SCHOOL INCENTIVE PAYMENTS FOR MENTOR ENGAGEMENT IN TRAINING

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Nimble Trial Report

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The project

The Early Career Framework (ECF) is an entitlement to a fully-funded, two-year package of structured support for all early career teachers (ECTs), including funded time off timetable in the second year of teaching and additional support from school-based mentors. To support the ECF, the Department for Education (DfE) has commissioned an early roll-out of the programme (autumn 2020) ahead of the national roll-out in September 2021. Given the key role that in-school mentors will play in offering early career support, this randomised controlled trial (RCT) was commissioned to test whether providing financial incentives to school-based mentors has an impact on the time ECF mentors spend in training and whether they stay in the role—the impact on retention of ECF mentorship.

This project was a two-armed school cluster-randomised controlled trial to test the impact of providing financial incentives to schools on the amount of time school mentors spent on their training as part of the first year of the early roll-out. A total of 216 schools with 708 mentorships (mentor/ECT pairings) were included in the trial; these were schools that took part in the ECT early roll-out through Ambition Institute or Teach First providers and had at least one mentorship. A payment of £775 per mentorship was made by the DfE directly to the intervention school via the school's local authority or via the school's multi-academy trust. It was suggested, not required, that schools used the additional funds to provide supply cover for mentors so they had more time to commit to their training. Control schools did not receive any payments.

School-mentor engagement was collected from the delivery providers' information systems. The project started in September 2020 and data collection ended in July 2021.

Table 1: Summary of findings

Research question	Finding
RQ1: What is the effect of financial payments to schools on mentors' time spent in training?	There was no evidence that financial payments to schools—as implemented in this trial—had an impact on the time mentors spent on their training (Hedges g = -0.032; 95% CI: -0.251, 0.187; p = 0.775).
RQ2: What is the effect of financial payments to schools on drop out of mentors in the programme as measured at the end of each term?	There was no evidence that financial payments to schools—as implemented in this trial—had an impact on dropout of mentors from the DfE-funded provider-led programme (OR = 1.266; 98.3% CI: 0.601, 2.694; p = 0.452). Termly analysis was not possible from the data collected. Just under three quarters of schools retained all their mentors to the end of the first year of the programme.
RQ3: What is the effect of financial payments to schools on mentorship dropout and school-level mentorship retention measured at the end of each term?	There was no evidence that financial payments to schools—as implemented in this trial—had any impact on mentorships terminating. This was measured by mentorship dropout (OR = 3.224; 98.3% CI: 0.256, 173.771; p = 0.315) and mentorship retention at school level (OR = 0.993; 98.3% CI: 0.438, 2.249; p = 0.982). Across both control and intervention groups, around 80% of schools had all their mentorships running at the end of the first year of the programme. At the other end of the spectrum, less than 3% of schools had all their initial mentorships terminated.

Limitations and interpretation

The 'intervention' intended to pay schools the incentives at the start of the project. Due to issues with establishing an effective process to distribute the payment, the payment was not received by the schools until the second half of the academic year. Although schools were informed of the forthcoming funds throughout the trial, this delay may have meant that schools did not allocate the funds to support school-based mentors, which would significantly compromise the intervention implementation and outcomes. Unfortunately, no process evaluation activities were planned to confirm whether this was the case. Further research is needed to understand how the timing of incentives distribution may affect how schools spend the funds.

Introduction

Background evidence

As part of its teacher recruitment and retention strategy (DfE, 2019), the Department for Education developed the Early Career Framework (ECF) to support teachers in the initial stage of their career. The ECF sets out what newly qualified teachers in their first two years of teaching need to learn. From September 2021, the government funded an entitlement for all early career teachers (ECTs) to access high quality professional development at the start of their career. New teachers will receive development support and training over two years instead of one. The offer for ECTs includes:

- 5% off timetable in the second year of induction, funded by DfE, for all early career teachers to undertake induction activities including training and mentoring;

- freely available high quality development materials based on the Early Career Framework;

- a dedicated mentor and training for these mentors including funding mentor backfill time off timetable to attend training; and

- funding for mentors to spend with early career teachers in the second year of induction—this is based on 20 hours of mentoring across the academic year.

The government took a phased approach to introducing these reforms, starting with early roll-out of the DfE-funded provider-led programme[1] to schools in the North East, Greater Manchester, Bradford, and Doncaster in the academic year 2020/2021. The purpose of the early roll-out, which is the subject of a separate evaluation, is to evaluate whether the ECF reforms meet the needs of teachers and schools working in a range of contexts and to inform the national roll-out. Four early providers, which specialise in teacher training and development, were appointed by the DfE to deliver the early roll-out: Ambition Institute, Education Development Trust, Teach First, and UCL Institute of Education. The National Foundation for Educational Research (NFER) are undertaking the evaluation of the early roll-out of the ECF. The primary aim of the evaluation is to explore the impact of the early roll-out on the retention rate of ECTs in the teaching profession. Secondary aims include an assessment of the impact on the retention rate of ECTs within their original schools, as well as on teaching quality and self-efficacy. The evaluation will run from July 2020 to December 2023. This report presents the findings from a separate but related evaluation to assess the impact of incentive payments made to early roll-out schools to support mentors' engagement in their training as part of the DfE-funded provider-led programme. The purpose for conducting this embedded trial is to test specific elements of the delivery and understand the factors that influence take-up to maximise implementation fidelity. The national roll-out of the ECF began in September 2021 and has been commissioned to be evaluated as a separate piece of work.

It has been well documented that there is a high drop-out of teachers in the early years of their careers, with leaving rates highest in the first few years after qualifying and within shortage subjects (Worth and De Lazzari, 2017).[2] Recent evidence shows that retention rates are falling over time. In 2020, 68.46% of teachers were still working in the profession after five years compared to 74.2% in 2010 (DfE, 2021; Fullard and Zuccollo, 2021). Mentoring and induction appears to be a significant factor associated with higher early career teacher retention alongside pre-service training and factors related to job satisfaction (Kelly *et al.*, 2019; Ingersoll and Strong, 2011). The quality of the mentoring and mentors in the same subject appear particularly significant to increasing retention (Smith and Ingersoll, 2004; DeAngelis *et al.*, 2013). While mentor training and preparation has been shown to be important (Aspfors and Fransson, 2015), the quality and provision is of variable quality (Hobson *et al.*, 2009). The ECF acknowledges the importance of the role of the mentor and mentoring and aims to ameliorate the barriers to quality mentoring support. A part of this is through providing mentors with training to support them in their role.

This project aims to assess whether payments of £775 per mentor would enable mentors to spend more time in training by covering the cost of supply cover and releasing them from some of their teaching commitments. Underpinning this is the assumption that lack of time is a key factor that could present a barrier to engagement in mentoring and associated training. Given the well-documented heavy workload for teachers, this appears a reasonable assumption (DfE, 2018). A qualitative study drawing on nearly 300 interviews and case study visits in England found that time was reported as the main barrier to providing quality support of NQTs and in terms of attending any training for their role (DfE, 2019d). Other international evidence drawing on research from Scotland, Malta, and Denmark found that practical challenges in terms of finding time to undertake the mentoring activities are raised consistently as well as mentors needing more training in terms of how to effectively mentor (Shanks *et al.*, 2020).

