

Motivating Parents Through a Parent Academy: Testing the impact of a parenting programme to improve attainment Updated proposal to the Education Endowment Foundation, November 2014

1.1 Parent Academy trial

The trial will test the hypothesis that educating parents in how to support their child's learning can improve the attainment of pupils from disadvantaged backgrounds, focusing on children in Key Stage 2 aged 7-11. This is the first study of its kind in the UK, as far as we are aware, adopting experimental methods of evaluation and would provide evidence of great value for closing the attainment gap between children of different backgrounds. There is, however, a broader relevant evidence base that supports the notion that parent involvement in children's learning promotes their attainment¹. The evidence suggests that to be effective, parental involvement needs to encompass engagement with schools but also how parents communicate their expectations and values around education to their children at home². The Parent Academy curriculum covers both of these aspects.

This research would also provide an interesting counter-balance to the current emphasis of policy which focuses on schools as the primary drivers of pupil attainment at Key Stage 2 and emphasises their responsibility for closing the attainment gap. Schools receive pupil premium payments for each disadvantaged child and schools are held accountable as to whether they use these funds effectively. A recent announcement from Ofsted suggested that schools would be precluded from receiving an 'outstanding' rating if they failed to raise the attainment of disadvantaged pupils.

The research from the Chicago Heights Early Childhood Center (CHECC) investigated the relative impacts of educating pre-school children and educating their parents, finding that the latter was more effective.³ The circumstances for the proposed Parent Academy trial are of course different in that the children are in the education system by virtue of being older. This trial focuses just on the added value of intervening with parents, and investigating the effect of incentives in motivating and engaging them. If successful, the project may lead to a further trial of more complex incentive structures.

The Parent Academy will be delivered over 8 months in the academic year November 2014 to June 2015. There will be approximately 20 parents in a class and each class will meet every fortnight, totalling 13 sessions across the 8 months. The classes will last an hour and a half and follow a curriculum which is aligned with the learning of the pupils at school so that similar topics are being taught at both simultaneously. The parents will be drawn from 16 schools in disadvantaged areas in Middlesbrough (8 schools) and Camden (8 schools). We note that the CANParenting Trial has also been operating in these Local Authorities which we believe will be overlapping with the Parenting Academy in content and so parents may be involved in both interventions which could undermine the findings from the trial, for example whether any impact observed is as a result of the Parenting Academy programme.⁴

We understand that the Parent Academy is intending to target mothers and fathers equally, and that either one or both parents would be expected to attend the sessions in two-parent families. Parental

¹ Grayson, H. (2013). *Rapid Review of Parental Engagement and Narrowing the Gap in Attainment for Disadvantaged Children*. Slough and Oxford: NFER and Oxford University Press.

Carter-Wall, C. and Whitfield, G. (2012). *The Role of Aspirations, Attitudes and Behaviour in Closing the Education Attainment Gap*. York: Joseph Rowntree Foundation.

² Hill, N. E., & Tyson, D. F. (2007). Parental involvement in middle school: A meta-analytic assessment of strategies that promote achievement. *Manuscript Submitted for Publication, Duke University, Durham, NC*.

³ Levitt, S., List, J., Metcalf, R., & Sadoff, S., 2013 Motivating Parents through a Parent Academy proposal.

⁴ Cullen et al 2014 CANparent Trial Evaluation: Second Interim Report, Department for Education.
<https://www.gov.uk/government/publications/canparent-trial-evaluation-second-interim-report>

attendance is a complex issue which may impact on the outcomes of the intervention. The qualitative process evaluation would provide a valuable opportunity to explore this further, for example the importance of father involvement.

The academic team will be responsible for recruiting the local authorities, instructors, schools and parents, managing any child consent process (if required) and maintaining their engagement throughout the course of the programme as well as designing the curriculum. The evaluator will be responsible for randomisation, pupil assessment, maintaining the engagement of participants in the evaluation and independent analysis for the EEF report.

