

The Craft of Writing

UCL Institute of Education and BIT

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| Evaluation Summary | |
|--------------------|---|
| Age range | 9-11 |
| Number of pupils | Approx. 3,000 |
| Number of schools | 96 |
| Design | Two-arm school-level clustered randomised trial |
| Primary Outcome | Writing Assessment Measure (WAM) with double-weighted "Ideas" sub-scale |
| Protocol date | 18 May 2018 |
| Version | 1 |

Intervention

The Craft of Writing (CoW) intervention provides a sustained 'Arvon experience' developing teachers as writers and is combined with a more explicit focus on pedagogical implications for the classroom. The intervention will be underpinned by a Framework of Craft Knowledge co-created with professional writers. This will make visible what the teachers are learning about writing – the process of writing; linguistic choices; and narrative/poetic techniques – and it will be used to support teachers in integrating what they learn from the Arvon experience into their routine teaching of writing.

The CoW evaluation is part of a broader programme of work entitled 'Learning about Culture', which aims to improve the evidence base around arts-based education programmes. This is coordinated by the Education Endowment Foundation and the Royal Society for the Arts.¹ It consists of five programmes: two in Key Stage 1 (Reception and Year 1) and three in Key Stage 2 (Year 5). Despite the unique aspects of these intervention models, there are many similarities in how they are delivered and what they hope to achieve.²

One self-nominated Year 5 teacher in each participating school will participate in the training, which will be led by tutors who are professional authors selected by Arvon. This includes two weekend (Friday-Sunday) Arvon Teachers as Writers residencies 6 months apart. These will comprise workshops and tutorials for teachers led by professional writers, with time and space for writing, plus structured sessions to consider pedagogical implications and establish clear expectations for follow-through. There will also be a programme of CPD days for teachers, each of which will include a further Arvon Teachers as Writers experience, but will also provide a programmatic sequence, linking teachers' experience as writers with focused consideration of pedagogical transfer to the classroom.

¹ <https://www.thersa.org/globalassets/pdfs/reports/rsa-learning-about-culture-report.pdf>

² For an overarching flow diagram of the programme similarities, please see appendix 1.

These activities aim to develop teachers' and, through this, students' confidence as writers. As noted above, the CPD days for teachers will be focussed on changing how teachers teach writing in the classroom with an emphasis on using their improved knowledge of the craft of writing and through fostering a community of creative writers. Where successful, this results in pupils' with a stronger sense of agency around writing and of ownership of their writing.

Significance

A focus on increasing attainment in literacy and numeracy has been criticised for leading to a marginalisation of art, music and cultural studies in English schools (Warwick Commission, 2015). The UK Government's Culture and Sport Evidence review (Newman et al., 2010), which summarised much of the observational and qualitative research in this area, showed student participation in cultural learning programmes (from piano training to theatre-based drama projects) to be correlated with higher levels of achievement in mathematics and literacy / English in both primary and secondary school. The review also linked participation in cultural learning programmes to faster language development in the early years and improved cognitive ability. Additionally, large cohort observational studies in the US have suggested that the mathematics and literacy gains to cultural participation are particularly large for students from low income groups (Catterall, 2009, 2012).

Research into the relationship between providing teachers with an opportunity to develop their own identities and skillsets as writers and student outcomes is patchy. There is some evidence of impact on teachers' skills, knowledge and confidence in facilitating creative writing (Redmond, 2010), and on students' attitudes and engagement as writers (Wilson, 2010), but a systematic review by Cremin & Oliver (2017) found that the evidence base regarding the impact of teachers' writing on students' outcomes is small and does not show a clear impact. Specifically, they review all studies from 1990-2015, finding only 22 that met the standards to be included in their systematic review; these studies lead to an inconclusive picture of whether or not teachers who teach writing 'need to be writers', but highlight the role of pre-service and professional development programmes in helping teachers develop their assurance and identities as writers.

This evaluation is part of a round of funding between the Education Endowment Foundation (EEF) and the Royal Society of Arts to test the impact of different cultural learning strategies in English schools. The programmes will be supported by Arts Council England.

Methods

Research questions

The primary objective of this evaluation is to estimate the effect of participating in the CoW over the course of one school year on pupils' writing skills.

In addition, the evaluation will seek to answer the following questions:

1. What is the effect of participating in the CoW over the course of one school year on pupils' writing self-efficacy?
2. Does participating in the CoW over the course of one school year have an impact on pupils' perception of their own capacity to generate ideas?

In addition, we will look at the long-term effects of participating in the CoW after one further year, looking at results from the end of Key Stage 2 SATS test in English grammar, punctuation and spelling. These long-term results will not be included in the initial EEF report due to the time lag in these assessments, but this analysis will allow us to look at the longer-term effects of participating in the CoW. The results from the long-term outcomes will be reported in early 2021.