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[1] The DfE-funded provider-led programme is a package of training and professional development for early career teachers and their mentors. It is underpinned by the early career framework: **https://www.gov.uk/government/collections/early-career-framework-reforms**.

[2] Shortage subjects are mathematics, science, and languages.

The aim of the trial was to consider whether the financial incentives impacted on the time mentors spent in training to prepare for their role—one specific factor in the theory of change for the ECF. The nature of the design was a light-touch design drawing on data already being collected through management information (MI) systems. This study does not make any assessment of mentors' views on the quality of the training they received nor the impact of this training on mentors' knowledge of effective mentoring: these questions are explored further as part of the main ECF evaluation.

ECF early roll-out

This trial considers an element in the delivery of the early roll-out of the ECF, that of mentor training. Mentor training is part of the DfE-funded provider-led programme that has been rolled out to schools in some areas in the academic year 2020/2021 as part of the early roll-out of the ECF. The logic model for the early roll-out can be found in Appendix E. The hypothesised logic model shows that mentor training is one of the core elements of the programme. It is based on the principle that better-trained mentors, more aware of good coaching practices, can—through frequent, one to one, within-school coaching sessions with their mentees—contribute to the improved retention of ECTs in their schools and beyond as well as to improved pupil attainment. Additional intermediary outcomes include improved confidence in mentoring, improved job satisfaction and wellbeing, and sharing of best practice through a professional community of support.

Each early career teacher must be assigned a mentor from their school. Mentors are expected to hold Qualified Teacher Status (QTS) and have the necessary skills and knowledge to work successfully in this role. They should also be given adequate time to carry out the role effectively and to meet the needs of the ECT. Mentors should share their expertise with ECTs to help them to develop and feel prepared for their future careers. There is a larger role for mentors under the ECF than preceding models and the induction period for early career teachers has been extended from one to two years. Training and resources have been developed by the providers for both ECTs and mentors based on the ECF. All of the training and support to mentors was delivered online due to the COVID-19 pandemic.

As part of the early roll-out, mentors should receive:
- 36 hours of training over the two-year induction period based on the ECF;
- high-quality resources to support their mentoring;
- funding to cover mentors' time with the mentee in the second year of teaching; and
- two years of funded, provider-led training.

After discussions with the DfE, it was agreed to focus on two of the early roll-out providers, Ambition Institute and Teach First.

Ambition Institute's offer to mentors included:

- induction sessions—all mentors joining were able to work through four hours of induction content prior to a 90-minute webinar on instructional coaching and delivery practice; and
- 'coaching on coaching' webinar sessions—two in year one, one in year two.

Teach First's offer to mentors included:

- online training—to develop skills in assessing teacher progress, providing effective feedback, using deliberate practice to accelerate progress, and how to provide further challenge to high performing early career teachers (there are over 200 video exemplifications for mentors on the online platform);
- webinars—delivered by Teach First Development Leads[3] to complement their early career teachers' modules so they can tailor their support; and
- mentor group seminars—one-hour sessions every half term to work with other mentors to develop their use of instructional coaching.

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[3] Development Leads work with trainees, mentors, and schools in order to support trainees through their training.

School incentive trial

The description above outlines the role of the mentor and mentor training in the early roll-out of the DfE-funded provider-led programme.[4] The research question in this trial is whether the payment of £775 per mentor paid to schools had any impact on the amount of time mentors spend on their own training during year one of the early roll-out of the ECF. The amount of £775 was calculated by the DfE based on teacher salaries and how long mentors were expected to spend on training activities. This trial is testing a specific implementation approach used as part of the broader programme, namely the payment of £775 per mentor to schools on a per-mentor basis to help support schools in enabling mentors to engage in their training. Providers informed NFER of the numbers of mentors in each school in the trial after they had completed recruitment to the programme in autumn 2020. NFER then passed on these details to the DfE for those schools in the intervention group in order for them to process these payments. The main mechanism through which the payment was thought to potentially improve engagement in training time was through covering the costs of lessons for mentors (that is, by providing teaching cover). The wording of the letter from the DfE informing schools of the payment stated 'how you choose to use this money to support [the mentor's] participation in the programme is at your discretion, but it might include securing supply cover to give your mentor the time to attend training sessions' (see Appendix G). While there is a clear suggestion for what the additional resource could be spent on, the letter also states that schools can spend the funding as they see fit provided it is 'used to support the mentors' participation in the programme'. The quality of the training received by mentors or whether the training itself leads to improved support for early career teachers are important questions but not ones considered as part of this evaluation.

In designing the evaluation we felt that the idea of 'mentorship' was an important one. We use 'mentorship' to define the partnership between the early career teacher and a mentor, though this mentor does not have to remain the same throughout the period. A mentorship continues as long as the early career teacher has a mentor associated with them (see Appendix F). We introduced this measure as we felt that the paramount consideration is whether the early career teacher is being mentored and therefore continuing to receive the programme as intended. Early turbulence in changes to mentors during initial stages of the programme would otherwise have not given a fair representation of what was happening in schools.

INTERVENTION NAME	School incentive payments for mentor engagement in training
WHY (THEORY/RATIONALE)	The ECF emphasises the importance of in-school mentors supporting early career teachers. The incentives intend to provide finance for schools to support mentors to attend their training.
WHO (RECIPIENTS)	Schools working with Teach First or Ambition Institute as part of the early roll-out of the ECF.
WHAT (MATERIALS)	None.
WHAT (PROCEDURES)	A payment of £775 per mentor/early career teacher pairing (mentorship).
WHO (PROVIDER)	Department for Education (commissioner), Ambition Institute, and Teach First (providers).
HOW (DELIVERY MODE)	A sum of £775 per mentorship (mentor/early career teacher pairing) is paid by the DfE to the school at onset of the programme (autumn term 2020).

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[4] For further detail on the evaluation of the early roll-out see: **https://educationendowmentfoundation.org.uk/projects-and-evaluation/projects/early-career-framework-early-roll-out**. The ECF provides a standards framework for early career teachers, details of which can be found here: **https://www.gov.uk/government/publications/early-career-framework**.

WHERE (LOCATION)	Not applicable.
WHEN AND HOW MUCH (DOSAGE)	The payment was a one-off payment at the onset of the programme (autumn term 2020). The amount was calculated as the number of mentorships (mentor/early career teacher pairings) multiplied by £775.
TAILORING (ADAPTATION)	The financial mechanisms in terms of how payments are received by schools varies depending on the governance of the school.

Research questions

Primary research question

- RQ1: What is the effect of financial payments to schools participating in the early roll-out of the ECF on the level of mentor engagement in training as measured by the time spent engaged in training?

Secondary research questions

- RQ2: What is the effect of financial payments to schools on the retention of mentors in the programme as measured at the end of each term?
  RQ3: What is the effect of financial payments to schools on the retention and dropout of mentorships as measured at the end of each term? If the early career teacher has a mentor (whether this is their original mentor, an additional mentor to replace the original mentor, or another early career teacher's mentor) then the mentorship is considered to be continuing.

