report noticing a slight improvement in attendance by the end of the trial. There was, however, a lack of clarity about the precise reasons for improved attendance. One or two schools attributed higher attendance rates to the text messages, although most schools felt improved attendance rates were due to the suite of support interventions being implemented by the school.

> *Because we haven't sent those normal [attendance] letters, you would expect our attendance to have dropped this academic year—but I think it's actually a bit higher than this time last academic year. This shows that in the absence of those letters, these text messages have had the same impact as those letters would have with parents.* (Urban location, Small school, Small proportion of pupils eligible for FSM, Medium proportion of pupils with EAL)

> *We try as a school to talk about attendance more all over the school and our attendance figures are improving, it's just hard to say what that's down to. I don't imagine it's any one particular thing. Every day we move further away from the COVID experience, the attendance improves. The mindset is shifting back.*

(Urban location, Large school, Small proportion of pupils eligible for FSM, Small proportion of pupils with EAL)

All schools interviewed felt that the utility of the text message depended on the circumstances surrounding the pupil absence. For example, in more complex circumstances, such as a significant bereavement or severe bullying, schools believed a text message would be inappropriate and could possibly undermine previous efforts to build trust between the school and the affected family. (This was reflected in the instructions provided to schools, which reminded them that they had discretion over which pupils were sent the text messages.) In these contexts, schools believed a face-to-face approach would be more supportive and effective and would be better for fostering collaboration between the school and parents/carers.

The text messages were perceived as having greater impact for pupils with attendance just below the threshold and among parents/carers who would be more responsive, compared to those with persistent absences. Schools indicated that if the text could be tailored to those pupils/families who the school perceived as being more responsive, text messages would better complement existing strategies.

> *I'd say it [the intervention] is not particularly helpful for those whose attendance drops very quickly, but it is very useful as a tool in our armoury for those whose attendance is just below what you want to be. I think the other strategies we've got in terms of getting families in, building relationships with children, incentivising them to come in, it's far more effective*. (Urban location, Large school, Small proportion of pupils eligible for FSM, Small proportion of pupils with EAL)

What did schools think about the intervention? How do schools perceive the impact of the intervention on their internal processes?

Schools generally felt the intervention supported their existing initiatives to increase attendance and perceived the text message as an additional, informal means of boosting attendance. Some schools, mainly those that tended to use emails or letters to communicate with parents/carers, welcomed the use of texts as a short-form and less formal communication style that offered the possibility of greater parental engagement.

> *It's friendly and it's casual and it's not punitive and I think that's what parents need from us as schools*. (Urban location, Small school, Small proportion of pupils eligible for FSM, Medium proportion of pupils with EAL)

Most interviewed schools felt that the intervention placed an administrative burden on staff, although the relative negative impact of this varied significantly. This was particularly the case for schools with limited capabilities for sending text messages, which led these schools to feel like the text message approach did not easily fit with their existing processes.

> *It wasn't difficult, but it's time-consuming to administer: adding the current salutation and then having to send them to individuals…it's an issue, unless you've got an automatic reporting system that your school buys into that allows you to do that*. (Urban location, Large school, Small proportion of pupils eligible for FSM, Small proportion of pupils with EAL)

Some schools felt that, while the initial administrative impact was high at the start of the trial, over time it lessened as attendance officers were able to build these responsibilities into their capacity and complete the process of sending out text messages more efficiently with practice. Schools that described themselves as 'well-resourced' were able to buffer the administrative impact across more staff members, sometimes enlisting administrative staff to support with the text messaging process. Other schools felt their limited capacity meant that the staff responsible for sending out text messages often had to forgo other responsibilities. For example, one attendance officer prioritised sending text messages over paper-based attendance letters to parents/carers, which was part of their business as usual approach. Furthermore, a few schools believed that the process was more time-consuming than the four to six hours suggested by BIT: the duration of time schools estimated spending on administrating and delivering the text messages ranged from three to four hours to up to three days, which was fitted around business as usual responsibilities.

> *We have an attendance officer who is able to do it as part of her role…with the bonus of having some admin staff who have some capacity to help. So, it doesn't have an impact on staff capacity*. (Urban location, Medium-sized school, Large proportion of pupils eligible for FSM, Large proportion of pupils with EAL)

> *Sending the message took a few hours of work, about half a day…when signing up we thought it would be less than that. I thought it would be simple, just that they give us a file and we send it.* (Urban location, Medium-sized school, Large proportion of pupils eligible for FSM, Large proportion of pupils with EAL)

Regardless, most interviewed schools felt the intervention was value for money and justified any financial costs associated with the administration and delivery of the text messages. Schools that had already invested in text message platforms believed the financial impact was minimal: these schools were able to implement the intervention easily because they were already familiar with sending messages for registration lates and school updates. One school that had to purchase the text messaging package in bulk felt the initial financial investment was justified by the school's intention to carry on sending text messages after the trial. Another school was able to minimise the financial impact by sending out text messages via the Edulink app rather than paying for text messages. This school reported this adaptation had enabled them to take part in the trial, which would have been too costly otherwise. A third school felt unsure whether the intervention had been value for money, as it was costly for them to buy and send the text messages; this school's Management Information System was more aligned to sending messages via email as a more cost-effective approach.

> *It is value for money because I don't have to sit and analyse that data, and think: 'how am I writing these messages, what phrasing am I using?'* (Urban location, Large school, Small proportion of pupils eligible for FSM, Small proportion of pupils with EAL)

> *It is really cost-effective if you are able to fit it into what systems you already have.* (Urban location, Large school, Small proportion of pupils eligible for FSM, Small proportion of pupils with EAL)

IPE evidence suggests there is appetite in some schools to continue using the intervention. In the endline school survey, most of those who responded (76%, N=32) said they would continue to implement some form of the intervention in the next academic year, 19% (N=8) were uncertain, and 5% (N=2) said their school would not continue to use the text messages (noting that there may be some response bias, in that schools who were more likely to complete the survey may have also been more likely to say they would continue implementing the intervention). Similarly, over half of schools interviewed stated they intended to continue with attendance messaging in the subsequent academic year, although most wanted to adapt the targeting, context, and frequency of the text messages.

> *We've put together, partly as a result of the trial, a suite of nudge messages. We already have several that go out weekly to parents through the MCAS [My Child At School] app…what I have ready to start, which I guess is more similar to the BITUP trial, is for bespoke dynamic groups…where trends will be spotted.* (Urban location, Large school, Large proportion of pupils eligible for FSM, Medium proportion of pupils with EAL)

By contrast, some interviewed schools did not feel they would continue to send text messages, either because they perceived the process as too time-consuming in relation to its perceived impact or because they did not have the capacity to continue sending them (which was especially the case for schools that had to manually adapt and send out text messages). Respondents in the endline school survey gave similar reasons for not continuing, or being uncertain about continuing, to send the intervention text messages after the trial ended.

> *Sending the messages is quite straightforward, but no one saw any benefit to it, it just felt like an extra admin task to do for not much impact.* (Urban location, Medium-sized school, Large proportion of pupils eligible for FSM, Large proportion of pupils with EAL)

Did parents/carers, pupils, and schools think the design and delivery of text messages could have been improved to have a bigger impact on attendance (tone, content, mode, frequency, reason for receiving it, timing)?

Interviewed schools were largely happy with the delivery of the messages: the process was felt to be smooth and most schools did not experience issues with it. One school found the translation function particularly helpful as they did not have the capabilities to offer this provision to families as usual practice. Other schools welcomed the structure of the process—it was clear to them when, how, and who they had to communicate with; these schools also felt the messages framed attendance well and in a positive way for parents/carers, making it clear and easy for them to understand. However, some felt the text messages would have had more impact if they were targeted rather than delivered as a blanket message to all eligible families. With the caveat that it is based on limited evidence, respondents thought that targeting text messages at families perceived to be more receptive to a text message, described by some schools as families whose children's attendance was just below the 95% threshold, would have a greater impact on attendance because positive changes to

attendance-related behaviours could be more easily achieved. Three survey respondents also mentioned negative parent/carer reactions to the text messages as a reason to discontinue sending them (e.g. backlash from parents/carers who thought the text messages were unprofessional and from parents/carers who were upset at receiving a text message when their child had been ill).

Some parents/carers perceived the text message as a helpful reminder and it made them feel like the school was trying to help improve their child's attendance: several parents/carers also welcomed the text message format viewing it as a quick and handy way to be communicated with rather than via email, which was seen as less accessible. There was a mixed response to the tone of the text messages with some parents finding it straightforward, positive, and encouraging whereas others found the tone to be patronising and accusative. The main criticism of the message from interviewed parents/carers who tended to respond negatively to the message was that it failed to acknowledge the reason for their child's absence. These parents/carers wanted the message to be further personalised and demonstrate empathy about their families' individual circumstances: this would indicate to them that the school understood their situation and cared.

> *I think it needs to appear individualised and compassionate as an invitation to explore the problem together. I think a message that conveys how we can work together and understand this together, etc.* (Parent of male pupil, Year 10, Received two or more messages)

> *I think there needs to be some reviewing, some refining, and I think there needs to be a greater consideration of the whole picture and the whole family relationship.* (Parent of male pupil, Year 10, Received two or more messages)

It felt important to some interviewed parents/carers that the text message should promote collaboration between the school and parent/carer to improve their child's attendance. They suggested the text message could be more supportive of the family situation and acknowledge the difficulties parents/carers can face with attendance.

> *Incorporating the fact that it's teamwork between the school and the family to get the child to school. It's not just all on the parent. I think there are probably parents who feel overwhelmed and can't get their kids to school…that [text message] just puts more pressure on them.* (Parent/carer of male pupil, Year 10, Received one message)

Some parents/carers recommended adding a direct telephone number and named contact to the text message to indicate the school's willingness to discuss attendance with the family. These parents/carers said that providing a direct contact, rather than the school's general number, would avoid them having to search for a telephone number and waste time trying to locate the right person to speak to. A few parents/carers also indicated they felt uncomfortable discussing sensitive issues with the staff in the school office. Parents/carers would want to talk with someone who understood their child and their attendance history, so potentially the child's form tutor, the pastoral lead, or attendance officer.

