

CLUSTER RANDOMISED TRIAL OF A METACOGNITION INTERVENTION TO INCREASE ATTAINMENT AMONG PRIMARY SCHOOL CHILDREN



11.2.13

BACKGROUND

Significance

An explanation of the scientific background, policy context and rationale for the evaluation

Mind the Gap seeks to create better learners by harnessing the power of effective 'learning to learn' thinking strategies. The learning to learn approach is based on the idea that effective learning is based on skills, attitudes and dispositions to learning, and that these can themselves be learned.

There is evidence to suggest that learning to learn approaches may be effective at raising pupils' academic attainment. A meta-analysis of related approaches found a large effect on curriculum outcomes. Furthermore, it is not just teachers who can help children to improve their learning skills. Parental engagement is potentially important; giving parents the skills to help their children to become confident, motivated learners could make a big difference to their performance at school. While there has been a large amount of research around this, few robust evaluations of the effectiveness of programmes designed to increase parental engagement have been undertaken. A small scale pilot project in Harrow showed encouraging results. Among the 40 participating primary school pupils, the percentage achieving the Government's expected level of progress in English rose from 15% to 73%.

The evaluation is intended to provide robust evidence on the effectiveness of learning to learn. A cluster randomisation approach was adopted since individual-level randomisation was felt to be both impractical, likely to cause contamination and to raise ethical concerns. Outcomes are at the level of individual pupils.

Intervention

Details of the intervention being tested

There are two aspects to the intervention. The first involves training school staff in how to embed learning to learn approaches in their work, and how to continue to effectively and strategically involve parents. The second is the parental engagement intervention. This involves 10 hours of sessions where children and their parents work together to create an animated film. Sessions are coordinated by a practitioner who helps participants to think about how they were learning, creating learning goals and reflecting on their progress.

RESEARCH PLAN

Research questions

Questions the evaluation is designed to answer

The primary questions the evaluation was designed to answer are:

1. what is the effect on children's attainment and other outcomes of parental engagement in learning?
2. what is the effect on children's attainment and other outcomes of training teachers in learning to learning techniques?
3. what is the combined effect on children's attainment and other outcomes of parental engagement and teacher training?

Design

Explain the type of trial including the unit of randomisation (e.g. whether pupil, school or class) and number of trial arms

Methods used to generate random allocation including details of any pairing or stratification

Randomisation takes place at both the school level and the class level.

- **School level:** Schools participating in the trial are randomly assigned to either the intervention group or a control group. Teachers in intervention schools receive training to embed learning to learn techniques in their work.
- **Class level:** In intervention schools, eligibility for the parental engagement course is randomly assigned to year 4 classes.

This assignment approach results in three types of year 4 classes being included in the trial:

- a) classes in intervention schools that are eligible for the parental engagement course
- b) classes in intervention schools that are not eligible for the parental engagement course
- c) classes in control schools.

Randomisation was carried out within blocks defined by area (Devon, Haringey or Birmingham) and by the proportion of pupils in each school shown in the 2011 school performance tables to achieve level 4 or higher at KS2 in both English and Maths (low, medium, high – where these thresholds were chosen to achieve equal sized groups in each area (and so could vary by area)). In addition, the practical arrangements around delivering the intervention meant that the Birmingham schools had to be randomised in two batches. In effect, this meant four areas (Devon, Haringey, Birmingham I, Birmingham II) and three school performance bands, so 12 blocking variables (or strata).

Randomisation of schools (to achieve a 50:50 allocation) was performed as follows:

- Each school was assigned a randomly generated number
- Schools were sorted by blocking variable and, within each block, by the random number
- The first school was randomised.
- Each subsequent school was assigned to have the opposite outcome of the previous school.

Randomisation of classes was performed as follows:

- In intervention schools that had a single year 4 class, that class was assigned to be eligible for the parental engagement course
- In intervention schools that had two year 4 classes, one class was randomly assigned to be eligible for the parental engagement course and the other class was then not eligible for the parental engagement course
- In intervention schools that had three year 4 classes, a randomly selected class was excluded from the trial and randomisation of eligibility proceeded as in the 2-class case
- In control schools that had a single year 4 class, that class was selected for the trial
- In control schools, where there were multiple year 4 classes, the class to be included in the trial was selected at random.