Deviation from protocol

We had originally planned to also study the effect of financial payments to schools on the replacement of mentors who drop out in (a) schools where there is only one mentor and (b) schools where there is more than one mentor. However, we were unable to collect this data due to the management information systems not capturing adequate data on why mentorships ended (that is, whether the mentor or the early career teacher withdrawing led to the termination).

Methods

Trial design

The intervention considered here is the payment of incentives to support mentor engagement in their training as provided by two providers, Ambition Institute and Teach First. The trial is assessing the impact of an implementation approach on an output measure (mentor training time) that is considered to be important in achieving the programme objectives downstream. The long term outcome of the ECF is higher teacher retention and increased pupil attainment (see evaluation logic model in Appendix E).

The trial is a two-arm cluster randomised trial with one arm receiving incentive payments and one not. School level allocation was used as payments would need to be made to schools and asking schools to allocate different levels of resource to different mentors within their schools would not be practical and would have ethical issues. Cluster size is the number of mentorships in a school and no upper limit was applied. In about half of the schools (49.3%), there were two or fewer mentorships per school and the highest number of mentorships is 15 (see Figure 2).

Table 1: Trial design

Trial design, including number of arms		Two-arm cluster randomised controlled trial with random allocation at school level.
Participants	inclusion criteria	Schools signed up to take part in the early roll-out of the DfE-funded provider-led programme with either Teach First or Ambition Institute by October 2020. Schools must have had a mentorship (mentor/early career teacher pairing) running in their school at onset of the programme (that is, autumn term 2020).
	exclusion criteria	None.
	target number	300 schools and 600 mentorships.
Unit of randomisation		School.
Stratification variables (if applicable)		Provider (Ambition Institute and Teach First).
Primary outcome	variable	School-level average time spent engaged in mentor training activities in a mentorship (minutes) (RQ1).
	measure (instrument, scale, source)	School-level bespoke measure. Total time of attendance at seminars, coaching sessions, and time spent accessing materials on the providers' online platforms divided by the number of early career teachers in the school (source: providers' MI systems).
	timeline of data collection	October 2020 to August 2021.[5]
Secondary outcome(s)	variable(s)	(1) Mentorship dropout (RQ3) (2) Mentorship retention (RQ3) (3) Mentor retention (RQ2)
	measure (instrument, scale, source)	(1) Mentorship dropout: **all** the mentorships established in the school at the beginning of the programme are terminated. (2) Mentorship retention: all of the mentorships established in the school at the beginning of the programme are still ongoing. (3) Mentor retention: all of the mentors in the school at the beginning of the programme are still enrolled in the programme. (Source: providers' MI systems.)
	time of collection	October 2020 to August 2021
	adjusting for multiple comparisons?	Yes (Bonferroni correction)

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[5] The providers recorded events in their MI systems as they happened throughout the October 2020 to August 2021 period. However not all the events in the trial's lifecycle were time stamped and the collection of data was continuous but not necessarily happening at regular intervals.

Although the data possesses a nested structure with mentorships clustered into schools, it was not amenable to multilevel modelling. As described in Figure 2, a large number of the schools in the sample were running a small number of mentorships throughout the trial, typically less than four, therefore multilevel models would not be able to estimate mentorship-level variance.

Figure 2: Frequency of number of mentorships per school at the onset of the trial
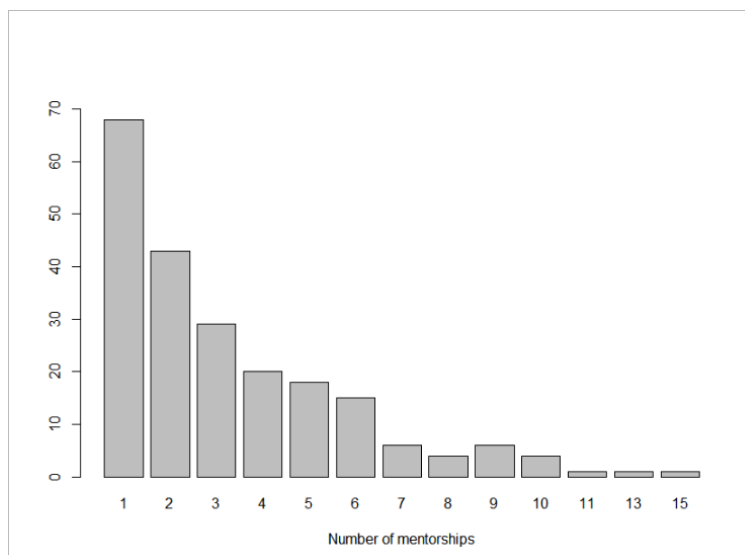
Given the clustered nature of the data and the unsuitability of multilevel models, we have decided to consider instead outcomes that are aggregated or summarised at school level and run the analyses resorting to single level OLS and logistic regression models.

We originally include a fourth secondary binary outcome—*mentor replacement*—that identified if the mentors in a school who left the programme during the first year were replaced with an additional mentor. However, it was not possible to implement it as the data we obtained from the providers would not allow us to identify whether a mentorship was terminated due to the withdrawal of the mentor or of the early career teacher consistently. We had also planned to report secondary outcomes 1, 2, and 3 (for research questions 2 and 3) on a termly basis but the data was not available in a way that allowed us to report this. It appeared that the data available through the MI systems was not reliable enough to give a termly snapshot of mentors and their ECTs. This was likely to be due to the schools not updating the MI systems frequently enough, so detailed changes were not always captured in 'real time'.

Although not specified in the analysis plan, we have decided to run the secondary analysis adjusting for the simultaneous testing of three hypotheses by introducing the Bonferroni correction that is used to compensate for the increase in likelihood of occurrence of false positives when testing several hypotheses simultaneously. When applying the Bonferroni correction to trial of n hypotheses at a confidence level of 95%, each individual hypothesis is tested considering a significance level $\alpha^* = 0.05/n$, which for three hypotheses corresponds to considering a cut-off value for p of 0.05/3 = 0.016.

Participant selection

Recruitment to the trial took place from schools that had opted to take part in the early roll-out of the ECF from September 2020. A separate programme was rolled out in autumn 2020 for recently qualified teachers whose training had been impacted by the COVID-19 pandemic.

While it would have reduced complexity in terms the measures needed (see Outcome section) and streamlined communication and data collection, it was not possible to achieve the desired sample size with one of the four providers of the early roll-out. After initial meetings with the DfE and some providers, the decision was taken to work with two providers as this should provide a large enough number of schools to detect an appropriate minimum detectable effect size (see section on sample size). Both Ambition Institute and Teach First have developed online platforms that capture management information that could be used for the trial.

In early October the two providers shared the contact details of the key contact from all schools that had signed up to the early roll-out using secure data portals set up by NFER (see Appendix B on data protection). All the schools that had signed up to take part in the early roll-out of the ECF through Ambition Institute and Teach First by the beginning of October 2020 were emailed by NFER and asked if they would like to take part in the trial. To be eligible to take part, schools had to:

- be signed up to the early-roll out of the ECF with Ambition Institute or Teach First; and
- have at least one mentorship starting in the autumn term 2020.

