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# A cluster-randomised trial of the impact of a policy of daily testing for contacts of COVID-19 cases on attendance and COVID-19 transmission in English secondary schools and colleges

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## Keywords

COVID-19; SARS-CoV-2; Lateral flow testing; Contacts; Testing; Schools

## Summary

### Background

School-based COVID-19 contacts in England are asked to self-isolate at home, missing key educational opportunities. We trialled daily testing of contacts as an alternative to test if this resulted in similar control of transmission, while allowing more school attendance.

### Methods

We performed an open-label cluster-randomised controlled trial in secondary schools and further education colleges in England (ISRCTN18100261). Schools were randomised to self-isolation of school-based COVID-19 contacts for 10 days (control) or to voluntary daily lateral flow device (LFD) testing with LFD-negative contacts remaining at school (intervention).

Co-primary outcomes in all students and staff were symptomatic PCR-confirmed COVID-19, adjusted for community case rates, to estimate within-school transmission (non-inferiority margin: <50% relative increase), and COVID-19-related school absence. Analyses were performed on an intention to treat (ITT) basis using quasi-Poisson regression, also estimating complier average causal effects (CACE). Secondary outcomes included participation rates, PCR results in contacts and performance characteristics of LFDs vs. PCR.

### Findings

Of 99 control and 102 intervention schools, 76 and 86 actively participated (19-April-2021 to 27-June-2021); additional national data allowed most non-participating schools to be included in co-primary outcomes. 2432/5763(42.4%) intervention arm contacts participated. There were 657 symptomatic PCR-confirmed infections during 7,782,537 days-at-risk (59.1/100k/week) and 740 during 8,379,749 days-at-risk (61.8/100k/week) in the control and intervention arms respectively (ITT-adjusted incidence rate ratio, aIRR=0.96[95%CI 0.75-1.22;p=0.72]) (CACE-aIRR=0.86[0.55-1.34]). There were 55,718 COVID-related absences during 3,092,515 person-school-days(1.8%) and 48,609 during 3,305,403 person-school-days(1.5%) in the control and intervention arms (ITT-aIRR=0.80[95%CI 0.53-1.21;p=0.29]) (CACE-aIRR 0.61[0.30-1.23]). 14/886(1.6%) control contacts providing an asymptomatic PCR sample tested positive compared to 44/2981(1.5%) intervention contacts (adjusted odds ratio, aOR=0.73[95%CI 0.33-1.61;p=0.44]); rates of symptomatic infection in contacts were 44/4665(0.9%) and 79/5955(1.3%), respectively (aOR=1.21[0.82-1.79;p=0.34]).

### Interpretation

Daily contact testing of school-based contacts was non-inferior to self-isolation for control of COVID-19 transmission, with similar rates of student and staff symptomatic infections with both approaches. Infection rates in school-based contacts were only around 2%. Daily contact testing should be considered for implementation as a safe alternative to home isolation following school-based exposures.

### Funding

UK Government.

## Introduction

In the COVID-19 pandemic, disease control in schools has ranged from no controls at one extreme, to school closure at another, the latter largely based on evidence regarding transmission of influenza.[1] Between these poles, different degrees of control have been applied, including isolation of suspected or confirmed cases, to isolation of close contacts of cases.[2] With widespread availability of SARS-CoV-2 point-of-care testing, daily contact testing (DCT) has been modelled and piloted as an alternative to compulsory unsupervised isolation of contacts.[3,4,5] DCT allows contacts to attend school provided a daily SARS-CoV-2 test is negative. Daily testing with antigen lateral flow devices (LFDs) is feasible,[6] with rapid turnaround times, relatively low cost and good detection of virus.[7,8,9] In addition to allowing students and staff to remain at school, DCT may make regular asymptomatic testing more popular and improve contact reporting, by removing the social penalty of positive cases triggering isolation in contacts.[10] This in turn may improve case detection and therefore may even reduce transmission.[3] However, concerns about LFD performance, especially outside of healthcare and expert settings, have left uncertainty about whether DCT is appropriate for schools or more widely.[11]

A policy of self-isolation of contacts assumes this reduces the risk of onward transmission in schools. In practice, its impact is unknown: adherence to isolation is incomplete,[12] and the number of isolation-days required to prevent one onward transmission has not been calculated. Evidence is lacking that the benefit of the policy outweighs the clear social[13,14] and educational[15,16,17] disadvantages. Contact-tracing data from England suggests that transmission following contact in secondary schools is infrequent, and occurs in <3% of contacts of infected teenagers.[18] Observational reports from England found educational outbreaks are uncommon, and strongly associated with community incidence.[19]

We undertook a cluster-randomised controlled trial of DCT in students and staff at secondary schools and colleges in England to determine if DCT increases school attendance and to assess the impact of DCT on SARS-CoV-2 transmission within the school.

## Methods

### Study design and participants

We conducted an open-label, cluster-randomised controlled trial to assess the effectiveness of offering daily testing to contacts of COVID-19 cases (ISRCTN18100261). The study took place in secondary schools and further education colleges in England. Secondary schools were studied as students at these schools were already participating in asymptomatic screening with LFDs, and so the trial built upon existing infrastructure which was not present in primary schools (students  $\leq 11$  years). Schools and colleges (hereafter collectively referred to as schools) were eligible to participate if willing to follow the trial procedures and able to operate assisted testing on site. A representative of the institution provided consent electronically. Participation by individual student and staff contacts was voluntary and followed written or electronic completion of a consent form. After randomisation, parents or guardians provided consent for participants <16 years old and for those otherwise unable to give consent. The study protocol was reviewed, and ethical approval granted, by Public Health England's Research Ethics and Governance Group (ref R&D 434).

