Education
Endowment
Foundation

Parent Academy

Evaluation report and executive summary

July 2016

Independent evaluators:

Fatima Husain, Nico Jabin, Sarah Haywood, Adetayo Kasim, Jonathan Paylor

NatCen
Social Research

The Education Endowment Foundation (EEF) is an independent grant-making charity dedicated to breaking the link between family income and educational achievement, ensuring that children from all backgrounds can fulfil their potential and make the most of their talents.

The EEF aims to raise the attainment of children facing disadvantage by:

- identifying promising educational innovations that address the needs of disadvantaged children in primary and secondary schools in England;
- evaluating these innovations to extend and secure the evidence on what works and can be made to work at scale; and
- encouraging schools, government, charities, and others to apply evidence and adopt innovations found to be effective.

The EEF was established in 2011 by the Sutton Trust as lead charity in partnership with Impetus Trust (now part of Impetus - Private Equity Foundation) and received a founding £125m grant from the Department for Education.

Together, the EEF and Sutton Trust are the government-designated What Works Centre for improving education outcomes for school-aged children.

This project was co-funded with the KPMG Foundation.

For more information about the EEF or this report please contact:

Danielle Mason
Head of Research and Publications

p: 020 7802 1679
e: danielle.mason@eefoundation.org.uk
w: www.educationendowmentfoundation.org.uk

About the evaluator

The project was independently evaluated by NatCen Social Research. The research team consisted of Nico Jabin, Research Director, Sarah Haywood, Senior Researcher, Jonathan Paylor, Researcher (now at Kings College, University of London), Fatima Husain, Research Director, Adetayo Kasim, Senior Research Statistician, Durham University.

For further information contact NatCen:

35 Northampton Square,

London

EC1V 0AX

p: 020 7250 1866

Contents

Executive summary

The project

The Parent Academy was a series of classes for pupils' parents, designed to improve the English and mathematics attainment of pupils in Years 3 to 6 in English primary schools. Parents were offered the opportunity to participate in 12 Parent Academy classes, 6 on English and 6 on mathematics, delivered fortnightly by tutors with teaching qualifications and experience of teaching adults. The programme also included an educational family trip.

The evaluation used a two-arm randomised controlled trial to test the efficacy of two versions of the intervention. In the first version, parents were incentivised to attend with a payment of £30 per session and in the second version they were not. Children of both groups of parents were compared with a similar group whose parents were not offered Parent Academy. Sixteen schools in two urban local authorities took part in the trial. A total of 2,593 children were involved. The project also included a process evaluation which assessed how the intervention was delivered and reported on its perceived benefits. The intervention was developed by the University of Chicago. It was not manualised and involved the development of a new adult learning course. The intervention and evaluation were funded by the Education Endowment Foundation and the KPMG Foundation. The trial took place between September 2014 and July 2015 with classes delivered between November 2014 and June 2015.

Key Conclusions
1. There is no evidence that offering free Parent Academy classes improved mathematics or reading outcomes for the children in the trial, even when parents were given a financial incentive to attend.
2. In general, parental attendance at sessions was very low. However, even when the evaluators took this into account they found no evidence that parental attendance improved pupil outcomes.
3. Offering financial incentives improved attendance at Parent Academy, suggesting this may be an effective way to engage and retain parents in interventions of this type.
4. In general, evaluation participants felt that attending Parent Academy gave parents the confidence and skills to engage more effectively with their children's learning.
5. Staff reported that having a designated project lead in each school and using multiple methods to engage parents (rather than just written communication) were necessary for successful delivery. Schools need to consider these cost implications when deciding whether to adopt this intervention.

How secure are the findings?

Security rating awarded as part of the EEF peer review

Findings from this study have moderate to high security. The study was designed as a three-arm randomised controlled trial which aimed to compare the progress of pupils whose parents were invited to participate in Parent Academy with those whose parents were not. The trial was an efficacy trial, which means that it tested the intervention under ideal, developer-led conditions. The trial was well designed and well powered, and the sample was well balanced on all observable characteristics. The proportion of pupils excluded from the analysis because they did not complete the tests at the end of the intervention was just over 10%.

What are the findings?

Table 1 below presents the main results. The evaluation did not find evidence that offering parents either incentivised or non-incentivised Parent Academy classes improved maths or reading outcomes for pupils in the trial. The estimated impact on each outcome was close to zero. This is true for the group of all pupils in Years 3 to 6 and for pupils eligible for free school meals. For children whose parents

attended the non-incentivised sessions, the estimated impacts on maths and English were actually negative, but these results may have been due to chance, so we would not conclude that Parent Academy has a negative impact.

The intervention had a very low attendance rate. Six in ten parents (60%) who were offered the Parent Academy did not take part in any sessions and only around one in ten (11%) attended all twelve sessions. To assess whether the low attendance rates might account for the lack of impact, additional analysis was done taking account of how many sessions parents had attended. There was no evidence that the intervention had more impact for parents who attended more sessions. Although the intervention does not appear to have had an impact on pupil outcomes, there is evidence that the use of financial incentives did increase ***attendance*** at Parent Academy sessions. This suggests that financial incentives can be an effective way to encourage attendance at this type of intervention.

This evaluation measured pupil outcomes within a few weeks of the intervention ending. Because the Parent Academy is designed to have an impact on pupils via changes in parent behaviour, it is possible that it would take longer than this for any impact to occur. It would therefore be valuable to monitor pupil outcomes over time.

The process evaluation collated the views of class teachers, Parent Academy tutors, and parents. Parents reported that they had a better understanding of the school curriculum and other school issues, which gave them more confidence to engage with their children's learning. Teachers felt parents who attended the Parent Academy became better at communicating with teachers about their children's schooling. Parents were more actively helping children with their English and maths homework. Participating parents who had been considered 'disengaged' were seen to be more engaged with their child's education after taking part. The full-time area programme manager appears to have been an important factor in ensuring that the Parent Academy was developed and delivered successfully. Schools generally also allocated a significant amount of staff time to engaging with parents and to supporting the intervention. Manualisation of the intervention would help to standardise the Parent Academy approach and support consistency in delivery across local areas and over time.

How much does it cost?

Total intervention costs comprise one-off set-up costs including tutor training and advertising, and ongoing running costs consisting mainly of the material and staff costs of running Parent Academy classes and, for the incentivised group, incentives.

Over three years, the average annual cost of running the Parent Academy would be £641/pupil for the incentivised group and £280/pupil for the unincentivised group.

Table 1: Summary of estimated impacts of the Parent Academy

Group	Effect size (95% confidence interval)	Estimated months' progress	Security rating	Cost rating
Incentivised classes (Maths)	0.01 (-0,20, 0.22)	0	🔒🔒🔒🔒🔓	**£££**
Incentivised classes (Reading)	0.00 (-0.17, 0.17)	0	🔒🔒🔒🔒🔓	**£££**
Unincentivised classes (Maths)	-0.04 (-0.24, 0.16)	-1[1]	🔒🔒🔒🔒🔓	**£££**
Unincentivised classes (Reading)	0.02 (-0.15, 0.19)	1	🔒🔒🔒🔒🔓	**£££**

[1] *Since this report was published, the conversion from effect size into months of additional progress has been slightly revised. If these results were reported using the new conversion, all measures would be reported as 0 months of additional progress. See* ***here*** *for more details.*

Introduction

The intervention

The Parent Academy intervention was designed by the University of Chicago and aimed to provide parents with guidance to supplement their child's learning at home through parent education classes comprising two topics: English and mathematics. It focused on the parents/carers of children in Key Stage 2 (English primary school Years 3 to 6). The Parent Academy reflected the primary school curriculum with the aim of improving pupil attainment.

To be successful and achieve the desired pupil level outcomes, the intervention had to successfully recruit parents to attend Parent Academy sessions. The hypothesis was that a high level of attendance and engagement with the intervention content would lead parents to apply their learning to the home learning environment and to engage in the appropriate and desired ways with their children's education. This change in parental behaviour would then lead to the expected attainment outcomes for their child/children.

The Parent Academy was delivered from November 2014 to June 2015. In each participating school, the parents of Key Stage 2 pupils were randomly allocated to one of three groups:

1. Invited to attend the Parent Academy;
2. Invited to attend the Parent Academy and receive a financial incentive of £30 per session; and
3. Not invited to attend the Parenting Academy: the control group.

Parents in the treatment groups attended fortnightly classes each covering a topic as set out in Figure 1. A family field trip took place during the school Easter holidays. The provision of sessions was initially split by pupil year group and receipt of a financial incentive. The parents of pupils in Years 3 and 4 pupils were taught together, as were those with children in Years 5 and 6. In addition, parents who received a financial incentive attended separate sessions to those who did not. All parents were offered onsite childcare and refreshments.

Parents in the incentivised treatment group received a £30 incentive for each class they attended. By financially rewarding attendance, the developers aimed to increase parental exposure to the learning offered in classes and thus increase the effect of the intervention.

The material presented in the classes was delivered in an interactive way so as to allow parents to learn from the tutors and each other. In addition to the classes, parents also undertook homework extension activities which enabled them to put into practice the knowledge they gained. These 'Fun Home' activities were designed to make learning enjoyable and to encourage parents and children to do things together and to foster new habits that ultimately enhance the home learning environment.

The Parent Academy built on a study carried out at the Chicago Heights Early Childhood Center (CHECC) which investigated the impact of a similar parent education programme. Findings from the CHECC study suggested the programme helped to improve pupil attainment and that incentives offered to participating parents were key to its success. Guided by the principles of the CHECC programme, the Parent Academy intervention set out to further develop the evidence on improving pupil attainment by improving parents' ability to enhance the home learning environment. However, it is to be noted that the CHECC intervention was focused on parents of pre-schoolers in the USA while the Parent Academy focused on the parents of primary school children aged 7 to 11 (Years 3 to 6) in England.

The intervention was not manualised and involved the development of a new programme which was delivered in a total of sixteen schools in two English local authorities (eight schools in each local

authority). A programme manager (full-time) and an assistant programme manager (part-time) were appointed in each local authority to oversee the development and delivery of the programme. With ultimate oversight by the University of Chicago, the local authority programme managers' role involved the creation of the curriculum and lesson plans for the Parent Academy, liaison with schools and parents, budget management, incentive disbursement, and day-to-day management of the intervention. The design specifically included flexibility for the programme managers and tutors to adapt the curriculum and course to meet the needs of participants.

Parent Academy tutors were appointed on a part-time basis to deliver the learning sessions. Four staff delivered the sessions in each local authority. These were qualified teachers with knowledge of family learning environments in addition to experience of teaching adults with basic skills needs and poor English proficiency. In addition to developing the Parent Academy scheme of work and teaching, their role included working with school staff to encourage parent retention in the intervention. Parent Academy tutors were also required to:

- attend four formal training events (a total of six days) organised for all tutors. These took place every few months throughout the intervention;
- spend a minimum of two hours preparing and planning for each session;
- attend a weekly 2–3 hour Parent Academy team meeting (area specific); and
- be available for parents 30 minutes before and after each session.

The development and delivery of the intervention were phased. A training session was held for all tutors in September 2014 where the English scheme of work was finalised and the first three sessions designed. The second meeting was held in December 2014 where the final three English sessions were developed. Mathematics sessions were developed in February 2015. A final meeting to reflect on good practice was held in June 2015.

The intervention delivery teams maintained constant communication via email to share resources and to make decisions about the content of sessions on an ongoing basis. While all Parent Academy tutors followed the same scheme of work and used the same materials and resources, modifications were made to ensure the content was relevant to the specific school and to meet individual learning needs.

In addition, each local authority organised two training sessions for tutors, covering topics outside the curriculum:

> **Area 1:** Safeguarding learners, the Prevent Strategy, Female Genital Mutilation (FGM) Awareness; and Promoting Equality and Diversity in the Classroom.
>
> **Area 2:** 'How to make the most of technology'; and using technology and office software packages such as iPads, Sharepoint and Office365.

In total the Parent Academy comprised 13 sessions for parents, 12 taught sessions and one family field trip. The first two taught sessions lasted 90 minutes and subsequent sessions were 2 hours long. Fifteen parents were expected to attend each session.

Figure 1 below provides a summary of the overall structure of intervention design and delivery.

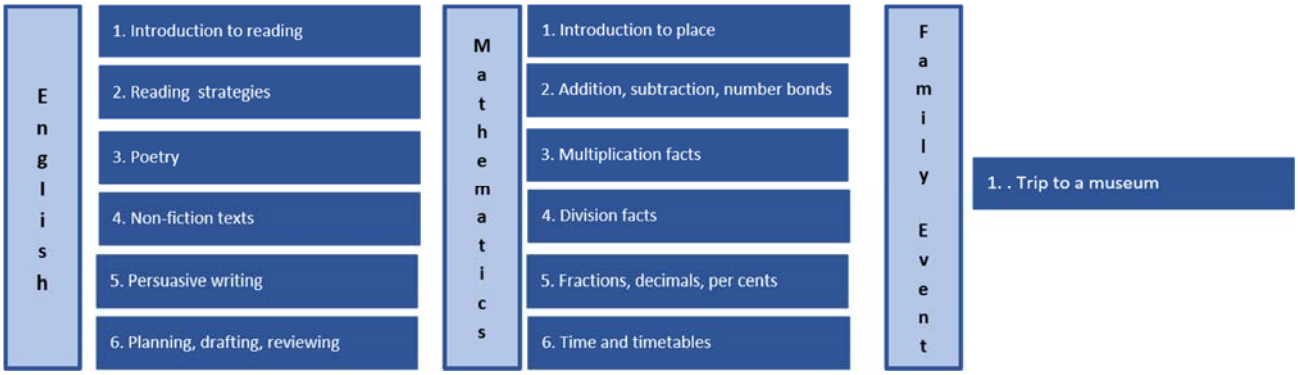
Figure 1: Intervention development and delivery

Each session was adapted to curriculum requirements for the Key Stage 2 year groups. Sessions were skills-based with a common set of skills underpinning each session:

- use of questioning to support learning;
- research;
- communications, including speaking and listening;
- building self-esteem of the child and the parent;
- assertiveness;
- study skills—resilience, persistence, reflection and self-assessment;
- use of internet resources to support learning; and
- role model learning behaviour.

Each session included a 'Fun Home Activity' that parents could complete at home with their child. These were designed to make learning enjoyable and encourage parents and children to do things together. The topics covered in each session are depicted in Figure 2.

Figure 2: Parent Academy session topic coverage



The control conditions

The control condition was 'business as usual'. The Parent Academy did not invite control group parents to take part in any alternative sessions, neither were they provided with any Parent Academy or other materials. Parents in the control group were also not included in any research activities. This means that there is no information about control parents' circumstances, their perceptions of 'business as usual', and any relevant support they were accessing. While our research gave no indication of this, it is possible that individual schools offered parents in the control group support to improve the home learning environment as part of other interventions within the school. The teachers and tutors who participated in the research did not have information on parent support activities that control parents may have engaged with.