> *It's embarrassing to ring up the general busy school office…you'd be better off with a direct number for a form tutor who knows your child.* (Parent/carer of male pupil, Year 10, Received one message)

> *There should be either an attendance officer or safeguarding officer that you should be able to reach out to when you have concerns about these messages.* (Parent/carer of female pupil, Year 8. Received two or more messages)

As discussed above, a few parents/carers welcomed the inclusion of both the number of days missed and the corresponding attendance percentage and a breakdown of the number of days that equates to 95%. It was felt that this would improve parents'/carers' knowledge about their child's attendance against statutory benchmarks.

Case study 3: A good fit with existing school practices

Urban location, large school, large proportion of pupils eligible for FSM, and medium proportion of pupils with EAL

This school felt the BITUP intervention positively complemented both its existing interventions to improve attendance, which dropped to 86.6% following the COVID-19 pandemic, and its technical capabilities to support parent communication. The school had a Management Information System in place prior to the intervention, with the functionalities to enable swift personalised text message delivery to eligible parents. Strategically, the intervention also fitted well with the school's staged absence response process, which is supported by several communication practices. For example, the school sends a text or email to parents/carers whose child is unexpectedly absent, which is followed up with a home visit from the school's safeguarding officer if the parent/carer fails to respond. The school also sends: half-termly attendance reports, reported in percentages, to all parents/carers; attendance nudge text messages including positive reminders sent on Sundays to encourage attendance for the coming week; and a message towards the end of the Summer Term to deter parents/carers from taking term-time holidays. The BITUP intervention complemented these existing systems and prompted the school to consider targeted, rather than blanket, text messages to specific groups where trends could be identified such as persistent absences on Mondays or Fridays.

Case study 4: More intense support needed for school community

Urban location, medium school, large proportion of pupils eligible for FSM, and large proportion of pupils with EAL

This school felt the intervention was less suited to its family population and demographic. Staff described the community as disadvantaged, with high numbers of single parent/carer families and pupils with caring responsibilities leading to very low attendance. The school's strategy for pupil absence has become less focused on the number of days pupils are absent and now takes a more holistic approach to attendance, promoting the value of good attendance on education and pupil mental health and well-being, friendships, and relationships. Greater emphasis is placed on collaborative approaches and the school working with parents/carers rather than attempting to enforce attendance. In future, the school plans to deliver parent/carer workshops to build relationships with its community and understand the perspectives of its parents/carers. It perceived face-to-face communications and phone calls as being more effective and having greater impact on their community than letters and text messages.

The Theory of Change hypothesised that the intervention would impact pupil attendance via the parent/carer by improving parent/carer accuracy about the days of school their child had missed and improving parents'/carers' perceived importance of attendance and the consequences of absence. Most parents/carers agreed that attendance is important, but few knew the exact number of school days their child had missed over the year. However, in most cases parents/carers felt the intervention did not improve their understanding of how many days of school their child had missed, and there was no evidence to suggest that the interventions improved parent/carer beliefs about the importance or consequences of attendance. In cases where the text message did correct parents/carers who underestimated their child's attendance, the intervention appeared to have prompted action from interviewed parents/carers, which to some extent positively affected their child's attendance. Whether parents/carers felt that they had control over their child's attendance varied depending on the reason for the absence, their child's age, and their child's emotional and physical health. We also find no support for the role of the pupil as a mediator as set out by the Theory of Change, as pupils were not generally absent from school due to negative perceptions about education or low perceived importance of attendance.

Mediators

How did the intervention impact pupil absence via the parent/carer through the mechanisms set out in the Theory of Change?

The Theory of Change sets out how the intervention's effect on pupil attendance is mediated by parents/carers. It hypothesises that if a parent/carer understands the consequences of absenteeism and has more accurate knowledge of

their child's absence record, they will have increased agency to monitor and support their child's attendance more carefully and engage with their child about the importance of going to school each day.

The parent/carer endline survey (sent only to parents/carers whose child had become eligible for the intervention at least once during the trial period at the end of the school year) found no evidence that parents/carers in the intervention arm had more accurate knowledge of their child's days of school missed than parents/carers in the control arm. In the parent endline survey we asked respondents how many days of school they thought their child had missed since the beginning of the year. In total, 575 (74%) respondents, 288 in the control group and 287 in the treatment group provided a response, and 206 (26%) said 'I don't know'. The median response was six days of school missed; this was the same for those in the control and intervention arms. We found that:

- 12% (N=69) of respondents who gave a response **knew the exact number of days** their child had been absent (10% N=29 control, 14% N=40 intervention). When we relax this criteria, 39% (N=222) were correct within one day (38% N=108 control, 40% N=114 intervention), and 55% (N=318) were correct within two days (55% N=158 control, 56% N=160 intervention).
- 54% (N=311) of respondents who gave a response **underestimated the days of school their child had missed** (54% N=154 control, 55% N=157 intervention). The median difference between actual days missed and parent/carer responses was three days in the control arm, and two and a half days in the intervention arm.
- 34% (N=195) of respondents who gave a **response overestimated the days of school missed** (37% N=105 control, 31% N=90 intervention). The median difference between actual days missed and parent/carer responses was one and a half days in the control arm, and two and a half days in the intervention arm.

Therefore, we find no evidence that parents/carers in the treatment group had more accurate knowledge of how many days of school their child had missed than those in the control group. While more than half of respondents who provided an answer underestimated number of days their child missed, most participants were correct within two days. These results should be interpreted with caution: it is also possible that participants underreported the days of school their child actually missed to be viewed more favourably by the research team.

Parent/carer interviews suggested a more nuanced picture of how parents/carers influence their child's attendance than set out in the Theory of Change. While most parents/carers interviewed remembered receiving a text message, only some felt that the presentation of the number of days their child was absent in the previous half-term was helpful in cementing their knowledge of the absence, as many were acutely aware of the number of days their child had not attended. The interviewed parents/carers who reported feeling that the intervention improved the awareness of their child's absences and their understanding of the consequences of missed days perceived the text message as having a positive impact on their child's attendance.

> *There would have been another one day a term that she may not have gone in, had I not had the message, because I might have been a bit more lenient…I do think it [the text] does make a difference.* (Parent of female pupil, Year 9, FSM, Received two or more messages)

There was no noticeable impact on parental beliefs and attitudes towards attendance during the interviews with parents/carers. Within the endline interviews, most interviewed parents/carers expressed strong support for education and reported recognising the importance of attendance. However, it is possible that participants overstated their beliefs about the importance of attendance due to social desirability concerns.

> *It would have been the same, regardless if we got the messages or not. We know it's important that he goes to school, whether I had the text messages or not we'd still be encouraging him to go to school.* (Parent of male pupil, Year 7, Received two or more messages)

The results of the endline parent/carer survey also found that parents/carers placed a high value on daily attendance, regardless of whether they were in treatment or control arm families. However, again it is possible that respondents overstated their beliefs about the importance of attendance due to social desirability concerns:

- 92% (N=718) of respondents agreed or strongly agreed that daily attendance is important for their child to succeed in school (92% N=365 control, 92% N=353 intervention).
- 67% (N=527) of respondents agreed or strongly agreed that every additional day of school missed has a big effect on their child's grades (67% N=265 control, 68% N=262 intervention).

Based on these results, we find no evidence that the text message improved parent/carer attitudes towards the importance of attendance, as was hypothesised in the Theory of Change. However, we note that parents/carers who responded to the endline survey and participated in the interviews may also be those who already placed high value on the importance of school attendance.

Although most interviewed parents/carers acknowledged that they had significant responsibility for their child's attendance, perceptions of their agency to influence their child's attendance varied. Parents/carers who felt they had greater agency to improve attendance tended to be those with younger children and children who were more academically motivated or compliant. Some of these parents/carers believed that they could adopt a firmer attitude to ensuring their child goes to school unless they were severely unwell.

> *I think after the text message I was even more like 'No you need to get up and go to school, like I'm not even debating it'.* Whereas before, there might have been a discussion about his health. (Parent of male pupil, Year 7, Received one message)

> *On Sports Day for example, he was begging me to let him have the day off but I told him, 'No, you have to go'.* (Parent of male pupil, Year 8, Received one message)

Other parents/carers felt they had little control over one-off episodes of unexpected illness and in more complex absence circumstances, for example, where children were awaiting SEND assessments, facing mental health challenges, or experiencing long-term health conditions, or bereavement. The intervention was not specifically designed to address complex absence, and instructions provided to schools reminded them that they had discretion over which pupils were sent the text messages. Schools were also specifically prompted to consider cases of serious illness or bereavement. Nearly all interviewed schools described an internal process, sometimes involving a number of school staff, for removing families from the distribution list in specific cases where the school was aware of and had authorised absences. These included ongoing medical issues, recent bereavements, social care orders, elite performers, military service families, Year 11 pupils, bullying, and emotional-based school avoidance. These were, however, not reported consistently across schools and were viewed on a case-by-case basis: some schools reported challenges with this approach, such as the school requiring a general practitioner (GP) note to authorise absences based on medical grounds, which parents do not always provide and frequently assume a phone call is adequate evidence. On reflection, a few schools felt they should have reviewed their selection/deselection criteria more closely and anticipated the impact of sending text messages to some families. While most schools appreciated that they had discretion over the selection/exclusion process and felt they were best placed to make this decision, two schools described how more specific guidance from the BIT team would have been helpful and could have made decision-making clearer for schools to ensure text messages were targeted correctly.

Parents/carers with older children also felt they had less agency over their child's attendance. For some parents/carers with children on neurodivergent pathways, or children with long-term health conditions, striking a balance between upholding attendance and tending to their physical and emotional health felt difficult.

> *My control over her attendance is slipping. I used to take her to school and now I have to put faith and trust into her that she's making it on time every day…there's not much more I can do, she's a young adult.* (Parent of female pupil, Year 8, Received one message)

> *While I recognise the school is looking at this issue through the lens of attendance for educational gain, it is beyond me that there seems no consideration of health needs in this equation.* (Parent of male pupil, Year 10, Received two or more messages)

Results from the endline parent/carer survey support the finding that most parents/carers acknowledged responsibility for their child's attendance, but fewer felt that they had control over whether their child attended school or not, to some extent:

- 96% (N=746) of respondents felt very or extremely responsible for ensuring their child attends school (96% N=380 control, 95% N=366 intervention).
- 88% (N=691) felt that they had moderate to complete control over whether their child attends school (89% N=351 control, 88% N=340 intervention).