The study was done in accordance with the Declaration of Helsinki and national legislation. A nested qualitative process study of acceptability and feasibility for students, parents and staff is reported separately.[20] The study protocol and analysis plan are provided as supplementary material.

### Randomisation

Schools were randomly assigned 1:1 to either a policy of offering contacts daily testing over 7 days to allow continued school attendance (intervention arm) or to follow usual policy of isolation of contacts for 10 days (control arm). Stratification was used to ensure schools representative of those in England were balanced between study arms (Table 1, details in supplement).

### Procedures

All control and intervention arm schools followed the national policy of offering twice weekly asymptomatic testing with LFDs. Individuals with positive LFD results were required to self-isolate immediately and requested to obtain a confirmatory PCR test within 2 days.[21] Those with indicator symptoms of possible COVID-19 (new cough, fever, loss or change in taste or smell) were required to self-isolate along with their household and obtain an urgent PCR test.

If a student or staff member tested positive by LFD or PCR, close contacts (“contacts”) were identified by schools using national guidelines (see supplement). Those in close contact with a case <48h prior to symptom onset (or a positive test if asymptomatic) were required to self-isolate for 10 days.[22]

At intervention arm schools, contacts were offered DCT as an alternative to self-isolation, provided the contact was school-based (i.e. with a staff member or student), the contact did not have indicator symptoms of COVID-19, and contacts were able to attend for on-site testing at school. Contacts were excluded from DCT if they had a household member who was isolating following a positive COVID-19 test. Contacts who did not consent to DCT were required to self-isolate for 10 days.[22]

Participants who agreed to DCT swabbed their own anterior nose; swabs were tested by school staff using a SARS-CoV-2 antigen LFD (Orient Gene).[23] Participants who tested negative were informed and released from isolation that day to attend education, but were asked to self-isolate after school and on non-testing days (weekends/holidays). Those with 5 negative tests over  $\geq 7$  days were released from self-isolation, allowing for no testing at weekends. Where a school-based close contact tested positive, they were instructed to self-isolate along with their household, their school-based contacts were identified, and the process repeated for these contacts.

### Data collection

A study worker was funded at each participating school. Schools provided a list of all students and staff, including personal identifiers and demographics. For consented randomised schools that stopped active participation, where available, a list of students was provided by UK Government Department for Education (DfE).

Schools reported the numbers of staff and students present on each school day, absent for COVID-19-related reasons and absent for other reasons. Data from schools who stopped participating, where available, were obtained from DfE.

Schools recorded each SARS-CoV-2 infection (“index case”) brought to their attention, including PCR-positive cases and LFD-positive cases without a subsequent PCR test. LFD-positive-PCR-negative individuals were not considered cases. The school-based contacts of each index case, whether the contact consented to study procedures, and LFD results were recorded. During the trial, the trial management team were blinded to the combined data.

### PCR testing

Results of routine SARS-CoV-2 tests performed outside of the study in staff and students were obtained from national public health data (“NHS Test and Trace”). Dedicated study PCR testing was also undertaken in consenting contacts in both study arms on day 2 and 7 of the testing/isolation period. In addition, study PCRs were obtained from consenting LFD/PCR-positive individuals for later analysis (see supplement).

### Outcomes

The co-primary outcomes, across all students and staff, were (i) the number COVID-19-related school absences amongst those otherwise eligible to be in school and (ii) the extent of in-school COVID-19 transmission. Non-inferiority in transmission was considered appropriate, as the intervention was hypothesised to produce beneficial increases in attendance. Transmission was estimated from rates of symptomatic PCR-positive infections recorded by NHS Test and Trace, after controlling for community case rates. Both these endpoints were assessed using study data for actively participating schools and using national administrative data on student attendance and student and staff lists for non-participating randomised schools. Rates of symptomatic PCR-positive community tests were compared as the incidence of these tests was not expected to be impacted by the study intervention, whereas more intensive sampling of asymptomatic contacts in intervention schools may have detected more asymptomatic infection. Twice weekly asymptomatic LFD testing was not reliably reported, so results were not compared between arms.

Secondary outcomes reported include DCT participation rates in the intervention arm, the proportion of contacts testing positive on asymptomatic study PCR tests and symptomatic routine PCR tests, and the performance characteristics of LFD vs. PCR testing.

### Statistical analysis

Rates of COVID-related absences and symptomatic PCR-positive SARS-CoV-2 infections were compared on an intention to treat (ITT) basis using quasi-Poisson regression to account for over-dispersion, considering each school as the unit of analysis. We adjusted for randomisation strata groups and participant type (student/staff) and accounted for repeated measurements from the same school over time (see supplement for details and for following analyses). Infection incidence models were also adjusted for community SARS-CoV-2 case counts at the lower tier local authority level (LTLA) in the prior week. To account for incomplete participation in DCT, we present complier average causal effects (CACE)

estimates for both primary outcomes, which estimate the impact of the intervention amongst those actively participating.

We report DCT uptake in intervention arm participants, on a per day and per participant basis. We used Poisson regression to investigate factors associated with per individual participation rates, including the randomisation stratification groups, participant type, age, sex, and ethnicity.

The proportion of close contacts testing positive on an asymptomatic study PCR test or symptomatic community PCR test was compared between study arms using logistic regression. Given there were relatively few PCR-positive contacts, adjustment was made only for randomisation strata groups and local case counts in the previous week.

We compared the performance of LFD to PCR testing in participants tested by both methods on the same day, or up to 2 days later for those testing LFD-positive, regarding PCR testing as the reference standard.