1.2 Evaluation design

There are two principal research questions to be answered by this project.

- What is the impact of the Parent Academy on pupil attainment?
- What is the impact of the Parent Academy on pupil attainment when incentives are given to parents for attendance?

The design is a three arm study (two treatment groups and a control group) individual-level randomised control trial. Within each school, parents of pupils in Years 3 to 6 will be randomly allocated to one of three groups:

1. Control group ('business as usual')
2. Intervention group
3. Intervention group (with incentives for attendance)

The numbers allocated to each group are illustrated in Table 1.

We would expect take-up in the two intervention arms of the trial to be less than 100%, so intention to treat analysis is appropriate though it should be noted that estimates of intention to treat are likely to be different to the effect of treatment on those treated. The key point, however, is that data which accurately measures take-up in the two treatment arms of the trial will be needed. The academic team will be responsible for ensuring that instructors keep accurate records of attendance for this purpose (this should include whether attendees are the mother, father or both parents). It will also be important to measure whether those who are assigned to the control group access classes either as the result of error or deliberate subversion. All communications with parents will clearly state from the outset that only some of the parents participating in the Parenting Academy will receive the financial incentive, and that parents in both treatment groups will receive childcare and free meals. This will help reduce problems occurring amongst the non-financial incentive group who are more likely to consider the process to be unfair which could undermine their engagement and motivation to participate. This will also help to manage the risk of 'resentful demoralisation' and 'compensatory rivalry' among the untreated control group.

Table 1: Number of parents in different groups

Group	Middlesbrough	Camden	Total
Control	950	950	1900
Intervention 1 - Parent Academy	300	300	600
Intervention 2 - Parent Academy + Incentives	250	250	500
Total	1500	1500	3000

The academic team will be responsible for managing parenting academy incentives, we are happy to provide more advice about NatCen's standard approach to using high street vouchers as incentives in research studies.

1.2.1 Randomisation

The randomisation will be conducted as follows:

- The randomisation aimed to allocate pupils from 16 schools and in four school years to three groups—PA with incentive, PA without incentive and control—subject to four requirements:
 - Similar pupils are allocated (matched) to triplets, and pupils within each triplet randomly assigned to the two treatments and the control;
 - In each school only a limited number of randomly selected pupils are assigned to the treatment groups, subject to the capacity constraints;
 - Only one of any group of siblings is allocated to a triplet;
 - It is ensured that pupils in the three matched groups are as similar to each other as possible, while still allowing for random assignment.
- To achieve this, the randomisation was carried out by first assigning pupils into strata based on the stratification variables: school, school year, consent to data linkage to NPD, pupil gender, ethnicity (2 groups: White-British/Non-White-British), and whether English is an additional language.
- Strata are created whereby every person in each stratum has the exact same value for each of the stratification variables. For example, all White-British males in Year 3, from school “A” with English as first language who had not been opted out of linkage would be in one stratum. All black females in Year 3 from school “A” with English as first language who had not been opted out of linkage would be in another stratum.
- Pupils in each stratum are then sorted on the matching variable, pre-intervention KS1 average point score. The first three students in each stratum would become a matched triplet. For example, the 3 white males in Year 3 with the highest KS1 average point scores are a matched triplet, then the next 3 are in the second triplet and so on.
- After all pupils are allocated to a triplet, a limited number of triplets are randomly selected for treatment, subject to the capacity constraint for the treatment delivery. The pupils from each such triplet in turn are then randomly assigned to the two treatment and the control groups.
- To ensure that the treatment groups are similar on a range of additional factors the randomisation was repeated many times. Each time, the balance of these factors across the assignment groups was examined using a multinomial logistic regression. Afterwards, the random assignment with the best balance was used for the study. The factors considered were:
 - the proportion of pupils with special educational needs;
 - proportion of pupils eligible for to receive free school meals;
 - the term in which a pupils were born;
 - the average level of deprivation in the area of the pupils’ home; and
 - average class attendance.
- To ensure that this approach does not find the one assignment that creates the greatest balance—as this would no longer be a random assignment—we restricted the number of randomisations to 1,000.
- If the number of children in a stratum was such that they could not be entirely allocated into triplets, including in cases where there were less than three pupils in a stratum, an additional, less stringent stratification was undertaken. For these pupils, strata were created on the basis of school, school year and consent for additional analysis, only. These pupils were then put into triplets as before, using the matching variable. Each of these less stringently allocated triplets was as likely to be selected for inclusion in the treatment assignment as the stringently allocated triplets.
- If a child does not have any siblings in years 3-6, then he/she is treated normally (with no additional constraints).
- If a child has a sibling in Years 3-6 who is also eligible for the Parent Academy programme then only one of the children can be in a matched triplet. The other child(ren) will be unmatched. The child who is in the matched triplet is chosen randomly by the randomization code. This means that if the code is run more than once, a different sibling can end up in the matched pair each time. Following this procedure it is possible for any sibling to be unmatched.
- If a child has a sibling in his/her age group (Years 3/4 or 5/6), then both children are allocated to the same treatment (Control, PA with financial incentives, or PA without incentives). The assignment is based on whatever treatment the matched child is assigned to. If selected for PA, both children will be allocated for their parent to be invited to attend PA sessions. The parents do not receive multiple vouchers for having more than one child. We do not want to vary the incentive to attend per family.
- This approach is necessary because we treat the parents, and so all siblings are being treated. By treating the matched child, the sibling is also being treated. Any siblings not selected for matching must remain unmatched because if he/she were included in a triplet and randomly assigned to the control group, then there are large spill-over effects and that child's triplet would