Background evidence

Parent education programmes are multiple and diverse, covering a wide range of formats and a variety of intended outcomes. While this diversity exists, a vast majority of parent education programmes have been directed at families and single mothers with very young children (with some programmes beginning in the prenatal months) and have consisted of regular home visits. Studies focusing on these types of programmes, such as Parents as Teachers and Nurse-Family Partnership, have found positive effects on parenting behaviours (for example, attitudes toward parenting) and child outcomes (such as cognitive skills) (Wagner and Clayton, 1999; Wagner et al., 2002; Olds, 2006). However, inconsistency in results across geographic locations as well as variation in outcome measures has made it difficult to draw any definitive conclusions about the effects of these programmes.

Group training programmes that work with parents later in their children's lives (but still in early childhood) tend to show more consistent effects. A notable number of studies, for instance, have demonstrated the positive effects interventions such as the Incredible Years Program and Positive Parent Program have had on children's behaviour (Sanders, Markie-Dadds et al., 2000; Sanders, Montgomery et al., 2000; Reid et al., 2001; Reid et al., 2004). However, other similar interventions (St. Pierre and Layzer, 1999) have proven to be less effective, suggesting that the effectiveness of these interventions depends more on the specific content than on the age of the children or the format of the programme.

Research also suggests interventions that target specific groups tend to be effective. A programme targeted at adolescent mothers, for example, had significant positive effects on children's cognitive development and their play behaviour (Fewell and Wheeden, 1998), while a meta-analysis of programmes targeted at ethnic minorities showed similar effects (Jeynes, 2003, 2007).

There is a paucity of research on parenting interventions that seek to directly affect academic outcomes. While a number of non-experimental studies show a correlation between parents' involvement in their

children's education and their children's academic performance (Fehrmann et al., 1987; Griffith, 1996; Marcon, 1999) very few parent education programmes aimed at improving children attainment have been evaluated through a randomised, controlled experiment. One notable project is Chicago Heights Early Childhood Centers' (CHECC) programme for parents of pre-school age children. The emerging findings from this field experiment suggest that programme participation helped improved pupil outcomes (Fryer et al., 2015).

The studies outlined above bring into focus the potential effectiveness and importance of parent education programmes. However, many of these studies were conducted in inner city schools in the USA. Hence the delivery, educational, and demographic contexts are different. Overall, existing research is largely inconclusive in terms of what makes an effective parent education programme, especially in relation to children's academic performance. By building on CHECC's study, the trial of the Parent Academy adds to an emerging body of research that aims to part-fill this knowledge gap.

Evaluation objectives

The evaluation consisted of an impact study and a process study. The impact evaluation sought to establish whether and to what extent the Parent Academy improved outcomes in reading and general mathematics of the children whose parents were invited to take part in the Academy. The impact evaluation tested the following hypotheses:

- Offering participation in the Parent Academy to parents of children in Key Stage 2 will improve attainment in reading and general mathematics of these children within a year.
- Providing parents a financial incentive to attend will increase the effect.

The process study gathered the views and experiences of those delivering and receiving the intervention to assess how the Parent Academy was delivered and its perceived benefits.

Project team

The team responsible for delivering the Parent Academy consisted of:

Edie Dobrez (University of Chicago)—Overall project manager
Programme manager in each area
Parent Academy tutors

NatCen Social Research were the independent evaluators. The research team consisted of:

Nico Jabin, Research Director
Sarah Haywood, Senior Researcher
Jonathan Paylor, Researcher (now at Kings College, University of London)
Fatima Husain, Research Director
Adetayo Kasim, Senior Research Statistician, Durham University

Ethical review

Ethical approval was obtained by the Chicago University project team for the Parent Academy intervention.

NatCen Social Research obtained ethical approval from its internal Research Ethics Committee for the evaluation comprising the process evaluation, pupil testing, data linkage, and analysis of test results. Approval was sought for the opt-out process, communications and interviews with parents, tutors, and school staff as well as the pupil testing, and data linkage. Approval was granted in September 2014.

Trial registration

This trial was retrospectively registered on 30 September 2015 on the international standard randomised controlled trial number (ISRCTN) registry at **http://www.isrctn.com/ISRCTN42518213**.

Methods

Trial design

The evaluation was designed as a three-arm randomised controlled efficacy trial, randomising individual pupils (except for siblings, see 'Randomisation' section, below) into:

1. A control group, in which parents were not invited to receive any support;
2. An 'unincentivised' treatment group, in which parents were invited to take part in the Parent Academy, but not given any financial incentive to attend; and
3. An 'incentivised' treatment group, in which parents were financially rewarded for each session they attended.

An individual-level randomisation design was chosen because the risk of spillover from one family to another was judged to be small, making this design the most powerful to detect any possible effects (Torgerson, 2001).

The developers' previous experience suggested that financially incentivising parents would increase attendance and thus improve outcomes. As a result, two treatment arms were implemented to test this hypothesis.

The trial was designed, conducted and reported to CONSORT standards (Schulz et al., 2010).

Outcome measures

The primary outcomes are the age standardised scores for Reading and General Mathematics of the Interactive Computerised Assessment System (InCAS) (Centre for Evaluation and Monitoring, 2015). Age standardised scores are standardised for each age group with a mean of 100 and a standard deviation of 15. This score was used because the data from several year groups was combined for the analysis.

Tests were completed interactively by pupils using a computer and head phones. The InCAS Reading test is a composite of three test components: word recognition, word decoding, and comprehension. The General Mathematics test consists of four components that measure informal arithmetic; formal arithmetic; measures, shapes and space; and data handling.

InCAS was chosen because the wide age range of participating pupils required a test applicable to pupils across four school years. InCAS tests accomplish this by interactively adapting the test difficulty to the performance of the pupil. This then avoids floor and ceiling effects that would be unavoidable using a more targeted non-adaptive test. In order to reduce variability in scores due to age, analysis was performed on the age standardised rather than raw scores.

The tests were administered by NatCen interviewers who were blind to the treatment assignment of the children tested. In addition, the test administrators were instructed not to seek out the treatment status of individuals by discussing the intervention with teachers or pupils.

Parent Academy sessions were delivered from November 2014 to June 2015 and pupil testing was conducted immediately after the end of the Parent Academy sessions, during the last two weeks of June and the first week of July 2015. The evaluation timetable and the school year (ending in mid-July) did not permit testing to be conducted at a later date. This time constraint limited the evaluators' ability to test for longer-term effects of the intervention.

The 'Pupil characteristics' section includes descriptive statistics of the outcomes.

Participant selection

Two local authorities, both urban (one in Greater London), were chosen for the trial. These two local authorities had experienced difficulties in engaging parents with their children's education and were actively trying to increase parent engagement and motivation to improve the home learning environment. The Parent Academy was seen as a vehicle to develop work with parents in the local area. Some schools in one area were already trying to find new ways to engage parents in their children's education. Following recruitment of these schools, the University of Chicago obtained a list of all primary schools across the two local authorities and emails were sent out inviting schools to join the trial.

The parents of all pupils in Years 3 to 6 in recruited schools were eligible to take part in the Parent Academy.

Opt-out consent was sought from all parents before randomisation. Information about the Parent Academy was provided, pupil data that would be accessed was listed, and NPD data linkage and archiving explained. Parents had the option to i) opt out of the trial and ii) opt out of data linkage and archiving, even if they chose to participate in the Parent Academy. A copy of the consent form is provided in Appendix A.

Sample size

The study aimed to recruit 3,000 pupils from 18 schools in two local authorities, assigned to two treatment groups and a control group. Due to resource limitations, 500 pupils were due to be assigned to the incentivised treatment arm, 600 to the unincentivised, and 1,900 to the control group.

The initial calculation of the minimum detectable effect size (MDES) assumed 5% attrition and the use of Key Stage 1 reading and mathematics outcomes as baseline measures. The baseline was assumed to explain between 20% and 60% of variation in the outcome.

Due to the different sizes of the two treatment arms, the MDES reported is based on the comparison of the smaller incentivised treatment with the control group (assuming unequal allocation). For the comparison of this group with the control, the MDES was calculated to lie between 0.13 and 0.09 standard deviations, depending on the assumed predictive power of the baseline measure. The minimum detectable effect size for this comparison without a baseline measure was 0.14 standard deviations.

The minimum detectable effect size for the comparison between the two treatment groups was estimated at 0.17 standard deviations without the use of a baseline measure. This dropped to 0.11 standard deviations with the most optimistic assumption about the predictive power of the baseline measure (60%, as above).

All estimates were calculated assuming 80% power and a two-sided statistical significance of 5%.

The final recruited sample consisted of 2,690 pupils in four years across 16 schools. Of these, 2,153 were allocated to treatment or control and 1,964 included in the analysis. More detail is given in the 'Participants' section below.

Randomisation

NatCen independently carried out the randomisation designed by the project team at the University of Chicago and agreed by the Education Endowment Foundation (EEF). It combines stratification, matching, and re-randomisation, in order to maximise balance of a range of covariates across the three trial groups and reduce heterogeneity among groups during the analysis.

Stratification is a frequently used device to enforce balance on a small number of categorical prognostic variables. It works by allocating an equal number of participants with a particular combination of observable characteristics to each treatment arm. This prevents undesirably unbalanced assignments on those characteristics and, when stratification is accounted for in the analysis, ensures greater homogeneity among participants.

Matching is an extension of stratification where a number of participants (one for each trial arm, in this case three) are matched to each other based on some prognostic criteria (for example, a baseline score) and then assigned at random within the pairs or triplets. Similar to stratification, matching restricts assignment to allocations that are considered balanced, and reduces heterogeneity among assignment groups at analysis stage.

Re-randomisation, finally, is another way to restrict the frame of possible treatment assignments to assignments with sufficient balance across a range of known covariates. In this way, it is similar to stratification, but it allows for the consideration of a greater number of covariates. Re-randomisation can be carried out either by pre-specifying an acceptance criterion on a balance check (such as an acceptable *p*-value on a logistic regression test for the predictability of treatment assignment using the covariates), or by using the best assignment[2] out of a pre-specified number of randomisations (where this number is smaller than the number of all possible assignments) (Bruhn and McKenzie, 2009, p.12ff).

In this trial, following the design set out, all three approaches were used. The practical implementation was carried out independently by the evaluation team using an anonymised dataset provided by the participating local authorities.

The implementation aimed to address four requirements:

1. Similar pupils were allocated (matched) to triplets, and pupils within each triplet randomly assigned to the two treatments and the control.
2. In each school only a limited number of randomly selected pupils were assigned to the treatment groups, subject to the schools' capacity constraints; the remainder were allocated to the control group.
3. Only one of any group of siblings within the four years was allocated to a triplet.
4. It was ensured that pupils in the three treatment groups were as similar to each other as possible across a range of variables, while still allowing for random assignment.

The randomisation thus used a six-step process:

1. From each family with more than one eligible sibling, one sibling was selected at random for assignment. If there existed a non-selected sibling in the same year grouping (either Year 3 & 4 or Year 5 & 6), the non-selected sibling was given the same assignment (but unmatched—see below) as the selected sibling, otherwise the non-selected sibling was excluded. Year groupings were relevant because the original intent was for parents of children in school years within a group (3 and 4 or 5 and 6) to be taught together, so that a parent of two children in the same year group could be considered trained for both children, but a parent of two children in different year groups could not.
2. Pupils were stratified by school, year, gender, English as an additional language (EAL), ethnicity (White British versus Other), and whether or not additional data linkage consent was given.
3. Within each stratum, pupils were ordered by Key Stage 1 Average Point Score (KS1 APS-RWM, comprising of reading, writing, and maths scores), and then matched consecutively with two

[2] The best assignment is the one resulting in the highest *p*-value when modelling the relationship between the chosen covariates and the treatment assignment.

other pupils to form a triplet. If the number of pupils in a stratum was not divisible by 3, one or two pupils were randomly removed to create adequately sized strata.

4. To deal with the fact that the stratification in step 2, due to the many stratification factors used, created many groups with only one or two pupils, pupils removed at step 3 were pooled; stratified by school, year and whether or not additional data linkage consent was given; and matched to triplets as described in step 3. If the number of pupils in a stratum was not divisible by 3, one or two pupils were randomly removed to create adequately sized strata. The removed pupils were excluded. All triplets formed in steps 3 and 4 were pooled.
5. By school, triplets were randomly assigned to one of three triplet assignment options (T1-T2-C; T2-T2-C; and C-C-C), reflecting the resource availability in each school for offering treatments 1 and 2[3]. Within each triplet, pupils were then randomly assigned the assignment options available.
6. Steps 1–5 were carried out 1000 times, each time testing balance on the original stratification variables (school, year, gender, EAL, ethnicity, data linkage consent) plus five additional balance variables, using multinomial logistic regression. The additional balance variables were special educational needs status (SEN), eligibility for free school meals (FSM), term of birth, banding of the school's location on the Index of Multiple Deprivation 2010 (IMD) and pupil attendance rate. The randomisation with the highest *p*-value was used for assignment.

The randomisation was implemented by NatCen using Stata 12 (StataCorp, 2011).

Analysis

In general, analysis of randomised controlled trials should take account of the method of randomisation. Ignoring the randomisation method still produces the correct mean estimate of the effect but results in unnecessarily large standard errors, and thus unnecessarily wide confidence intervals. However, in the case of this study, we believe that the randomisation method was so complex that taking all aspects of randomisation into account risked introducing error. As a result, we conducted a more straightforward analysis as primary analysis, accepting wider confidence intervals as the more robust approach. We considered other aspects of randomisation as secondary sensitivity analysis.

The **primary data analysis** compared each intervention arm with the control group using a multilevel model to account for heterogeneity between schools and for differential treatment effects by school. This model adds to the classical multilevel model which only assumes school as random effect if the intervention effect is constant across schools. We presented an unadjusted model which estimated the effect of the treatment considering only the relevant baseline Key Stage 1 outcomes (KS1 reading for the reading outcome and KS1 maths for the maths outcome). The main (adjusted) model included baseline scores as well as the stratification variables available (school year and gender) and FSM eligibility. Due to restrictions encountered in accessing de-anonymised data from the National Pupil Database, two stratification variables could not be included in the analysis: EAL and ethnicity. In order to allow for comparability with the subgroup analysis of pupils eligible for free school meals, we included FSM in the primary analysis. Pupils with missing outcome data were excluded from analysis, but a sensitivity analysis looked at the effect of imputing missing outcome data.