#### Sample size and power

The challenge with setting a non-inferiority margin for transmission events is that the margin's meaning is highly dependent on the control group event rate. It was not possible to determine the transmission event rate in the control group prior to the trial start, and it is subject to on-going change. However, it was considered at the time of writing the study protocol that with an example infection rate in contacts of 20%, an upper bound of the confidence interval of an absolute increase of 10%, i.e., relative increase in transmission of up to 50% would be acceptable. Given the uncertainties in the absolute rates of transmission events in each arm, we powered the trial to detect a difference in school attendance (details in supplement).

#### Role of the funding source

The UK Government Department of Health and Social Care sponsored the trial and was involved in study design, matching of NHS Test and Trace data with study records, data curation and interim monitoring. Otherwise, the study sponsor had no role in data analysis and interpretation or writing and submission of the report.

## Results

201 schools were randomised (Table S1; Figure S1) and started participating in the 10-week study between 19-April-2021 and 10-May-2021 and continued until the pre-appointed stop date 27-June-2021; 76/99(77%) control and 86/102(84%) intervention schools actively participated, returning student/staff lists and attendance data (Figure 1).

#### Baseline characteristics

Schools were randomised using 9 school-type strata (Table 1). Schools in the control and intervention arms had a median(IQR) 1014(529-1376) and 1025(682-1359) students and 142(91-189) and 125(91-173) staff respectively. Ages, sex and ethnic groups in students and staff were similar between the study arms, most students were aged 11-18y (Table 2).

### Index cases and contacts

The 76 and 86 actively participating control and intervention schools reported 338 and 450 index cases (students or staff) respectively, resulting in 5097 and 6721 recorded school-based contact events in 4400 and 5797 individuals. A total of 247 and 343 control and intervention arm index cases had  $\geq 1$  contact, where the 10 days following the contact event included  $\geq 1$  study school day. The remaining index cases had no reported close contacts, e.g. having tested positive during a weekend/holiday. These 4463 and 5763 contacts in 47 and 59 control and intervention schools involved a total of 22,466 and 27,973 school days where without the intervention students and staff would have been asked to isolate at home. In the intervention arm, this represented a theoretical maximum of 27,973/4,105,826(0.68%) school days where DCT could potentially prevent COVID-related absences. On 13,846/27,973(49.5%) days an LFD result was recorded (or the contact had already completed follow-up, i.e., recorded  $\geq 5$  tests or a positive test). In 1241 contact episodes, the contact declined to participate in DCT (5598 person-school-days;19.9%) and on 2600(9.2%) person-school-days a participating contact was unavailable testing (i.e. did not attend school or declined testing). Testing on 4457(15.8%) person-school-days did not occur after the whole cohort of contacts or school was sent home to isolate, following either school or public health agency intervention (Figure 2A). These participation pauses occurred at 14 schools, 5 due to school capacity issues, 6 following school or public health agency concern about the Delta variant, and 3 after public health concern about cases in the school arising from community transmission. No pause was instituted because of excess transmission attributed to the intervention.

Per day DCT participation was highest at the start of the study and lowest in the week prior to the “half-term” holiday (31-May-2021 to 04-June-2021) when participation fell, predominately due to school-wide participation pauses (Figure 2A,2B).

Using reporting of  $\geq 3$  LFD results or an LFD-positive result to summarise participation per contact rather than per day, 2432/5763(42.4%) contacts participated, with differing rates by school (Figure 2C). The median(IQR) participation across the 59 schools was 63%(40-79%). Staff were more likely to participate than students (adjusted rate ratio=1.40;95%CI 1.09-1.80;p=0.009). Amongst schools with  $\leq 17\%$  of students receiving free school meals, participation rates were higher in schools with students aged 11-16 years compared to 11-18 years (Table 3).

### COVID-related absences

Rates of student and staff COVID-related absence, due to known or suspected COVID or as a contact, were compared. Student attendance data were available for part or all of the study from 91(92%) of control and 99(97%) intervention schools; with data for 3551/4146(86%) and 3836/4261(90%) of possible school-school day combinations (Figure S2). Similarly, staff attendance was available from 94(95%) control and 100(98%) intervention arm schools, for 3767/4146(91%) and 3925/4261(92%) days. 95,545 and 102,134 students and 14,687 and 14,811 staff were reported in control and intervention arm attendance data. (Total numbers of students and staff in aggregate attendance data differ to totals from student/staff identifier lists used to identify symptomatic cases [Table 2], reflecting different underlying data sources and different schools with available data).



Students had 55,718 COVID-related absences during 3,092,515 person-school-days in the control arm (1.80%), and 48,609 during 3,305,403 person-school-days in the intervention arm (1.47%, Figure 3). Rates of staff COVID-related absences were 3704/566,502(0.65%) in control schools and 2932/539,805(0.54%) in intervention schools.

On an ITT basis, adjusting for the randomisation strata group and participant type, the adjusted incidence rate ratio, aIRR, for COVID-related absence in the intervention arm was 0.80 (95%CI 0.54-1.19;p=0.27) (Table 4;Table S2). Overall, staff were less likely to be absent for COVID-related reasons than students (aIRR=0.39;95%CI 0.31-0.48;p<0.001), but there was no evidence a difference in the effect of the intervention between students and staff (heterogeneity p=0.98). As no covariate changed with time, the originally proposed approach has a more conservative confidence interval than required. We repeated the analysis aggregating the data per school and participant type, yielding an aIRR of 0.80 (95%CI 0.62-1.03;p=0.085;Table S3).

As per day participation in the intervention arm was 49.5%, we estimated the impact of the intervention among those participating; the point estimate showed a greater reduction in absences (CACE aIRR=0.61 (95%CI 0.30-1.23;Table S2). Applying this point estimate (with the caveat the range of uncertainty is wide) to COVID-related absence in control arm students (1.80%), would equate to a 39% relative and 0.70% absolute reduction in school days missed due to COVID. CACE estimates were relatively unaffected by the choice of imputation strategy for schools with no contacts and therefore no participation data (Table S4). See Tables S5-S6 for separate ITT and CACE results for students and staff.