not be valid. Also, we could not require that child to be assigned to a triplet and in the same treatment group as the already-matched sibling, because this would no longer be random.

- If a child has a sibling that is in the other age group, then the unmatched child will be in the control group, regardless of the placement of the matched child. This is because if the unmatched sibling were to be in treatment, it would change the investment opportunity for the matched child (the parent would have to choose how to split time between the two parent academy sessions). Therefore, this would violate the validity of the first child's match triplet. The parent of the matched child in treatment would face a different investment structure than the matched child in control if he/she was in treatment.
- If the number of children in a stratum was such that they could not be entirely allocated into triplets, including in cases where there were less than three pupils in a stratum, an additional, less stringent stratification was undertaken. For these pupils, strata were created on the basis of school, school year and consent for additional analysis, only. These pupils were then put into triplets as before, using the matching variable. Each of these less stringently allocated triplets was as likely to be selected for inclusion in the treatment assignment as the stringently allocated triplets.

1.3 Process evaluation

We will conduct a process evaluation alongside the RCT to evaluate the implementation of the programme and the extent of fidelity to the model, plus necessary conditions and barriers to success. The process evaluation will comprise:

- Development of a simple logic model
- Depth interviews with instructors and parents

We considered inviting both instructors and classroom teachers to fill out an online survey in order to gather more information about the delivery of the programme. However, the small numbers within our sample mean that we will be able to gather the required information during the depth interviews which will also reduce the burden on schools and instructors.

1.3.1 Logic model

As evaluators, we will work with the project team to understand their delivery model and build up an 'impact map', or logic model.

A logic model approach holds that programme interventions, in almost all cases, are based on an underlying logic or theory and a set of assumptions about how an intervention works. At its core, this approach provides an explanation of how a group of stakeholders expects to reach a commonly understood goal.⁵