The results of the analysis are presented as effect sizes. Effect sizes are a unit-free and normalised way of presenting results that allows for comparison between the effects of different treatments. The calculation of effect sizes here followed the methodology for Hedges' *g*, using the differences in means between treatment and control groups as numerator and the pooled standard deviation based on the total variance from a multilevel model as denominator. The effect size calculation was based on Hedges' (2007) approach assuming unequal sample size per school.

[3] Every school offered at least as many T2 spaces as T1 spaces, hence there was no need for T1-T1-C triplets.

A number of **secondary analyses** were carried out. First, as part of sensitivity analyses, a classical permutation test to estimate the main model's *p*-value was performed. Following Fisher (1935), this presumes that the null hypothesis of no treatment effect is true and that therefore the treatment effect calculated from any permutation of treatment assignment is equally likely. This then allows the estimation of the likelihood of the observed effect if the null hypothesis is true (the *p*-value) by examining the proportion of estimated effects greater than the one actually observed. We estimated the *p*-value by permuting treatment allocation 1,000 times to generate a distribution of effect size under the null hypothesis. This permutation test was based on simple randomisation without replacement and without any further acceptance criteria.

Second, we analysed results taking account of the re-randomisation used in the original allocation process. The principle of this analysis was similar to that of the permutation test described, but the random allocations were subject to the same acceptance criteria as the original randomisation.

The re-randomisation analysis should therefore have created 1,000 random assignments of which each was the most balanced of a run of 1,000 possible assignments. However, randomly selecting siblings for matching in each round of randomisation as well as the inclusion of pupils without data linkage consent (see 'Participants' section below) caused a change in the identity of the pupils actually assigned to matched triplets and a fluctuation in the number of matched pupils assigned to each treatment. This was problematic in terms of re-randomisation, as each new assignment instance was no longer a reshuffle of treatment assignments. The assumptions underlying the permutation test were thus violated and the re-randomisation test could no longer be run.

One way to approach this problem at the re-randomisation stage would have been to restrict the set of valid randomisations to instances including only the pupils included in the original final assignment. However, due to the use of matching based on a continuous variable, holding all required parameters fixed during the re-randomisation would have resulted in re-assigning pupils merely within their triplets. As a result, at the analysis stage, we altered the acceptance criteria for a valid random assignment from 'most balanced allocation in 1,000 runs' to 'achieving a minimum balance in the group assignment'. This minimum, measured as *p*-value from a logistic regression predicting group assignments, was set at $p=0.5$.

The 2,153 children originally assigned to matched triplets were thus re-randomised to generate a matrix of 1,000 acceptable random assignments according to the new acceptance criterion.

Under the null model of no treatment effects, post-test scores are expected to be independent of re-randomisation runs (Morgan and Rubin, 2012, p.3). Using the pre-defined re-randomisation matrix, we performed:

- simple unadjusted analysis between post-test scores and the intervention groups. We reported quantile-based 95% confidence intervals for the average intervention effects across the re-randomisation runs. The proportion of re-randomisation results greater than the actual "trial intervention groups" was reported as *p*-value; and
- adjusted analysis for the stratification and balance variables using a multilevel model. 95% confidence intervals and *p*-value were calculated as described above.

Third, we conducted a sensitivity analysis for missing data by reporting the distribution of missingness by intervention groups and the baseline factors. A logistic regression model was used to investigate whether the missing data were independent of the stratification variables and intervention groups. We also performed multiple imputations to assess the impact of missing data by comparing the results with and without imputations. The multiple analysis was based on 50 imputations. Each of the 50 imputed data was analysed separately and the results were converted into a single result based on weighted average accounting for both within and between imputation variation.

Fourth, we assessed the impact of the interventions on pupils eligible for free school meals by carrying out the primary analysis using only FSM eligible pupils.

Finally, we explored the influence of parental attendance at parenting classes. This type of analysis allows the estimation of a complier average causal effect (CACE). Our analysis estimated the intervention effect taking account of the dose of treatment received by participants (measured as proportion of possible sessions attended by each invited parent). The estimate was biased as the factors inducing parents not to attend were also likely linked to the measured outcome, and non-attending parents were effectively removed from the treatment but not from the control group. Apart from the addition of the attendance measure, the estimation used the same model as the primary analysis.

All analyses were conducted on an intention-to-treat basis. Analyses were restricted to pupils matched to triplets, plus any siblings in the same year grouping (who were automatically assigned to the same treatment condition as set out in the 'Randomisation' section). Pupils that could not be matched to triplets were excluded from the analysis. This affected cases:

- with missing stratification or matching data;
- that were randomly excluded to allow for the formation of full triplets; and
- where siblings were not selected for a triplet and were in a different year group to the selected sibling.

The primary analysis and all multi-level modelling was carried out in R (R Core Team, 2015) using the lme4 package (Bates et al., 2015). All randomisations and re-randomisations were carried out using Stata 12 (StataCorp, 2011). Multiple imputation of missing data was carried out in SAS software, version 9.2 of the SAS System for Windows (SAS Institute Inc., 2010).

The analysis plan is attached in Appendix B.

Implementation and process evaluation

The process evaluation was designed to explore how parents, Parent Academy tutors, and staff in participating schools viewed the delivery and implementation of the Parent Academy, and what they perceived the impact to be. The process evaluation also investigated the extent of fidelity to the intervention and collected cost information from the delivery team.

A longitudinal approach was taken involving two stages of interviews. This approach aimed to capture the views of parents and tutors at the start of the programme and once the programme was almost complete. This meant that there were two main fieldwork periods; stage one took place between December and February 2015 and stage two took place between May and July 2015.

Parents, Parent Academy tutors and school staff were all interviewed over the phone by NatCen researchers.

Parents

Only parents in the treatment group (incentivised and non-incentivised) were invited to take part in the research. Parent Academy managers and tutors supported recruitment by distributing research information materials at teaching sessions. From the parents who agreed to take part in the research 18 were sampled for the first stage of fieldwork based on a range of demographic characteristics. These included:

- locality (Area 1 or Area 2);
- child's school year;
- whether the parent was receiving an incentive for attending the Parent Academy;

- the relationship to the child (parent/carer/grandparent/guardian); and
- the relationship status of the participating adult.

At the second stage of fieldwork, fewer (12) parents were interviewed. A lower number of interviews were planned for the second stage of fieldwork in order to take into account the attrition of parents between the start and end of the programme. As these were follow-up interviews, parents who participated in the first stage of the process study were invited to take part in a second interview, whether or not they had been attending the sessions. All those who took part in a second interview reported they had attended at least half the sessions.

Parent Academy tutors and school staff

All Parent Academy tutors were invited to take part in the research and at both stages of the research.

School staff were interviewed during the second stage of fieldwork. Initially, headteachers were invited to take part in the interview. If they felt they were not the most suitable participant, they were asked to nominate a staff member who had been most involved in the logistics and day-to-day running of the Parent Academy in their school. Two headteachers, one deputy headteacher, two parent support advisors and one family support worker took part in the research.

School staff from participating schools were interviewed in order to understand how the Parent Academy fitted into school life and what school staff themselves perceived the impact, if any, to be for pupils and parents.

Conduct of interviews

The content of each interview was based on a topic guide to ensure systematic coverage of key issues that addressed the process evaluation research objectives. It was intended to be flexible and interactive, allowing issues of relevance to be covered through detailed follow-up questioning. Separate topic guides were produced for incentivised and unincentivised parents.

To minimise the burden on participants, all interviews were conducted over the telephone. Interviews lasted approximately 45 minutes. The interviews were digitally recorded and then analysed using Framework, a systematic approach to qualitative data management developed by NatCen Social Research and now widely used in social policy research. All participants were assured that everything discussed in the interview would remain confidential and would be treated in accordance with the Data Protection Act.

Timeline

Planning for the intervention began in July 2013 with the recruitment of schools. The intervention was delivered during the 2014-2015 school year (September 2014 to July 2015). Evaluation activities in the field started in October 2014 and ended with pupil testing in June & July 2015. Table 2 sets out the key evaluation milestones.

Table 2: High level evaluation timetable

Date	Activity
July 2013	Schools recruited in Area 2
January 2014	Schools recruited in Area 1
July 2014	Opt-out parental consent obtained by delivery partners
October 2014	Opt-out parental consent obtained by NatCen
October 2014	Randomisation of pupils performed based on data received from local authorities
November 2014	Intervention delivery begins

June 2015	Intervention delivery finishes
June 2015	Post-intervention data collection (pupil testing) begins
September 2015	NPD outcome data extract obtained
October 2015	Analysis
January 2016	Delivery of report to EEF

Costs

Information on costs was provided by the delivery team project manager. This included set-up costs, and staff and material costs for delivery of the intervention. Costs for each area were calculated separately and combined to give a per pupil cost. A list of engagement activities was provided by area project managers, however we were not given estimates of time spent by Parent Academy staff and/or school staff on these activities. The trial was commissioned before new guidance on the systematic collection of cost data was developed by EEF.

Impact evaluation

Participants

The parents of all pupils in Years 3 to 6 of the 16 recruited schools were invited to take part in the intervention. Of those, 2,690 pupils in 2,306 distinct families did not opt out. However, parents of 97 of these pupils, in 89 families, opted out of an additional request to consent to data linkage. This consent was required to allow linking of pupil outcome data to baseline and covariate data from the National Pupil Database (NPD). Children whose parents refused data linkage were not included in the analysis but the parents were able to attend the Parent Academy.

Of the 2,690 originally recruited pupils, 1,895 (70%) and 1,923 (71%) pupils were included in the analysis for, respectively, reading and maths outcomes. The full detail on attrition is set out in Figure 3.

It is important to note that the randomisation was carried out using data provided in anonymised form by the two local authorities where the Parent Academy was delivered. This meant that when the same information was obtained from the NPD for the analysis, some differences existed:

1. First, data on English as additional language, ethnicity and special educational needs status could not be obtained from the NPD, as sensitive data requires parental opt-in consent, whereas an opt-out process was used for this study. These variables were thus included only in the randomisation and not in the analysis.
2. One variable, free school meals (FSM) eligibility was defined differently at randomisation and analysis stage. At randomisation, FSM related to pupils' current status, whereas the NPD variable records whether pupils had ever been in receipt of free school meals.

Due to these small differences between the pupil data provided by local authorities and that obtained from the NPD, the balance of covariates achieved across treatment groups according to the school-based baseline data was not synonymous with the balance achieved according to the NPD data. This is discussed in the section 'Pupil characteristics' below.

Figure 3: Pupil attrition

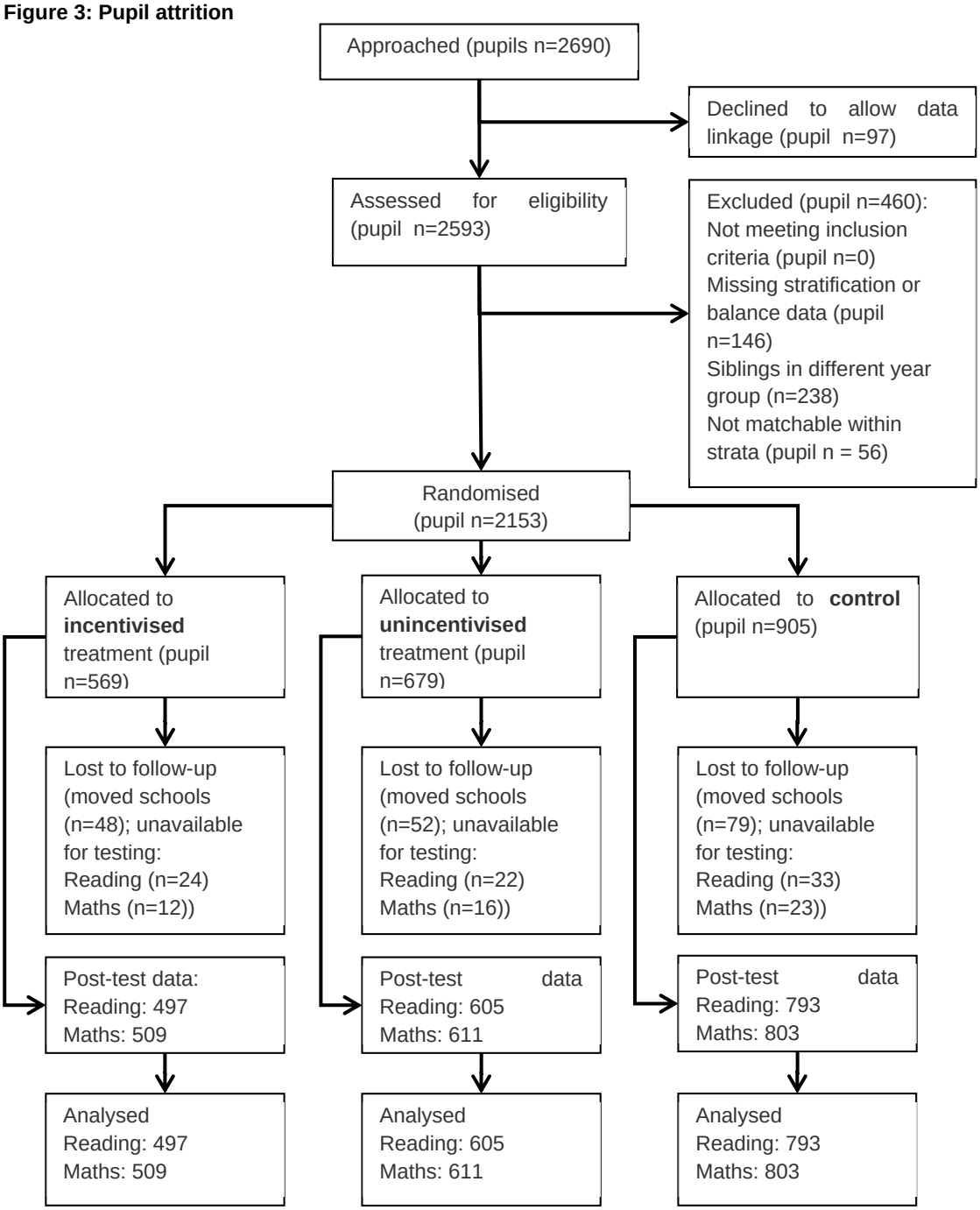


Table 3 shows the change in the minimum detectable effect size across difference stages of the trial. Our calculations at the protocol and randomisation stage were based on the comparison of the smaller treatment arm (incentivised) with the control group. The calculations at protocol and randomisation stage did not take account of any clustering of pupil outcomes within schools. In our final analysis, however, we used a random effects model to estimate the effect of the intervention. This model allowed for school effects, expressed in the intra-class correlation coefficient (ICC). In practice, the occurrence of clustering increased the minimum detectable effect size, because the reduced variability of outcomes within schools reduced the effective sample size. On the other hand, our baseline scores were more predictive of the measured outcomes than we had assumed, which reduced the MDES.