Participants

Who is eligible and how they will be identified and recruited

The study comprises primary schools in three areas of England: Devon, Haringey and Birmingham. Within each school, the trial focuses on year 4 pupils. Within schools that were randomly selected to be intervention schools, teachers are trained in metacognition techniques, with the aim of influencing teaching throughout the whole school. The parental engagement course is offered to a single year 4 class within each intervention school. Participation in the course is voluntary.

In Haringey and Devon, existing networks were approached. The main mechanism in Devon was through the Devon Association of Primary Heads (<http://www.devon.gov.uk/daph>.) In Haringey we went through Haringey's Head of Primary Standards, who was asked to select schools that met the

EEF criteria and might benefit from support. In Birmingham we originally approached clusters and networks but had limited success. We then looked at the national database for schools and selected schools based on their attainment results for key stage 2 in Maths and English – one of the key indicators for EEF.

Outcome Measures

Clearly defined primary and secondary outcomes and how they are measured

Details of any plans to ensure tests are blinded (eg, random allocation after pre-test and blind assessors at post-test)

The primary outcomes are: pupil attainment, measured using the combined literacy and numeracy scores on INCAs (CEM) tests administered 6 months after the completion of the intervention. Secondary measures will include separate literacy and numeracy scores on the INCAs tests, pupils' views on their relationship with their parents (measured using SDQi, running to the same timetable as the CEM tests); pupils' metacognitive development using a measure constructed from the Pupil Views Templates; KS2 outcomes. Similar data will be collected on children in control classes. We hope to link to the National Pupil Database in order to observe KS2 outcomes, although these will not be available within the project timetable. We hope also to be able to identify the effect on siblings of children involved in the parental engagement course. This relies on being able to identify siblings in the NPD. All outcomes are at the individual level.

The analysis will not be blinded. Assessors and data analysts will know the intervention status of each school.

Sample size calculations

How sample size is determined and any power calculations that are being used, including assumptions

The aim of the project is to recruit 50 schools to the study. A minimum detectable effect of 0.42 was estimated, based on 25 schools randomised into treatment and control, 30 children per cluster, 0.05 significance level, 0.8 power and a 0.25 intra-cluster correlation. This is a conservative ICC and the calculation does not factor in stratification, which would increase the power. The effect size was predicted to be in the range of 0.35 to 0.45 standard deviations.

Analysis plan

Describe the statistical methods to be used to compare the groups on the primary and secondary outcome measures. Also, details of any subgroup or additional analysis

The analysis will be carried out using multilevel regression models to reflect the clustered nature of randomisation. The three types of classes included in the trial are:

- a) classes in intervention schools that were eligible for the parental engagement course
- b) classes in intervention schools that were not eligible for the parental engagement course
- c) classes in control schools.

The model will be specified in order to allow three types of comparison:

- vs c) will give the combined impact of teacher training and parental engagement
- vs b) will give the impact of parental engagement only
- vs c) will give the impact of teacher training only.

We will consider a number of subgroups defined by pupil characteristics. These include:

- children receiving free school meals (FSM) compared to non-FSM children;
- ethnic minority children compared to white children
- children with low attainment scores on the pre-test compared with children with higher attainment scores on the pre-test
- children with a low estimated probability of participation in the parental engagement course compared with children with a higher estimated probability.

We will also consider subgroups defined by school characteristics. These include:

- sub floor standard schools and schools that are not sub floor standard,
- faith schools and non-faith schools.