Design

This trial has been designed as a two-armed clustered randomised trial with randomisation occurring at the level of the school. This level of randomisation has been selected since

entire classes participate in the programme and thus the risk of contamination within schools is very high. The two arms are as follows:

- Participation in the Craft of Writing (Treatment)
- Business as usual (Control)

Randomisation

Blocking will be used to improve cross-arm comparability of schools, to improve precision of estimates, and to allow schools that sign up early to receive their allocation sooner than they otherwise would (this is important because of the nature of the intervention requiring as much notice of allocation be given to teachers, given it requires activity outside of their normal working hours). There will be eight blocks, defined on the basis of, timing of sign-up (first 48 vs. last 48), class composition by English as an Additional Language (EAL) (high vs. low) and class composition by eligibility for Free School Meals (FSM) (high vs. low). This approach will help to ensure that our treatment and control groups are well balanced in terms of these characteristics, which are likely to be correlated with our outcome measures (EEF, 2015b). High and low EAL and FSM in these definitions will be defined as above and below by the sample median (calculated separately for the first 48 and the last 48) in each case to ensure that block sizes are approximately equal (which may not be the case if we used population, rather than sample, characteristics).

Randomisation will be designed to achieve an equal number of schools in each arm (i.e. 48 control and 48 treatment). It will be carried out twice (as per details about blocking above) and the same procedure will be followed each time:

- Each school will be assigned a randomly generated number (setting a stable seed for the random number generation);
- Schools will be sorted by block and random number;
- Schools will be assigned to the treatment arm and to the control arm in turn.

Randomisation will be carried out by UCL in Stata and the code used to carry out the process will be recorded and reported in the final report.

Participants

Ninety-six English state-funded primary schools will be recruited principally from the following geographical regions: the North-West and North-East with a small number from London. One class from Year 5 will be selected for participation in the trial; all children in the teacher's class will participate in the trial. It is important that the teacher and pupils who will participate are selected prior to randomisation to ensure that this cannot introduce differences between the intervention and control groups; this will be assured by collection of pre-randomisation data (see the following section on sample size calculations).

In order to be considered, schools will have to agree to distribute opt-out consent forms to parents, provide student data in order that it is possible to apply for data from the National Pupil Database, to identify the teacher who will participate in the trial, and to cooperate with the project and evaluation teams during the trial (further details of these requirements are outlined in the Memorandum of Understanding with Schools).

The project team will advertise the trial and also approach schools through their existing networks. Where possible it will aim to recruit schools that have larger populations of individuals receiving Free School Meals than the national average of 15.3 per cent of pupils aged 5-10 (DfE, 2016).

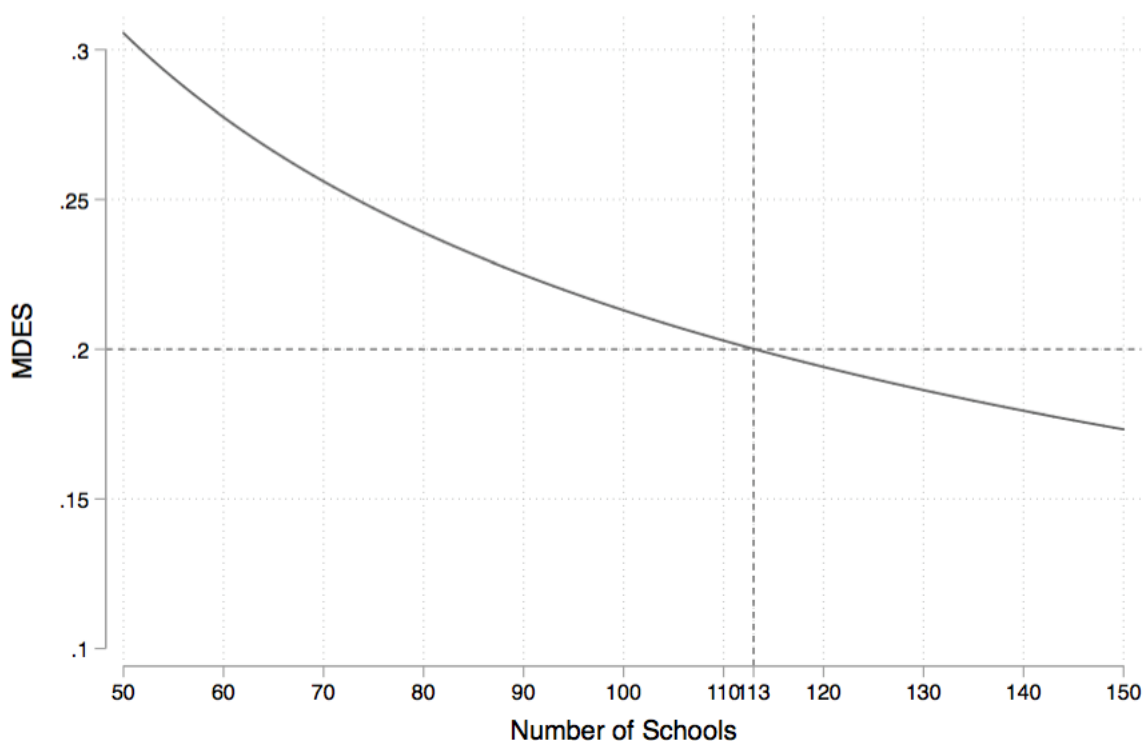
Sample size calculations

We conduct our sample size calculation for the Writing Assessment Measure, since this is the primary outcome of interest. Sample size calculations are based on an estimated Minimum Detectable Effect Size (MDES) of 0.20 and the following assumptions: power of

0.8 for a two-tailed 0.05 significance test, randomisation at school level, an intra-cluster correlation of 0.15³ (EEF, 2015a) and 25 pupils involved in the trial per school with 10 per cent pupil-level attrition.