A total of 365 letters were sent out via email inviting schools to take part in the trial and a total of 230 (63%) signed up and were randomised. Shortly after the initial randomisation, the evaluation team realised that one of the lists contained both individual schools and multi-academy trusts (MATs). A total of five MATs were randomised. In four instances several schools that made up the MAT and the MAT itself as a separate unit had been randomised separately and, in a separate instance, a MAT was randomised but not its associated schools.

The evaluation team decided to exclude the randomised MATs from the analysis as invalid cases while inviting the schools in the five MATs that were not randomised as individual schools to join the trial. The MAT schools that were eligible and willing to take part in the trial could then be randomised at a later stage and considered to have joined the trial on a second stage of recruitment. A second round of letters were sent out to the key contacts in these five MATs detailing which of their schools (if any) were signed up to the trial and gave them the opportunity to sign up any further schools in their MATs that met the eligibility criteria. No further schools were signed up. Related to MATs signing up as schools, it is also important to highlight that two schools in the control group received incentive payments in error. The evaluation team decided to include them in the analysis (in the control group) as although these schools received payments, they were not informed that they would be receiving them (conversely they were told they were in the control group). When the payments were made in the spring term there was no reference to the trial or mentors in the transaction. It was considered by the DfE and the evaluation team that the payments were likely to be considered by the school as part of the standard payments made to schools for taking part in the early roll-out of the ECF.

Outcome measures

A bespoke measure was designed relying on the information collected in the providers' customer relationship management (CRM) systems. The record of participation in training across the different types of provision (live seminars, coaching, and online self-study)[6] was combined in order to estimate how long, measured in minutes, the mentor(s) associated to a given mentorship engaged in training. The initial time-per-mentorship estimate was calculated differently according to provider: while Ambition Institute was able to provide actual estimates of the time spent in each specific activity, Teach First was only able to provide lists of the activities each participating mentor attended and estimates of the expected time spent in each activity (see Figure 4).

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[6] The provisions are specific to each provider and differ across the programme.

Figure 4: Expected time spent on each activity in the Teach First DfE funded provider-led programme (as stated by the provider)

Mentor Brightspace Content	Mins	Topics
Overview of Mentor Induction	0	2
Session 1 (Induction)	60	12
Session 2 (Induction)	50	9
Session 3 (Induction)	75	11
Session 4 (Induction)	30	8
Weekly Content Overview Videos (5-6 8min videos per Module)	232	31
Mentor Training Sessions (hosts recordings)	0	11
Seminars (1 per module)	90	0
ECT / Mentor Meetings (1 per week)	60	0
Totals:	597	84

The total engagement in training time for Ambition Institute mentorships was simply derived by adding up the times corresponding to all the activities the mentor(s) associated to the mentorship took part in, while for Teach First mentorships the total engagement in training time was estimated by crediting 90 minutes to each seminar attended by the mentor(s) associated to the mentorship and 5.32 minutes to each completed topic.[7] We acknowledge that the latter procedure corresponds to a cruder estimate of engagement in training time for Teach First mentorships but unfortunately this provider did not record the times spent in specific activities in its CRM system.

The school-level primary outcome measure was computed as the total engagement time measured across the mentors of each school divided by the number of early career teachers being mentored, which corresponds to an estimate of the average length of time spent in mentor training per mentorship. If a mentor dropped out of the programme but a replacement was found for the ECT's mentorship, then the new mentor's training time was included in the measure. We were thus defining the mentorship by the original ECT, that is, the original list of ECTs enrolled at the beginning of the programme determined the mentorships in the analysis.[8] The aim of the programme is to train ECTs and provided the paired ECT still had a mentor (whether a replacement or the original mentor) the programme did continue. Following the same rationale, mentors that were not paired with ECTs were excluded from the analysis as not meeting the eligibility criteria for the trial. For the primary outcome measure, when an ECT withdrew from the programme then the ECT, as well as their associated mentor training time, were not included in the analysis (unless the mentor carried on mentoring another ECT that completed the first year of the programme).

We averaged the total time spent on mentor training by dividing by the number of ongoing mentorships in a school at the end of the first year of the programme including in the measure all the training time associated to active mentorships. One consequence of this is that schools where more replacement took place had the opportunity to receive more total hours mentor training per mentorship. We have chosen to divide by the number of ECTs (mentorships) rather than number of mentors as the programme (including the mentor training) is ultimately for the benefit of the ECT and the training time of replacement mentors needs to be accounted for as having been necessary to the maintenance of the mentorships.

We defined three secondary binary measures: *mentorship dropout*, *mentorship retention*, and *mentor replacement.* The first and second variables are related. *Mentorship dropout* indicates if *none* of the mentorships in a school are ongoing and *mentorship retention* indicates if *all* of the mentorships in a school are still ongoing. If there is only one mentorship in a school then the two variables will be identical but if not, the two variables measure different ends of the scale—one where all mentors drop out and the other where no mentors drop out. It is useful to include both variables in the analysis as *mentorship dropout* could be considered to reflect implementation failure where the programme has failed in a school as no mentorships have continued. The *mentorship retention* variable captures *any* termination of mentorship as it signals when just a single mentorship in a school has ended. Mentorship can end due to ECT dropout, for personal reasons, for reasons associated with the school, as well as factors relating to the programme itself.

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[7] The total number of topics is 84 and the total time corresponding to those topics is 60 + 50 + 75 + 30 + 232 = 447 minutes, which corresponds to an average time of 5.32 minutes per topic.
[8] We have included all the early career teachers that enrolled, although some of them were only assigned a mentor at a later stage. The training time of the mentor was accounted for, even if the training occurred before the assignment of the mentor to an early career teacher.

Mentor retention asks whether all the mentors originally enrolled in the programme remain in the programme in a particular school. Mentors could leave the programme for a range of reasons including if their ECT drops out. These first three measures were calculated as intended from the management information (MI) provided from Ambition Institute and Teach First.

The last variable, *mentor replacement*, measures whether any mentors that dropped out of the programme have been replaced. This last outcome measure could not be calculated as the data available across the two providers did not consistently report whether a mentorship ended due to the ECT or the mentor withdrawing from the programme.

Sample size

Providers were aiming to recruit around 200 schools each to the early roll-out (total 400 schools), which would have powered the trial to detect an effect size as small as 0.28. At the time of designing the trial, a success rate of 75% from the recruitment of schools to take part in the trial was taken as a reasonable estimate, and out of proposed 400 schools we estimated that 300 would take part. This yielded an effect size of 0.33 at protocol. In fact, at the time of recruitment to the trial the providers had recruited slightly fewer schools, which meant that 365 schools were approached to take part. Subsequently only 225 signed up, which increases the minimum detectable effect size (MDES) to 0.38 at randomisation as shown in Table 2. Of the initially randomised schools, 212 reached the end of the trial, which lead to an increase of the MDES from 0.38 to 0.39 (see Table 3 for detail).

Power calculations were completed using a bespoke Excel spreadsheet assuming a two-group independent sample t-test design and a pre-post correlation of zero, as there was no baseline for this trial.