Table 3: Minimum detectable effect size at different stages, comparing incentivised treatment against control

Stage	Number of pupils (incentivised; control)	Correlation between pre-test & post-test	ICC	Blocking / stratification or pair matching	Power	Alpha	Minimum detectable effect size (MDES)
Protocol	2,275 (475;1,800)	0.0 – 0.6	Not consi-dered	Stratification, matching and re-randomisation	80%	0.05	0.14 – 0.09
Randomi-sation	1,500 (578; 922)	0.0 – 0.6	Not consi-dered	Stratification, matching and re-randomisation	80%	0.05	0.15 – 0.09
Analysis Reading	1,290 (497; 793)	0.66	0.06	Stratification	80%	0.05	0.11
Analysis Maths	1,312 (509; 803)	0.66	0.06	Stratification	80%	0.05	0.11

Pupil characteristics

Table 4 to Table 9 below present the characteristics of pupils after their random assignment to the three trial arms. The tables exclude: pupils for whom stratification variables were missing and who were thus excluded from assignment (n = 146); pupils who were excluded from analysis because an older or younger sibling was selected at random[4] (n = 238); and pupils selected at random within strata in order to allow the formation of proper triplets (n = 56).

The randomisation relied on anonymised statistics provided by local authorities, which were not available at the analysis stage. Instead, the analysis used equivalent data extracted from the National Pupil Database, requested after the randomisation was completed. Due to restrictions accessing sensitive personal data, we were not able to obtain data on ethnicity and English as an additional language (EAL). In addition, the measure for free school meal eligibility provided by local authorities used eligibility in the year prior to the study, while the data used in the analysis measured whether a pupil had ever been eligible. Finally, the randomisation, in the matching, used Key Stage 1 Average Point Score for reading, writing and maths, while the analysis used Key Stage 1 reading and maths scores as baseline for the reading and maths outcome measure, respectively. Table 4 to Table 7 thus presents the variables used in the randomisation and Table 8 and Table 9, for comparison, the data used in the analysis.

Ensuring balance at baseline was an integral part of the randomisation procedure. The logistic regression examining the relationship between the group assignment and the stratification and balance variables had a *p*-value of 0.9998, implying a high degree of balance between groups. However, due to attrition as well as the differences in data used at randomisation and in the analysis described above, analysis variables may not exhibit the same degree of balance at baseline. Table 10 sets out a comparison of baseline reading and maths scores between each treatment and the control group. Expressed in standard deviations the differences range from 0.04 to 0.07 and are thus very small. None of the differences have statistical significance.

[4] Since the parents of treatment-assigned pupils attend the Parent Academy, siblings of assigned pupils were considered 'matched' if they were in the same year group (Years 3 and 4 or Years 5 and 6), but 'unmatched' otherwise. Siblings from the same year group as the assigned pupil are thus included in the analysis but siblings from a different year group are excluded. The same rule applied to control group pupils.

Table 4: Baseline comparison—stratification variables used in the randomisation

Variable	Incentivised group		Unincentivised group		Control group	
	n/N (missing)	Percentage (sd)	n/N (missing)	Percentage (sd)	n/N (missing)	Percentage (sd)
Year 3	155 / 569 (0)	27.2	182 / 679 (0)	26.8	254 / 905 (0)	28.1
Year 4	141 / 569 (0)	24.8	185 / 679 (0)	27.2	251 / 905 (0)	27.7
Year 5	135 / 569 (0)	23.7	157 / 679 (0)	23.1	193 / 905 (0)	21.3
Year 6	138 / 569 (0)	24.3	155 / 679 (0)	22.8	207 / 905 (0)	22.9
Female	287 / 569 (0)	50.4 (0.5)	339 / 679 (0)	49.9 (0.5)	465 / 905 (0)	51.4 (0.5)
EAL	253 / 569 (0)	44.5 (0.5)	312 / 679 (0)	45.9 (0.5)	401 / 905 (0)	44.3 (0.5)
White British	259 / 569 (0)	45.5 (0.5)	301 / 679 (0)	44.3 (0.5)	402 / 905 (0)	44.4 (0.5)

Table 5: Baseline comparison—categorical balance variables used in the randomisation

Variable	Incentivised group		Unincentivised group		Control group	
	n/N (missing)	Percentage (sd)	n/N (missing)	Percentage (sd)	n/N (missing)	Percentage (sd)
SEN status	116 / 569 (0)	20.4 (0.4)	136 / 679 (0)	20.0 (0.4)	184 / 905 (0)	20.3 (0.4)
FSM eligibility	229 / 569 (0)	40.2 (0.5)	262 / 679 (0)	38.6 (0.5)	348 / 903 (2)	38.5 (0.5)
Birth term Spring	177 / 569 (0)	31.1	222 / 679 (0)	32.7	290 / 905 (0)	32.0
Birth term Summer	203 / 569 (0)	35.7	236 / 679 (0)	34.8	312 / 905 (0)	34.5
Birth term Autumn	189 / 569 (0)	33.2	221 / 679 (0)	32.5	303 / 905 (0)	33.5
IMD (<=6th decile)	102 / 565 (4)	18.1	117 / 679 (0)	17.2	176 / 896 (9)	19.6
IMD (7th decile)	85 / 565 (4)	15.0	112 / 679 (0)	16.5	142 / 896 (9)	15.8
IMD (8th decile)	167 / 565 (4)	29.6	184 / 679 (0)	27.1	245 / 896 (9)	27.3
IMD (9th decile)	211 / 565 (4)	37.3	266 / 679 (0)	39.2	333 / 896 (9)	37.2

Table 6: Baseline comparison—continuous balance variables used in the randomisation

Variable	Incentivised group		Unincentivised group		Control group	
	N (missing)	Mean (sd)	N (missing)	Mean (sd)	N (missing)	Mean (sd)
Attendance (mean percentage)	567 (2)	95.1 (4.8)	678 (1)	95.1 (4.3)	899 (6)	95.1 (4.5)

Table 7: Baseline comparison – matching variable used in the randomisation

Variable	Incentivised group		Unincentivised group		Control group	
	N (missing)	Mean (sd)	N (missing)	Mean (sd)	N (missing)	Mean (sd)
KS1 Average Point Score, Reading Writing Maths	569 (0)	14.6 (3.5)	679 (0)	15.0 (3.3)	905 (0)	14.8 (3.2)

Table 8: Baseline comparison – categorical NPD variables from the used in the analysis

Variable	Incentivised group		Unincentivised group		Control group	
	n/N (missing)	Percentage (sd)	n/N (missing)	Percentage (sd)	n/N (missing)	Percentage (sd)
Female	285 / 564 (5)	50.5 (0.5)	339 / 677 (2)	50.1 (0.5)	465 / 901 (4)	51.6 (0.5)
FSM eligibility	320 / 558 (11)	57.3 (0.5)	372 / 677 (2)	54.9 (0.5)	481 / 894 (11)	53.8 (0.5)
IMD (<=6th decile)	111 / 558 (11)	19.9	129 / 677 (2)	19.1	191 / 894 (11)	21.4
IMD (7th decile)	84 / 558 (11)	15.1	113 / 677 (2)	16.7	140 / 894 (11)	15.7
IMD (8th decile)	124 / 558 (11)	22.2	142 / 677 (2)	21.0	196 / 894 (11)	21.9
IMD (9th decile)	239 / 558 (11)	42.8	293 / 677 (2)	43.3	367 / 894 (11)	41.1
Birth term Spring	175 / 564 (5)	31.0	221 / 677 (2)	32.6	290 / 901 (4)	32.2
Birth term Summer	202 / 564 (5)	35.8	236 / 677 (2)	34.9	310 / 901 (4)	34.4
Birth term Autumn	187 / 564 (5)	33.2	220 / 677 (2)	32.5	301 / 901 (4)	33.4

Table 9: Baseline comparison—continuous NPD variables used in the analysis

Variable	Incentivised group		Unincentivised group		Control group	
	N (missing)	Mean (sd)	N (missing)	Mean (sd)	N (missing)	Mean (sd)
KS1 Reading score	564 (5)	15.0 (4.1)	677 (2)	15.4 (4.0)	901 (5)	15.2 (3.8)
KS1 Maths score	564 (5)	14.9 (3.6)	677 (2)	15.3 (3.6)	901 (5)	15.1 (3.4)
Attendance	564 (5)	95.3 (4.7)	677 (2)	95.3 (4.2)	900 (6)	95.2 (4.7)

Table 10: Balance of NPD data at baseline

Variable	Incentivised vs. Control		Unincentivised vs. Control	
	Mean difference	Effect size	Mean difference	Effect size
KS1 Reading score	0.19	0.05	0.17	0.04
KS1 Maths score	0.14	0.04	0.23	0.07

Schools

Local authorities and the participating schools were recruited on a convenience basis, utilising the fact that the two participating local authorities had a pre-existing interest in developing parent-school relationships. This can limit the applicability of the findings from this trial to other geographical areas, since the local interest may stem from an acknowledged problem with parent-school relationships (comparatively low baseline), or, reversely, because of successful previous work with parents and schools (comparatively high baseline). However, this type of recruitment is appropriate for the efficacy-focused nature of the trial.

Outcomes and analysis

Primary analysis

Table 11 shows the means of the age standardised reading and maths outcomes of pupils in the two treatment and the control groups. Pupils in the control group had a mean score of 99.28 in the General Mathematics test and 102.60 in the Reading test; pupils in the unincentivised treatment group had an average General Mathematics score of 99.61 and a Reading score of 103.07, while pupils in the incentivised treatment group had an average General Mathematics score of 99.11 and Reading score of 102.06.

Table 11: Age standardised test scores

	Incentivised group	Unincentivised group	Control group
General Mathematics post-test scores, age standardised			
Number of pupils (missing)	509 (69)	611 (79)	803 (119)
Mean (95% confidence interval)	99.11 (97.4, 100.78)	99.61 (98.2, 101.02)	99.28 (98.09, 100.47)
Reading post-test scores			
Number of pupils (missing)	497 (81)	605 (85)	793 (129)
Mean (95% confidence interval)	102.06 (100.63, 103.49)	103.07 (101.89, 104.25)	102.60 (101.60, 103.60)

Table 12 presents the results of our analysis. The estimates of our main model for the effects of the treatments on maths outcomes are 0.01 standard deviations for the incentivised and -0.04 for the unincentivised interventions. For the effect on reading outcomes, the main model's estimate of the incentivised treatment is 0.00 and for the unincentivised treatment 0.02. None of the estimates are statistically significantly different from zero.

For comparison, Table 12 also presents the estimated effect sizes of the unadjusted model, which takes account only of baseline KS1 outcomes. The results are in line with the results of the main model. The table also presents the main model's confidence intervals around the effect size estimate, the *p*-value and the intra-class correlation for each treatment and outcome.

Table 12: Effect sizes using primary analysis

	Maths outcome	Reading outcome
Incentivised vs. control		
Unadjusted effect size	0.00	-0.01
Effect size (main model)	0.01	0.00
Confidence intervals (main model)	(-0.20, 0.22)	(-0.17, 0.17)
***p*-value (main model)**	0.39	0.48
Intra-class correlation (main model)	0.06	0.03
Unincentivised vs. control		
Unadjusted effect size	-0.04	0.01
Effect size (main model)	-0.04	0.02
Confidence intervals (main model)	(-0.24, 0.16)	(-0.15, 0.19)
***p*-value (main model)**	0.76	0.33
Intra-class correlation (main model)	0.06	0.03

*Note: Statistical significance of effect sizes is indicated by *, such that * indicates that the treatment effect is significant at the 10* level, ** at the 5% level and *** at the 1% level.*

Direct comparison of the two treatments was not undertaken due to the fact that no statistically significant effect for either treatment arm was found. However, the similarity in effect sizes of incentivised and unincentivised treatment suggests that the increased attendance resulting from incentivising parents to take part does not improve outcomes.

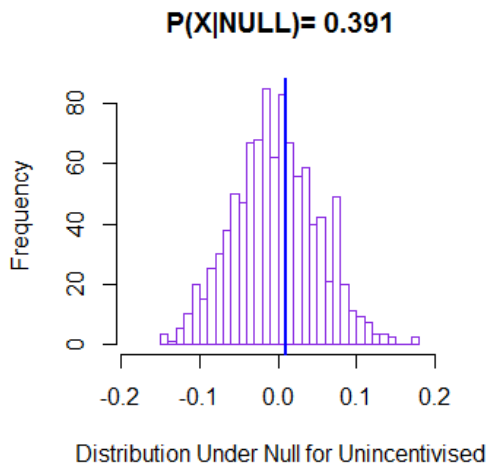
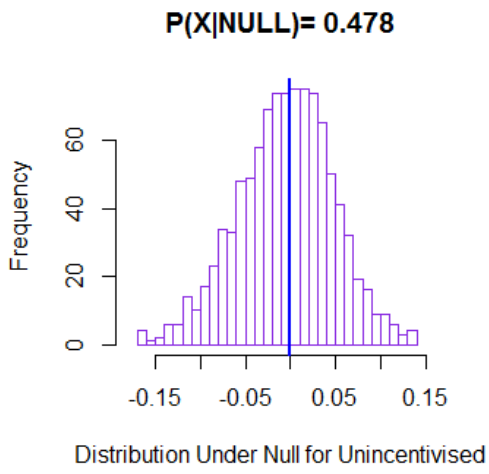
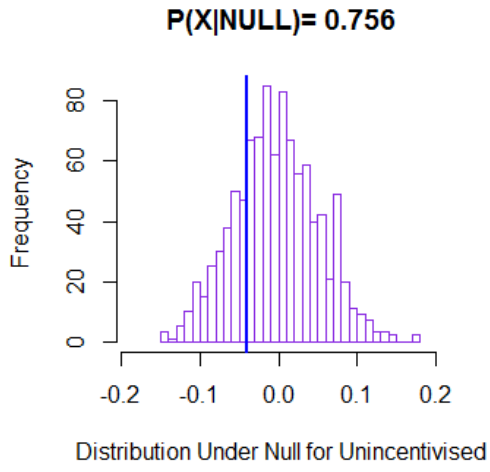
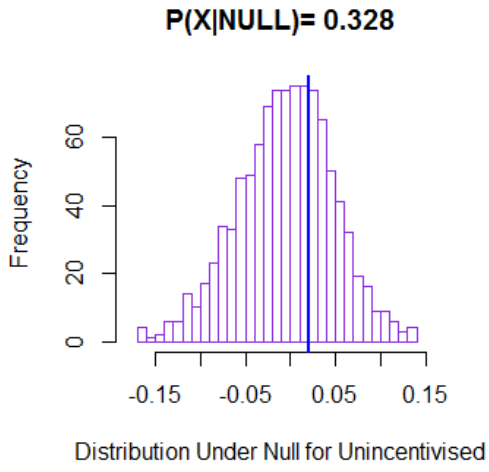
Secondary analyses

1. *Sensitivity analysis: p-value calculation*

Figure 4 shows the distribution of effects calculated using a simple permutation of treatment assignments. The solid blue bar indicates the effect actually calculated in the primary analysis. In line with the reported confidence intervals in Table 12, the *p*-values from the permutation analysis show that for either treatment the null-hypothesis of no treatment effect cannot be rejected for either outcome.