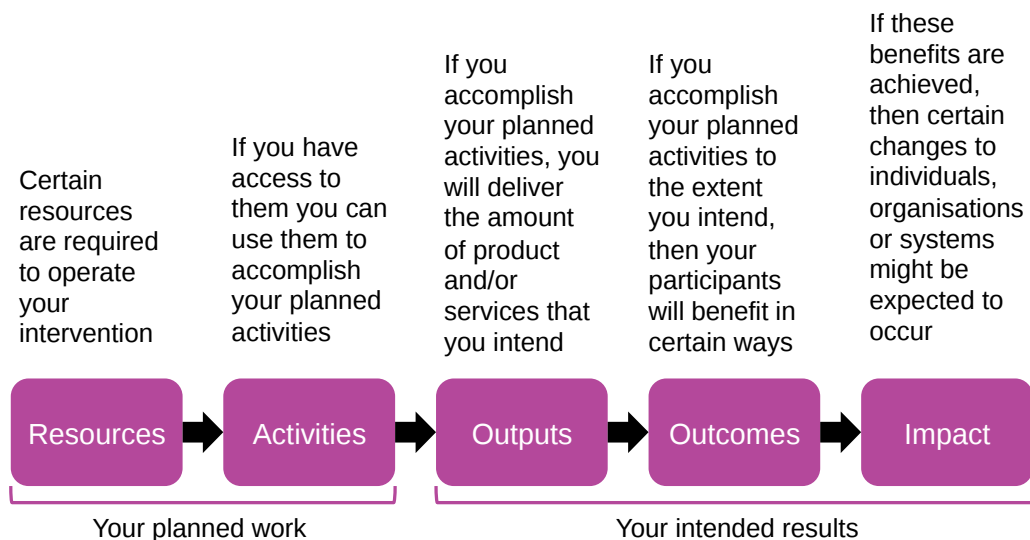
This approach promotes a systematic and visual way to represent a shared understanding of the component parts of a programme. The development process considers the programme's planned work and its intended outcomes. A logic model separates the key components of a programme which are usually structured in a linear model as shown in the diagram below (see Figure 1). Mapping a programme in this way helps to visualise and understand how human and financial investments can contribute to achieving intended programme goals and can lead to programme improvements.

Programme logic models are useful in the design and planning stages of a programme and in implementing programme components and activities. Logic models are used to identify data collection points for monitoring and evaluation purposes and to devise an evaluation plan. Finally, evaluations underpinned by logic models help to understand how well a programme is functioning, whether it is achieving the desired outcomes and where the programme has encountered delivery challenges.⁶

⁵ Anderson, A. (2007). The community builder's approach to theory of change: A practical guide to theory development. The Aspen Institute. <http://www.epa.gov/evaluate/pdf/eval-guides/logic-model-development-guide.pdf>

⁶ W. K. Kellogg Foundation. (2004). Logic model development guide. <http://www.epa.gov/evaluate/pdf/eval-guides/logic-model-development-guide.pdf>

Figure 1. How to read a logic model (adapted from W. K. Kellogg Foundation)



For this evaluation we will develop a simple logic model in consultation with stakeholders so that the delivery team can use this to inform and review their programme activities. The standard process for developing a logic model is to have an initial workshop with the delivery team to understand the programme activities, associated outputs and link these to the outcomes. Following this workshop the logic model diagram is drafted and shared with the delivery team for further discussion before agreeing a final model for the programme. For this evaluation we will use a slightly different approach; the first draft logic model diagram will be based on detailed programme plans and documents to form a basis of the version to be shared with the delivery team. The delivery team will then discuss their comments on the model and work with the evaluation team to refine the expected outputs and outcomes to agree a final model which the evaluation team will update. The logic model will inform our development of research instruments (e.g. topic guides) and will support our interpretation of the impact evaluation findings. The logic model will be included in the final report on the evaluation.

1.3.1 Instructor interviews

We will interview all eight of the Parent Academy instructors. As such a small number of instructors are delivering to such a large number of families we consider it appropriate to capture the experience of all of them rather than to try to select a representative group. These interviews will cover their views on:

- training;
- workload;
- delivering the curriculum;
- working with schools and collecting data from teachers;
- working with parents;
- any differences in parents receiving the Parenting Academy and those also in receipt of the incentive;
- how the Parent Academy was working for parents;
- how the incentive works; and;
- impact on children and reasons for this.