There was no evidence of an impact on all-cause absence rates (ITT aIRR=0.97, 95%CI 0.82-1.16, p=0.77), with non-COVID-related reasons responsible for most absences (Table S7).

#### Symptomatic PCR-confirmed SARS-CoV-2 infection

PCR results from symptomatic SARS-CoV-2 infections in students were available for 96/99(97%) control schools and 101/102(99%) intervention schools and staff results for 76(76%) and 85(83%) respectively.

614 and 683 students at control and intervention schools tested PCR-positive and reported symptoms during 6,966,653 and 7,541,525 days-at-risk (61.7 and 63.4 cases/100,000 population/week). Rates in staff were 43/790,219 (38.1/100,000/week) and 57/819,487 (48.7/100,000/week). Incidence rose during the study, as the Delta variant spread nationally,[24] similarly in each arm (Figure 4A). Incidence was higher than the number of index cases reported by schools, partly because not all randomised schools actively reported cases and in active schools not all community-diagnosed infections were reported or recorded (Table S8).

Adjusting for the randomisation strata, participant type, and the community rate of SARS-CoV-2 infection in the previous week, there was no evidence of difference between study arms in symptomatic PCR-confirmed infection (ITT aIRR=0.96;95%CI 0.75-1.22;p=0.72) (Table 4;Table S9). Overall rates of infection were lower in staff than students (aIRR=0.75;95%CI 0.61-0.92;p=0.006), but there was no evidence that the effect of the

intervention differed in staff and students (heterogeneity  $p=0.41$ ). Infection rates in students were approximately linearly related to local case counts, plateauing as community incidence rose (Figure S3); estimates were similar with varying plausible lags between community case counts and student and staff infections (Table S10).

A CACE analysis allowing the impact of the intervention to be estimated given theoretical full participation, also showed no evidence of difference between study arms in symptomatic PCR-confirmed infection (aIRR=0.86;95%CI 0.55-1.34). CACE estimates were relatively unaffected by the choice of imputation strategy for schools with missing participation data (Table S11).

Similar results were obtained in a secondary analysis of any positive PCR-result from routine community-based testing (Figure 5B) (ITT aIRR=0.96;95%CI 0.76-1.20; $p=0.71$  and CACE aIRR=0.88;95%CI 0.57-1.41) (Table S12). There was no evidence of a difference in the effect of the intervention for students and staff (ITT model, heterogeneity  $p=0.21$ ). Separate analyses for students and staff for symptomatic and any PCR-positive infection are presented in Tables S13-S16.

#### Incidence of PCR-confirmed infection in contacts

PCR testing of asymptomatic contacts was undertaken in 886 non-overlapping contact episodes in the control arm, 14(1.6%) tested PCR-positive, 1(0.1%) indeterminate and 871(98%) negative. In 2981 intervention arm contacts, 44(1.5%) tested positive, 14(0.5%) indeterminate and 2923(98%) negative. Adjusting for randomisation stratification group and community case counts in the prior week, there was no evidence that the proportion of contacts testing positive varied between study arms (adjusted OR, aOR=0.73;95%CI 0.33-1.61; $p=0.44$ ) (Table S17). Of control and intervention arm contacts testing positive/indeterminate, 4/15(27%) and 19/58(33%) went on to have a positive symptomatic test (exact  $p=0.76$ ).

We also compared the proportion of contacts with a symptomatic PCR-positive test, which included those initially testing positive while asymptomatic above who went on to have a symptomatic test. This analysis is contingent on schools reporting contacts, with several control arm schools with higher incidence not actively participating and reporting contacts (Figure S4). In the control arm 44/4665(0.9%) contacts tested PCR-positive within 10 days, compared to 79/5955(1.3%) in the intervention arm. Adjusting for randomisation strata groups and community case counts, there was no evidence that the proportion of contacts testing positive differed between arms (aOR=1.21;95%CI 0.82-1.79; $p=0.34$ ) (Table S18).

#### Performance characteristics of LFDs vs. PCR

Across the study, and the non-randomised pilot phase, 4757 contacts completed at least one LFD during DCT generating 20,289 LFD results overall. For 3226 a paired PCR test was available from the same day, or up to 2 days later for those testing LFD-positive. 3166 were PCR-negative and 60 PCR-positive. Specificity was 3164/3166 (99.93%, exact binomial 95%CI 99.77-99.99%) and sensitivity 32/60 (53%, 40-66%) (Table S19). These results largely reflect performance in students (Table S20,S21), as 3003/3226(93.1%) of participants with paired tests were students. PCR-positive cycle threshold (Ct) values were lower in those testing

LFD-positive (median 18.5, IQR 16.3-22) than LFD-negative (median 25.3, IQR 21.6-28.5) (Kruskal-Wallis  $p < 0.001$ ; Figure S5).

## Discussion

Daily LFD testing of school-based COVID-19 contacts was trialled as a voluntary alternative to 10 days of self-isolation. Although DCT avoids students and staff missing school while isolating, at the conception of the trial there was uncertainty whether it would substantially increase SARS-CoV-2 transmission, e.g. via infections missed by LFD testing.[3] The trial provides evidence this was not the case.