An appropriate pre-test/post-test correlation assumption cannot be estimated empirically directly for this trial, since correlation data between the pre- and post-tests used in this trial are unavailable. This is because the pre-test (score in the year 1 phonics screening check; used consistent with EEF policy to use an administrative measure rather than an additional pre-test) has only been in place since 2012, and our post-test (the WAM) is an even newer measure. EEF guidance suggests that a pre- and post-test correlation of 0.7 in education research is common (Torgerson and Torgerson, 2013), however we see this as too optimistic in this case. The 21-day test-retest correlation coefficient of the WAM is reported to be 0.82 (Dunsmuir et al., 2015) but the time elapsed between the pre- and post-test in this trial is much longer, and we will not be using the WAM itself as a baseline. Our proposed pre-test (score in year 1 phonics screening check) has less variance than would be ideal, due to a degree of bunching between the pass (32) and highest available mark (40). Nevertheless, given its closer temporal proximity to the post-test point, we believe it is likely to explain more variance in our post-test than earlier measures available in the NPD (which would have to be measured at the Early Years Foundation Stage). While there is no direct measure of the pre-test/post-test correlation between the WAM and the phonics screening check available, a value has been calculated using year 1 phonics screening check scores and Progress in International Reading Literacy Study (PIRLS) scores (DfE, 2017) (taken in year 5, the same year as the WAM will be administered). This value is estimated to be 0.52. Given the similar time period between pre-test and post-test administration, and the related domain, we believe this estimate is likely to approximate the value that will be observed in this trial. Based on this, we assume that 25% of post-test variance at both pupil- and school-level is explained by the pre-test (equivalent to pre-test/post-test correlation of 0.5).

Figure 1. Minimum detectable effect size estimate as a function of number of schools



³ EEF guidance on ICCs (EEF, 2015a) is provided for NPD outcomes. In the absence of ICC data for our outcomes of interest we use this guidance, specifically for the reading fine points score, and, given uncertainty about the geographical spread of participating schools, we use the highest regional ICC (which happens to be Inner London) to the nearest two decimal places.

These assumptions suggest a requirement of 113 schools to achieve an MDES of 0.2 (see Figure 1). Based on discussions with the Craft of Writing team at the set-up meetings, we agreed on a sample size of 96 schools. This number is due to a maximum of 16 participants per Arvon retreat. The Craft of Writing team confirmed that recruitment of 96 schools and intervention delivery to 48 treatment schools are reasonable and achievable numbers given their capacity. Given the sample size of 96 schools and the assumptions mentioned above this trial should be able to detect an effect of 0.22.

Assuming the FSM sub-group is 15.3 per cent of the total size of the sample (based on pupils aged 5-10 in data from DfE statistics (DfE, 2016) and ignoring that it may be higher if recruited schools are in more disadvantaged areas), and maintaining all other assumptions (which is likely to be a conservative approach, given lower levels of within-group variation in this sub-group), there is an estimated minimum detectable effect size for this group of approximately 0.33 standard deviations.

Outcome Measures

The primary outcome of interest is writing attainment and the secondary outcome of interest is writing self-efficacy. The “ideation” sub-measure of the same writing self-efficacy measure will be an additional secondary outcome measure. These are discussed in turn below.

Writing attainment

To measure the primary outcome, we will use the Writing Assessment Measure (WAM) (Dunsmuir et al., 2015; Murphy et al., 2013). The WAM was developed in order to create a valid and reliable writing assessment measure, relevant within the context of the English educational system. This measure is designed to assess narrative writing in response to a written prompt, to which pupils are given 15 minutes to write. Previous evidence suggests that this measure is reliable (test-retest correlation $r=0.82$ over 21 days with different prompts) and valid ($r=0.786$ with Wechsler Objective Reading Dimensions–WOLD–Written Expression subtest) (Dunsmuir et al., 2015). The WAM is based on the structure and format of the WOLD Written Expression subtest, with modified dimensions that incorporate descriptors from the National Curriculum writing attainment targets, including: ideas development, organisation and planning, vocabulary, sentence structure and grammar, spelling, punctuation and handwriting. The WAM is unique as an assessment because it incorporates “ideas development”. Given the nature of the intervention, we will double-weight the score on the “ideas development” dimension. Final scores range from 4 to 32 (after accounting for double-weighting).