We will collate reported case studies of differing impacts and outcomes. These one to one interviews will be carried out in the first half of the Spring term 2015. We will then recontact all eight of these instructors to carry out another interview at the end of the programme. This second interview will focus on how they feel the programme has worked and whether this has been different to what they were expecting and why. It will also cover how they would have implemented the programme differently and any changes they would have made retrospectively.

At the first interview participants will be asked if they would be willing to be contacted again for the follow up interview.

1.3.2 Parent interviews

Parents attending the Parent Academy sessions will be invited to participate in interviews with the evaluation team. Parents will be given a letter by their instructors and will be invited to opt-in to interview by phone, email or returning an opt-in slip using a pre-paid envelope.

The sample would consist of 18 parents in total, split amongst each of the intervention groups (PA and PA + incentive). Interviews will be carried out by phone or in person (guided by the participant's preference) and will take place in two phases; between December 2014 and January 2015 and then a sub sample of 12 will be interviewed again towards the end of the programme in May/June 2015.

We will aim to achieve a sample which represents a range of number and ages of children, work status, family structure, maternal education level, ethnicity and geographical location (Camden or Middlesbrough). The initial interviews will cover:

- How involved they are with their child's school
- Reasons for this
- Their personal experience of education
- The impact their behaviour has on their children
- How they have found the programme
- Its benefits and challenges
- Any behaviour change they feel it has led to
- The impact of the incentive

A further 12 interviews will take place towards the end of the programme and will focus on any changes which may have occurred since the parenting academy and the reasons for these. If parents have dropped out of the Parenting Academy during this time they will still be included within the sample and interviewed where possible to establish the reasons they had for dropping out.

All participants will receive payments (£20 high street vouchers) for taking part in the interviews. It is anticipated both interviews would take a maximum of 30 minutes. We would offer to carry these out via Language Line if the parent/instructor does not feel they have sufficient English skills to fully explain their views.

1.3.3 Class Teacher interviews

Six teachers in total will be interviewed, three from each participating area. This interview will take place on the phone and last approximately 30 minutes. Interviews will be arranged for a time that suits teachers and they will be sent an information leaflet and will have the opportunity to opt-out. An online booking system, BookingBug will be used as a recruitment aid with teachers. This online system allows participants to follow a link sent via email and choose a time slot for interview that suits them.

The teacher interviews will take place at the end of the year and will include topics such as: whether they have seen any change in parent or pupil engagement and motivation, any change in how parents interact with teachers, or changes in how often parents attend parents evenings or other school events, whether the teachers' workload has been affected at all by the academy and whether they feel the academy is helping to increase pupil attainment.

1.3.4 Costs of delivering the Parent Academy

An important aspect of the evaluation is to understand the costs involved in delivering the Parent Academy to estimate the cost if schools (or parents) wished to purchase this in the future. Calculating the average cost of delivery enables comparisons to be made with other interventions based on both the average effectiveness and costs incurred.

The total cost per pupil will be calculated based on information provided by the delivery team, comprised of:

- Direct costs that are associated with the Parent Academy such as employment of instructors and Directors, venue hire, incentives and other expenditure;
- Indirect costs such as any additional school/head teacher staff time during curriculum development or liaison with Parent Academy staff.

1.4 Data collection process

In this section, we outline the approach to contacting parents to gain informed consent for pupil data collection, participation in the Parent Academy, pupil data and eligibility for testing.

1.4.1 Parental consent

The approach to gaining informed consent from parents is:

1. An opt out letter sent from the University of Chicago to parents of Year 3 to 6 pupils in participating schools. This letter explains that the school is taking part in the study and provides the opportunity to opt themselves out of the Parent Academy and their children from testing.
2. An opt out letter sent to all parents who did not opt out following letter one, accompanied by a Parent Academy Brochure. This letter was sent in October 2014, asking for permission to link child data to the NPD, store the data in EEF's data archive managed by the FFT and anonymously in the UK data archive.
3. Allocation letter informing parents if they have been allocated (offered) the parenting academy with or without incentives. Parents in the control group will not receive a letter.