How did the intervention impact pupil absence via the pupil through the mechanisms set out in the Theory of Change?

The Theory of Change also sets out the role of the pupil as a mediator of their attendance: it assumes that if a parent/carer speaks to their child about the importance of attendance, the pupil will develop positive beliefs about the value of

attendance and, as a result, will factor this into daily attendance decision-making. Once again this was not borne out fully from the evidence of the interviews with pupils: the text message may have prompted some parents to speak to their children, but interviewees felt that no further changes in behaviour or changes in belief occurred.

Interviewed pupils, whose parents/carers received at least one attendance text message, reported that their parents/carers were taking action to support their attendance. However, most pupils felt their parents'/carers' behaviour had not changed because of the text message(s). The text message may have prompted a conversation, but it did not lead to further changes in behaviour. Most pupils felt their parents/carers were already interested in their attendance and took steps, albeit in varying degrees, to encourage them to go to school.

Nearly all pupils said their parents/carers had conversations with them about their attendance; the tone of these conversations was more commonly described as positive rather than punitive. A few pupils mentioned their parents/carers being firm in tone. Conversations between parents/carers and children tended to focus on the importance of school and education, for example, getting good grades and finding a good job. Telling children it would be harder for them to catch up on their work if they missed school was also mentioned.

> *[The conversation is] positive because she [mum] encourages me to go to school so I can then get good grades, a good job and be rich.* (Female pupil, Year 9, Received one message)

> *My mum was just like knuckle down, and if you feel a bit ill, just go in anyway and see…she says it's good for me in the long-term.* (Female pupil, Year 10, Received one message)

Pupils appeared to appreciate these conversations, acknowledging that their parents/carers were supportive and wanted them to make the best of their time at school and have a good future.

> *My mum wants me to have a good education, good friends…she cares about my mental health, my education and who my friends are.* (Female pupil, Year 7, FSM, Received two or more messages)

Some pupils described parents/carers supporting them to establish healthy bedtime and morning routines to ensure they got sufficient rest and could wake up on time; however, as mentioned above, this type of support was not necessarily the result of the text message. Other actions included parents/carers getting their children's school uniform ready and driving them to school. Pupils felt this support helped their attendance and punctuality.

> *It's [support from mum] made me feel I'm in more of like a routine and I've more structure to my day.* (Female pupil, Year 9, Received one message)

> *It's [support from mum] definitely been a big help for me because I probably would have really bad attendance if she didn't help me in the morning.* (Female pupil, Year 10, Received two or more messages)

Most pupils felt the message did not have a significant impact on them or their parents/carers; however, the message did prompt some pupils to reflect on their attendance and, when feeling ill, consider whether they were well enough to attend or remain in school.

> *It hasn't really changed my view, like it didn't really affect me. I'm just going in as much as I can, and I go off school when I really have to.* (Male pupil, Year 10, Received two or messages)

> *If it's only a couple days, a lot of people won't really think about it. But, if it was a higher amount of days, I think it would make you reflect on how much you're missing.* (Female pupil, Year 9, Received one message)

> *It gives me a bit more of an understanding of how much of an effect it can have, and probably I should go in a bit more…some days I'm just not feeling 100% but [if] I think I'm able to go in, so I should make the effort.* (Male pupil, Year 9, Received two or more messages)

Most pupils interviewed stated they were positive about education and saw the value of attendance for their academic attainment. This was less prominent for those in Year 7 and Year 8, who felt absences were less relevant at their age, although acknowledged that absences would be more significant in the pre-GCSE years. Pupils also reported a range of factors that motivated them to attend school, such as wanting to see friends, feeling confident at school, and not wanting to miss out on peer group experiences.

In this context, pupils did not report generally being absent from school due to negative perceptions about education. As was the case for parents/carers, they tended to feel that their absences were out of their control, which was especially the case for those who had experienced unexpected events, such as one-off illness, injury, or bereavement. In more complex situations, pupils described experiencing barriers to attendance, such as bullying or friendship issues at school, perceptions of the school environment as strict or hostile, anxiety about attendance or long-term health conditions that made more regular attendance difficult to secure for them.

> *I've been trying to get to school a bit more but sometimes it is difficult…the rules are strict and they make me want to go less than I already do. There are situations at school that are not really fair and some teachers are rude.* (Female pupil, Year 8, Received two or more messages)

> *If there's a change in plan or routine—I do like a really strict routine in the morning. If something goes wrong, it really stresses me out and I could be upset for like an hour and then I would get really exhausted.* (Female pupil, Year 7, Received two or more messages)

Context/moderators

There were no clear trends to suggest that the effectiveness the intervention varied based on demographic factors of interviewed parents/carers. However, interviewed parents/carers and pupils with positive perceptions of the school environment were more receptive to the intervention text message than those with negative perceptions. Contextual factors about the reason for absences also potentially moderated the effectiveness of the intervention. In particular, when absences were complex (e.g. caused by bereavement, medical conditions, anxiety, autism, and SEND) the intervention was not perceived as effective by parents/carers and schools. In fact, schools felt that sending the intervention to parents/carers when the absences were the result of these complex factors could have undermined the impact of other attendance management interventions. The Theory of Change did not account for the role of absence context on parent/carer and pupil ability to improve attendance.

What factors influenced the impact of the intervention on parents/carers and pupils (i.e. who did it work for and under what circumstances)? What role did parents'/carers' perceptions of the importance of attendance (e.g. on educational outcomes), their perceptions of the school and the importance of education, and their behaviours relating to their child's attendance, and other contextual and demographic factors play?

The intervention broadly appeared to have been more effective for interviewed parents/carers who felt they had a greater sense of control over their child's attendance and who had children with a greater predisposition to pleasing their parent/carer or wanting to do well at school. The text message encouraged these parents/carers to have more regular conversations about attendance with the child and for some to be firmer in their decision-making than before. In this context, the causal mechanisms set out in the Theory of Change were borne out in practice.

These causal mechanisms were less apparent, however, within the absence context of bereavement, long-term medical conditions (e.g. missing school to attend doctor's appointments, operations, and allergy tests), anxiety, Autism Spectrum Disorder, and SEND. Instructions provided to schools reminded them that they had discretion over which pupils were sent the text messages and specifically prompted them to consider cases of serious illness or bereavement. However, in some cases interviewed parents reported receiving text messages after their child had missed school for these reasons. Many interviewed parents/carers of pupils with these types of absences felt they had less control over their child's attendance and felt it was very challenging and in some circumstances upsetting to try to balance the emotional needs of their child with consistent attendance. In these cases, parents/carers tended to be distressed or frustrated by the text messages they received because they already felt concerned about their child's attendance and the text messages compounded feelings of disempowerment and helplessness to address attendance-related behaviours. Parents/carers of children who were absent due to genuine one-off episodes of illness, such as chicken pox, vomiting, or high temperatures also felt no agency over their child's attendance. The Theory of Change did account for the contextual factors of unavoidable and complex absences under outputs and theorised the intervention would not have an impact in these cases. As such, schools were instructed to use their discretion to not message these families and relied on school discretion and following guidance around this. However, some schools did not apply this discretion.

One further factor that influenced the impact of the intervention on interviewed parents/carers and pupils was their perceptions of their school environment and school efforts to work collaboratively to empathise with their circumstances and support their attendance. Interviewed pupils who viewed their school environment positively, or enjoyed learning, felt more motivated to attend, compared to those who perceived the school environment as hostile, strict, or overbearing. Pupils with a positive perception of school were more receptive to the text message and more likely to adopt positive attendance-related behaviours. The availability of pastoral care at school, and the accommodation of the pupils' needs by the school, appeared to enhance pupils' predisposition to attend school. Similarly, parents/carers who viewed their child's school unfavourably were less concerned about their child being absent from school.

Parents'/carers' perceptions of the importance of attendance and education did not appear to affect the impact of the intervention on parents/carers and pupils. As discussed above, most interviewed parents/carers expressed strong support for education and recognised the importance of attendance. Many pupils also confirmed their parents'/carers' commitment to their attendance: parents/carers initiated regular conversations; reinforced their beliefs about the importance of education; and took action to support their child's attendance and punctuality.

There were no clear trends in demographic factors playing a role to affect the impact of the intervention on interviewed parents/carers. There were no particular differences identified by ethnicity, however, a limitation of the interviews with parents/carers and pupils was that only five families from minority ethnic backgrounds were recruited. There were also no particular differences in impact identified between FSM and non-FSM eligible pupils.

Cost

Time costs

Schools were asked in a short endline survey about the number of additional hours it took to implement and deliver the BITUP text message intervention. Schools delivered the intervention using a wide variety of staff types (or 'roles') and there were no consistent patterns of the combination of roles, which were involved in delivery. Therefore, we report the average total amount of staff time used and the average number of roles this was split between, followed by the average number of time spent by staff in each role.

The time costs reported for year one (Table 38) reflect how the BITUP intervention was implemented during the trial, with the delivery team providing schools with the list of eligible pupils and a template for the text message. Preparation time costs for the schools included the time they spent assessing text message eligibility and amending text message content. Schools were also given training by the delivery team, which is a 'start-up' activity, and thus, would not need to be repeated in subsequent years of the programme.

The time costs presented for the following years (Table 39) reflect the full responsibility of the schools preparing and delivering the intervention, without the delivery team's direct support. This includes all preparation activities (such as assessing pupil eligibility and creation of message content) and delivery activities (such as sending the text messages and responding to parents/carers).