This can be demonstrated using the example of the effect of the incentivised treatment on maths outcomes (top left chart in Figure 4). A hypothetical treatment status (treatment or control) was repeatedly and randomly assigned to each pupil during the analysis. In each case, the average outcome of the 'treatment' and 'control' group was estimated and the difference recorded. After 1,000 random assignments the 'treatment effects' of the hypothetical assignments were larger than the one actually found in 39.1% of assignments. We thus have no evidence to reject the null hypothesis.

Figure 4: Permuted distribution of effect sizes under the null hypothesis

	Maths outcome	Reading outcome
Incentivised vs control		
Unincentivised vs control		

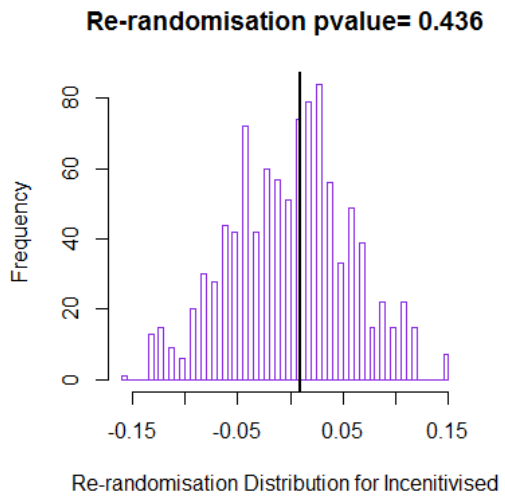
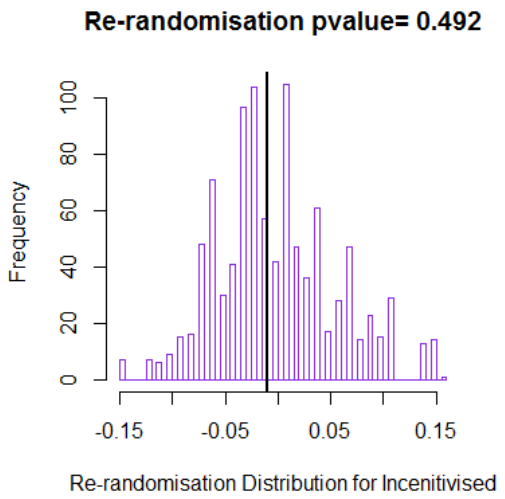
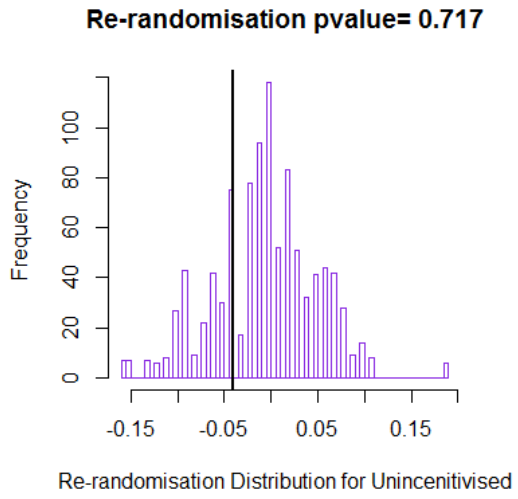
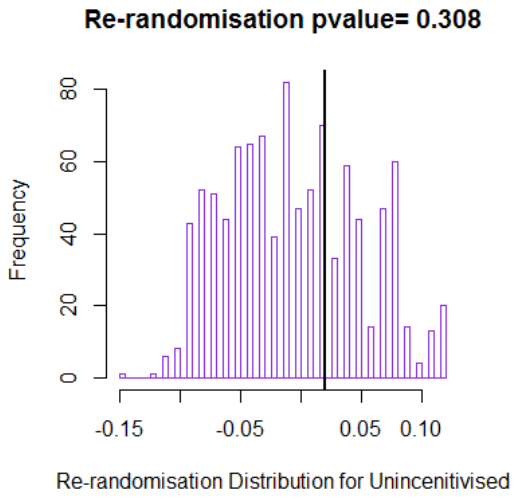
2. *Re-randomisation*

The results of the re-randomisation analysis support the findings from the main analysis. The likelihood of obtaining the maths and reading outcomes measured assuming the intervention has no effect (the *p*-value) is 0.44 and 0.49 for the incentivised Parent Academy classes, respectively (compared to 0.39 and 0.48, respectively, in the primary analysis) and 0.72 and 0.31 for the unincentivised classes (compared to 0.76 and 0.33 in the primary analysis). The distribution of effect sizes is displayed in Figure 5.

As above, the *p*-value is derived using a repeated and random assignment of a hypothetical treatment status to pupils participating in the trial. This time, the random assignment is restricted in a similar way that it was restricted in the original randomisation (see the 'Methodology' section). The results for the maths outcome of the incentivised group vs control group show that in 43.6% of hypothetical random

assignments, the calculated effect size was larger than the one actually measured. We thus cannot reject the hypothesis that the treatment had no effect.

Figure 5: Distribution of effect sizes with re-randomisation analysis

	Maths outcome	Reading outcome
Incentivised vs control		
Unincentivised vs control		

3. *Missing data*

Loss to follow-up is very low in both the reading and maths outcome as shown in Table 13**Table 13**, as well as the CONSORT diagram. The loss to follow-up rate for maths was 2.3%, 2.6% and 2.8% for incentivised, unincentivised and control groups, respectively. The loss to follow-up rate for reading was 4.6%, 3.5% and 4.0%, respectively.

Table 13: Distribution of missing data

Outcome	Incentivised group	Unincentivised group	Control group	Total
Maths	2.3% (12/517)	2.6% (16/627)	2.8% (23/820)	2.6% (51/1,964)
Reading	4.6% (24/517)	3.5% (22/627)	4.0% (33/820)	4.0% (79/1,964)

The loss-to-follow-up model did show higher odds of missing data for Year 4 than for the other years for the reading outcome. Also, the multiple imputation analysis resulted in a similar effect size as the analysis without imputation. There is no evidence based on this sensitivity analysis to suggest that the conclusion of the intervention effect would have been different had there been no missing data.

The results of the model and multiple imputation analysis are presented in Table 14. They show that the multiple imputation analysis resulted in a similar effect size as the analysis without imputation. The loss-to-follow-up model did show higher odds of missing data for Year 4 than for the other years for the reading outcome. However, overall there is no evidence based on this sensitivity analysis to suggest that the conclusion of the intervention effect would have been different had there been no missing data.

Table 14: Logistic regression analysis of dropout r and multiple imputation results

Group	Dropout Model: LogOR (95% CI)		MLM with Imputation: Difference/Slope (95% CI)	
	Maths	Reading	Maths	Reading
Intercept	-3.23 (-5.21; -1.26)	-2.50 (-3.55; -1.46)	-4.01 (-4.25; -3.77)	-4.05 (-4.31; -3.79)
Incentivised – Control	-0.09 (-0.53; 0.36)	0.12 (-0.24; 0.47)	0.00 (-0.11; 0.11)	-0.01 (-0.14; 0.13)
Unincentivised – Control	-0.12 (-1.23; 0.99)	-0.20 (-0.84; 0.44)	-0.05 (-0.15; 0.06)	0.01 (-0.12; 0.14)
Baseline	-0.03 (-0.09; 0.03)	0.02 (-0.05; 0.01)	0.26 (0.25; 0.27)	0.28 (0.27; 0.28)

Year 3–Year 6	-0.15 (-1.56; 1.26)	-0.08 (-1.26; 1.00)	-	-
Year 4–Year 6	0.69 (-0.29; 1.68)	-1.11 (-1.76; -0.46)	-	-
Year 5–Year 6	-0.17 (-0.93; 0.59)	-0.56 (-1.35; 0.24)	-	-
FSM (0–1)	-0.24 (-1.01; 0.52)	0.06 (-0.38; 0.49)	-	-
Gender (F–M)	-0.03 (-0.74; 0.68)	-0.09 (-0.51; 0.33)	-	-

4. *FSM eligibility*

The analysis of effects on only those pupils eligible for FSM showed a similar picture to the main results. As shown in Table 15, the estimated effect of the incentivised treatment was -0.02 standard deviations for maths, and 0.00 for reading outcomes. The estimate for the effect of the unincentivised treatment was -0.01 for maths and 0.00 for reading outcomes. However, none of the effects were statistically significantly different from zero. In addition, the differences in point estimates between the outcomes for all pupils and FSM pupils are of a negligible size, and an interaction test of the overall intervention with an FSM indicator (not presented here) did not result in a statistically significant different effect for FSM pupils.

Table 15: Effect sizes for effect on FSM subgroup

	Maths outcome	Reading outcome
Incentivised vs. control		
Effect size	-0.02	0.00
Confidence intervals	(-0.26, 0.22)	(-0.18, 0.19)
Unincentivised vs. control		
Effect size	-0.01	0.00
Confidence intervals	(-0.25, 0.23)	(-0.18, 0.18)

5. *Attendance*

Attendance at the Parenting Academy was very low. Over four in ten parents (41%) invited to the incentivised and nearly three quarters (73%) of parents invited to the unincentivised group did not attend any classes (see Table 16). However, even when controlling for attendance, the Parent Academy did not show an effect on outcomes (see Table 19).

Table 16: Attendance at the Parent Academy

	Incentivised classes (%)	Unincentivised classes (%)	Overall (%)
0% attendance	41.2	73.37	58.83
Less than 50% attendance	7.93	11.96	10.14
50% or more attendance	28.82	10.85	18.97
100% attendance	22.05	3.83	12.06
Total	**100**	**100**	**100**

The average percentage of classes attended in the incentivised group was 47%, while it was 14% for the unincentivised group. As shown in Table **17** 17, incentivising attendance increased attendance by a statistically significant 33 percentage points.

Table 17: Difference in attendance between incentivised and unincentivised groups

	N	Mean proportion of classes attended by parent	95% confidence interval
Incentivised group	517	0.47	0.43 – 0.51
Unincentivised group	627	0.14	0.12 – 0.17
Difference		0.33	0.29 – 0.37

Examining observable factors associated with parental attendance at the Parent Academy using a zero inflated beta model[5] (Buis, 2012) found that incentivising parents was a strong predictor of attendance even when controlling for pupil prior achievement, free school meal eligibility, pupil class attendance and local authority area[6]. The model also showed that pupil class attendance was a strong predictor of parental attendance, perhaps indicating parental attitudes to their child's education. Overall, neither FSM eligibility nor pupil prior attainment (measured as Key Stage 1 Average Point Score for reading, writing and maths) had any discernible influence on parental attendance (Table 18).

Table 18: Marginal effects of influences on parental attendance using a zero inflated beta model

[5]This approach models the proportion of classes attended by parents, but treats parents that never attended any classes differently from those that attended at least some.

[6] Local authorities led the implementation of the trial in their area and thus had an influence on the effectiveness of recruitment and parental engagement.

	Coefficient	*p*-value	95% confidence interval
Incentivised (dummy)	0.15	0.00	(0.11, 0.18)
KS1 APS (RWM) (prior attainment)	0.00	0.30	(0.00, 0.00)
FSM eligibility (dummy)	0.00	0.64	(-0.02, 0.01)
Attendance	0.33	0.00	(0.11, 0.55)
LA (dummy)	0.01	0.19	(-0.01, 0.03)

Estimates of the complier average causal effect (CACE) are presented in Table 19, showing the estimated treatment effect taking account of the same covariates as the primary model plus the addition of an attendance measure for parents in the treatment group (defined as proportion of possible sessions attended). As before, the point estimates are very close to zero and indicate that the treatment did not have an effect at the time of measurement. The results are, again, not statistically significant.

Table 19: Effect sizes from model taking account of attendance

	Maths outcome	Reading outcome
Incentivised vs. control		
Effect size	-0.05	-0.03
Confidence intervals	(-0.26, 0.16)	(-0.20, 0.14)
Unincentivised vs. control		
Effect size	-0.06	0.01
Confidence intervals	(-0.27, 0.15)	(-0.16, 0.18)

Cost

Upfront costs for the intervention include costs for promotional activities (including an event for headteachers), staff recruitment, equipment, and training.

To deliver the 13 sessions of the Parent Academy, the main costs incurred were for staff time and parent incentives. Staff appointed to deliver the Parent Academy included a full-time programme manager, and an assistant programme manager in each area. In addition, four part-time tutors were appointed in each area to plan and deliver the sessions. Tutors committed to six training days (four separate events) and weekly meetings, and were required to be available to parents before and after each session. Parents were given £30 in vouchers for each session attended, making incentives the largest non-staff costs.

Other running costs included sessional childcare staff, refreshments, payment for transport to sessions (paid to parents in Area 1 only), printing/photocopying, and venue hire (in instances where suitable space on school sites was not available).

In schools, the headteacher in each school took on certain responsibilities associated with organising the intervention. School staff in varying roles worked with Parent Academy staff to support engagement activities, and attend meetings to discuss progress. The time and associated cost of school staff involvement varied across schools and an estimate was not provided by the programme developers or collected from schools.

Cost per pupil

The three-year average annual cost per pupil[7] including all trial participants recruited into the two treated groups independent of attendance status (n=1,248[8]) is £641 for the incentivised and £280 for the unincentivised group. Table 20 sets this out in more detail. It is important to note that set-up costs have been divided equally between incentivised and unincentivised groups, although some costs arguably would be incurred in full if only one group was run (for example, the event for headteachers).

Table 20: Total cost per pupil

	Incentivised group (n=569)		Unincentivised group (n=679)	
	Cost (£)	Cost (£) per pupil	Cost (£)	Cost (£) per pupil
Start-up cost	9,865	17	9,865	15
Running costs per year	361,617	636	186,657	275
Total cost—first year	**371,482**	**653**	**196,552**	**289**
Total cost over three years	1,094,716	1,924	569,836	839
Average cost/year over three years	**364,905**	**641**	**189,945**	**280**

When these figures are disaggregated by local authority, the results are quite different: respectively £754 and £366 for incentivised and unincentivised groups in Area 1, and £549 and £207 for Area 2 (see Table 21 and Table 22, respectively). The reason for the differences in per pupil costs is due to three interacting factors:

1. Staff costs were lower in Area 2.
2. The greater recruitment success in Area 2 reduced the impact of some annual fixed costs.
3. The lower attendance rate in Area 2 reduced expenditure on incentives for the incentivised group.