Writing self-efficacy

As highlighted in the logic model, the impact of the intervention on writing outcomes may have an effect through pupils’ engagement with and motivation for writing, which may in turn have an effect on children’s sense of efficacy as a writer. For this reason, we consider writing self-efficacy as our secondary outcome. In addition, self-efficacy has been highlighted in EEF’s review of non-cognitive skills: the evidence “indicate that self-efficacy for a particular task is malleable and that improved self- efficacy is associated with greater persistence, interest, and performance” (Schoon & Gutmann, 2017, p.11) and that “the best predictors of specific academic performance are self- efficacy beliefs regarding those specific academic domains” (Pajares, 1996). To measure writing self-efficacy, we will use a version of the measure proposed by Bruning et al. (2013), which has been adapted for primary school pupils with some simplification of language. This involves sixteen statements capturing aspects of writing, including “I can think of many ideas for my writing” and “I can avoid distractions while I write”, with pupils giving marks out of 100 for their self-assessment in each of these. We plan to use slightly simplified versions of some of these to better suit the primary school context; in addition, we will request responses on a 5 point Likert scale. Bruning et al. (2013) develop a multi-factor model of writing self-efficacy, however since we do not have specific factors (beyond those listed below) that we hypothesise our intervention to effect we will use a simple aggregate of self-assessments across all sixteen statements

(all are positively framed so there is no need for reverse coding). As such, possible scores range from 16-80 for each child.

Ideation

The CoW team also identified increased creativity in the pupils who have participated in their programme. To explore this, we will report differences in the “ideation” sub-measure of the writing self-efficacy measure as an additional secondary outcome measure. This measure was jointly chosen with RSA and allows us to address secondary research question 2 on “idea generation”. This uses the first five questions of the writing self-efficacy measure and, as such, possible scores range from 5-25 for each child.

Collection and marking

Both the WAM and the writing self-efficacy measures will be invigilated and collected in summer 2019 by a team of research assistants (RAs) coordinated by the Behavioural Insights Team (BIT); they will also mark the writing self-efficacy measure. RAs will be blind to trial arm assignment of schools. An additional group of RAs, also blind to trial arm assignment of schools, will mark the writing exercises against the WAM scoring sheet. A sub-sample will be independently double marked to allow us to assess inter-rater reliability of the measure.

In addition, we will look at pupil performance on national KS2 SATS tests in English grammar, punctuation and spelling. These results will not be available until 2020, which is after the trial concludes; therefore, this analysis will not be included in the initial report. The results from this outcome are planned to be included in a separate report reflecting on all the projects from this round of funding to be published in early 2021.

Analysis plan

We will estimate the effect of the trial using a linear model on pupil-level data with school-level clustered standard errors including a school-level treatment indicator, blocking dummy variables (EAL composition, FSM composition, and timing of sign up) and pre-test covariate (Kahan and Morris, 2012). Scores in outcomes as described in the outcome measures section above will be used in all models.

The coefficient on this treatment indicator will recover the Intention to Treat (ITT) estimate of impact. We will calculate Hedge’s *g* effect size by dividing this coefficient by an estimate of the pooled total variance of the outcome variable and applying the appropriate correction factor. 95% confidence intervals will be estimated by inputting the upper and lower confidence limits of the coefficient from the regression model into the effect size formula.

An estimate of the intra-cluster correlations of the outcome measure will be extracted by estimating a variance components model for this purpose.

As noted above, the regression model will include a pre-test variable in order to improve the precision of the estimates. This will vary depending upon the outcome being estimated:

- We will use pupils’ marks in the Year 1 phonics check (extracted from NPD PHONICS_PHONICS_MARK) as a pre-test for writing attainment outcome and the KS2 English grammar and punctuation outcome measure.
- We will use assessment of pupils’ Personal, Social and Emotional Development skills from the EY Foundation Stage Profile (aggregated scores from NPD FSP_PSE_G06, FSP_PSE_G07 and FSP_PSE_G08) as a pre-test for writing self-efficacy outcomes (including the ideation sub-measure).

Following EEF guidance, we will first test for an interaction of the treatment and FSMever status. If a significant interaction is found, we will estimate a separate model on the restricted sample of only FSMever pupils. This procedure will be carried out for both our primary and our secondary outcomes.

We will estimate treatment effects for compliers using a Complier Average Causal Effect (CACE) analysis using a school-level measure of compliance with the intervention, discussed below.

Definition of fidelity/on-treatment minimum

We outline below the fidelity measure and on-treatment minimum for CoW. This measure assesses the minimum standards required in order for the delivery team to be satisfied that it is on-treatment – it is not an assessment of quality of engagement. The purpose of this measure is to be able to exclude schools which have not engaged in the intervention in the way we expected, which also provides useful contextual information for the process evaluation. For example, it may help us decide which schools to sample for the case studies.