Table 38: Total time in year one devoted to training, as well as for preparation and delivery

	Average (mode) no. of roles used across schools (min–max)	Mean no. of hours per school, across all roles (min–max)	Role	Time needed in year one			
				Mean no. of hours	SD	Range (min–max)	No. of schools
Preparation	1 role (1–4)	32.74 hours (2.5–250)	Headteacher	5	0	0 (5–5)	1
			Assistant or deputy headteacher	13.11	9.91	36.50 (2–37.50)	26
			Teachers in welfare positions	10	12.75	37.50 (2.5–40)	8
			Middle leadership or classroom teachers	5	0	0 (5–5)	2
			School administrator	24.55	33.85	92.50 (2.5–95)	11
			Attendance officer	21.15	43.11	224 (1–225)	27
Delivery	1 role (1–4)	41.85 hours (5–200)	Assistant or deputy headteacher	19.70	25.28	107.50 (2.5–110)	20
			Teachers in welfare positions	5	7.16	17.50 (2.5–20)	5
			School administrator	29.82	27.62	85 (5–90)	14
			Attendance officer	32.52	40.48	149.50 (0.5–150)	24
Training	1 role (1–4)	19.24 hours (2–135)	Assistant or deputy headteacher	11.04	14.44	72.50 (2.5–75)	24

			Teachers in welfare positions	6.25	4.33	7.50 (2.50–10)	4
			School administrator	13.41	27.60	99 (1–100)	12
			Attendance officer	12.43	26.50	124 (1–125)	21

Table 39: Total time in future years devoted to training, as well as for preparation and delivery

	Average (mode) no. of roles used across schools (min–max)	Mean no. of hours per school, across all roles (min–max)	Role	Time needed in each future year			
				Mean no. of hours	SD	Range (min–max)	No. of schools
Preparation	2 roles (1–5)	60.65 hours (10–300)	Headteacher	15	0	0 (15–15)	1
			Assistant or deputy headteacher	19.71	19.92	92.50 (2.5–95)	26
			Teachers in welfare positions	11.18	14.28	57.50 (2.5–60)	17
			Middle leadership or classroom teachers	10.31	7.13	17.50 (2.5–20)	8
			SEND coordinator role	6.50	3.35	7.50 (2.5–32.5)	5
			School administrator	21.36	12.81	37.50 (2.5–40)	11
			Attendance officer	35.08	45.69	247.50 (2.5–250)	37
Delivery	1 role (1–4)	41.85 hours (5–200)	Assistant or deputy headteacher	19.70	25.28	107.50 (2.5–110)	20
			Teachers in welfare positions	5	7.16	17.50 (2.5–20)	5
			School administrator	29.82	27.62	85 (5–90)	14
			Attendance officer	32.52	40.48	149.50 (0.5–150)	24
Training	1 role (1–4)	19.24 hours (2–135)	Assistant or deputy headteacher	11.04	14.44	72.50 (2.5–75)	24
			Teachers in welfare positions	6.25	4.33	7.50 (2.50–10)	4
			School administrator	13.41	27.60	99 (1–100)	12
			Attendance officer	12.43	26.50	124 (1–125)	21

Financial costs

Table 40 provides the financial costs associated with implementing and delivering the BITUP intervention.

The BIT, as the developer, provided an overall cost estimate related to their involvement in the BITUP intervention. This total cost encompassed several key components:

- **Assessment of pupil eligibility.** This includes the financial cost of determining which pupils were eligible to receive a BITUP text message. In the trial, BIT was responsible for this task.
- **Briefing schools on the intervention.** BIT also provided information to schools on how to implement the intervention.
- **Troubleshooting and communication with schools.** BIT incurred costs for troubleshooting and maintaining communication with schools throughout the intervention.
- **Wonde license fee.** BIT incurred financial costs for a Wonde license fee, which was used to access information on pupils such as pupil's attendance from school's Management Information Systems. This information was used to personalise the BITUP text messages.

The costs of briefing, troubleshooting, and communicating with schools were considered start-up costs. In considering the costs of the intervention as implemented within the trial, the cost of assessing pupils' eligibility to receive BITUP text messages and the Wonde license fee were considered recurring costs. However, for future implementations of the BITUP intervention, it is assumed that schools would take on responsibility for assessing pupil eligibility, and eliminating the need for BIT's involvement in this task. As such, when calculating the total cost per pupil per year, we have produced an alternative costing model without the costs of BIT's involvement.

Set-up of a texting platform and Management Information System by schools was considered a pre-requisite. Among cost data received at baseline from 90 schools, 56.7% reported currently using an automated text messaging service to communicate directly with parents/carers and guardians. To facilitate the intervention, schools were required to have a Management Information System to record and track attendance and a texting platform, which the BITUP text messages could be sent. Across the 90 schools providing baseline cost data, 93.3% of these schools reported having an existing Management Information System, with 4.4% of schools unsure if their school had one. Only 2.2% of schools reported definitively not having a Management Information System. It should be noted that while schools having a Management Information System was a pre-requisite of the BITUP intervention and evaluation, if the BITUP intervention was delivered without the BIT's support, a Management Information System would not necessarily be required by the school, as schools would only need a way of tracking and identifying pupils' attendance.

Recurring costs were reported by schools at baseline and endline. To send text messages using a text message platform, schools were required to have a license fee, the average cost (median) of which was reported as £40 per year. Schools at endline also reported that text messages cost 6p on average (median) to send.

The cost per pupil per year calculation uses an estimate of the number of pupils who would benefit from the intervention. From data collected through monitoring forms, across the 105 schools 36,037 Years 7–11 pupils in the treatment group became eligible over the school year, averaging as approximately 343 pupils per school. Of those who became eligible, most were eligible for more than one term. Based on this, the total number of text messages that were eligible to be sent was 100,543. As schools who decide whether to take up the BITUP intervention would need to account for the costs of sending a text message to all eligible pupils (including those in the control group), we also report below the costs for schools if they send text messages to all pupils who become eligible.

Table 40: Costs of the implementation of the programme adjusted for inflation,[32] per ingredient

Category	Cost ingredient	Start-up or recurring	Mean quantity required	Price per unit required	Mean cost	Mean quantity required	Price per unit required	Mean cost	Mean quantity required	Price per unit required	Mean cost
			2023–2024			2024–2025			2025–2026		
Personnel for preparation and delivery	BIT personnel cost assessing text eligibility	Recurring	1	£133.28 per school	£133.28	1	£136.61 per school	£136.61	1	£140.03 per school	£140.03
Personnel for training	BIT personnel cost briefing schools on intervention (delivered by BIT)	Start-up	1	£133.28 per school	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Facilities, equipment, and materials	License fee for school texting platform	Recurring	1	£40 per year	£40.00	1	£41 per year	£41.00	1	£42.03 per year	£42.03
	Cost of text messages – Treatment group	Recurring	958 text messages	£0.06	£57.48	958 text messages	£0.06	£58.92	958 text messages	£0.06	£60.39
	Cost of text messages – Treatment and control groups	Recurring	1,890 text messages	6p per text message	£113.42	1,890 text messages	£0.06	£116.26	1,890 text messages	£0.06	£119.17
	Management Information System to record and track attendance	Pre-requisite	1	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
	Wonde platform fee for BIT to access attendance data	Recurring	11 (months)	£5	£55	11 (months)	£5.13	£56.38	11 (months)	£5.25	£57.78
Other costs	BIT personnel cost associated with troubleshooting and communicating with schools	Start-up	BITUP cost	£137.20 per school	N/A	N/A	N/A	N/A	N/A	N/A	N/A

N/A, not applicable.

[32] Prices provided for 2024–2025 and 2025–2026 have been adjusted to account for inflation. Both years have been adjusted by 2.5% from the previous year, which was the inflation rate according to the Bank of England as of 4 February 2025. https://www.bankofengland.co.uk/monetary-policy/inflation

To report the cost per pupil per year, two main costing models were produced: i) the costs of the intervention over three years with BIT's involvement in assessing pupil eligibility, which reflects how the intervention was implemented within the BITUP trial (Table 41); and ii) the costs of the intervention over three years assuming that BIT would have no involvement, and that the intervention would instead be delivered entirely by schools (Table 42). In this second costing model, schools would take on the responsibility for assessing pupil eligibility and managing the intervention.

The cost to schools of delivering the BITUP intervention with BIT's involvement over a three-year period was £1.11. per pupil per year, as shown in Table 41 (reducing to £0.56 per pupil per year if schools send text messages to all pupils, not just the treatment group). Over three years, the cost per school would be £1,145.43—an average cost per year of £381.81. Without BIT's involvement, the cost per pupil per year decreased to £0.29, as shown in Table 42 (reducing to £0.23 if text messaging all pupils). Although the cost without BIT's involvement is lower, it is important to note that schools felt their staff would need more time in future years to prepare materials for the intervention compared to year one of the intervention when BIT assisted schools with preparation tasks, such as providing a list of eligible pupils and a template for the text message. The increased preparation time needed by schools is presented in Table 39.

Table 41: Recurring costs of the implementation of the programme adjusted for inflation,[33] per ingredient with BIT costs (main costing model)

Category	Cost ingredient	Start-up or recurring	£ Year			Total
			2023–2024	2024–2025	2025–2026	
Preparation and delivery	BIT cost assessing text eligibility	Recurring	£133.28	£136.61	£140.03	£409.92
Training	BIT cost of briefing schools on intervention	Start-up	£133.28	N/A	N/A	£133.28
Other costs	BIT cost associated with troubleshooting and communicating with schools	Start-up	£133.28	N/A	N/A	£133.28
Facilities, equipment, and materials	License fee for texting platform (per year)	Recurring	£40.00	£41.00	£42.03	£123.03
	Cost of text messages (treatment only)	Recurring	£57.45	£58.92	£60.39	£176.76
	Wonde platform fee for BIT to access attendance data	Recurring	£55.00	£56.38	£57.78	£169.16
Cost per school per year			£552.30	£292.91	£300.22	£1,145.43
No. of pupils per school per year[34]			343	343	343	1,029
Total cost per pupil per school year						£1.11

[33] Prices provided for 2024–2025 and 2025–2026 have been adjusted to account for inflation. Both years have been adjusted by 2.5% from the previous year, which was the inflation rate according to the Bank of England as of 4 February 2025. https://www.bankofengland.co.uk/monetary-policy/inflation

[34] This is an estimate of the number of pupils eligible by year, based on the total number of eligible pupils in our analysis (71,916) divided by the number of schools in the trial (105).