We investigated the incidence of symptomatic infection as an unbiased outcome measure that could be ascertained across nearly all schools, as national public health policy was that all symptomatic children and adults, whether or not they had a LFD test, should obtain a PCR test for SARS-CoV-2. As the intervention was not expected to impact the relative incidence of asymptomatic versus symptomatic infection this measure should also indicate the impact on all infections. Based on a non-inferiority margin of ensuring any relative increase in symptomatic infection, as a proxy for transmission, did not exceed  $>50\%$ , we show allowing student and staff contacts to remain in school after a negative lateral flow test was non-inferior to routine isolation. On an ITT basis, i.e. implementing DCT at participation rates seen in the trial, using data for students from 197/201 schools and staff data from 161/201 schools, we can be 97.5% confident that any increase in the rate of symptomatic infection did not exceed 22% more than seen in the control arm. If all those eligible to participate did so, then, based on a CACE model, we can be 97.5% confident that any increase does not exceed 34%. In both analyses the point estimate favours a slight to modest reduction in incidence with the intervention.

The range of absolute changes in symptomatic infection rates potentially seen with the intervention, depends on prevailing incidence. At the average incidence in the control arm during the study (0.06% students/week), the range of uncertainty in the impact of the intervention is equivalent to 1.2 fewer to 0.9 more infections/1000-student-school/month, or 3.6 fewer to 2.7 more at the highest weekly rate seen (0.18% students/week). Throughout the study, cases in both arms remained well below the  $>1\%$  level seen in 2020 when schools remained open.[25] Staff had lower rates of infection than students. There was no evidence of a difference in the effect of the intervention for students and staff.

Asymptomatic and symptomatic infections were uncommon in school-based contacts in both study arms: 1.6% and 1.5% of students and staff participating in study PCRs tested positive while asymptomatic, and 0.9% and 1.3% tested positive in symptomatic testing in the control and intervention arms respectively. These figures are comparable to the estimates for school-age children from national contact-tracing data.[18] Therefore, given precautions in place in schools during the trial (routine mask use was discontinued during the trial on 17-May-2021, but other precautions were maintained), the overall risks to students and staff following exposure to a contact at school are low. Indeed, whether the extent of transmission and performance of LFDs (discussed below) is sufficient to make contact testing necessary and cost-effective will require careful discussion and may vary with changes in incidence, virus transmissibility or the prevalence of any vaccine evasive strains. Participation in study PCR testing in control schools was lower than in the

intervention schools, in part because participation in DCT facilitated intervention arm PCR-testing and because the greater awareness of the study in intervention schools. It is unclear whether this introduced bias in the results for the study PCR tests, however we also found no evidence of difference in symptomatic infection rates in contacts.

We did not clearly demonstrate superiority of the intervention for avoiding student and staff COVID-related school absences. This possibly reflects that the trial was relatively underpowered given the large extent of variation in absence rates over time and between schools, requiring overdispersion to be accounted for in regression models fitted. Pooling data on a per school basis, in an ITT analysis, our point estimate showed a 20% decrease in COVID-related absences, but with a broad range of uncertainty (95%CI 0.62-1.03), similarly in the CACE analysis amongst those who participated the point estimate was a 38% reduction, but with broader uncertainty (95%CI 0.29-1.33).

Reductions in COVID-related absences were not greater because not all those eligible participated, and not all absences were amenable to the intervention, e.g. household contacts were ineligible. However, despite the lack of statistical evidence from the trial, in the absence of increased transmission, it is reasonable to assume that a policy allowing students and staff to remain in school would lead to increased attendance, but this may be more limited than initially anticipated.

DCT participation rates in intervention arm contacts were 42% on a per-person basis with marked variation between schools (range 0-100%). Staff were more likely to participate than students. Although contacts at government-funded schools with students 11-16 years old with a low percentage of free school meals were most likely to participate, other school types were similar, such that differences in participation related to factors other than school type. A qualitative analysis of interviews with participants to understand why some participated and others did not will be presented separately.[20] Additionally, at some stages, schools paused the intervention because of capacity limitations or public health officials' concerns about the Delta lineage or rising transmission in the community. No local public health teams reported concern that transmission increased because of this study. We did not formally assess compliance with isolation in the control arm, although it was school policy that known cases and contacts did not attend school. However, it is still possible that in both study arms there was incomplete compliance with isolation at home outside of school hours and during school hours in the control arm, particularly as lockdown restrictions eased.

Previous estimates for the performance of antigen LFDs compared to PCR testing have varied markedly.[7,9,26] Here we estimate the overall sensitivity of school-based LFD testing in largely asymptomatic individuals as 53%, i.e., within the range of previously reported rates. It is worth noting our findings on transmission in this study are in the context of this level of performance. Specificity was 99.93%. As LFD performance varies by viral load[27] performance can change as the population viral load distribution changes. Consistent with previous reports[7] we find higher viral loads, i.e. lower PCR cycle threshold values, are associated with increased sensitivity, and therefore LFDs are more likely to detect those who are most infectious.[18]

The study has several limitations. Schools and colleges, despite provision of dedicated resources, were not always able to participate due to competing pressures. It is also likely as a result that data capture was imperfect, e.g. it is possible that not all PCR-positive cases were reported to schools, and not all contacts were documented for all index cases. However, our primary outcomes are robust to this. We used the incidence of symptomatically driven testing as a primary endpoint as this was least likely to be affected by the two testing strategies; in fact, there was little difference in the incidence of all community PCR tests between the study arms. Relying on linkage to Test and Trace data is a potential weakness, as it depended on imperfectly recorded identifiers, however this would not be expected to differ between study arms. Furthermore, using incidence data means we do not directly measure within-school transmission, rather we estimate it by controlling for the rate of community infections, as a proxy for the extent of introductions into the school. The trial was conducted during periods of low to moderate COVID-19 incidence. We therefore did not estimate the impact of DCT in high incidence settings; monitoring of the impact of DCT may be needed if it is deployed when incidence is high. Changes in incidence may relate to new variants, which may impact LFD performance, and so on-going assessment of LFD performance would be needed as well. High incidence may also pose logistical challenges, in the last two weeks of the study, community incidence rose making the DCT protocol unwieldy for some schools, given the space and staff required to perform testing. We did not have sufficient power to study if the intervention had different impacts across different school types and settings.