All letters will have consistent branding with the Parenting Academy logo, NatCen's logo and the LA logo for consistency.

1.4.2 Participation in the Parent Academy

One potential problem is that parent instructors, for a variety of reasons, subvert randomisation, or administrative errors might occur, leading to crossovers (parents joining arms of the study they were not assigned to). Attendance at the Parent Academy should be monitored closely by the academic team asking instructors to record attendance at the beginning of each session and to forward these records to our research team on a frequent basis so that we can monitor attendance and compliance with randomisation.

Loss to follow-up of study subjects will be minimised through positive engagement with schools, drawing on our experience of working with schools as research participants. A Memorandum of Understanding (MoU) will be put in place with schools, the academic and NatCen team as part of this engagement strategy. This would also include maintaining a project page hosted on the NatCen website with material of relevance to schools, teachers and parents and timely communication with schools by letter and email. Contact details for NatCen would be provided to schools in case of queries or concerns.

1.4.3 Pupil data and eligibility for testing

EEF's standard guidelines on testing procedures for evaluation of RCTs recommends the use of National Pupil Database records of key stage attainment as a pre-test score where high correlations are observed between key stage scores and tests.⁷ Therefore, pupil KS1 scores obtained from the NPD will be used as the pre-intervention scores. Post-intervention assessments will be carried out in the summer 2015 term using InCAS tests, provided by the University of Durham's Centre for Evaluation and Monitoring (CEM).

Any parents of pupils who may join the trial schools in October, after the enumeration and randomisation processes will already be underway will not be eligible for the study. We don't anticipate this to be a problem given that not all the parents within the school will be taking part in the trial. It would however, be worth exploring the typical level of pupil turnover for the schools taking part.

⁷ http://educationendowmentfoundation.org.uk/uploads/pdf/Pre-testing_paper.pdf [Accessed 21-02-14]

We will work with the Parent Academy Directors and schools from the start of the intervention to fully brief them on conducting the pupil assessments to ensure that they understand the importance of the assessments and that these are complete by the end of term. NatCen would support the schools in conducting the testing through use of a dedicated website supplemented by telephone and email support (provided as standard from CEM for the InCAS test administration and by NatCen for other queries).

The academic project team will be responsible for liaising with the local authorities to obtain pupil level data which will include: School, Gender, Year, Key stage 1 grade, Ethnicity, SEN status, English as an additional Language (EAL), Free School Meals (FSM) eligibility, Indices of Multiple deprivation band, attendance percentage, month and year of birth, flag indicating any siblings in the data.

The National Pupil Database records at the individual level will provide socio-demographic information about the pupils in the trial for use as control variables in the analysis. Having the UPNs allows EEF to continue to track the future academic progress of these pupils if desired. NatCen will be responsible for applying for the NPD records in Spring 2015.

Teachers will provide data on the performance of participating pupils on a termly basis. We have assumed that these will be collected by local authorities and the academic team would be responsible for overseeing this.

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Effective collaboration

At NatCen, we have built up valuable learning from the numerous projects we have conducted for EEF. One of the most important ways in which we can ensure high quality and cost-effectiveness is through the clear allocation of roles and responsibilities between EEF, the academic team, ourselves as evaluators, research participants and other stakeholders.

1.5 Outcome measures

Pupil measures

The InCAS test is a standardised assessment system that is designed to monitor pupils' educational progress throughout the Primary phase. The tests cater for pupils aged 5 through to 11 years providing an assessment of attainment in the core curriculum areas of reading and mathematics plus attitudes to these subjects and school in general. These can be administered in groups of 5-6 pupils or as a class and are completed on stand-alone computers/laptops or the school network with each child having a pair of headphones⁸ to hear the questions being read out. Each module takes around 30 minutes to administer. We will use the two modules of mathematics and reading.