Table 42: Recurring costs of the implementation of the programme adjusted for inflation,[35] per ingredient without BIT costs (main costing model)

Category	Cost ingredient	Start-up or recurring	£ Year			Total
			2023–2024	2024–2025	2025–2026	
Facilities, equipment, and materials	License fee for texting platform (per year)	Recurring	£40.00	£41.00	£42.03	£123.03
	Cost of text messages (treatment only)	Recurring	£57.45	£58.92	£60.39	£176.76
Cost per school per year			£97.45	£99.92	£102.42	£299. 79
No. of pupils per school per year			343	343	343	1,029
Total cost per pupil per school year						£0.29

[35] Prices provided for 2024–2025 and 2025–2026 have been adjusted to account for inflation. Both years have been adjusted by 2.5% from the previous year, which was the inflation rate according to the Bank of England on 4 February 2025. https://www.bankofengland.co.uk/monetary-policy/inflation

Conclusion

Table 43: Key conclusions

Key conclusions	
1.	Eligible pupils (<95% attendance) in the intervention group were on average absent for 0.21 fewer days in total across five terms (Terms 2–6), compared to eligible pupils (<95% attendance) in the control group. This is a 0.10% percentage-point increase in overall attendance during Terms 2–6. This finding has a high security rating.
2.	Eligible pupils in the intervention group had on average 0.26 fewer days of authorised absences across five terms (Terms 2–6) compared to eligible pupils in the control group. The trial found no impact on unauthorised absence rates.
3.	Eligible FSM pupils, Year 8 pupils, and female pupils in the intervention group were on average absent for 0.51, 0.61, and 0.35 fewer days in total over five terms relative to their counterparts in the control group. These results have a lower security rating as they are based on a smaller number of pupils.
4.	There was no conclusive evidence that receiving a text message improved parent knowledge about their child's absence rate or changed their perceived importance of attendance.
5.	School staff showed support for the intervention, with many schools intending to continue using the intervention in the next academic year, albeit with some adaptations (e.g. targeting the text messages specifically at pupils whose attendance fell just below the 95% benchmark). Although schools generally saw the intervention as low cost, this was influenced by staff capacity and access to text message software.

Impact evaluation and IPE integration

Evidence to support the logic model

The findings of the BITUP evaluation partially support the Theory of Change, highlighting both strengths and areas for refinement. Regarding inputs and outputs, the intervention adhered to the design set out by BIT, with schools actively sending attendance-related text messages with the number of days a child had been absent to parents/carers whose child had an attendance rate of less than 95% in the previous term. However, there were variations in the implementation process, for example, schools used different communication platforms or Management Information Systems, and some sent messages in different formats (largely because of technological limitations of their platform). The results of the IPE suggest that the intervention worked best when school staff had the time to review eligible pupils each term to decide, which parents/carers should or should not be sent a message. This involved taking into consideration the context of the absence and previous parent/carer communications.

There was little to no support for the short-term school staff outcomes set out in the Theory of Change. While for some schools the administrative burden of sending out the text messages lessened over time, there is no evidence that the intervention systematically freed up staff time. In all schools, the intervention was sent out alongside and in addition to other attendance monitoring and support strategies and, in some cases where schools had limited staff capacity, those responsible for sending out text messages had to forgo other responsibilities to do so. Similarly, there was no evidence that the intervention further clarified attendance roles or responsibilities among staff. In most cases, the intervention was delivered by staff members with existing responsibility for attendance monitoring and management. There is more positive evidence that staff developed skills and became more proficient in sending personalised attendance text messages over the trial. The administrative burden of delivering the intervention did lessen over time as staff were able to build these responsibilities into their capacity and send out text messages more efficiently with practice. However, one of the biggest factors that determined how easy it was for schools to send the text messages out was the school's messaging platform: some platforms allowed staff to upload personalised data and send out text messages in bulk, others required staff to input names and days of absence manually for each pupil and send text messages individually.

The short-term outcomes for parents/carers and pupils were mixed, reflecting partial alignment with the Theory of Change. There was no noticeable impact on parental beliefs and attitudes. In both the intervention and control arms parents/carers felt attendance was important but few knew the exact number of days their child had been absent from school. Evidence from the IPE suggests that the text message did not improve parents'/carers' knowledge of their child's attendance record; and parents/carers already recognised the importance of school attendance. The existing high level of parental awareness casts doubts as to whether the intervention can increase parental engagement with their child's attendance any further than business as usual. However, some parents/carers spoke to their child in response to receiving the text message. It seems that the text message acted as a prompt for parents/carers to talk to their child and to be less lenient the next time their child asked to stay home from school, if they had positive perceptions of the school and the circumstances of the

absence were such that the parents/carers felt they had control over their child's attendance. Pupils generally did not perceive a change in their parents'/carers' attitudes or behaviours due to the intervention, as many already experienced regular, supportive communication at home. There is also an opportunity to refine the Theory of Change based on the type of absence being targeted by the messaging intervention, as in the context of persistent or unauthorised absences the intervention was perceived as less appropriate and effective, and the dosage analysis suggested that the intervention was not effective for pupils who were eligible in all five terms, namely, those who consistently had high absence rates.

The evaluation identified some important moderators that influenced alignment with the medium-term outcomes of the Theory of Change: the extent to which school messaging systems made it easy to send the messages; and the amount of staff capacity for intervention tailoring and delivery. While all schools viewed the intervention as cost-effective, those with limited technological resources found it labour-intensive, highlighting the need for the Theory of Change to consider school-level resource disparities. The perceived impact of text messages on attendance was modest and largely contingent on specific circumstances, such as the reason(s) for and number of absences, and parental responsiveness. Additionally, while some parents/carers and schools found the text messages complementary to broader attendance strategies, others saw them as insensitive, particularly in cases of complex absences. BIT invited schools to exclude pupils/families for whom the school felt it would be inappropriate to send a text message to their parent/carer, including for those reasons. However, the intervention did not target any specific reason for or type of absence; both unauthorised and authorised absences from school counted towards becoming eligible for the intervention. These findings suggest the need for targeted messaging, and a more context-sensitive approach in the Theory of Change and in school delivery, to enhance its relevance and effectiveness across diverse school settings.

Contextual and moderating factors at the school, parent/carer, and pupil levels influenced the perceived efficacy of the intervention. The evaluation found that the schools with greater capability to personalise, review eligible pupils, and send the text messages (e.g. access to better technological platforms and greater staff capacity) perceived the intervention as being less burdensome and more worthwhile in relation to the effort needed to implement the intervention. There was limited or no explicit evidence that school characteristics such as location and the proportion of FSM or EAL pupils influenced the efficacy of the intervention.

At the parent/carer level, the evaluation found no clear trends to suggest that the effectiveness of the intervention varied based on demographic factors of interviewed parents/carers. However, there was some evidence that parents/carers with a positive view of the school were more likely to engage with the text messages.

The evaluation found that the intervention had an effect for FSM, female, and Year 8 pupils (although the IPE did not raise clear reasons why this was the case); and contextual factors about the reason for absences may have moderated the effectiveness of the intervention. When absences were complex (e.g. caused by bereavement, medical conditions, anxiety, autism, and SEND) or outside the control of parents/carers and pupils (e.g. a serious or one-off illness) the intervention was perceived as less effective by parents/carers, pupils, and schools. In fact, schools felt that sending the intervention to parents/carers when the absences were the result of complex factors could have undermined the impact of other attendance management interventions. The intervention left agency with schools to remove individuals with complex absences, but in future it would be worth exploring the level of guidance provided to schools about who to exclude from such an intervention, to avoid creating more stress for families with complex needs. Schools received the following guidance: ‘ You can choose not to send the message to any parents, where you do not feel it would be appropriate to do so. For example, if a pupil has been seriously unwell, or suffered a family bereavement’.

Interpretation

The evaluation found a small positive effect of the intervention on overall pupil absence rates. There is some statistical uncertainty regarding this impact, and the results of the study are consistent with the intervention having a higher positive impact or a negative impact. The trial data showed an average reduction in absence of 0.26 days per eligible pupil in the intervention schools in Terms 2–6, but we cannot be confident that the effect is genuine, and the result would be replicated if the trial was repeated. This equates to 15 days per pupil in the intervention group in Terms 2–6, since only 79% of pupils were eligible (with attendance <95% in at least one of Terms 1–5), which is a 0.10% percentage-point increase in the attendance rate in Terms 2–6 of the trial.[36] The schools in our trial were slightly more likely than average to be local authority maintained schools, located in urban areas, and Ofsted rated as ‘Requiring improvement/Inadequate/Serious weakness’. They were also slightly larger compared to the national average, with a slightly higher proportion of FSM pupils and a slightly lower proportion of EAL pupils. There was limited or no explicit evidence that school characteristics influenced the efficacy of the intervention.

[36] The intervention can only improve the attendance of pupils who became eligible to receive it, which was 79% of the pupils in the trial. That means that the average difference in days attended per pupil (over all Years 7–11 pupils in the schools) was 15 days over Terms 2–6. In the trial, there were on average 150.69 attendable days in Terms 2–6.

The impact of the BITUP intervention on attendance can be compared with other evaluations of text messages that aimed to improve attendance. A trial in New York that evaluated personalised text messages informing parents/carers about their child's absence found no impact of the text messages on attendance, despite only including schools that were not already using text messaging to communicate with parents about attendance in order to create a clear contrast between intervention and control arms (Balu, Porter, and Gunton, 2016). Nor did the UK pilot study conducted by BIT find an impact on its primary outcome measure, pupil's attendance rate, although that trial had to be stopped prematurely after three months due to the COVID-19 pandemic (BIT, 2020).

Our size of the impact contrasts with the two other trials that found an effect of sending letters informing parents of the number of days of school that their child had missed. Rogers and Feller (2018) sent the information in repeated rounds of letters (up to five), targeting key misbeliefs held by parents/carers of at-risk pupils and showing attendance visually as a bar chart; and found attendance increased by 1.1 days per eligible pupil during the school year. BIT and Ceibal (2024) sent three letters that informed parents of days of school missed but also included information about the importance of attendance, tips for improving attendance, and calendars for monitoring attendance. This intervention resulted in a 6% increase in attendance, equivalent to 1.7 days per pupil over the school year. These interventions used more behaviour change techniques than BITUP, which was possible because they were sent by mail. Indeed, Rogers and Mapp (2024) argued in a recent paper (published after the BITUP trial was complete) that printed letters have a greater impact on pupil attendance than text messages. These interventions also had different samples and contexts from the BITUP trial. Rogers and Feller (2018) ran their study in the US (from kindergarten through 12th grade), and only included pupils who were absent for three or more days more than the mode, but who were within 2 SDs from the mean; while BIT and Ceibal's (2024) sample was primary school pupils in Uruguay, with a rate of absence of 28.64% in the control arm. Further, Rogers and Feller's (2018) intervention was a response to a pilot survey showing that parents tended to underestimate their children's absences, whereas in the post-COVID-19 UK context, school attendance is high profile and the schools in our evaluation tended to have intensive business as usual monitoring and communication strategies in place.[37]

The results from the impact evaluation suggest that if the intervention worked, it likely did so through reducing authorised absences, when the school accepts an explanation from the parent (such as illness). However, the IPE found that authorised absences were often complex and that the text messages were perceived less positively by parents/carers in these cases. This suggests that not all authorised absences should be treated as the same type of absence, and that more work is needed to understand, which types of authorised absences the intervention worked for, and which they did not.