Future work includes whole-genome sequencing of positive samples from school members and from the community, which may help analyse transmission networks in schools, including during periods of higher incidence in a manner successfully achieved for SARS-CoV-2[28,29] and a number of healthcare-associated pathogens.[30,31] This study includes staff and students from secondary schools and colleges of further education but most of the participants were students aged 11-18 years. Therefore, it is unclear the extent to which it can be generalised to other settings, and other context-specific studies are required.

Our findings have implications for policy makers seeking to balance control of COVID-19 with student well-being, education and avoiding social inequalities. We show DCT is a safe alternative to home isolation for school-based contacts, which has potential to facilitate increased school attendance and therefore to reduce the wider long-term negative consequences of the pandemic.

Overall, this study shows that in secondary school and college of further education, student and staff infection of following contact with a COVID-19 case at school occurs in only around 2%. We found switching from isolation at home to DCT, at least in the settings of the schools studied, kept rates of symptomatic COVID in students and staff at similar levels. DCT is a safe alternative to home isolation in school-based contacts and should be considered an alternative to routine isolation of close contacts following school-based exposures.

## Evidence in context

### **Evidence before this study**

We searched PubMed for research articles for any date up until 26<sup>th</sup> June 2021. We used the terms “SARS-CoV-2” and “school” and “transmission”, as well as “COVID-19” and “school” and “transmission”. No clinical trials have been reported on interventions to impact SARS-CoV-2 transmission in schools or other educational settings.

Evidence synthesis on COVID-19 transmission has found the evidence for school closure relies on extrapolating from studies of influenza transmission. Further data from schools has accrued from observational data and modelling. Public health data after school opening in England in summer 2020 showed that school related outbreaks were uncommon, and strongly associated with community incidence of infection. A review of all case-contact pairs in the UK Test and Trace system estimated a low chance of transmission following educational contact with COVID-19 in young people. Modelling studies have suggested that implementing daily contact testing in place of contact isolation may be neutral or advantageous with regard control of transmission.

### **Added value of this study**

We report the first randomised-controlled trial of a public health intervention on COVID-19 transmission in secondary schools and colleges of further education, during a period of low to moderate community incidence, predominantly with the Delta variant. Infection in close contacts in these educational settings was uncommon and around 2%. Supervised daily testing with lateral flow devices as an alternative to self-isolation for close contacts was non-inferior for control of COVID-19 transmission. School absence was reduced where testing was available, but did not demonstrate statistically significant reduction.

### **Implications of all available evidence**

Safe alternatives to mass isolation for young people in education are crucial to reducing the impact of the COVID-19 pandemic. With low transmission to contacts, in the context of other mitigations, the results here show daily testing of contacts is an acceptable alternative. Further randomised controlled trials of public health policy interventions can ensure an evidence-based response to the pandemic.

## Contributions

FI, JH, ST, VB, RO, DC, PM, NH, TF, SH, LY and TEAP contributed to the protocol and design of the study. BCY, SK, CW, SS, IanD, ER, FD, leuanD, LD, PS, AL, JM, FJ, JK, UB contributed to the implementation of the study or data collection. BCY, DWE, GB, TN, FI, leuanD and TEAP accessed and verified the data. DWE, BCY and TEAP analysed the data and wrote the manuscript. All authors contributed to revising the manuscript, had full access to all the data in the study and had final responsibility for the decision to submit for publication.

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## Transparency declaration

DWE reports lecture fees from Gilead outside the submitted work. VB, RO and DC are consultants employed by DHSC as part of Deloitte's broader project work supporting the delivery of NHS Test and Trace. TF reports honoraria from Qatar National Research Fund (QNRF) outside the submitted work, no other author has a conflict of interest to declare.

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## Data availability

Data from the trial will be available within the Office for National Statistics Secure Research Service. Applications for access can be made by Accredited Researchers. For more details please see -

<https://cy.ons.gov.uk/aboutus/whatwedo/statistics/requestingstatistics/approvedresearcherscheme>.



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## Tables

Characteristic	Control n = 99 <sup>1</sup>	Intervention n = 102 <sup>1</sup>
Strata		
Government-funded, 11-18y, free school meals ≤17%	32 (32%)	34 (33%)
Government-funded, 11-16y, free school meals ≤17%	8 (8.1%)	8 (7.8%)
Government-funded, 11-18y, free school meals >17%	22 (22%)	24 (24%)
Government-funded, 11-16y, free school meals >17%	19 (19%)	18 (18%)
Any residential school	5 (5.1%)	6 (5.9%)
Special needs or alternate provision	5 (5.1%)	5 (4.9%)
Further education college, 16-18y	3 (3.0%)	2 (2.0%)
Independent day school ≥500 pupils	3 (3.0%)	3 (2.9%)
Independent day school <500 pupils	2 (2.0%)	2 (2.0%)
Students attending school	1,014 (529, 1,376)	1,025 (682, 1,359)
Missing data	3	1
School staff	142 (91, 189)	125 (91, 173)
Missing data	23	17

**Table 1. School level baseline characteristics by study arm.** The number of students and staff at each school are based on participant lists provided as part of the study and for students from the UK Government Department for Education (DfE) for schools not actively participating after randomisation. <sup>1</sup>n (%); Median (IQR). Four schools had missing student lists as schools stopped participating before this was provided and the school had not submitted student lists to DfE previously. Forty schools had missing staff lists as schools stopped participating before this was provided and only student data were available from DfE.