The tests would be administered by a NatCen interviewer who is blinded to which group the pupils are in. Each module would be administered during a separate session. Children will be tested in groups of 20 in ICT suites or normal classroom settings at their school. Early discussions with schools via Parent Academy Directors suggest that schools have the equipment for this to take place with online and telephone support from the research team. The evaluation team will ask schools to complete a short questionnaire about ICT facilities in their school and plan the best approach for each school, in conjunction with the appropriate school staff. Schools will be asked to sign an agreement outlining when and how testing will take place in their school prior to the summer 2015 term.

⁸ We have assumed that schools will already have headphones that can be used for this purpose, however, this will be included in the school questionnaires to plan a school by school approach.

Two InCAS test modules; Maths and Literacy (each 30 minutes) will be completed with invigilation by a NatCen interviewer and a break for pupils in between test sessions. As a computer based test, the InCAS test has the advantage of not requiring data entry and marking so being more cost-effective.

Safeguarding Pupil Anonymity

Pupil testing requires schools to enter pupil data into the InCAS testing software. The data required includes pupil names, full date of birth and UPN. Test data is also stored securely by CEM and may be used for research purposes by CEM or third parties. As required by the two Local Authorities where the Parent Academy is to be implemented, additional measures to protect pupil anonymity will be put in place. These include:

- Offering schools the option to use proxy pupil names (generated by the Local Authority) instead of their real names;
- The removal of pupil names (proxy or real) from test results before transfer to NatCen; and
- Deletion of all test data by CEM so that test results are available to only Parent Academy evaluators, schools, and upon successful set up of a data sharing agreement, to the University of Chicago who are delivering the intervention.

1.6 Impact analysis

Due to random allocation and matching, the three study groups should be well-balanced at the point of randomisation. Differential loss to follow-up may, however, occur during the course of the study, and once post-test data are available we will need to check the sample to ensure that it remains well-balanced on pre-test/baseline measures. If this proves not to be the case, we will need to use regression methods more extensively, multiple imputation, or other non-experimental analyses to control for biases resulting from sample loss.

Assuming that randomisation produces three study groups that are well-balanced, the 'primary' impact analysis will compare average reading and maths ability at post-test in the three groups, on an intention to treat basis. This will be achieved by estimating a series of multiple regression models controlling for triplet group membership and other factors. The primary outcome measure for the analysis will be the numeracy component from InCAS and the secondary outcome measure will be the literacy component from InCAS.

We will also conduct some 'secondary' analysis of differences across subgroups. Such an analysis will be greatly enhanced with background demographic information. This subgroup analysis will look at outcomes by FSM receipt, gender and parental attendance at the Parenting Academy sessions (if available from the academic team). There will also be an on-treatment analysis looking at whether the effect on attainment is moderated by levels of attendance.

The delivery team expects the Parenting Academy to have the same effect on the different year groups participating in the intervention, therefore, analysis will primarily look at the effect on all pupils in years 3 to 6 together.

The trial analysis plan will be designed and agreed in consultation with the EEF and external experts to draw on experience of similar analysis based on matched pair/triplet randomisation. This will be agreed by the end of March 2015.

Minimum detectable effect sizes

For this study we define a minimum detectable effect size⁹ as the smallest effect size that if true will yield an estimate of impact statistically significant at the 5 percent level with 80 percent power. The estimates below are based on a comparison of outcomes between one treatment arm and another, and between the smallest treatment arm and control. A loss to follow-up rate of five percent is assumed and that there

⁹ By effect size we refer to the standardised mean difference.

is no differential loss across the study groups.¹⁰ Moreover, we assume that impacts are estimated using regression adjustment and that two-tailed statistical tests will be performed.

We estimate that for subgroup analysis based on comparisons between two groups containing approximately 100 children in each an effect size of 0.4 standard deviations will be detectable, at 5 percent statistical significant and 80 percent power. This assumes an unadjusted comparison.