Process evaluation methods

Summary of methods used in the process evaluation or to assess programme fidelity

The project will include a full qualitative process evaluation conducted by the University of Durham. This evaluation will have two strands:

1. Qualitative survey will be used to sample pupils', parents' and teachers' perspectives across the sample. This will be done in multiple ways:
 - a. Pupil Views Templates will target pupils' metacognitive development and will be completed by all children across the three class types (pre, immediately post and delayed post) treatment. The qualitative data will be coded using a previously validated system with high inter and intra rater reliability to allow statistical analysis across the data set.
 - b. Telephone interviews will be undertaken with all teachers of treatment classes who have undertaken the parental engagement course. This will happen within one month of the end of the course. There will also be an additional follow up telephone interview 6 months delayed to explore the sustained nature of any perceived impact.
 - c. Mediated interviews will be undertaken with a voluntary sub-sample of families who have taken part in the parental engagement project. The interviews will be 6 months delayed so as not to impact on the quantitative evaluation and will focus on the learning experience and perceived impact. The Family handbook and the families' films will be used as prompts to support this process.
2. A multiple case study will focus on the impact of the intervention in a sample of 3 schools drawn from each of the 3 regions taking part in the project. The study will draw on a wide range of data produced as an output of the intervention pedagogy, gathered through non-participant observation, through interviews with teachers and sampled families and via research tools including Pupil Views Templates, an SDQ questionnaire exploring parental relationships and baseline data questionnaires. Additionally, the case studies will be informed by the results of INCAS assessments for all pupils, gathered before and after the intervention in order to capture impact on attainment. Visual methods will feature strongly in the research, including using video in observation and in the use of highly visual data collection and pedagogic tools. Exploiting the range of expertise within the research team, the case study and data from the entire sample of schools will be used to identify and discuss emerging themes.

PERSONNEL

Roles and responsibilities

Design of the trial

- sample size calculations - University of Durham
- refinement of randomisation approach - NIESR

Delivery of the intervention

- recruitment of schools - Campaign for Learning
- delivery of teacher training - Campaign for Learning
- delivery of parental engagement course - Campaign for Learning

Measurement of outcomes and construction of database

- administration of CEM tests – University of Durham
- administration of SDQi tests – University of Durham
- construction of database – University of Durham
- incorporation of NPD – NIESR

Impact analysis - NIESR

Qualitative analysis - University of Durham, with support and guidance from NIESR

TIMELINE

Timetable including specification of who completes each task

- Delivery of the intervention is scheduled to complete by end-May 2013 (Campaign for Learning)
- Monitoring will continue throughout the intervention (Campaign for Learning, University of Durham)
- Qualitative analysis will be on-going from March 2013 to end-January 2014; with reporting scheduled for end-March 2014 (University of Durham)
- Advice and support for the qualitative analysis will continue throughout the study (NIESR)
- Immediate impacts will be estimated by end-August 2013 (NIESR)
- Tests six month post-intervention are scheduled to complete by end-Nov 2013 (University of Durham)
- Six-month impacts will be estimated by end-January 2014 (NIESR)
- Final report on impact analysis by end-March 2014 (NIESR)

RISKS

Risks to the evaluation and how they might be addressed.

Data protection statement

Some of the key risks are listed below:

- A low level of volunteering for the parental engagement course would reduce the power of the trial. To guard against this, a very robust engagement strategy has been developed from over three years of successful engagement work in schools. This starts 13 weeks before delivery of the programme begins, and involves a range of approaches designed to engage as many families as possible.
- Among those parents and children who start the parental engagement programme, some will not complete it. Such attrition creates analytical problems. The design of the course lends itself to retention, as each week the family complete another element of their animation. Each school actively communicates with all families and sends out reminder texts or letters. Monitoring information is collected in order to understand attendance patterns.
- Schools dropping out post-randomisation reduces the integrity of the experimental design. Efforts can be made to achieve buy-in from schools by explaining the reason for the trial and what it hopes to achieve. Importantly, schools stand to gain regardless of their assignment outcome: control schools receive the intervention, just with a delay. Where schools drop out, prior to receiving the intervention, they will be replaced by substitute schools who are given the assignment outcome of the dropout schools. Sensitivity to this will be assessed at the analysis stage.
- There is the possibility that delivery of the intervention varies across schools. We are able to observe in the data who is the facilitator for the parental engagement course and so control for this source of variation.
- When randomising clusters rather than individuals, the chances of a 'bad draw' increase. We used blocking to limit this problem.