Characteristic	Students		Staff	
	Control, n = 102,859 <sup>1</sup>	Intervention n = 111,693 <sup>1</sup>	Control, n = 11,798 <sup>1</sup>	Intervention, n = 12,229 <sup>1</sup>
Ethnicity				
Asian	14,735 (14%)	12,885 (12%)	562 (4.8%)	522 (4.3%)
Black	6,240 (6.1%)	5,772 (5.2%)	239 (2.0%)	204 (1.7%)
Chinese	491 (0.5%)	703 (0.6%)	12 (0.1%)	20 (0.2%)
Mixed	4,975 (4.8%)	4,565 (4.1%)	120 (1.0%)	96 (0.8%)
Other	2,137 (2.1%)	2,123 (1.9%)	65 (0.6%)	57 (0.5%)
Prefer not to say	8,709 (8.5%)	9,948 (8.9%)	3,411 (29%)	3,502 (29%)
White	65,339 (64%)	75,470 (68%)	7,389 (63%)	7,828 (64%)
Missing data	233	227	0	0
Age group				
11 to 14	48,396 (47%)	50,400 (45%)		
15 to 18	49,461 (48%)	52,185 (47%)	16 (0.1%)	5 (<0.1%)
19 to 34	3,602 (3.5%)	6,974 (6.2%)	3,453 (29%)	3,411 (28%)
35 to 44	744 (0.7%)	1,232 (1.1%)	2,807 (24%)	3,015 (25%)
45 to 54	418 (0.4%)	672 (0.6%)	2,865 (24%)	3,145 (26%)
55 to 64	143 (0.1%)	209 (0.2%)	2,215 (19%)	2,193 (18%)
65+	95 (<0.1%)	21 (<0.1%)	442 (3.7%)	460 (3.8%)
Sex				
Female	49,502 (48%)	58,148 (52%)	8,092 (69%)	8,395 (69%)
Male	53,356 (52%)	53,545 (48%)	3,706 (31%)	3,834 (31%)
Missing data	1	0	0	0

**Table 2. Student and staff level baseline characteristics by study arm.** Note students aged  $\geq 19$  years attended further education colleges providing courses for students at any age. Data based on 96 control schools and 101 intervention arm schools with data on student demographics and 76 and 86 schools respectively with data on staff. <sup>1</sup>n (%).

Characteristic	Descriptive		Univariable			Multivariable		
	Did not participate, n = 3,331 <sup>1</sup>	Participated, n = 2,432 <sup>1</sup>	RR <sup>2</sup>	95% CI <sup>2</sup>	p-value	RR <sup>2</sup>	95% CI <sup>2</sup>	p-value
<b>Study week of first contact test</b>								
1	7 (17%)	34 (83%)	1.10	0.77, 1.58	0.60	1.45	0.92, 2.27	0.11
2	70 (25%)	213 (75%)	—	—		—	—	
3	147 (43%)	195 (57%)	0.76	0.58, 0.99	0.041	0.81	0.60, 1.09	0.17
4	138 (41%)	200 (59%)	0.79	0.60, 1.02	0.075	0.96	0.68, 1.36	0.82
5	306 (72%)	118 (28%)	0.37	0.14, 0.95	0.038	0.43	0.20, 0.95	0.036
6	412 (93%)	30 (6.8%)	0.09	0.02, 0.43	0.003	0.12	0.03, 0.49	0.003
8	206 (42%)	280 (58%)	0.77	0.59, 0.99	0.041	0.82	0.62, 1.09	0.17
9	332 (31%)	755 (69%)	0.92	0.79, 1.08	0.32	1.03	0.84, 1.28	0.75
10	1,713 (74%)	607 (26%)	0.35	0.24, 0.50	<0.001	0.39	0.25, 0.60	<0.001
<b>Strata group</b>								
Government-funded, 11-18y free school meals ≤17%	1,018 (51%)	979 (49%)	—	—		—	—	
Government-funded, 11-16y free school meals ≤17%	70 (22%)	252 (78%)	1.60	1.17, 2.19	0.003	1.44	1.06, 1.95	0.020
Government-funded, 11-18y free school meals >17%	987 (66%)	501 (34%)	0.69	0.39, 1.22	0.20	0.71	0.45, 1.11	0.13
Government-funded, 11-16y free school meals >17%	904 (67%)	439 (33%)	0.67	0.31, 1.44	0.30	0.76	0.47, 1.23	0.26
Other	209 (58%)	154 (42%)	0.87	0.51, 1.47	0.59	0.82	0.49, 1.36	0.45
Independent day school	143 (57%)	107 (43%)	0.87	0.64, 1.19	0.39	1.00	0.68, 1.47	>0.99
<b>Ethnicity</b>								
White	2,320 (57%)	1,764 (43%)	—	—		—	—	
Asian	394 (63%)	236 (37%)	0.87	0.49, 1.53	0.62	1.06	0.85, 1.31	0.61
Black	167 (61%)	106 (39%)	0.90	0.62, 1.30	0.57	1.03	0.82, 1.30	0.82
Chinese	12 (23%)	40 (77%)	1.78	1.18, 2.69	0.006	1.72	1.15, 2.55	0.008
Mixed	134 (64%)	75 (36%)	0.83	0.61, 1.13	0.24	0.93	0.79, 1.10	0.39