[7]Costs were calculated by adding a third of the start-up costs to the annual running costs, to give an estimate of average annual costs incurred if the intervention was run for three years.

[8] See section 'Participants', where the recruitment process is explained in detail.

Table 21: Area 1 total cost and cost per pupil

Area 1	Incentivised group (n=256)		Unincentivised group (n=311)	
Item	Cost (£)	Cost (£) per pupil	Cost (£)	Cost (£) per pupil
Start-up cost	7,265	28	7,265	23
Running costs per year	**190,730**	**745**	**111,493**	**358**

Table 22: Area 2 total cost and cost per pupil

Area 2	Incentivised group (n=313)		Unincentivised group (n=368)	
Item	Cost (£)	Cost (£) per pupil	Cost (£)	Cost (£) per pupil
Start-up cost	2,600	8	2,600	7
Running costs per year	**170,888**	**546**	**75,164**	**204**

The above costs exclude school staff time and cost, therefore an estimate of cost for school staff involved in parent engagement is likely to be needed in order to provide a more accurate per pupil cost.

Attendance data collated by the University of Chicago (refer to Table 16) suggests that incentivisation helped to engage and retain parents in the intervention. The cost for incentivising all parents should be considered when calculating the costs of delivering similar but different interventions in the future.

Process evaluation

This section synthesises the findings from the process evaluation by bringing together the perspectives and experiences of stakeholders who participated in the evaluation research. These include: Parent Academy tutors who developed and delivered sessions for parents; teachers who supported delivery of the intervention; and parents who attended Parent Academy sessions. It discusses the main issues related to implementing the Parent Academy, sets out the key challenges, and identifies areas of improvement that may strengthen fidelity and replication. All Parent Academy tutors were invited to take part in the research along with six purposively selected school staff. The research was conducted in two waves, the first designed to understand the development and early delivery of the intervention, and the second to understand how delivery progressed, the challenges encountered, and the perceived benefits of participation. Table 23 sets out the number of achieved interviews for each type of participant.

Table 23: Achieved interviews

Wave	Participant	Achieved interviews
Wave 1	Parents	18
	PA tutors	7
Wave 2	Parents	12
	PA tutors	7
	School staff	6

Implementation

The discussion on implementation is structured around the key elements that were found to be related to successful delivery of the Parent Academy, namely:

- attractiveness of intervention and school motivation to take part;
- role of the area programme manager;
- developing Parent Academy session content;
- flexibility in content development and teaching techniques;
- practical delivery arrangement;
- relationship with schools and the role of school staff;
- parent engagement and retention; and
- attendance rates.

Attractiveness of intervention and motivation to take part

Encouraging parental engagement in their children's education was identified by some teachers as one of their school's central aims. As such, the Parent Academy aligned with the existing school ethos. There

was a similar understanding among tutors of the need for an intervention that bridged the relationship between parents and teachers to support children's education.

> *'Well for me the main goal is to, for us to give parents some skills and strategies to help them to help their children's education…'*

Teachers saw the Parent Academy as another approach to engage parents in their child's education (see 'Parent engagement' section below). However, their enthusiasm was tempered by a concern among teachers of the 'inequality' of incentivising some parents and not others to take part.

Role of the area programme manager

The area programme managers played a prominent role in ensuring the smooth running of the Parent Academy. They were able to provide the dedicated time and effort required to facilitate the development of intervention content as the intervention was delivered. Their role included developing strategies to engage and retain parents, school liaison, developing the timetable, and managing Parent Academy tutors. Their knowledge and experience of teaching adults and project management and leadership within an educational context were believed to be critical to the role. The area managers' local authority base supported access to data, provided office space, and helped to publicise the intervention widely.

Developing Parent Academy session content

The content of all sessions was developed based on a core framework and lesson plan agreed with the developers. Four formal training sessions were held with all tutors from both areas to develop the programme. Tutors felt well supported to develop sessions while delivery of the Parent Academy was in progress, and weekly meetings in each area facilitated a collaborative approach.

Concern was expressed in relation to varying skill levels and experience among the tutors and the differences in teaching styles which made it more difficult to develop a consistent teaching approach. Although working with the University of Chicago staff was viewed positively, the distance was identified as a challenge to effective communication.

> '*…we have a very different view on how to put the curriculum and the course across... and we feel it's been ignored time and time again. We've actually stopped commenting on the resources, we've just totally ignored them and written our own.'*

A collaborative working approach was facilitated and led by the area programme manager, who provided support to tutors on an ongoing basis, was seen as instrumental to the success of the programme. Although the frequency of meetings and collaborative approach had come about naturally, tutors felt that collaborative content development was beneficial to the programme as a whole. A suggestion would be to formalise a collaborative approach in any documents developed to guide future delivery.

Flexibility in content development and teaching techniques

Flexibility in content development was identified as important for the effective delivery of the Parent Academy. Ownership of content by tutors adapting the content to suit individual teaching styles and parents' abilities were two aspects of flexibility that were considered to underpin successful delivery.

Moreover, establishing a relaxed learning environment that encouraged social interaction during sessions was believed to be a key component of success. Activities and teaching techniques that supported this included paired and group work and the provision of refreshments. Importantly, tutors strove to create a learning environment where parents '*[felt] they can make mistakes*'.

This flexibility to adapt teaching approaches was thought to be important in order to accommodate parents with varying skills and abilities. The greatest challenge was believed to be delivering sessions

to parents with limited or no proficiency in English and poor mathematics skills. The needs of this group of parents were a priority when Parent Academy tutors discussed materials and teaching techniques. At the other end of the spectrum were parents with a high level of skill who felt they '*already knew it all*'. Therefore, the ongoing challenge was to deliver sessions that appealed to parents across the ability spectrum which meant that tutors had to adapt each session based on the types of parents attending. While the topics and content were covered consistently across sessions and areas, Parent Academy tutors and schools suggested that the content could be developed further to complete curriculum gaps and include topics such as 'parenting skills'. Developing a 'toolbox' of materials and techniques that Parent Academy tutors can draw on to help engage diverse parents with lesson content was highlighted as a possible method for increasing engagement and retention.

Practical delivery arrangements

Participating schools were asked to identify rooms within school grounds to deliver the Parent Academy. Where this was not possible external venues were used. The settings where the Parent Academy was delivered varied with some sessions delivered in well-equipped and adequately sized rooms and other instances where the tables and chairs (in a school setting) were designed for children but tutors and parents had to make do. Important for effectively delivery was a consideration of:

- sufficient space to allow paired and group work and for parents to interact;
- accessible venue to parents—close to school or home but not necessarily within a school setting; and
- access to appropriate IT facilities.

Multiple sessions were delivered during the week and on weekends to accommodate parents who were in employment and to separate incentivised and non-incentivised groups of parents. The original intention was to deliver the sessions for Year 3 and 4 parents separately from those for Year 5 and 6 parents. This approach was altered due to low attendance rates and parents across all years were invited to attend any session while maintaining the separation between the incentivised and non-incentivised groups.

Based on early feedback from parents, the session timetable was revised to offer lessons during school time (so parents could attend while their child was in school) and during the evening and on weekends (for parents with weekday commitments). However, weekend sessions were poorly attended.

Relationship with schools and the role of school staff

As delivery of the Parent Academy was organised and managed at local authority level, the perception of tutors was that the initial engagement of teachers was challenging because:

- teachers were perceived to be extremely busy;
- Parent Academy tutors felt they were intruding on the school; and
- some school staff felt 'threatened' by the research element of the project.

Allocating sufficient time to engage with and build relationships with school staff, and face-to-face meetings arranged at times suitable for teachers, were thought to be important in overcoming these challenges.

Parent engagement and retention

The initial information letter sent out by the Unversity of Chicago informing parents about the Parent Academy received a mixed response. Those who had ignored the letter or failed to engage with its content learned about it by word of mouth from other parents, teachers, and school-based Parent Support Advisors. Involvement of school staff to convince parents helped with initial parent engagement.

> *'[The staff member] said that, 'This course would be quite good for you to have a better understanding of the way your child learns'…I thought, oh yeah, it sounds good.'*

Face-to-face contact with school staff was thought to help parents more than written communication (letter and text). However, a letter that stood out was enclosed in a gold envelope and contained what was perceived to be useful information about their group allocation and a timetable of sessions.

A desire to support their child (and in some cases, grandchild) to improve their learning, along with the potential to improve their own skills, were motivating factors for parents joining the PA.

> '[*I decided to join the academy] so it could help me, really. I'm more aware of how they're teaching in class. How can I get my kids to improve. You know, to do better in life, maybe better in their exams.'*

Clarity in communicating the aims of the Parent Academy and explaining, in accessible ways, the potential benefits to parents would be important to successful engagement.

Retaining parents in the programme required additional efforts by school staff and Parent Academy tutors, who used various approaches (email; text messaging; door knocking; telephone calls; speaking to parents when they picked up their child from school) to remind parents of up-coming sessions. Due to data protection considerations, school staff were mainly responsible for contacting parents, with face-to-face contact perceived to be the most promising method.

The provision of refreshments was thought to have supported engagement and retention of parents and contributed to the relaxed and informal learning environment necessary to engage diverse parents, including those who may have had barriers to learning due to their own experiences of schooling.

Attendance rates and financial incentives

The offer of £30 per session to attend the Parent Academy motivated some of the parents from the incentivised group to continue attending. This group of parents felt it was a '*fair payment*' for their time, without which attending the Parent Academy would not have been appealing.

In some cases, the financial value of the incentive provided participants with additional motivation to attend sessions more regularly than they would have done otherwise.

> *'My honest opinion was I would've attended but probably didn't make that much effort to attend every week 'cause obviously I've got three children…but with the vouchers I knew I was getting £30 vouchers, which is a lot. That was my one week's shopping out of the way for the food—so I was attending regular.'*

Attendance data was collected for each area by the University of Chicago. Table 24 below presents attendance data by local area. Table 25 presents attendance rates by treatment group.

Table 24: Attendance rates by area

	Area 1 (%)	Area 2 (%)	Overall (%)
100% attendance	15	9	11
50% or more attendance	20	17	18
Less than 50% attendance	8	12	10
0% attendance	57	63	60

Base: Pupils assigned to treatment groups (n = 1,248)

Table 25: Attendance rates by treatment group

	Incentivised classes (%)	Unincentivised classes (%)	Overall (%)
100% attendance	21	4	11
50% or more attendance	28	10	18
Less than 50% attendance	8	12	10
0% attendance	43	74	60

Base: Pupils assigned to treatment groups (n = 1,248)

Between the incentivised and the unincentivised groups, there was an 18 percentage point difference between the proportion of parents attending at least half of the classes, and a 17 percentage point different in the full attendance figures. The variation in attendance suggests that the £30 voucher given to parents after attending each session was successful in increasing attendance rates[9].

Those who attended the Parent Academy regularly found the fortnightly sessions and the 90 minutes to2 hours length of sessions to be appropriate and manageable. Poor attendance meant that Parent Academy tutors found it difficult to deliver sessions in the intended way.

[9] This is based on a final milestone report on the Parent Academy prepared by the University of Chicago team and submitted to the Education Endowment Foundation in September 2015.

Fidelity

When assessing fidelity to the programme delivery model there are two aspects to consider. The first relates to whether the programme content was developed as intended. The second relates to fidelity in how the Parent Academy sessions were delivered across the different sites.

The Parent Academy was inspired by the Chicago Heights Early Childhood Center (CHECC) parent programme. The CHECC programme focuses on parents of pre-school children (ages 3 to 5), while the Parent Academy is delivered for the parents of English primary school children in Years 3 to 6 (ages 7 to 11). Programme developers agreed a core framework—a Scheme of Work—for the Parent Academy (based on the English primary school curriculum) based on which lesson plans were developed. This development of the programme was executed as intended, however gaps in topic coverage were highlighted by Parent Academy tutors.

Delivery of sessions based on these lessons plans was reported as being consistent across both areas. Flexibility was introduced in the use of materials and teaching techniques deployed in order to engage parents across a range of skills.

A number of factors that may risk fidelity were identified:

- Tutors had insufficient time to build effective relationships with schools—more time was needed to work with school staff, and to involve them with the Parent Academy, primarily to support parent engagement and participation.
- A designated member of school staff with time ring-fenced to support the Parent Academy was considered important to its' success. In some schools the support of Parent Engagement Officers or Family Support Workers was critical to the successful engagement of parents.
- A lack of suitable space to deliver lessons that involved group-based activities. This was identified as an issue in some locations. Availability of a suitable space may also have restricted the number of sessions that were delivered in any given week to cover parents of children in Years 3 to 6 and to provide separate sessions for non-incentivised and incentivised parents. Parents preferred a location that was familiar to them (such as a school setting) and the use of external settings may have affected attendance rates.
- A lack of coordination among Parent Academy tutors could threaten consistent delivery. Weekly meetings and sessions to develop content in a coordinated way worked well. Even if content is manualised for consistent delivery, frequent meetings would need to be scheduled to facilitate a collaborative approach.
- Poor or ineffective communication about the Parent Academy and what it was trying to achieve for parents and children alongside a weak understanding of personal constraints (such as disengagement from education or limited time and resources to attend) were identified as risks to successfully delivery. Sufficient resources would need to be allocated for face-to-face parent engagement activities.

Crucial to ensuring a consistent delivery approach was the area programme manager. This role helped to foster a collaborative approach among Parent Academy tutors and facilitated the relationship with schools. Face-to-face communication with school staff helped to secure delivery locations and develop a session timetable that suited schools and parents. In addition, flexibility on the part of Parent Academy tutors was an important skill so that the session timetable could be amended to suit parents' availability.

Perceived outcomes/benefits

The overall view among school and Parent Academy staff was that there is an ongoing need for parenting education programmes that teach parents the skills and knowledge they need to support their child's education.

With regard to the specific Parent Academy intervention, perceived benefits included:

- *Improved knowledge of the curriculum*—parents were better able to understand educational terminology and the structure and content of the educational curriculum. This familiarity was believed to have increased parents' confidence to become more involved in their child's learning.
- *Increased confidence of parents in engaging with their child's school*—the perception was that parents were communicating in different, improved ways with teachers during informal catch-ups and at parents' evenings.