Randomisation

Randomisation took place in October 2020. As specified in the protocol, the project statistician was not blinded to group allocation. The randomisation, stratified by provider in order to guarantee a balanced distribution of the incentives, was based on two lists of schools provided by Ambition Institute and Teach First.

The R code used to perform the stratified randomisation is included in Appendix D. All the calculations were performed in R 4.0.3.

Statistical analysis

The primary and secondary analyses were undertaken as planned and following EEF guidelines. Intention-to-treat was assumed in both cases. There was no evidence to suggest that payment of £775 as a financial incentive to schools has an impact on the amount of time the mentors spent on their training or their dropout and retention rates in the programme.

As described in the Statistical Analysis Plan,[9] multilevel modelling was rejected as the majority of schools in the sample only had a few mentors (usually one or two). Instead, given the clustered nature of the data, outcomes were aggregated or summarised at school level and analyses were run as single level OLS (primary outcome) and logistic regression models (secondary outcomes). A summary of the analysis approach for each research question can be found in Table 2.

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[9] **https://educationendowmentfoundation.org.uk/projects-and-evaluation/projects/early-career-framework-early-roll-out**
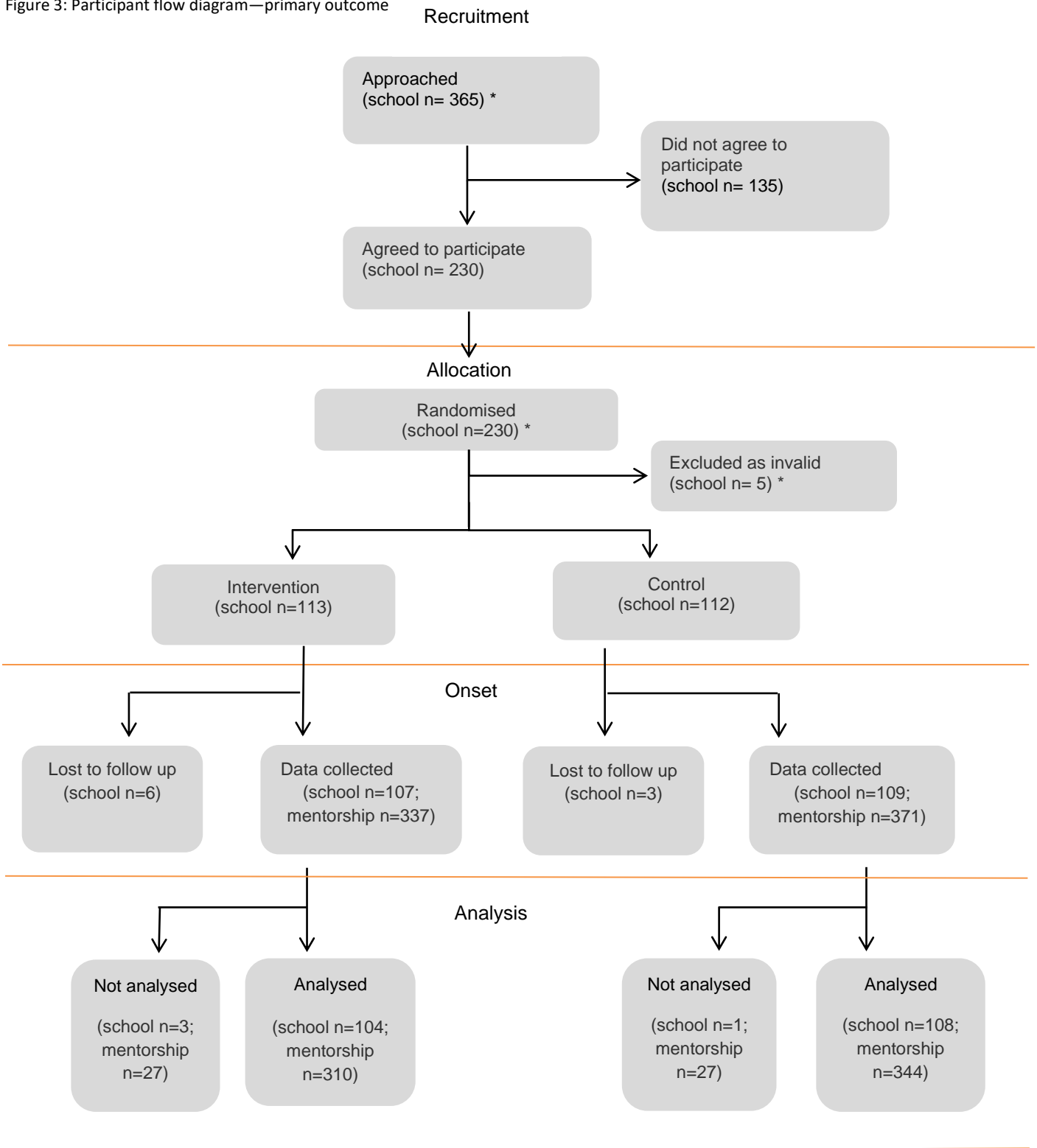
Table 2: Statistical analysis summary

RQ	Sample	Dependent variable	Independent variable	Control variable(s)	Analytical method	Interpretation
RQ1 (prim.)	212 schools	Average training time per mentorship (minutes)	Intervention	Provider	OLS model	No evidence of the intervention having had an effect on the outcome
RQ2 (sec.)	216 schools	Mentor retention	Intervention	Provider	Logit model	No evidence of the intervention having had an effect on the outcome
RQ3 (sec.)	216 schools	Mentorship dropout and mentorship retention	Intervention	Provider	Logit model	No evidence of the intervention having had an effect on the outcome
RQ4 (sec.)	N/A	Mentor replacement	Intervention	Provider	Logit model	It was not possible to run this analysis

Findings

Participant Flow

Figure 3: Participant flow diagram—primary outcome

Recruitment

Approached
(school n= 365) *

Did not agree to
participate
(school n= 135)

Agreed to participate
(school n= 230)

Allocation

Randomised
(school n=230) *

Excluded as invalid
(school n= 5) *

Intervention
(school n=113)

Control
(school n=112)

Onset

Lost to follow up
(school n=6)

Data collected
(school n=107;
mentorship n=337)

Lost to follow up
(school n=3)

Data collected
(school n=109;
mentorship n=371)

Analysis

Not analysed

(school n=3;
mentorship
n=27)

Analysed

(school n=104;
mentorship
n=310)

Not analysed

(school n=1;
mentorship
n=27)

Analysed

(school n=108;
mentorship
n=344)

* Of the 365 schools initially approached to take part in the trial, five were in fact multi-academy trusts (MATs). This was not realised until after randomisation. The decision was taken to remove the MATs from the trial. See the section on Participant Selection for further details. Where individual schools within the MATs signed up to the trial they were retained.

Minimum Detectable Effect Size (MDES)

The MDES at different stages of the study is presented in Table 3.