Table 2: Minimum detectable effect sizes – whole sample estimates – Intention to treat

	Comparison of outcomes among the PA group with outcomes among the PA + incentive group	Comparison of outcomes among PA with incentive group and outcomes among control
Total assumed sample size	1,045**	2,275***
Unadjusted	0.17	0.14
.20*	0.16	0.13
.40*	0.13	0.11
.60*	0.11	0.09
*proportion of residual variance explained by covariates		
** assumed achieved sample sizes in treatment group one (PA with incentive) 475 and treatment group two (PA without incentive) 570		
*** assumed achieved sample sizes in treatment group one of 475 and 1,800 controls		

1.7 Project team

The project is managed in the Children, Families and Work Group at NatCen. The trial manager will be Fatima Husain (Research Director), assisted by Sarah Haywood (Senior Researcher) who is experienced working on school based, and other EEF projects. Sarah and Fatima will be supported by Jonathan Paylor (Researcher). The researchers would work closely with other departments and specialists at NatCen including the evaluation team, led by Triin Edovald, statisticians and the Operations Department. CVs and experience of the project team are available on request.

¹⁰ We have not made an estimate of the likely loss to the study resulting from an inability to secure parental consent.

1.8 Main risks to evaluation and mitigating actions: Analytical, procedural and managerial

Risk	Likelihood	Impact	Mitigation
Crossovers - parents participating in groups they were not allocated to, due to error/subversion	Medium	Medium	We propose to monitor allocation to groups carefully to ensure the integrity of the study design is maintained and ensure that crossovers are minimised
Drop out – school may decide to leave the study thus reducing the study sample size	Low	High	We will draw on our experience of working closely with schools to ensure interest in and cooperation with the study is maintained, addressing practical concerns as necessary, and ensuring the benefits of continued participation are made clear.
Difficulty in obtaining parental consent for access to pupil data – risks to sample size and under-representation of pupils with certain characteristics. Consent rates may vary across schools.	Low	Medium	We will work with schools and research partners to obtain consent from as many parents as possible, ensuring parents are aware of the benefits. If handled carefully, this consent should not be a significant problem – parents are used to being asked for their consent on a regular basis for a variety of school activities.
Variations in quality of instructor – could lead to false conclusions regarding effectiveness of interventions.	Medium	High	We will explore variation through the survey and observations and will explore ways in which the analysis can be adjusted for instructor effects. We will ensure for each member of the study we record that instructor they were assigned to.
Loss to follow-up – could be a problem if overall sample loss is large (reducing absolute sample numbers)/patterns of loss differ between study groups.	Low	High	The best way to tackle loss to follow-up is through well-designed fieldwork procedures. Our approach to data collection minimises teacher burden and our communication procedures are of a high quality.
Variations in treatment delivery – significant departures from Academy curriculum may affect interpretation of treatment effects/or reduce effectiveness.	Low/medium	High	This risk will be controlled by study partners who will deliver treatments. The process evaluation will explore variation in treatment delivery across schools and consider the implications for study results. There may also be potential for feedback/corrective action.
NPD data access denial	Low	Medium/high	Were we unable to negotiate access to NPD records, we propose to work closely with schools in order to gain access to school level records used to compile the NPD
Difficulty scheduling case study visits.	Medium	Low	We will schedule case study visits in the North on consecutive days so as to allow flexibility in rescheduling if necessary. Interviews can be completed by phone.
Changes to project specification affecting timetable/costs	Medium	Medium	We have allocated sufficient resources to the set-up stage for the design to be clarified in detail allowing us to revise costs/timing where necessary and provide a robust estimate.
Poor project management	Low	Medium	We have proposed a strong team experienced in managing complex evaluations. We will monitor progress closely and identify areas of concern early.
Staff illness / unavailability / turnover	Low	Low	We forward-plan research capacity, and have a sufficient number of experienced staff members. Our procedures ensure that decisions and progress are fully documented.
Loss of or damage to data	Low	High	NatCen has high quality data security procedures with which team members are experienced. EEF will be notified of any breaches and contingency plans put in place.