The ease and administrative burden of implementation depended on the schools' existing attendance and messaging systems and their functionality. However, for schools where implementation of the intervention is low cost, using (elements of) the BITUP messages may be useful. Schools should consider what steps can be taken to ensure families dealing with complex absences do not receive text messages so as to avoid upsetting them. For instance, by improving the internal review process to decide who should get a text message each term, or by updating the eligibility criteria to always exclude parents whose children missed school due to circumstances beyond the parents'/carers' control.

Parents/carers broadly welcomed the communication of the number of days their child was absent for, compared to communication of the absence rate as a percentage. This simplification in communicating absence was one of the two core components of the intervention.

Limitations and lessons learned

There are several limitations to the evaluation design and implementation:

- With treatment and control arm families in the same schools, it is possible that our design risked some contamination between arms, which may have reduced the ability of the evaluation to detect an effect. Insights from the IPE suggests that some control arm parents/carers may have heard about the intervention from other parents/carers or their child's school, but the extent of the spillover is unclear. However, even if they knew about the intervention, control arm parents/carers would not have been given an update on the number of days their child missed, which is the key mechanism of the intervention, so any spillover should have had a low impact.
- Issues concerning the pupil absence data used by the delivery and evaluation teams. During the trial, the delivery team found that some attendance codes were used inconsistently between and within schools to mark pupil

[37] You can see the worsening attendance by comparing eligibility for this trial to the Bristol pilot: in the pilot, 40.5% of pupils were eligible for the intervention over three terms; whereas in our trial 79% were eligible, with 50.4% of them becoming eligible in the first term

attendance in each session. This led to miscalculations for the number of days of school missed in Term 1 and Term 2 for some pupils in the intervention arm. As a result, 25 schools in Term 2 and 22 schools in Term 3 were instructed to send text messages to parents/carers with an incorrect number of school days missed.[38] In both cases, the problem was caught by schools and addressed quickly by the delivery team before messages had been sent out in most cases. Relatedly, there were some differences in pupil attendance data depending on when the data was accessed by the delivery and evaluation teams. This suggests that some schools update pupil attendance records ex post facto. Therefore, it is possible that attendance records were not up to date for some pupils when the delivery team accessed the data between terms to calculate who was eligible for the intervention and how many days of school they had missed. As a result, it is possible that some parents/carers received text messages with incorrect information, and this may have reduced the ability of the evaluation to detect an effect of the intervention.

- The evaluation was limited in its ability to accurately assess compliance and implementation fidelity, such as how many messages each school sent and to which parents/carers. Schools were required to manually complete and return a monitoring form each term, including which pupils' families were sent a message, on which date, and the language the message was sent in, on top of reviewing the list of eligible pupils and sending out the text messages. While efforts were made to reduce the administrative burden this placed on schools, such as providing the forms with pre-filled information and offering schools a cash incentive to return completed forms in the last three terms, the number of monitoring forms returned remained low over the intervention. It is also possible that the schools who did return the forms had more staff capacity, were more engaged in the trial, and more compliant (e.g. were more likely to send out the text messages within the first two weeks, or send out any text messages at all). Future research should seek to access data on and from messaging platforms used by schools to assess whether certain systems make compliance easier (and to reduce missingness of compliance data)—we explored this for the present study but the intervention developer indicated that these requirements would pose too great a risk for school recruitment into the trial.
- There was a large amount of missing data, mainly due to three schools withdrawing from the trial and 29 schools not providing any data for pupils from Year 11. Because a higher proportion of pupils were eligible for the intervention that we had estimated when running the power calculations, the trial was still highly powered (the actual power was higher than the anticipated power at time of trial design). A sensitivity analysis that excluded all Year 11 pupils showed that the missing Year 11 data did not affect the results. There are also no demographic differences between trial arms. However, attrition was also more likely in pupils with FSM and/or EAL status, compared to the whole sample that was randomised. We did not explore this further because the missing data was clearly due to school-level characteristics, not participant-level characteristics.
- The IPE findings are limited to the perspectives of those who took part in the research, and it is possible that other perspectives, which could have influenced findings, have not been included in this evaluation. The evaluators had to rely on schools to inform parents/carers and pupils about the qualitative research and how they could take part. This meant that the research team was limited in their ability to recruit parents/carers and pupils purposively based on absence contexts and pupil circumstances. In particular, there was low coverage of EAL parent/carer and pupil perspectives. As parents/carers had to opt in to being contacted about the research, there is a risk that only the most engaged parents/carers did so.
- The evaluation did not include an assessment of pupil attainment, and there is no long-term follow-up planned to assess the medium- and long-term impacts of the intervention on either attendance or attainment.

Future research

Questions for future research may include:

[38] Two attendance codes created problems. The 'Planned whole or partial school closure' category is a null mark used to track holidays, weekends, and weekdays of school closure. However, this code was not used consistently between schools, and some schools did not use it at all. The 'All should attend/no mark recorded' attendance code is also a null mark referencing future potential attendance dates. Each pupil record has a value of 'All should attend/No mark recorded' corresponding to dates they could be present or absent in the future. This value is supposed to be replaced with the correct attendance value for the date once that date happens. However, for a few schools, the 'All should attend/No mark recorded' code did appear in the attendance record data, which led to an overcounting of the days of school missed for some pupils. This also implies that information is missing from the database, either because it has not been uploaded properly or because it has not been properly recorded by the school.

- Is there a mode of delivery of the BITUP intervention that would be more effective?
- Can the larger effect size for FSM, Year 8, and female pupils be replicated in a study with sufficient power? If it is more effective for these groups, then why?
- Does the effect of the programme differ for pupils who fell below 90% attendance (i.e. pupils with more frequent absences)? Or for pupils whose baseline attendance was near 95%?
- What type of interventions (including messaging strategies) can be used by schools to better target and reduce unauthorised or persistent absences (below 90% termly attendance)?

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Appendix A: EEF cost rating

Table A1: Cost rating

Cost rating	Description
£ £ £ £ £	*Very low:* less than £80 per pupil per year.
£ £ £ £ £	*Low:* up to about £200 per pupil per year.
£ £ £ £ £	*Moderate:* up to about £700 per pupil per year.
£ £ £ £ £	*High:* up to £1,200 per pupil per year.
£ £ £ £ £	*Very high:* over £1,200 per pupil per year.

Appendix B: Security classification of trial findings

OUTCOME: Annual pupil absence rates (school administrative records)

Rating	Criteria for rating			Initial score		Adjust		Final score
	Design	**MDES**	**Attrition**					
5 🔒	Randomised design	<= 0.2	0-10%					
4 🔒	Design for comparison that considers some type of selection on unobservable characteristics (e.g. RDD, Diff-in-Diffs, Matched Diff-in-Diffs)	0.21 - 0.29	11-20%	**4**				4
3 🔒	Design for comparison that considers selection on all relevant observable confounders (e.g. Matching or Regression Analysis with variables descriptive of the selection mechanism)	0.30 - 0.39	21-30%			Adjustment for threats to internal validity **[0]**		
2 🔒	Design for comparison that considers selection only on some relevant confounders	0.40 - 0.49	31-40%					
1 🔒	Design for comparison that does not consider selection on any relevant confounders	0.50 - 0.59	41-50%					
0 🔒	No comparator	>=0.6	>50%					

Threats to validity	Risk rating	Comments
Threat 1: Confounding	**Low**	Randomisation across schools minimises confounding variables.
Threat 2: Concurrent Interventions	**Low**	There is a lot of reported business-as-usual activity in relation to pupil attendance communications, but given within-school randomisation, this would have been experienced equally by the treatment and control groups.
Threat 3: Experimental effects	**Low**	There is no meaningful evidence that text messages about the number of days the pupil missed were sent to pupils in the control group or that parents discussed the intervention with parents of children in the control group. As most schools already reported closely monitoring attendance and sending parental communications, these messages are unlikely to have significantly changed control parent level of perceived monitoring.
Threat 4: Implementation fidelity	**Low**	The low rate of return of compliance data means that it is not possible to know whether text messages were sent out as planned in over half of schools. Schools that returned compliance data reported a high rate of compliance, suggesting potential qualitative differences between compliant and non-compliant schools. There were limitations to the definition of compliance, in that the evaluators were unable to determine whether text messages had been received and read by parents, only that they had been sent in schools where compliance was reported. Implementation fidelity was reasonably good in schools that returned compliance data.
Threat 5: Missing Data	**Low**	Total missingness is low <5%. The trial was unable to identify eligibility for a cohort of 13,090 pupils due to reasons concerning data quality
Threat 6: Measurement of Outcomes	**Low**	Outcome data was obtained directly from school MIS. No notable issues on reliability and validity of data.
Threat 7: Selective reporting	**Low**	Trial was preregistered and analysis conducted and reported according to the published SAP.

- **Initial padlock score: 4 padlocks –** two-armed cluster randomised controlled trial; MDES of .012 at randomisation stage; 12.59% pupil attrition led to a loss of 1 padlock.
- **Reason for adjustment for threats to validity**: 0 padlocks.
- FINAL PADLOCK SCORE: 4 Padlocks.