Table 3: Power analysis and MDES summary

		Protocol	Randomisation	Analysis
Minimum Detectable Effect Size (MDES)*		0.33	0.38	0.39
Alpha		0.05	0.05	0.05
Power		0.8	0.8	0.8
One-sided or two-sided?		Two	two	Two
Single-level or clustered design?		single	single	single
Number of schools	**Intervention**	150	113	104
	Control	150	112	108
	Total	300	225	212

* There is no baseline for the trial so pre-post correlation is assumed to be zero.

Attrition

Table 4 shows that there was some attrition between randomisation and onset of the data collection around mentor engagement. We have taken the point at which the majority of the recruitment to the early roll-out was completed as the point at which no further mentors could be added to the trial. We refer to this as the 'onset' of the data collection and was in December 2020. Some schools were lost from the trial between randomisation and onset due to schools withdrawing from the programme.

It was not always possible to obtain usable data for the analysis. Where we have attrition between onset and analysis this could be due to a range of reasons including schools withdrawing from the early roll-out of the DfE-funded provider-led programme or available data not providing enough information to allow the necessary analysis. The systems used by providers were in their infancy and while in the majority of cases the necessary data was adequately captured in the providers' online platforms and MI systems, there were cases where this was not possible. Additional tests were undertaken as described in the Sensitivity Analysis section to test whether this attrition was likely to lead to any bias in the results.

Table 4: Attrition from the trial—number of schools (primary outcome)

		Intervention	Control	Total
Number of schools	Randomised	113	112	225*
	Onset	107	109	216
	Analysed	104	108	212
School attrition (from randomisation to analysis)	Number	9	4	13
	Percentage	8.0 %	3.6 %	5.8 %

* 225 schools were randomised (this number excludes the five MATs that were excluded as invalid cases).

Table 5: Attrition from the trial—number of mentorships (primary outcome)

		Intervention	Control	Total
Number of mentorships	Onset	337	371	708
	Analysed	310	344	654
Mentorship attrition (from onset to analysis)	Number	27	27	54
	Percentage	8.0%	7.3%	7.6%

Participant characteristics

The Participant Characteristics tables below were compiled by linking the schools taking part in the trial to the relevant information contained in the 2019 edition of NFER's registry of schools, the most up to date edition at the time of the evaluation (August 2021).

Table 6: Participant school characteristics—randomisation

% pupils eligible for FSM 2019 (5 pt scale)	Intervention Group		Control Group		National level*
	Count	Percentage	Count	Percentage	Percentage
Lowest 20%	5	4.4 %	5	4.5 %	17.5%
2nd lowest 20%	10	8.8 %	13	11.6 %	17.5%
Middle 20%	15	13.3 %	20	17.9 %	17.2%
2nd highest 20%	31	27.4 %	33	29.5 %	17.5%
Highest 20%	48	42.5 %	35	31.2 %	17.3%
Unknown (FSM 2019 not reported by school or missing)	4	3.5 %	6	5.4 %	13.0%
Rural/urban	Count	Percentage	Count	Percentage	Percentage
Rural	12	10.6 %	7	6.2 %	24.8%
Urban	101	89.4 %	105	93.8 %	75%
Unknown or missing	-	-	-	-	0.2%
School governance	Count	Percentage	Count	Percentage	Percentage
Academy or free school	69	61.1 %	76	67.9 %	37.1%
Maintained	42	37.2 %	35	31.2 %	50.7%
Independent	-	-	-	-	^
Unknown or missing	^	^	^	^	^
Latest Ofsted rating (overall effectiveness)	Count	Percentage	Count	Percentage	Percentage

Outstanding	15	13.3 %	10	8.9 %	16.9 %
Good	55	48.7 %	62	55.4 %	54.9 %
Requires improvement	29	25.7 %	24	21.4 %	9.3%
Inadequate	5	4.4 %	4	3.6 %	2.6 %
Unknown or missing	9	8 %	12	10.7 %	16.4%

* English primary, secondary, and nursery schools

Table 7: Participant school characteristics—analysis)

	Intervention group		Control group		National level*
% pupils eligible for FSM 2019 (5 pt scale)	Count	Percentage	Count	Percentage	Percentage
Lowest 20%	5	4.8 %	4	3.7 %	17.5%
2nd lowest 20%	8	7.7 %	13	12 %	17.5%
Middle 20%	15	14.4 %	19	17.6 %	17.2%
2nd highest 20%	27	26 %	32	29.6 %	17.5%
Highest 20%	46	44.2 %	34	31.5 %	17.3%
Unknown (FSM 2019 not reported by school or missing)	3	2.9 %	6	5.6 %	13.0%
Rural/urban	Count	Percentage	Count	Percentage	Percentage
Rural	11	10.6 %	6	5.6 %	24.8%
Urban	93	89.4 %	102	94.4 %	75.0%
Unknown or missing	-	-	-	-	0.2%
School governance	Count	Percentage	Count	Percentage	Percentage

Academy or Free school	64	61.5 %	74	68.5 %	37.1%
Maintained	39	37.5 %	33	30.6 %	50.7%
Independent	-	-	-	-	^
Unknown or missing	^	^	^	^	^
Latest Ofsted rating (overall effectiveness)	Count	Percentage	Count	Percentage	Percentage
Outstanding	14	13.5 %	10	9.3 %	16.9 %
Good	51	49 %	60	55.6 %	54.9 %
Requires improvement	28	26.9 %	23	21.3 %	9.3%
Inadequate	5	4.8 %	4	3.7 %	2.6 %
Unknown or missing	6	5.8 %	11	10.2 %	16.4%

* English primary, secondary, and nursery schools, ^ Removed to minimise risk of disclosing schools due to low cell count

Outcomes and analysis

Primary analysis

The primary analysis aimed to determine if giving the schools in the programme a financial incentive (in addition to funding received as part of the existing programme) had an effect on how long their mentors spent engaging in training activities.

Histograms describing the distributions of average training time for mentorship can be found in Appendix I. Each histogram includes a normal curve with the same mean and standard deviation of the underlying distribution. Included in the same appendix are also:

- a QQ plot of fitted versus observed residuals and some goodness of fit tests (KS; Kolmogorov-Smirnov);[10]
- dispersion—tests if the simulated dispersion is equal to the observed dispersion, and
- outliers—tests if there are more simulation outliers than expected.

Although the primary outcome displays deviation from normality, the QQ plots of fitted versus observed residuals and the tests suggest that the analysis model can fit the data adequately.

Displayed in Table 8 are the findings for the primary analysis. As described in the Methods section, we have run an ordinary least square model and presented its results as effect sizes (Hedges' g). The effect size for the primary analysis was -0.032 (-0.251, 0.187). Due to the uncertainty around this result, as indicated by the confidence interval that

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[10] See Hartig (2021) for a description of the diagnosis features and goodness of fit tests include in the DHARMa package.

straddles zero, we are unable to conclude that the payment of school incentives had an effect on the average mentor training time invested in the early career teachers' mentorship.