The fidelity of this intervention will be measured at the teacher level, which reflects the intervention delivery method. These are as follows:

- Teachers attend both residential weekend workshops
- Teachers attend at least 2 out of the 3 CPD sessions

All measures will also double up as continuous measures to assess the range of basic engagement within the sample, and will help us understand which schools to ask to participate in the case studies as well as provide data to cross reference against the survey results.

Implementation and process evaluation methods

Introduction

A robust and in-depth implementation and process evaluation (IPE) is vital to ensure we understand the extent to which CoW achieves positive outcomes for young people.

In the first section, we outline the overarching implementation questions that will be explored across all Learning about Culture projects, including CoW. The cross-project similarities in delivery and in what they are aiming to achieve are outlined in the appendix. We highlight, for each question, which dimension or factor affecting implementation it relates to, as specified in the guidance set out by the EEF.⁴

The second section outlines the IPE questions that are unique to CoW. Additionally, we outline the fidelity measure for each project-the individual on-treatment minimum.

A flexible research approach will be employed to capture the unifying and distinct elements of the five programmes. We will use similar methods to capture both the overarching IPE questions, as well as the project specific questions.

Cultural Learning overarching IPE questions

1. In what ways was the programme implemented? What are the barriers and facilitators of delivery (Fidelity)? In particular:
 - a. Senior Leadership Team buy-in;
 - b. Delivery of training – a) the extent to which is it consistent across sites; and, b) whether it appears to be effective in ensuring that teachers understand the

⁴ Humphrey, N., Lendrum, A., Ashworth, E., Frearson, K., Buck, R., & Kerr, K. (2016). Implementation and process evaluation (IPE) for interventions in education settings: An introductory handbook. *Education Endowment Foundation (Ed.)*.

- aims and main features of the intervention;
- c. Delivery of the intervention – a) consistent across sites; b) whether it appears to be effective in supporting children’s attainment c) whether it appears to facilitate children’s engagement
2. To what extent did the schools engage with the intervention in line with the intervention aims? (Responsiveness).
 3. How was the quality of the intervention perceived by teachers, senior leaders and teaching assistants? (Quality)
 4. To what extent is the knowledge of arts practitioners delivering the intervention integrated with the pedagogic knowledge of teachers involved? (Implementer support system)

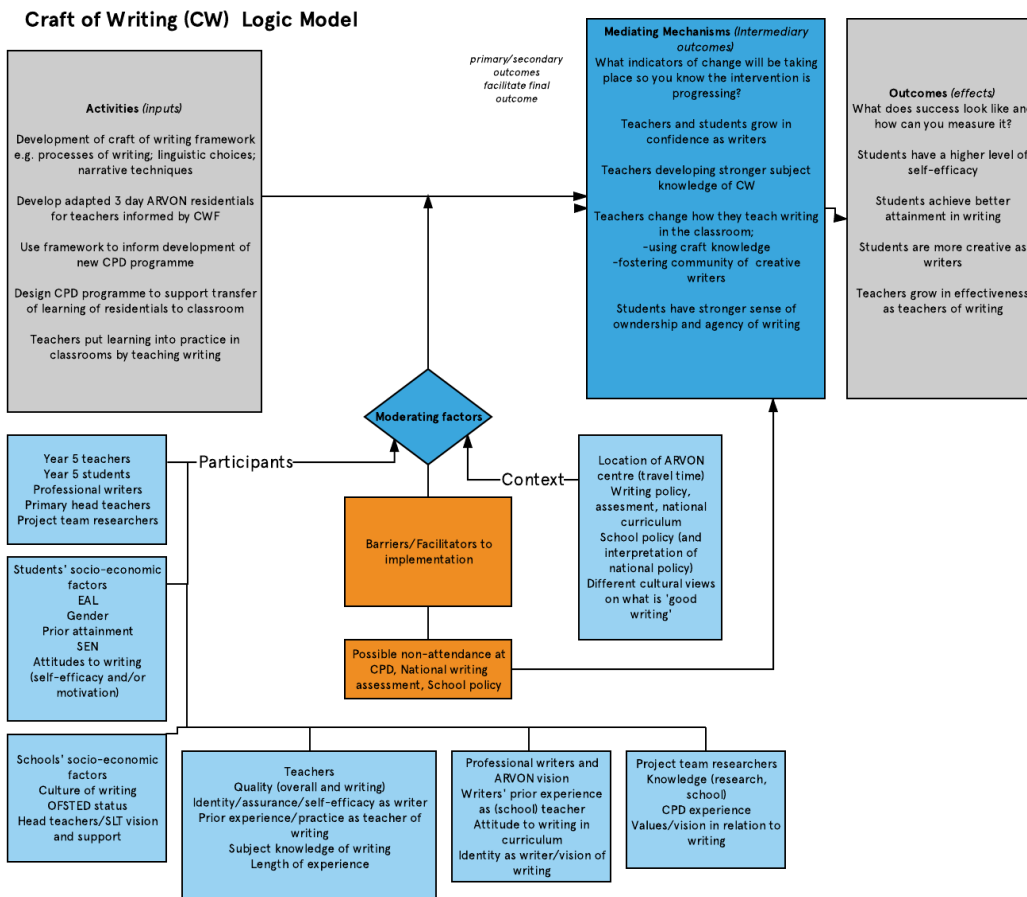
Craft of Writing specific questions

Beyond the overarching questions listed above, additional areas which will be important to explore are as follows:

1. To what extent does the intervention improve teacher confidence as a writer?
2. How do the strategies and techniques from the CPD and residential training emerge as part of teaching, and in what ways does this lead to improved teaching practice?
3. How does the intervention benefit the pupils: what are the mechanisms by which the teacher implements the intervention with their pupils?
4. How does teachers’ practice in relation to pupils revising their writing change?

Logic Model

An IDEA workshop was held, utilising the TIDieR framework, to develop a logic model in collaboration with CoW. The Logic Model will be instrumental in directing the IPE. Throughout the IPE, we will attempt to monitor the proposed mediating mechanisms as well as understand the role played by potential moderators. A summary of the similarities across all the logic models for the Cultural Learning interventions can be found in the appendix.



Methods

A suite of methods will be used to answer the research questions outlined above. The data generated from these methods will be analysed in conjunction with the other sources of data to provide an in-depth yet broad understanding of the intervention (see Triangulation section below for further details)

These methods will be the same across all projects to ensure consistency, but will vary according to the project delivery timetables that are yet to be defined. We will work closely with the CoW team to ensure we conduct the data collection when appropriate.

In addition to main project team input Professor Andrew Burn, specialist in English, Media and Drama; and Professor Gemma Moss, literacy specialist (all at UCL Institute of Education) will be invited to give feedback on the methods.

Observation of training. The IPE team will attend and observe at least one residential training and one CPD session delivered by the training provider, as well as review the materials used in the courses. Members of our team with expertise and knowledge of arts in education will lead the observations and fieldwork. We anticipate that the project team and/or training providers would also carry out evaluation of the training for their own purposes; where these overlap and with appropriate consent, we would look to triangulate insights. This will be particularly valuable around measuring engagement in programmes and consistency of training.

Administrative data. Working closely with the delivery partners, we will devise measures of engagement in the intervention and triangulate these metrics with the sampling to ensure our case studies (see below) target a variety of intervention settings. These measures may include online metrics, attendance or other relevant engagement related data.

Case studies of schools. These will consist of interviews and classroom observations with a subset of approximately 6 schools. These case studies will consist of

- Teacher interview both before and after the lesson observation
- Observation of a lesson featuring writing
- Interview with SLT

The schools will be sampled based on a range characteristics such as location; Ofsted rating and engagement (see defining fidelity). Case study is a powerful research strategy to use within sequential explanatory mixed method designs and adds completeness to the exploration of complex issues in situ (Yin, 2013).

Online surveys. To gather data from all participating schools, we propose carrying out an online survey of control and treatment schools. The purpose of this survey would be to collect information on “business as usual” schools and classrooms, differences between “business as usual” and intervention classrooms, cost data, and a wider view of implementation and/or impact as measured qualitatively. To encourage participation and minimise the burden on respondents, it is expected that the survey would take teachers no more than 20 minutes to complete.

Triangulation

Multiple sources of data will be brought together to best answer the IPE questions. How these methods will be triangulated are outlined in the table below.

| Cultural Learning IPE Questions | Methods |
|---|--|
| In what ways was the programme implemented? What are the barriers and facilitators of delivery (Fidelity)? In particular: <ol style="list-style-type: none"> a. Senior Leadership Team buy-in; b. Delivery of training – a) the extent to which is it consistent across sites; and, b) whether it appears to be effective in ensuring that teachers understand the aims and main features of the intervention; c. Delivery of the intervention – a) consistent across sites; b) whether it appears to be effective in supporting children’s attainment c) whether it appears to facilitate children’s engagement | Survey; Administrative Data; Case studies; Observation |
| To what extent did the schools engage with the intervention, in line with the intervention aims? (Responsiveness) | Survey; Administrative Data |
| How was the quality of the intervention perceived by teachers, senior leaders and teaching assistants? (Quality) | Case studies; Survey |
| To what extent is the knowledge of arts practitioners delivering the | Case studies; Survey |

| | |
|---|---|
| intervention integrated with the pedagogic knowledge of teachers involved? (Implementer support system) | |
| Craft of Writing questions | |
| To what extent does the intervention improve teacher confidence as a writer? | Survey; Case studies; Observation |
| How do the strategies and techniques from the CPD and residential training emerge as part of teaching, and in what ways does this lead to improved teaching practice? | Survey; Administrative Data; Case studies |
| How does the intervention benefit the pupils: what are the mechanisms by which the teacher implements the intervention with their pupils? | Survey; Case Studies |
| How does teachers' practice in relation to pupils revising their writing change? (Mechanisms) | Survey; Case studies |

IPE Analysis

Structurally, this will draw upon the analytical strategy of multi-case studies – whereby a programme is first coded individually and then a large cross-sectional analysis is conducted, which encompasses all programmes (Stake, 2013). This deductive analysis will be conducted on Nvivo by the lead researchers, who will co-code 3 transcripts to ensure coder similarity and robustness of coding framework. Codes will be pre-specified in a coding framework which reflect the research questions, but additional codes will be created as new themes emerge.