Appendix C: Additional absence rate results

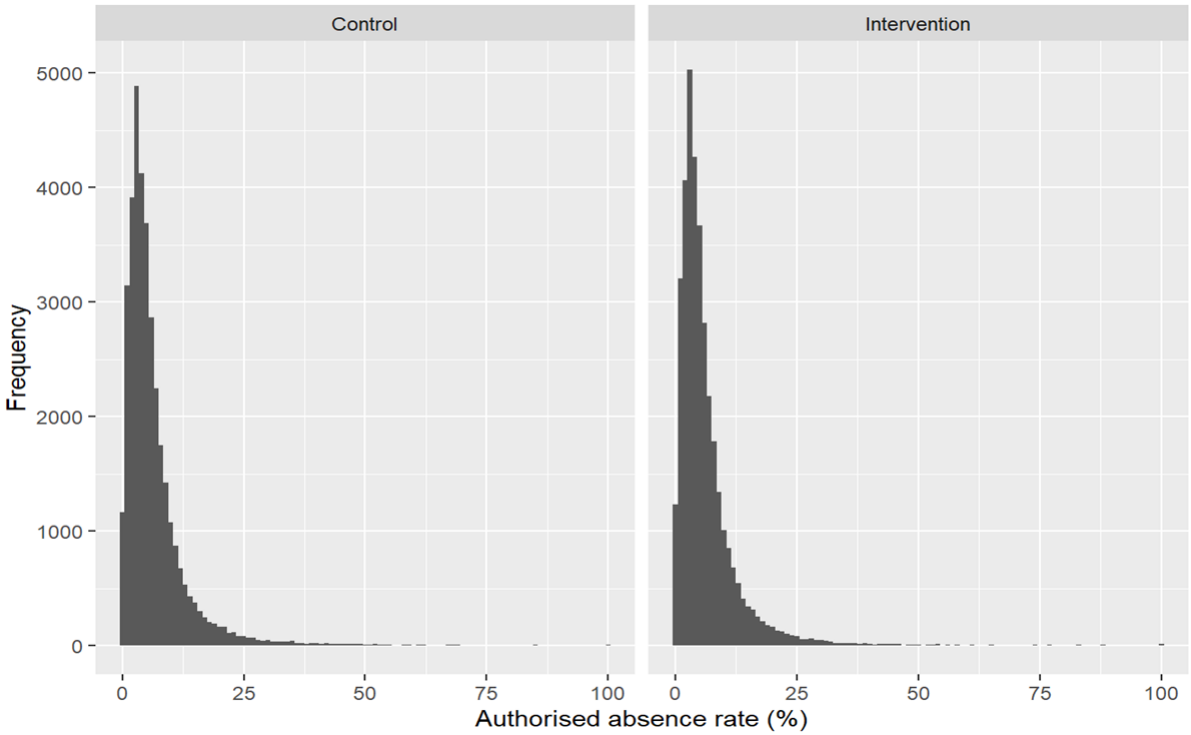

Figure C1: Distribution of authorised absence rate (%) across Terms 2–6 (secondary outcome), by trial arm

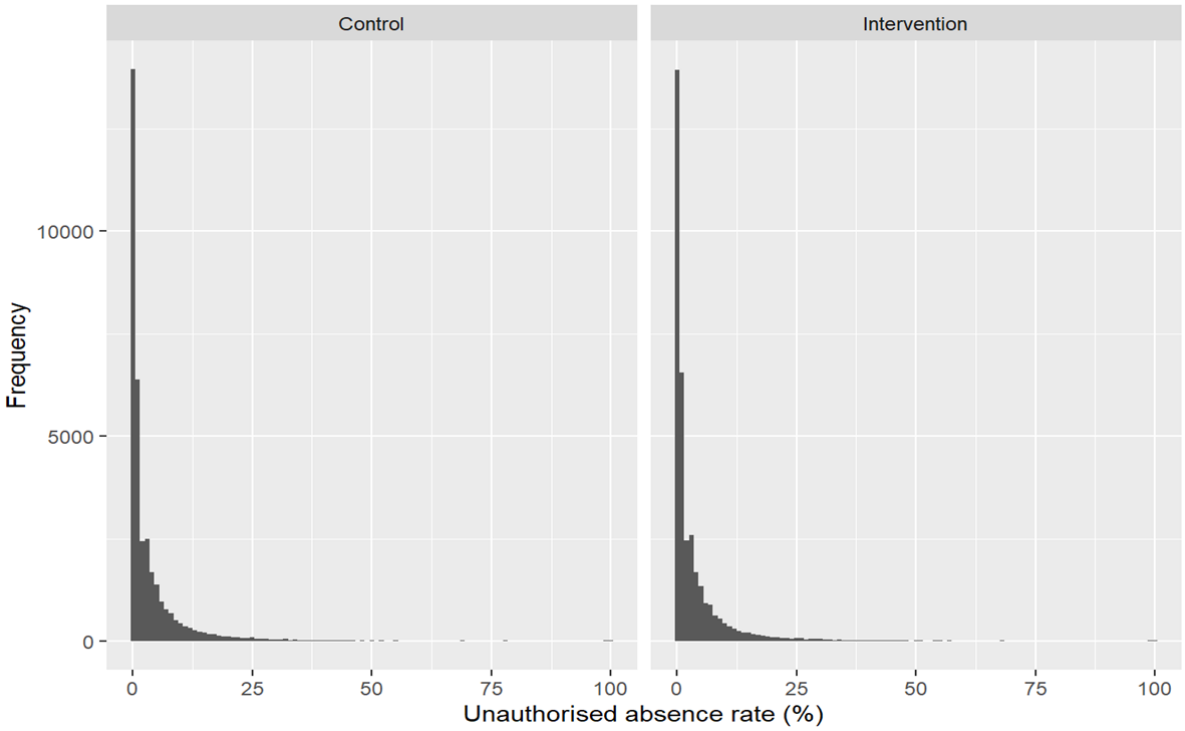

Figure C2: Distribution of unauthorised absence rate (%) across Terms 2–6 (secondary outcome), by trial arm

Appendix D: Additional compliance analysis

Table D1: Results from the first stage, primary compliance analysis

Predictors	Estimates	95% CI	P-value
(Intercept)	0.0145	-0.0002 – 0.0292	.054
Intervention	0.9198	0.9158 – 0.9239	<.001
Absence rate in Term 1	-0.0015	-0.0017 – -0.0014	<.001
EAL status: Non-EAL	-0.0023	-0.0090 – 0.0045	.510
EAL status: Not known	0.0038	-0.0053 – 0.0128	.417
FSM status: Non-FSM	-0.0009	-0.0055 – 0.0037	.699

Table D2: Results from the ITT analysis of the primary compliance analysis sample

Predictors	Estimates	95% CI	P-value
(Intercept)	5.5131	4.6378 – 6.3884	<.001
Intervention	-0.3847	-0.6242 – -0.1453	.002
Absence rate in Term 1	0.8083	0.7990 – 0.8176	<.001
EAL status: Non-EAL	0.7184	0.3174 – 1.1194	<.001
EAL status: Not known	0.7657	0.2249 – 1.3065	.006
FSM status: Non-FSM	-2.6528	-2.9290 – -2.3766	<.001

Table D3: Results from the first stage, secondary compliance analysis

Predictors	Estimates	95% CI	P-value
(Intercept)	0.0188	-0.0106 – 0.0482	.211
Intervention	0.8307	0.8246 – 0.8369	<.001
Absence rate in Term 1	-0.0022	-0.0025 – -0.0020	<.001
EAL status: Non-EAL	0.0034	-0.0068 – 0.0135	.517
EAL status: Not known	0.0003	-0.0134 – 0.0139	.972
FSM status: Non-FSM	0.0005	-0.0065 – 0.0076	.888

Table D4: Results from the ITT analysis of the secondary compliance analysis sample

Predictors	Estimates	95% CI	P-value
(Intercept)	4.4640	3.9157 – 5.0123	<.001
Intervention	-0.3973	-0.6806 – -0.1140	.006
Absence rate in Term 1	0.8106	0.8001 – 0.8211	<.001
EAL status: Non-EAL	0.9329	0.4803 – 1.3855	<.001
EAL status: Not known	0.6983	0.1256 – 1.2710	.017
FSM status: Non-FSM	-2.5017	-2.8229 – -2.1805	<.001

Appendix E: Missing data analysis

Table E1: Characteristics of pupils in the analysed sample with missing outcome variables

Family level (categorical)	Control	Intervention
	Count (%)	Count (%)
FSM categorisation		
FSM	52 (61.9%)	33 (48.5%)
Non-FSM	32 (38.1%)	35 (51.5%)
EAL categorisation		
EAL	12 (14.3%)	5 (7.4%)
Non-EAL	72 (85.7%)	63 (92.5%)
No. of siblings		
1	65 (77.4%)	50 (73.5%)
2	15 (17.9%)	17 (25.0%)
3	3 (3.6%)	1 (1.5%)
4	1 (1.2%)	0 (0%)
Pupil level (categorical, using endline data)	**Count (%)**	**Count (%)**
FSM status		
FSM	51 (60.0%)	33 (47.1%)
Non-FSM	34 (40.0%)	37 (52.9%)
EAL status		
EAL	12 (14.1%)	5 (7.1%)
Non-EAL	53 (62.4%)	49 (70.0%)
Not known	20 (23.5%)	16 (22.9%)
Gender		
Female	45 (52.9%)	33 (47.1%)
Male	40 (47.1%)	37 (52.9%)
SEND status		
Non-SEND	45 (52.9%)	37 (52.9%)
SEND	40 (47.1%)	33 (47.1%)
Ethnicity		
White	68 (80.0%)	57 (81.4%)
Asian	5 (5.9%)	2 (2.9%)
Black	2 (2.4%)	3 (4.3%)
Mixed or multiple ethnicity	6 (7.1%)	6 (8.6%)
Age		

12	6 (7.1%)	7 (10.0%)
13	12 (14.1%)	10 (14.3%)
14	21 (24.7%)	18 (25.7%)
15	30 (35.3%)	19 (27.1%)
16	13 (15.3%)	13 (18.6%)
17	3 (3.5%)	3 (4.3%)
Year group		
7	9 (10.6%)	7 (10.0%)
8	17 (20.0%)	11 (15.7%)
9	24 (28.2%)	23 (32.9%)
10	25 (29.4%)	19 (27.1%)
11	10 (11.8%)	10 (14.3%)

Appendix F: Additional subgroup analysis

Table F1: Subgroup analysis – FSM interaction

Predictors	Estimates	95% CI	P-value
(Intercept)	6.3518	5.9815 – 6.7221	<.001
Intervention	-0.3396	-0.6094 – -0.0698	.014
Absence rate in Term 1	0.7364	0.7311 – 0.7417	<.001
EAL status: Non-EAL	1.0147	0.7780 – 1.2514	<.001
EAL status: Not known	0.8485	0.5470 – 1.1501	<.001
FSM status: Non-FSM	-3.1357	-3.3729 – -2.8985	<.001
Intervention x FSM status: Non-FSM	0.2931	-0.0325 – 0.6187	.078