Table 8: Primary analysis

	Unadjusted means				Effect size		
	Intervention group		Control group				
Outcome	n (missing)	Mean (S.D.)	n (missing)	Mean (S.D.)	Total n (intervention; control)	Hedges g (95% CI)	p-value
Average training time per mentorship (minutes)	104 (9)	406 (219.2)	108 (4)	422 (244.4)	212 (104;108)	-0.032 (-0.251, 0.187)	0.775

Termly analysis

We had originally envisaged to take advantage of having access to data collected termly throughout the academic year to run two additional analyses in order to better understand the profile of mentor engagement with training. However, due to the timing of feedback received from schools being inconsistent and miscommunication on the timing of intervention activities, it was not possible to map a considerable number of observations to a specific school term and, as such, we could not analyse the evolution of training over the school year consistently.

Sensitivity analysis of the primary analysis with imputed missing data

As the trial relied on data collected from the providers' MI systems, when designing the analysis we did not expect missingness levels to exceed 5%. This turned out not to be the case as 13 out of 225 (5.8%) of the randomised schools ended up not being included in the primary analysis due to the termination of mentorships throughout the duration of the trial. It is also necessary to take into account that the dropout of early career teachers from the trial and the termination of mentorships had an impact in terms of the accuracy of measurement of the primary outcome, as the dropouts lead to the removal of data from the analysis and the estimation of averages based on fewer cases .

As per the Statistical Analysis Plan (SAP) we have evaluated the impact of the withdrawal of data from the analysis, either due to schools being lost to follow up or due to mentorship termination, by fitting logistic models that determine if a school not submitting data or submitting just data from part of its initial mentorships was conditional on intervention allocation and also by running sensitivity analysis. In deviation from the SAP we considered not only loss of data between the randomisation and analysis stages but also between the onset and the end of the trial.

In both cases the logistic regression models were run with provider and the school characteristics as covariates—proportion of FSM-eligible pupils within the school, whether the school is rural or urban, type of school governance, and latest Ofsted rating—and, in the case of the onset to end of trial model, number of initial mentorships. Intervention allocation was not a significant predictor of loss of mentorship/school data in any of the models.

We have run the primary ITT analysis in datasets with imputed outcome values based on the same variables included in the logistic regression models described above for all the schools that were absent at analysis stage or that terminated

mentorships during the first year of the programme. The missing values for the primary outcome and covariates were imputed using predictive mean matching, with five plausible values derived for each case. The primary ITT model was re-run on the five sets of imputed plausible values and the estimates for each model were then pooled into a single set of estimates and standard errors that was compared to the results of the original analysis. The comparison results can be seen in Table 9. Although the magnitudes of the coefficients and standard errors are inflated in the pooled estimate models, the signal of the randomisation coefficient is not significantly different from zero in the three cases and so the sensitivity analysis also does not support the case for the intervention having had, on average, an effect in mean time spent on mentorship training.

The results from the sensitivity analysis are consistent with the primary ITT model and confirm that introducing an incentive payment does not seem to have an impact on average time spent on mentorship training at a school.

Table 9: Comparison of primary ITT model and pooled estimates from imputed values models

Model including all the schools at	Randomisation group (intervention) coefficient	Standard Error	p-value
Pooled estimate (randomisation)	-34.43	39.95	0.316
Pooled estimate (onset)	-13.23	32.79	0.688
Primary ITT model	-7.4	25.83	0.775

Secondary analysis

The secondary analyses assessed the impact of giving schools an extra financial incentive on *mentorship retention*, *mentorship dropout*, and *mentor retention* at the end of the first year of the programme. These measures aimed to assess whether schools were able to support and sustain the programme's mentorships and also how much it was possible to maintain the original and desirable setting of pairing an ECT to a mentor throughout the duration of the programme. We have fitted logistic regression models that estimated the odds of schools receiving extra incentives retaining mentorships and mentors or terminating the programme when compared schools in the control group. As in the primary analysis we have controlled for provider as the randomisation stratifier.

A fourth secondary outcome, *mentor replacement*, that described if the withdrawal of mentors from the trial led to their replacement or the termination of the mentorships was also considered in the SAP, however, it was not possible to derive this binary measure as the data obtained from the providers did not allow us to identify the reasons for the termination of a substantial number of mentorships. We were able to pinpoint the replacement of a total of 41 mentors, as well as the reassignment of another five, throughout the trial but could not identify the mentorships that were terminated by withdrawal of a mentor.

Since we were testing multiple hypotheses, we have introduced the Bonferroni correction and adjusted the significance level and confidence intervals accordingly. The results of the three tests are displayed on Table 10.[11]

As all the confidence intervals for the odds ratio crossed zero, we could not conclude that there are significant differences between treatment and control schools when considering the likelihood of any of the secondary outcomes being verified.

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[11] Barplots of these variables are included in Appendix J.

Table 10: Secondary analyses (the confidence levels were adjusted for the Bonferroni correction from 95% to 98.3%)

| Outcome | Unadjusted proportions | | | | Effect size | | |
| | Intervention group | | Control group | | | | |
	N (missing)	Proportion (S.D.)	n (missing)	Proportion (S.D.)	Total n (intervention; control)	Odds ratio (98.3% CI)	p-value
Mentorship retention	107 (6)	79.4% (0.406)	109 (3)	79.8% (0.403)	216 (107; 109)	0.993 (0.438, 2.249)	0.982
Mentorship dropout	107 (6)	2.8% (0.166)	109 (3)	0.9% (0.096)	216 (107; 109)	3.224 (0.256, 173.771)	0.315
Mentor retention	107 (6)	75.7% (0.431)	109 (3)	71.6% (0.453)	216 (107; 109)	1.266 (0.601, 2.694)	0.452

Conclusion

Table 6: Summary of findings

Research question	Finding
RQ1: What is the effect of financial payments to schools participating in the early roll-out of the early career framework on the level of mentor engagement in training as measured by the time spent engaged in training?	There was no evidence of financial payments to schools, as implemented in this trial, having any impact on the time mentors spent on their training (Hedges g = -0.032; 95% CI: -0.251, 0.187; p = 0.775).
RQ2: What is the effect of financial payments to schools on the retention of mentors in the programme as measured at the end of each term?	There was no evidence of financial payments to schools, as implemented in this trial, having any impact on dropout of mentors from the DfE funded provider-led programme (OR = 1.266; 98.3% CI: 0.601, 2.694; p = 0.452). Termly analysis was not possible from the data collected. Just under three quarters of schools retained all their mentors to the end of the first year of the programme.
RQ3: What is the effect of financial payments to schools on the retention and dropout of mentorships as measured at the end of each term?[12]	There was no evidence of financial payments to schools, as implemented in this trial, having any impact on mentorships terminating. This was measured by mentorship dropout (OR = 3.224; 98.3% CI: 0.256, 173.771; p = 0.315) and mentorship retention at school level (OR = 0.993; 98.3% CI 0.438, 2.249; p = 0.982). Across both control and intervention groups, around 80% of schools had all their mentorships running at the end of the first year of the programme. At the other end of the spectrum, less than 3% of schools had all their initial mentorships terminated.

[12] If the early career teacher has a mentor (whether this is their original mentor, an additional mentor to replace the original mentor, or another early career teacher's mentor) then the mentorship is considered to be continuing.