A cross-project analysis of the Cultural Learning aspects of the data will be conducted to ensure we identify significant patterns relevant to all interventions. This will take the form of a flexible, yet robust, thematic framework, which will include elements that are unique to each, but also relevant to all projects. It will be important to understand how the same theme may be manifested in a different way for different programmes (Bazeley, 2013). The findings from this cross-project analysis may be included in individual reports where appropriate but will also be part of the wider report of Cultural Learning to be published in early 2021.

IPE Data Collection Timeline

We understand that each project will follow a similar delivery schedule, with variation in the numbers and timing of training sessions across the year. This similarity allows us to map our data collection activities on to one timeline. We have arranged the timeline by term, as we are yet to confirm the exact dates for observation. We can therefore consider this an indicative schedule of events across the academic year of 2018-19.

| Date | Item |
|-------------|-------------|
| | |

| | |
|------------------------------|--|
| June-October 2018 | Observation of residential training |
| | Collection of baseline survey to measure school buy-in and teacher attitude towards intervention |
| | Collection of school characteristics |
| January-February 2019 | Observation of mid-point training |
| | Collection of fidelity data to inform case study sampling |
| | Finalise sampling strategy |
| | Conduct in-school case studies |
| June-July Term 2019 | Conduct second in-school case studies |
| | Administer end of intervention survey |
| | Conduct analysis |

Costs

An estimate of the per-pupil cost of the intervention will be calculated by the evaluation team. This estimate will focus on cost from the perspective of a participating school and will be based on the direct, marginal costs of implementing the intervention. This could include anything which the school needed to pay for beyond the business as usual.

The cost estimates will make use of information from the project team (particularly regarding the actual cost of delivering the intervention, e.g. the cost of providing the training), as well as that collected directly by the evaluation team from schools about the costs of preparing and implementing the intervention. Information on costs, especially any hidden costs or resource implications, will be explored through the process evaluation as part of the interviews with teachers and school visits. The purpose of collecting such data in the process evaluation would be to identify the main areas of expenditure required by the project. This process will also help to establish whether it may be appropriate to include any questions on costs/resource use in the survey. This will need to strike a balance between collecting sufficient cost information and not damaging response rates; it will also need to take account of whether a teacher is well placed to provide accurate information on particular types of costs.

Time spent by schools, such as the amount of time for which schools need to arrange cover for teachers to attend training will be reported separately from the financial costs. Any costs in terms of prerequisites will also be considered, for example iPads or other resources. Control group schools will also be asked about the time they invested in CPD, to ascertain how much time above and beyond business as usual is needed. We may also triangulate national data on this if available.

An estimate of cost per pupil per year will also be calculated based on the trial period, as once trained, teachers would also be able to deliver the programme in subsequent years. Any costs associated purely with the evaluation will be excluded.

Ethics and registration

Ethical approval has been sought following UCL Institute of Education staff ethics approval procedure. It was approved on 14 December 2017.

Personal data for this trial will be processed under the public task provision of the GDPR. Nevertheless, parents will be provided with the option to object to this processing of their child's data, which we will respect. This use of data has been allocated the following UCL Data Protection Registration Number: Z6364106/2017/11/69 social research.

This trial protocol has been pre-registered at www.controlled-trials.com, and assigned an [International Standard Randomised Controlled Trial Number \(ISRCTN\)](#) of XXX.

Personnel

Project team

Debra Myhill (Exeter), Teresa Cremin (Open), Becky Swain (Arvon)

Evaluation team

Jake Anders, Nikki Shure, Dominic Wyse (UCL), Pantelis Solomon, Florentya Farghly, Jessica Heal, Michael Sanders (BIT)

The teams will have the following roles within the evaluation:

Design of the trial

- Sample size calculation – UCL
- Refinement of randomisation approach – UCL

Delivery of the intervention

- Recruitment of schools – Project Team
- Delivery of intervention – Project Team

Measurement of outcomes

- Writing outcomes - BIT
- Writing self-efficacy outcomes – BIT
- NPD application and linkage – UCL

Impact analysis – UCL (lead) and BIT

Qualitative analysis – UCL (lead) and BIT

Risks

The data security policies of UCL and BIT and the Data Sharing Agreement between BIT and UCL are included with this protocol.