Table F2: Subgroup analysis – EAL interaction

Predictors	Estimates	95% CI	P-value
(Intercept)	6.3332	5.9404 – 6.7259	<.001
Intervention	-0.3037	-0.6815 – 0.0740	.115
Absence rate in Term 1	0.7365	0.7311 – 0.7418	<.001
EAL status: Non-EAL	0.9192	0.6021 – 1.2362	<.001
EAL status: Not known	0.7443	0.3557 – 1.1329	<.001
FSM status: Non-FSM	-2.9888	-3.1609 – -2.8167	<.001
Intervention x EAL status: Non-EAL	0.1908	-0.2318 – 0.6134	.376
Intervention x EAL status: Not known	0.2095	-0.2797 – 0.6987	.401

Table F3: Subgroup analysis – gender interaction

Predictors	Estimates	95% CI	P-value
(Intercept)	6.5089	6.1390 – 6.8789	<.001
Intervention	-0.2186	-0.4331 – -0.0040	.046
Absence rate in Term 1	0.7362	0.7308 – 0.7415	<.001
EAL status: Non-EAL	1.0124	0.7757 – 1.2491	<.001
EAL status: Not known	0.8458	0.5442 – 1.1473	<.001
FSM status: Non-FSM	-2.9869	-3.1589 – -2.8148	<.001
Gender: Male	-0.4825	-0.6889 – -0.2761	<.001
Intervention x gender: Male	0.1512	-0.1385 – 0.4408	.306

Table F4: Subgroup analysis – year group interaction

Predictors	Estimates	95% CI	P-value
(Intercept)	6.5887	6.1827 – 6.9947	<.001
Intervention	0.0807	-0.2396 – 0.4010	.621
Absence rate in Term 1	0.7383	0.7329 – 0.7436	<.001
EAL status: Non-EAL	1.0207	0.7840 – 1.2574	<.001
EAL status: Not known	0.8169	0.5133 – 1.1204	<.001
FSM status: Non-FSM	-2.9648	-3.1371 – -2.7925	<.001
Year 8	-0.6166	-0.9330 – -0.3001	<.001
Year 9	-0.5435	-0.8570 – -0.2299	.001
Year 10	-0.0665	-0.3824 – 0.2493	.680
Year 11	-0.7102	-1.0578 – -0.3627	<.001
Intervention x Year 8	-0.4683	-0.9138 – -0.0228	.039
Intervention x Year 9	-0.1387	-0.5794 – 0.3020	.537
Intervention x Year 10	-0.2370	-0.6794 – 0.2053	.294
Intervention x Year 11	-0.2486	-0.7286 – 0.2313	.310

Table F5: Subgroup analysis – absence level in the first term interaction

Predictors	Estimates	95% CI	P-value
(Intercept)	6.1832	5.8272 – 6.5391	<.001
Intervention	0.0005	-0.1778 – 0.1789	.995
Absence rate in Term 1	0.7440	0.7366 – 0.7513	<.001
EAL status: Non-EAL	1.0131	0.7764 – 1.2498	<.001
EAL status: Not known	0.8472	0.5457 – 1.1488	<.001
FSM status: Non-FSM	-2.9882	-3.1603 – -2.8161	<.001
Intervention x absence rate in Term 1	-0.0154	-0.0258 – -0.0051	.004

Appendix G: Effect size estimation

Table G1: Effect size estimation

			Intervention		Control			
Outcome	Unadjusted differences in means	Adjusted differences in means	n	Variance of outcome	n	Variance of outcome	Pooled variance	Population variance (if applicable)
Absence rate (across Terms 2–6)	-0.28	-0.14	36,037	–	35,879	–	14.99	–
Authorised absence rate (across Terms 2–6)	-0.19	-0.17	36,037	–	35,879	–	8.31	–
Unauthorised absence rate (across Terms 2–6)	-0.08	0.02	36,037	–	35,879	–	11.72	–

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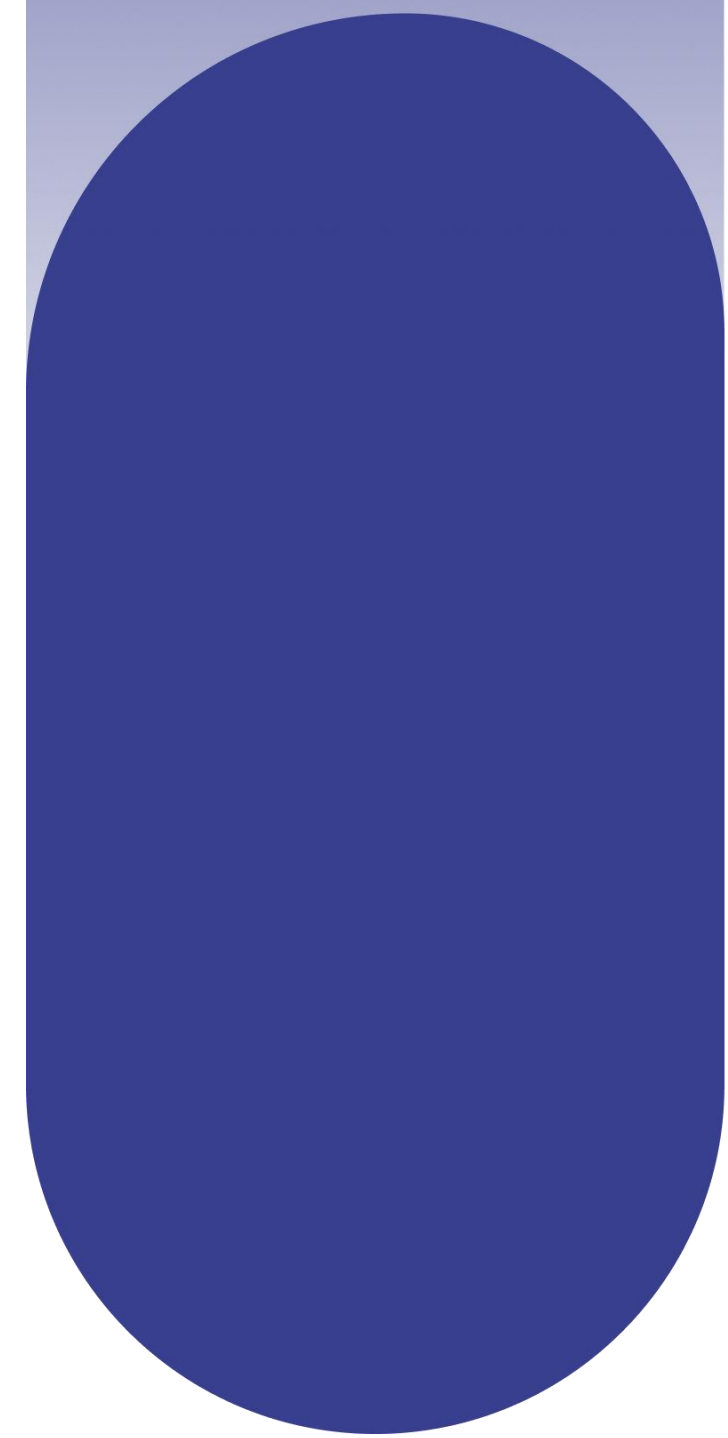

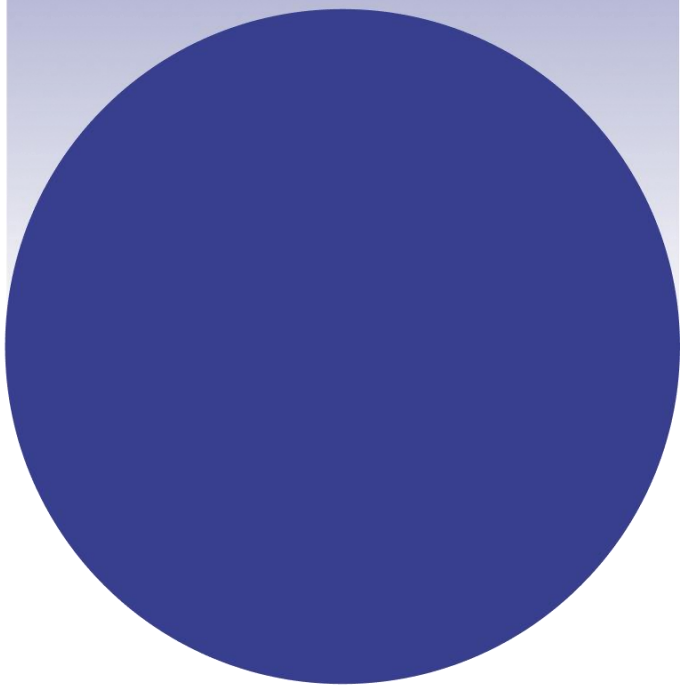

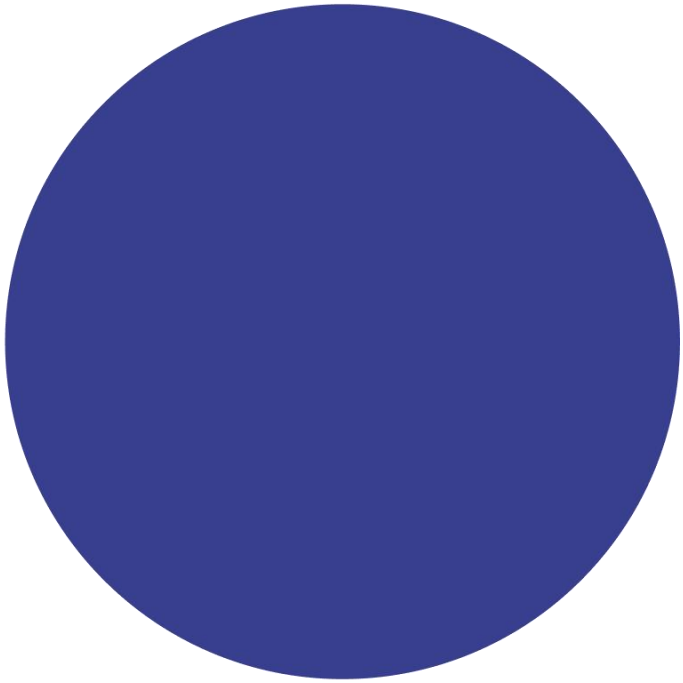

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