  > *'I can ask them [child's teachers] more questions as well, how well she's doing in her maths or what she's been taught…'*

- *Increased involvement of parents in their child's learning*—the evidence suggests that parents were more actively helping children with their English and maths homework. This was perceived to have been partly due to an improvement in parents' listening and questioning skills when interacting with their child. The 'Fun Home Activity' which accompanied each session was perceived to have helped parents to bond with their child.

  > *'Sometimes when walking to school, we'll play games on working out percentages and divisions, and mathematics problems, which I didn't do beforehand.'*

- *Improvement in pupils' English grades*—school staff noted an improvement in pupils' English grades which they speculated was due to improvements in parent-child interactions in relation to schooling and school work.

  > *'The major benefit, I thought, was improvement with my child's education… yesterday I had his parents' evening with his classroom teacher… He made improvement, 'specially the English. He exceeded his English grade… He was below the average most of time. This time he's exceeded his Year 5 English level.'*

- *Increased parental engagement*—the perception was that parents who had previously appeared 'disengaged' were more engaged with their child's education after having taken part in the Parent Academy. These parents were thought to have overcome their discomfort of being in a school setting.

Moreover, a renewed interest among parents to improve their own skills and education was evidenced, as was an increased awareness of their role in their child's education. Parents with English as an additional language (EAL) found that their English had improved as they were forced to speak English during sessions. Alongside this, children were reported to be pleased that their parents were attending the Parent Academy.

Attending classes with other parents and taking part in group activities (helped by the provision of refreshments) meant that parents were interacting more with each other. This was perceived to have strengthened relationships and led to the formation of new friendships. This was valued and appreciated as an additional benefit of the intervention.

> '*Gosh it was just, it was just good to get out of the house, and you know, and just sit and have a cup of tea, and not worry*.'

There was an acknowledgement among school staff that change among parents and their children is likely to be gradual with further benefits to be accrued over time.

Formative findings

The findings from the process study indicate that the intervention in its current form was well received by attending parents and schools alike. In both areas there is a commitment to continue to deliver Parent Academy courses. Intervention delivery could be strengthened by manualising the topic coverage and lesson content or by developing a database of resources. At the same time some flexibility has to be encouraged to allow tutors to develop teaching techniques collaboratively in order to deliver content in ways that accommodate parents' skills and abilities. Incentives motivated parents to attend and information about this offer should be included in the initial publicity materials to encourage engagement. Lastly, the provision of refreshments and childcare, for those who needed it, was believed to facilitate initial engagement and ongoing attendance.

Aspects of intervention delivery that should be reviewed are:

Communications: a range of approaches to communicating information about the intervention and its objectives could be set out during the planning stage. Establishing a personal relationship with schools and parents and regular communication with parents was found to be essential to keeping parents engaged.

Promoting the Parent Academy (to encourage retention): ongoing promotion to retain parents in the intervention was a modification introduced during the delivery of the Parent Academy. To be successful, approaches such as promoting the intervention during parent-school meetings, may need to be agreed with schools from the start.

Topic coverage: parenting skills was a topic which was not covered in the Parent Academy. This was identified as an important gap should a review of the content be conducted. The challenge to delivery is developing content that engages parents with varying skills and at the same time covers the KS2 curriculum in sufficient detail to be relevant for each year group.

Sessions for parents and children: It was believed that delivering a small number of sessions where children and parents attend together (perhaps at the start of the Parent Academy and at the end of each scheme of work) may encourage parents to take part.

Modifications suggested by the University of Chicago team
To be considered in any future development of the project are additional suggestions made by the intervention developers. These include:

- weekly sessions (instead of the fortnightly sessions delivered);
- each session to last 2 hours;
- higher level of incentivisation than the £30 shopping vouchers per session given to parents;
- parents champions to support recruitment and attendance;
- self-selection of parents to the intervention; and
- adaption of the content to cover the Key Stage 1 curriculum and to address the transition from Key Stage 1 to Key Stage 2 (that is, from Year 2 to Year 3).

To replicate the area-based delivery approach, local authority involvement by appointing an area programme manager is likely to be important. Four Parent Academy tutors per area to deliver sessions for the Key Stage 2 curriculum across eight schools was perceived to be sufficient. A school-based delivery model may be logistically less complicated but would require dedicated staff time to plan and deliver the intervention.

Although the Parent Academy was well received by those who attended sessions, the high percentage of 'no-shows'—parents who never attended—means that identifying effective ways to engage and retain parents should be a priority for initiatives with similar aims.

Control group activity

The trial assumes a 'business as usual' approach to parenting involvement in the control group. This means that parents of children in the control group were not invited to take part in any Parent Academy sessions but individual schools may have run other parent-focused interventions. We are not aware that such interventions actually took place, but research with parents in the control group was not included in the study.

An individual-level randomisation design was chosen because the risk of spillover effects if parents participating in the Parent Academy shared their learning with non-participating parents was judged to be small. However, a measurement of any spillover was not part of the research design.

Conclusion

Key Conclusions
1. There is no evidence that offering free Parent Academy classes improved mathematics or reading outcomes for the children in the trial, even when parents were given a financial incentive to attend.
2. In general, parental attendance at sessions was very low. However, even when the evaluators took this into account they found no evidence that parental attendance improved pupil outcomes.
3. Offering financial incentives improved attendance at Parent Academy, suggesting this may be an effective way to engage and retain parents in interventions of this type.
4. In general, evaluation participants felt that attending Parent Academy gave parents the confidence and skills to engage more effectively with their children's learning.
5. Staff reported that having a designated project lead in each school and using multiple methods to engage parents (rather than just written communication) were necessary for successful delivery. Schools need to consider these cost implications when deciding whether to adopt this intervention.

Limitations

There are four main limitations on the security of findings from this trial:

- Parental take up of the intervention was very low.
- A complex randomisation process excluded many participating pupils.
- The intervention was not sufficiently developed.
- There was a high level of flexibility built into content development.

The very low take-up of the intervention makes finding any impact very unlikely. This trial directly and unbiasedly assessed the effect of the *offer* of the Parenting Academy, with or without an incentive to take part, rather than the effect of taking part itself. This trial provides less insight into the actual effect of training for parents among those who take part in the tuition on the educational outcomes of their children. However, the analysis accounting for attendance that we have conducted does not suggest that there are strong effects among the groups of children whose parents did attend the Parenting Academy. Any future research on this approach could test different ways of engaging parents, including focusing on particular subgroups.

The randomisation approach, due to missing data and the matching requirements, led to the exclusion from analysis of some 30% of participating pupils. While all pupils were excluded at random and mean effects should therefore not be biased, a simpler randomisation strategy could have had a larger sample size and thus a smaller minimum detectable effect size.

Important aspects of the intervention were developed during the trial. This includes specific lesson plans and also activities undertaken to engage parents. As a result, even if take-up had been higher and the randomisation had not affected the sample size, it would have been difficult to assess fidelity and to determine what exactly had caused any estimated effect since the intervention was reacting to circumstances.

Assessing fidelity was all the more difficult due to the high level of flexibility built into content development. Programme managers and tutors felt able to disregard the advice and resources provided by the University of Chicago to develop lesson content.

There are other limitations, particularly the short follow-up period, the risk of spillover, and external validity problems, as discussed below.

Pupils were followed up only two weeks after the end of the intervention. Given that the Parent Academy aims to influence pupils' results only indirectly, a longer follow-up period would be more appropriate to detect any effects.

Spillover may have occurred if parents participating in the Parent Academy transmitted their knowledge or attitude to engaging in learning with their children to other parents. Although we have not measured what the control group parents did, we judge the spillover risk to be small. We hypothesised that the intervention's main transmission mechanism is the knowledge parents gain from participating, and that the likelihood of parents teaching other parents is low. It is possible, however, that an intervention such as this, if it were effective, would work primarily via a change in attitudes rather than learning, which could be transmitted to other parents, making spillover more likely. However, we do not explore that possibility here as we do not have any insight into the attitudes and views of control group parents.

Finally, the trial took place in 16 schools in two urban local authorities which means that the scope of the intervention (and the trial) was limited. The extent to which schools engaged with the intervention varied. Those more committed to parent engagement or with more enthusiastic or involved headteachers were more willing to allocate resources to support the intervention. This contextual variability, alongside issues with initial engagement and retention of parents and the availability of suitable venues, limits the applicability of findings to other areas.

Interpretation

The trial indicates that inviting parents to English and maths sessions to improve their children's home learning environments does not affect outcomes of pupils in the short term. This is true for both maths and reading outcomes. There is an indication that financial incentives increased parents' attendance but that this did not alter the mean estimate of outcomes.

Even if an effect had been found, in the first year, the intervention was moderately expensive, at £653 per pupil per year for the incentivised and £289 for the unincentivised group. Averaged over three years, the annual cost would be £641 per pupil for the incentivised and £280 per pupil for the unincentivised group. Intervention effects would have to be very large to justify this level of expenditure.

Evidence from the process study found that parents who attended sessions appreciated the additional learning which they saw as benefitting themselves and their children. Approaches to engaging and retaining parents were modified during the intervention. As engagement approaches were not the focus of the study, it is difficult to determine which strategies were most effective.

Parent Academy tutors valued the collaborative process undertaken to develop and deliver the intervention. Flexibility to adapt teaching techniques to include parents with a range of skills was considered an important aspect of the approach undertaken. Even if the content of the Parent Academy is manualised, there would be value in fostering a collaborative approach to facilitate the promotion of good practice in teaching and to help to build relationships between Parent Academy and school staff. The involvement of school staff depended on the level of effort expended to engage and retain parents. A designated school contact for the Parent Academy programme manager and for parents was helpful, but it was difficult to estimate the time schools would need to set aside due to the range of engagement activities deployed.

The null results of this trial raise the possibility that the intervention logic may contain weaknesses—perhaps due to the initial lack of specificity of the Parent Academy programme or its development from an intervention intended for the parents of pre-schoolers. It may be that insufficient consideration was given to the local cultural context within which parental behaviour and perceptions of this type of intervention are likely to be different from those of parents in Chicago Heights. We would also question

whether the dosage (the number of sessions delivered) was sufficient to achieve the level of change needed to observe strong effects.

Future research and publications

This intervention would merit further evaluation once the shortcomings of the intervention have been considered and addressed to the extent possible.

Intervention specificity: The intervention was developed while delivery was taking place, which means that context specific modifications may have been made to lesson plans and content. While some flexibility in teaching techniques is important to accommodate the range of skills among parents, steps to manualise the intervention may help to support consistent delivery across a wider geographical area. This includes setting out clear lesson plans aligned with parental and pupil learning requirements, and specifying effective ways to engage and retain parents.

The small number of schools: The small number of schools taking part in this trial may mean that the evaluation findings are anomalous, in part as a result of context specific issues relating to the recruitment of schools as well as the type of parents who attended (and those who did not). Conducting a larger trial should help to minimize the influence of contextual factors.

Parental involvement: Testing the effectiveness of parent engagement activities would help to better understand how attendance rates for this type of parenting intervention can be boosted.

Evaluation timescales: The evaluation assessed the outcome of the intervention immediately after the end of the intervention. Given that parental behaviour change is an intermediate step to improvements in attainment outcomes for children, this short follow-up period is likely to limit the detection of any effect that the intervention may have. If the evaluation is limited to a short evaluation timetable, or where it focuses on an intervention where the target group is different from the group on which impact is being measured (such as the Parent Academy), a study of intermediate outcomes would be beneficial. In this case, a study of intermediate parent outcomes and behaviour change within the home learning environment would provide valuable insight into the effectiveness of the intervention.

References

Bates, D. et al. (2015) 'Fitting linear mixed-effects models using lme4'. *Journal of Statistical Software* [online] 67: 1, 1–48.

Bruhn, M. and McKenzie, D. (2009) 'In pursuit of balance: Randomization in practice in development field experiments'. *American Economic Journal: Applied Economics* [online] 1: 4, 200–232.

Buis, M. L. (2012) *ZOIB: Stata module to fit a zero-one inflated beta distribution by maximum likelihood*. Statistical Software Components. Boston College Department of Economics.

Centre for Evaluation and Monitoring (2015) *Interactive Computerised Assessment System (InCAS) manual 2015*. Durham, UK: University of Durham.

Fehrmann, P.G. et al. (1987) 'Home influence on school learning: Direct and indirect effects of parental involvement on high school grades'. *The Journal of Educational Research*, 80: 6, 330–337.

Fewell, R.R. and Wheeden, C.A. (1998) 'A pilot study of intervention with adolescent mothers and their children: A preliminary examination of child outcomes'. *Topics in Early Childhood Special Education* [online] 18: 1, 18–25.

Fisher, S.R.A. (1935) *The design of experiments*. Oliver and Boyd.

Fryer, R., Levitt, S. and List, J. (2015) *Parental Incentives and Early Childhood Achievement: A Field Experiment in Chicago Heights*. Chicago, USA: University of Chicago and NBER.

Griffith, J. (1996) 'Relation of parental involvement, empowerment, and school traits to student academic performance'. *The Journal of Educational Research*, 90: 1, 33–41.

Hedges, L.V. (2007) 'Effect sizes in cluster-randomized designs'. *Journal of Educational and Behavioral Statistics*, 32: 4, 341–370.

Jeynes, W.H. (2003) 'A meta-analysis of the effects of parental involvement on minority children's academic achievement'. *Education and Urban Society* [online], 35: 2, 202–218.

Jeynes, W.H. (2007) 'The relationship between parental involvement and urban secondary school student academic achievement: a meta-analysis'. *Urban Education* [online], 42: 1, 82–110.

Marcon, R.A. (1999) 'Positive relationships between parent school involvement and public school inner-city preschoolers' development and academic performance'. *School Psychology Review*, 28: 3, 395.

Morgan, K.L. and Rubin, D.B. (2012) 'Rerandomization to improve covariate balance in experiments'. *The Annals of Statistics* [online], 40: 2, 1263–1282.

Olds, D.L. (2006) 'The nurse–family partnership: An evidence-based preventive intervention'. *Infant Mental Health Journal* [online], 27: 1, 5–25.

R Core Team (2015) *R: A language and environment for statistical computing*. Vienna, Austria: R Foundation for Statistical Computing.

Reid, M.J. et al. (2004) 'Halting the development of conduct problems in Head Start children: the effects of parent training'. *Journal of Clinical Child & Adolescent Psychology* [online], 33: 2, 279–291.

Reid, M.J. et al. (2001) 'Parent training in Head Start: a comparison of program response among African American, Asian American, Caucasian, and Hispanic mothers'. *Prevention Science* [online], 2: 4, 209–227.

Sanders, M.R., Montgomery, D.T. et al. (2000) 'The mass media and the prevention of child behavior problems: The evaluation of a television series to promote positive outcomes for parents and their children'. *Journal of Child Psychology and Psychiatry* [online], 41: 7, 939–948.