Other	76 (77%)	23 (23%)	0.54	0.31, 0.92	0.024	0.69	0.48, 0.98	0.037
Prefer not to say	228 (55%)	188 (45%)	1.05	0.70, 1.57	0.83	0.94	0.70, 1.28	0.71
<b>Age group</b>								
11 to 14	1,840 (65%)	984 (35%)	—	—		—	—	
15 to 18	1,400 (53%)	1,258 (47%)	1.36	0.91, 2.03	0.14			
Over 18	91 (32%)	190 (68%)	1.94	1.26, 2.99	0.003			
<b>Sex</b>								
Female	1,619 (54%)	1,390 (46%)	—	—		—	—	
Male	1,712 (62%)	1,042 (38%)	0.82	0.72, 0.93	0.002	0.92	0.82, 1.03	0.14
<b>Participant type</b>								
Student	3,257 (59%)	2,253 (41%)	—	—		—	—	
Staff	74 (29%)	179 (71%)	1.73	1.33, 2.25	<0.001	1.40	1.09, 1.80	0.009
<b>School size, students and staff, RR per 100</b>	1,274 (958, 1,410)	1,070 (801, 1,506)	0.99	0.97, 1.01	0.35	0.99	0.98, 1.00	0.18

0 **Table 3. Associations with participation in lateral flow testing in 5763 contacts in intervention arm schools where the 10 days following the**  
1 **positive test in the index case included  $\geq 1$  school day.** Participant age is omitted from the multivariable model due to collinearity with  
2 participant type. Results from Poisson regression, with robust variance estimation, adjusting variance to account for repeated measurements  
3 from the same school (for univariable and multivariable models). <sup>1</sup>n (%); Median (IQR); <sup>2</sup>RR = Rate Ratio, CI = Confidence Interval. Note week 7  
4 is the school “half-term” holiday, when school-based lateral flow testing was not undertaken. Note participation in the final week of the study  
5 appears lower than in Figure 2, as participation is summarised as completion of  $\geq 3$  LFDs, and contacts in the final week may not have  
6 completed testing before the end of the study.

	End point	Intention to treat			Complier average causal effect	
		aIRR / aOR	95% CI	p value	Effect	95% CI
<b>Primary end points</b>	Rate of COVID-related absence	0.80	0.54, 1.19	0.27	0.61	0.30, 1.23
	Rate of COVID-related absence (aggregated dataset)	0.80	0.62, 1.03	0.085	0.62	0.29, 1.33
	Rate of symptomatic PCR-confirmed infection	0.96	0.75, 1.22	0.72	0.86	0.55, 1.34
<b>Secondary end points</b>	Rate of any absence	0.97	0.82, 1.16	0.77	0.89	0.71, 1.18
	Rate of any community testing PCR-confirmed infection	0.96	0.76, 1.20	0.71	0.88	0.57, 1.41
	Proportion of asymptomatic contacts testing PCR positive on a research PCR test	0.73	0.33, 1.61	0.44	-	-
	Proportion of contacts testing PCR-positive while symptomatic on a routine community test	1.21	0.82, 1.79	0.34	-	-

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**Table 4. Co-primary and secondary end points.** aIRR, adjusted incidence rate ratio for rates; aOR, adjusted odds ratio for proportions; CI, confidence interval.



## 12 Figure legends

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14 **Figure 1. Consort diagram of participating schools for two co-primary outcomes: COVID**  
15 **related school absence (panel A) and symptomatic PCR-positive infection (panel B).** The  
16 former depends on availability of daily school attendance data for students and staff  
17 aggregated at school level. The latter depends on provision of student and staff lists to  
18 enable matching of identifiers with NHS Test and Trace national community testing data.  
19 DfE, UK Government Department for Education. School participation was defined based on  
20 submission of student/staff lists and attendance data for at least part of the study. A total of  
21 2000 schools were notified of the study by email, 226 attended webinars to learn more  
22 about the study, within this group 204 schools were taken through the consent process,  
23 during which 3 decided not to participate further. Of the 39 schools that stopped active  
24 participation between randomisation and the study starting, 26 provided reasons: 20 stated  
25 resource constraints, 3 intervention schools cited concerns about the protocol, 2 control  
26 schools did not wish to be in the control arm, 1 intervention school on local authority public  
27 health advice.

28

29 **Figure 2. Study participation during 27,973 potential isolation school days in 5763**  
30 **intervention arm contacts.** Panel A shows the number of contacts in the intervention arm  
31 by study day, by participation or reason for non-participation. Note the school “half-term”  
32 holiday (31-May-2021 to 04-June-2021). Panel B shows the percentage of contacts in the  
33 intervention arm participating, by study day; the bars are coloured according to the number  
34 of contacts under follow up on a given day. Panel C shows the percentage of contacts  
35 participating in LFDs in 59 intervention arm schools reporting  $\geq 1$  contact affecting school  
36 days. For each contact event return of  $\geq 3$  LFD results or a positive LFD result is used to  
37 summarise participation in the intervention. The bars are coloured by strata group, which  
38 summarises the 9 strata used for randomisation. LFDs, lateral flow tests. Schools with no  
39 contacts participating are shown with a small negative value on the y-axis to aid  
40 visualisation.

41

42 **Figure 3. Co-primary outcome: Percentage of students (panel A) and staff (panel B) absent**  
43 **for COVID-related reasons as a proportion of all those not absent for other reasons by**  
44 **study day.** Note the school “half-term” holiday (31-May-2021 to 04-June-2021).

45

46 **Figure 4. Co-primary outcome: incidence of symptomatic PCR positive results in students**  
47 **and staff by study arm (panel A), and secondary outcome: all PCR positive results (panel**  
48 **B).** Weekly incidence is shown per 100,000 at risk. The shaded area is the mean rate  $\pm 1$   
49 standard deviation using a negative binomial model to account for over-dispersion  
50 ( $\theta=0.28$ ).