Interpretation

The impact analysis showed no evidence of any impact of the payment of a £775 financial incentive to schools on the amount of time mentors spent on their training. The minimum detectable effect size in the analysed sample was 0.39. Due to slightly lower levels of recruitment to the early roll-out programme and to lower recruitment rates to the trial itself, the MDES was higher than anticipated. As in the adopted trial design the size of the effects detectable in the analysis is inversely proportional to the number of schools taking part in the trial, a larger sample of schools would have allowed for a possible detection of a smaller effect but we were constrained by the limited number of schools the providers were able to recruit to the trial.

This evaluation tested the mechanism of paying the extra resource to schools and whether that had an effect on mentor engagement in their training. In order to elucidate possible explanations for the findings, it is worth looking at the fidelity of the intervention as delivered as well as more closely examining the different stages of the theoretical mechanism being evaluated.

While a measure of fidelity was not explicitly part of this evaluation it is worth highlighting two areas where implementation of the intervention delivered could be considered to have deviated from what was set out initially. Firstly, there was a delay between schools being notified that they were going to receive payment and the payment being made. While schools were notified whether or not they were going to receive the payment (as the per mentor value) in November 2020, the payments were not made until February or March 2021. It is also worth considering that for state-maintained schools, payments were made via local authorities; for schools belonging to MATs, payments were made via the central trust. It is likely that the payments did not reach the bank accounts of schools of this type until some time later. It is unknown whether the staff responsible for arranging cover, or the mentors themselves, would have been aware of the forthcoming payment when schools were initially informed of the group allocation in the autumn term. It could be that the delay in payments had an impact here or that the structure of school (and MAT) finances and decisions around resource allocation are to far from day to day decisions made by teachers to have had an impact here. Lack of IPE also means we have no insight into whether funding was used as intended (that is, to support reduced teaching timetables). A further potential difference to the intended intervention versus the delivered intervention was that there was not an effective process to determine precisely the exact number of mentorships in schools in order to make the appropriate payment amount early on in the programme delivery. The process relied on data received from schools through providers and there was considerable flex in the system as the final stages of onboarding to the providers' systems was happening concurrently with the onset of the programme.

Aside from the delays in administering the payments, the mechanisms of school funding and the timelines involved could also impact the potential for payments to have any impact during the timeframe in this trial. The timings of this trial meant that many decisions around budget and resource allocation will have happened before knowledge of the trial arm allocation—or even the existence of the trial. Budget plans are discussed and submitted before the start of the academic year. The main mechanism through which the payments were hypothesised to support increased engagement in mentor training was through covering supply costs for mentors. The intention was that this would allow mentors to spend more time training but it may be that for additional funds to impact on a teacher's timetable a much longer lead-in time is needed. It is also worth noting that while this payment notionally covered the costs of supply cover for schools in terms of salaries on an hourly rate, the true costs are likely to be higher both in the time costs to administrative staff but also to the mentors. There is also a distinction to be made between *ad hoc* supply cover of lessons and a more permanent arrangement whereby responsibility for a class lies at least in part with another teacher. The second scenario here is likely what would be needed to lead to possible changes in mentor behaviour but is likely to require more lead-in time and possibly higher levels of funds.

The evaluation did provide some insight into the use of management information to construct outcome measures. As the early roll-out of the ECF was the first year of the delivery of the DfE-funded provider-led programme for the providers, management information systems were in their infancy and had not been in use in the schools. Despite considerable attempts to ensure the necessary data was fully and accurately available, it was an unknown as to how schools and providers would use the online platforms. The accuracy of data management systems used by providers at any given time also relies on school staff keeping the providers up to date with any changes. Schools were considerably affected by the COVID-19 pandemic over this time period and workloads were high as staff had to work flexibly, with competing priorities often with fewer staff (Worth and Faulkner-Ellis, 2021).

More broadly, the COVID-19 pandemic led to schools operating very differently than normal, which brought additional challenges. The DfE's operational guidance required schools to arrange their pupils and staff into 'bubbles' in order to minimise mixing of pupil groups in order to reduce transmission of COVID-19, possibly making mentoring and observing teaching more challenging. Data from an NFER survey in July 2020 found that schools were offering fewer Initial Teacher Training placements than they had the preceding year with primary schools realising a 20% drop in placements offered and secondary schools a 7% drop (Worth and Faulkner-Ellis, 2021). In a second survey in autumn 2020, senior leaders were asked what had influenced their decisions regarding ITT placements. For primary senior leaders there were 'concerns about the burden on school staff to provide support for trainees' (41%) and 'concerns about having too many different people in school' in light of COVID-19 guidance (39%) (ibid). It could be that challenges associated with increased workload around the pandemic and procedures put in place to reduce transmission of COVID-19 have led schools to deprioritise mentor training activities across the board regardless of any additional funds provided. Due to the nature of this trial and the focus on a specific implementation element of the programme, there was no implementation and process evaluation planned and thus it is not clear how the additional finance has been allocated.

Limitations

There were several limitations to the design and implementation of this trial. First, it was necessary to use a single-level design in the analysis rather than a cluster randomised design, and a cruder measure of school-level average time spent on mentor training rather than actual estimates of training time by individual mentors; as a result of this, the analysis was only powered to detect rather large effect sizes at school level. Second, the trial was underpowered from initial calculations due to lower numbers of schools recruited by providers than originally envisaged, a lower percentage of recruited schools signing up for the trial, and the unavailability of some necessary data. Thirdly, in terms of generalisability to all schools, this trial took place within those geographical areas selected for the early roll-out of the early careers framework and with those schools who had opted to take part in the new programme. While comparisons included in the report do not flag any significant differences, there may be unobserved differences between these schools and the whole population of schools. Fourthly, there are limitations to the data provided through the early roll-out providers' systems. For the primary outcome measure used in this evaluation, some time estimates had to be attached to training tasks for one of the providers to provide estimates of how long training modules are likely to have taken according to the provider. In terms of the retention measures, this does not provide any details around how engaged the mentor was—how many times the mentor met with their early career teacher, for example—just that they have not formally withdrawn from the programme. Finally, the late payments to schools (due to both the administrative delay but also financial mechanisms) may also be considered a limitation as it could be considered to change the research question to look at the effects of a *promise* of a financial payment. Although this limitation is due to implementation challenges rather than trial design, it is noted that the lack of implementation and process work means that we do not understand where in the hypothesised mechanism the incentives do not have their intended outcome. Related to this is the need to understand better how resource allocation differs for schools in MATs. As a handful of MATs signed up initially (rather than as individual schools), it could be that consideration should be given to MAT membership at randomisation.

Future research

In terms of further research, it would be useful to gain insight into the challenges facing the individual mentors in relation to committing time to their training to understand barriers they face as well as their motivations for engaging or not engaging. It is also important to understand the resourcing allocation mechanisms in both schools and MATs. Future research looking at the use of financial payments in this area could consider using interviews with headteachers, school business managers, and MAT finance officers to understand how decisions around allocating the extra resource are made. Interviews with chairs of finance committees could also be useful. It would also be useful to better understand the workings of resource allocation within multi-academy trusts as it may be the case that decisions are taken across the MAT so the effects of any payments allocated to individual schools within a MAT may be diluted.

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