Sanders, M.R., Markie-Dadds, C, et al. (2000) 'The Triple P-Positive Parenting Program: A comparison of enhanced, standard, and self-directed behavioral family intervention for parents of children with early onset conduct problems'. *Journal of Consulting and Clinical Psychology* [online], 68: 4, 624–640.

SAS Institute Inc. (2010) *SAS software, version 9.2*. Cary, NC, USA: SAS Institute Inc.

Schulz, K.F. et al. (2010) 'CONSORT 2010 Statement: updated guidelines for reporting parallel group randomised trials'. *BMJ* [online], 340.

StataCorp (2011) *Stata Statistical Software: Release 12*. College Station, TX: StataCorp LP.

St. Pierre, R.G. and Layzer, J.I. (1999) 'Using home visits for multiple purposes: the Comprehensive Child Development Program'. *The Future of Children* [online], 9: 1, 134–151.

Torgerson, D.J. (2001) 'Contamination in trials: is cluster randomisation the answer?' *BMJ : British Medical Journal*, 322 (7282), 355–357.

Wagner, M. et al. (2002) 'The effectiveness of the parents as teachers program with low-income parents and children'. *Topics in Early Childhood Special Education* [online], 22: 2, 67–81.

Wagner, M.M. and Clayton, S.L. (1999) 'The parents as teachers program: Results from two demonstrations'. *The Future of Children* [online], 9: 1, 91–115.

Appendix A: Parent consent form

Parent Academy
Middlesbrough & Camden

NatCen
Social Research that works for society

Education Endowment Foundation

9th October 2014 Reference: P10684

Dear Parent or Carer,

RE: Parent Academy Programme

As you may know, your child's school is taking part in the Parent Academy Programme which is funded by the Education Endowment Foundation. The project involves offering some parents the chance to attend a course to find out more about what their children are learning at school. This will allow researchers at NatCen Social Research and the University of Chicago to look at the effects of the course on children's achievements in English and Maths at school. Please find enclosed a leaflet with more details about the Parent Academy Programme, including the organisations involved.

We are writing to ask for your permission to link your child's Unique Pupil Reference Number, age, gender and test results, and whether or not you were offered the Parent Academy Programme to the National Pupil Database, which is managed by the Department for Education. This database holds information at individual pupil-level about children's educational attainment at different stages so being able to link data from this project to the National Pupil Database means that researchers can access more information to understand the effects of the Parent Academy Programme. This information will be shared with NatCen Social Research and then stored in the Education Endowment Foundation's archive which is managed by the Fischer Family Trust and in anonymised form to the UK Data Archive. More information about this including some frequently asked questions can be found overleaf.

The data from this study will be stored securely and treated with the strictest confidence. Information will be used for research purposes only and no schools, parents or pupils will be identified in any reports.

Yours sincerely,

Senior Researcher
NatCen Social Research

Tel: 0800 6524569
Email: Parentacademy@natcen.ac.uk

If you are happy with everything in this letter you do not need to do anything. If you do not want your child's data to be matched with the National Pupil Database or stored in the Education Endowment Foundation Data Archive please fill in the slip below and return it to the school by the **23rd October**.

OPT-OUT SLIP ☐ I do **not** want my child to:

- Have their data linked to the National Pupil Database
- Have their data stored in the Education Endowment Foundation Data Archive

Your child's name.....

Name of School.....

Your full name.....

Your telephone number (optional).....

Your signature..... Date.....

Please return this slip to your school by the **23rd October** if you wish to opt-out. This will not affect your chance of selection to the Parent Academy courses.

Frequently Asked Questions:

What about data and confidentiality?

Pupil test responses and other data requested from the school will be collected and accessed by the research teams at NatCen Social Research and the University of Chicago and will be treated in the strictest confidence and in line with the Data Protection Act. Data will be submitted in anonymised form to the UK Data Archive.
With the permission of the Department for Education, data will be matched with the National Pupil Database held by the Department for Education and shared with NatCen Social Research and the Education Endowment Foundation's contractor, Fischer Family Trust Data Archive.

What is the National Pupil Database (NPD)?

The NPD is a database, managed by the Department for Education (DfE), which holds information at individual pupil-level about children's education at different stages, including test and exam results for all children in the state sector in England, as well as non-maintained special schools, sixth form and Further Education (FE) colleges and (where available) independent schools. The NPD also holds information about pupils such as age, gender, free school meal entitlement and educational attainment.

How will the data be used?

The data from this study will be used by the University of Chicago and NatCen Social Research who will prepare research reports for publication. The University of Chicago will only have access to test responses and anonymous data from the local authority. NatCen Social Research will have access to all data. No individual schools, parents or pupils will be identified in any reports arising from this research. Data from each of the schools involved in the project will be analysed anonymously, and findings will be reported anonymously, for example "x % of children had numerical skills at x level or above", so it will not be possible to identify individual pupils or schools.

Test responses will be collected by NatCen Social Research. Data will be matched with the National Pupil Database held by the Department for Education and shared with NatCen Social Research, Education Endowment Foundation's Data Archive which is managed by the Fischer Family Trust and in anonymised form to the UK Data Archive. Your child/children's data will be treated with the strictest confidence.

Appendix B: Analysis plan

Trial objective

Primary objective

To compare performance in English (reading) and mathematics between children whose parents were:

- offered an *incentivised* place in the Parenting Academy;
- offered an *un-incentivised* place in the Parenting Academy; and
- not offered a place.

Secondary objective

To assess whether the estimate of impact differs when using alternative analysis approaches, in particular when considering the use of re-randomisation.

Sample size

Minimum detectable effect sizes were calculated assuming 2200 children across 15 schools, based on the number of pupils in the participating schools. The intra-class correlation coefficient was assumed to be between 0.05 and 0.2, and the explanatory power of covariates, at individual level only, between 0 and 60%. Making these assumptions, the following minimum detectable effect sizes were calculated, expressed in standardised means, and given at the 5% significance level and 80% power:

	Values for intra-class correlation coefficient (rho)		
	0.05	**0.10**	**0.20**
Unadjusted	0.13	0.12	0.12
.20*	0.11	0.11	0.10
.40*	0.10	0.10	0.09
.60*	0.08	0.08	0.07
* proportion of residual variance at level 2 explained by covariates			

Note that these effect sizes are updated from the ones given in the original proposal.

Randomisation

The randomisation used a six-step process:

1. From each family with more than one eligible sibling, *one sibling was selected at random* for assignment. If there existed a non-selected sibling in the same year group (either Year 3 /4 or Year 5 / 6), the non-selected sibling was given the same assignment (but unmatched – see below) as the selected sibling, otherwise assigned to the (unmatched) control group.

2. *Pupils were stratified* by school, year, gender, English as additional language (EAL), ethnicity (White British versus Other), and whether or not additional data linkage consent was given.

3. Within each stratum, pupils were ordered by Key Stage 1 Average Point Score (KS1 APS, comprising of reading, writing and maths scores), and then paired consecutively with two other pupils to form a triplet. If the number of pupils in a stratum was not divisible by 3, one or two pupils were randomly removed to create adequately-sized strata.

4. Pupils removed at step 3 were pooled; stratified by school, year and whether or not additional data linkage consent was given; and paired to triplets as described in step 3. If the number of pupils in a stratum was not divisible by 3, one or two pupils were randomly removed to create adequately-sized strata. The removed pupils were assigned to the (unmatched) control group. All triplets formed in steps 3 and 4 were pooled.

5. By school, triplets were randomly assigned to one of three triplet assignment options (T1-T2-C; T2-T2-C; and C-C-C), reflecting the resource availability in each school for offering treatments 1 and 2[10]. Within each triplet, pupils were then randomly assigned the assignment options available.

6. Steps 1-5 were carried out 1000 times, each time testing balance on the original stratification variables (school, year, gender, EAL, ethnicity, data linkage consent) plus five additional balance variables, using multinomial logistic regression. The additional balance variables were special educational needs status (SEN), eligibility for free school meals (FSM), term of birth, banding of the school's location on the Index of Multiple Deprivation 2010 (IMD) and pupil attendance rate. The randomisation with the highest *p*-value was used for assignment.

As a result of randomisation, the 2690 pupils available were assigned as outlined in Table 26.

Table 26: Treatment assignment from randomisation

NUMBERS	A T1-T2-C assignment	B All triplets (excl. siblings)	C Groups 1-3 (incl. siblings in same year group)	D All cases (excl. pupils with missing data)	E All cases (incl. pupils with missing data)
Incentivised	550	550	578	578	578
Unincentivised	550	656	690	690	690
Control (matched)	550	876	922	922	922
Control (unmatched)				237	500
	1650	**2082**	**2190**	**2427**	**2690**

Outcomes

InCAS assessments in reading and maths, provided by the Centre for Evaluation and Monitoring at the University of Durham, will be used to test outcomes. Outcomes are only measured after the intervention was delivered, without baseline. Key Stage 1 outcomes in reading and maths will be used as baseline predictors.

[10] Every school offered at least as many T2 spaces as T1 spaces, hence there was no need to T1-T1-C triplets.

Analysis

Analysis will be conducted on an intention-to-treat basis, including all children matched to groups (column C in Table 26). Analyses will be conducted in STATA version 12, using 2-sided significance tests at 5% significance level, both for comparisons between treatment and control and for any comparisons between treatments.

Baseline characteristics

Baseline characteristics (gender, age, Key Stage 1 reading and maths scores, ethnicity, EAL, SEN, FSM, attendance, IMD,) will be summarised by intervention group per year for each school and across all schools. Continuous variables (age, attendance) will be summarised with descriptive statistics (n, mean, standard deviation, range and median). Frequency counts and percentages of pupils by intervention group will also be provided by school and overall.

Trial completion

CONSORT diagram will be used to present summary of the flow of eligible children and their schools from recruitment through baseline assessment, randomisation, post intervention assessment and analysis. The number of children and schools included or excluded at each stage will be clearly stated and the reasons for exclusion will also be stated.

Primary analysis

The primary analysis will compare the two intervention arms with the control group using multilevel model to account for heterogeneity between schools. Unadjusted analyses with only pre-test scores and the intervention groups will be performed to investigate the impact of the stratification variables. However, effect sizes with 95% confidence intervals based on analysis adjusted for stratification variables will be reported as the main finding for this study. If either of the intervention groups is significantly different from the control group, comparison between the intervention arms will also be reported.

Secondary analysis

Secondary analysis based on a randomisation test will be performed to investigate the impact of balancing variables, matching and re-randomisation. The 2190 eligible children will be re-randomised to generate a matrix of 1000 acceptable re-randomisation runs. The acceptance criteria is p-value (no effect of the stratification variables) greater than 0.5. Under the null model of no treatment effects, post-test scores are expected to be independent of re-randomisation runs (Morgan and Rubin, 2012, p.3). Using the pre-defined re-randomisation matrix, we will perform:

1. Simple unadjusted analysis between post-test scores and the intervention groups. We will report quantile based 95% confidence intervals for the average intervention effects across the re-randomisation runs. The proportion of re-randomisation results greater than the actual “trial intervention groups” will be reported as *p*-value.

2. Adjusted analysis for the stratification and balance variables using a multilevel model. 95% confidence intervals and *p*-value will be calculated as described above.

Further analyses will be performed to explore the influence of parental attendance at parenting classes. The degree to which attendance account for the effects attributed to the particular treatment will be estimated. This mediational analysis formally establishes the functional relationship between offering treatment and improved outcomes, ruling out influences attributable to other causes or simply chance. This type of analysis also allows the investigation of dose-response. Also, sensitivity analysis

for missing data will be performed by reported the distribution of missingness by intervention groups and the baseline factors.

Finally, if one of the two treatment arms shows an effect but not the other, or if both show effects that are statistically significantly different from each other, we will explore the impact of incentives on attendance. This will help explore the extent to which incentives mediate outcomes via attendance.

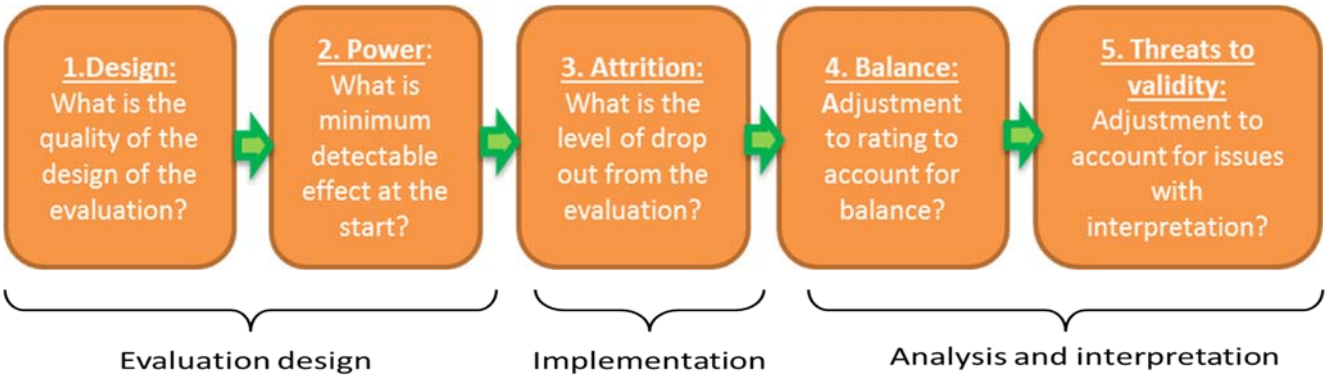
Appendix C: Cost rating

Cost ratings are based on the approximate cost per pupil per year of implementing the intervention over three years. Cost ratings are awarded using the following criteria.

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

Appendix D: Padlock rating

7th July 2016 Complete by Elena Rosa Brown



Rating	1. Design	2. Power (MDES)	3. Attrition	4. Balance	5. Threats to validity
5 🔒	Fair and clear experimental design (RCT)	< 0.2	< 10%	Well-balanced on observables	No threats to validity
4 🔒	Fair and clear experimental design (RCT, RDD)	< 0.3	< 20%		
3 🔒	Well-matched comparison (quasi-experiment)	< 0.4	< 30%		
2 🔒	Matched comparison (quasi-experiment)	< 0.5	< 40%		
1 🔒	Comparison group with poor or no matching	< 0.6	< 50%		
0 🔒	No comparator	> 0.6	> 50%	Imbalanced on observables	Significant threats

The final security rating for this trial is 4 🔒.

Education
Endowment
Foundation

The Education Endowment Foundation
9th Floor, Millbank Tower
21–24 Millbank
London
SW1P 4QP
www.educationendowmentfoundation.org.uk