Some of the key risks are summarised in the table below:

| Issue/risk | Risk level | Action to address issue/reduce risk |
|------------|------------|-------------------------------------|
| | | |

| | | |
|---|----------------|---|
| Dropout / non-compliance of settings | Medium | We want to avoid attrition of schools from the project as much as possible. We plan to minimise attrition by ensuring that schools that sign up are committed (by asking them to sign a Memorandum of Understanding). Keeping them informed of progress and providing reminders of next steps will be important for retention. The project team as a matter of course monitor changes in key personnel to ensure ongoing commitment. Minimising the data collection burden on schools will also be important for retention. We will also randomise only after schools have followed consent collection procedures, provided the necessary student data. |
| Difficulty in collecting data needed prior to randomisation (i.e. pupil data and consent) | Medium | Data will be submitted directly to BIT who will conduct quality checks prior to its acceptance and a school being considered eligible for randomisation. This will also be subject to quality assurance by the UCL team, including random checks of a sample of data collection spreadsheets during this process ensuring no missing data or discussing where this has been unavoidable and understanding the reasons for this. |
| Difficulty recruiting schools | Medium to high | We are confident that the project team will convey the importance of the evaluation to settings and the value to them of taking part. To understand whether recruited settings are atypical in some way (which would affect external validity), we ask that the project team keep records of settings approached and, where possible, of reasons for not participating. |
| Withheld consent to link to NPD | Medium | <p>We plan to collect the necessary data to allow this long-term follow up. We believe this processing of personal data is justified under the legitimate interests/public purpose of data protection regulations. Nevertheless, we will offer parents the opportunity to opt their child out of all processing of their data. We believe we should be able to provide the necessary information to parents in treatment and control settings and do not anticipate high or non-randomly varying levels of opt-out.</p> <p>There is some risk that regulatory change (introduction of the GDPR) may change DfE's attitude towards allowing access to NPD data on this basis. We think there are minimal steps we can take to mitigate this risk directly (short of changing to unambiguous-opt-in-consent, which has its own drawbacks). However, our primary analysis models only rely on NPD data to improve precision; as such we should still be able to recover unbiased estimates, albeit with lower levels of precision.</p> |
| Missing Outcome Data | Medium | <p>For directly collected assessments, attrition is a potential risk. BIT and UCL will ensure schools and research assistants understand the need to collect post-test measures for as many students as possible in order to maximise internal and external validity.</p> <p>Schools will also be contacted sufficiently far ahead of data collection window to ensure we arrive at a convenient time for RAs to visit and run the writing assessments.</p> <p>RAs will report to the BIT project coordinator the number of children not able to sit the assessments after each visit. If the rate is high (>5% of sample) the project coordinator will contact the school for further detail if required and alert UCL, the EEF and project team.</p> |

| | | |
|---|--------|--|
| | | Linking to children's outcomes in the NPD for long-term follow-up offers some protection against attrition but not for the primary outcomes of the project. This is not entirely without risk as it relies on the legal basis and technical ability to identify children in the NPD (see above). |
| Parent and teacher concern about 'over-testing' | Medium | Communications to schools (during recruitment) and parents (when obtaining consent) will emphasise that these assessments (referred to as "writing samples" in all school-facing communication) will be fairly short and their children will not be judged upon the outcomes. As such, they will be kept low-stakes and low-pressure for pupils. |
| Problematic randomisation | Medium | When randomising clusters rather than individuals, the chances of a 'bad draw' increase because of the reduction in the number of units being randomised for a given number of participating schools. To protect against this, we plan randomisation within blocks, as described in the Randomisation section. |
| Treatment variation | Medium | We view this not so much as a risk but as the reality of implementing such an intervention. The impact estimates (Intention to Treat) therefore relate more to the type of treatment likely to prevail in practice rather than the type of impact that could be seen were it possible to achieve laboratory-type conditions. Nevertheless, understanding treatment variation is important and will be explored through CACE analysis of the on-treatment sample as well as being a key focus of the implementation and process evaluation. |
| Unexpected absence or loss of team members | Low | The team will substitute for each other during any short-term absence. In the event of longer periods of unplanned absence or departure, we will recruit replacements. Both BIT and UCL have other experts in evaluation and education who could substitute for members of the team, should this be necessary. |

Timeline

| Date | Activity |
|------------------------------|--|
| October 2017 – February 2018 | Recruitment (Project Team) The CoW team will begin recruitment halfway through the Autumn term 2017 using its existing network of schools and continue into early 2018. |
| October 2017 – February 2018 | Pre-Randomisation Data Collection (BIT and Project Team) |
| March 2018 | Randomisation (earlier if possible) (UCL) Evaluation team will randomise schools and inform CoW of outcome. |
| June 2018 | First Retreat for Intervention Group (Project Team) |
| September 2018 – July 2019 | Intervention in Schools (Project Team) |
| September 2018 – July 2019 | Implementation and Process Evaluation Fieldwork (BIT and UCL) – see detailed timeline above |
| May- July 2019 | Outcome Testing (BIT) Pupils' writing and self-efficacy outcomes will be measured by BIT. These assessments will be marked by PCGE students at UCL in a process overseen by BIT. |
| September – December 2019 | Impact Analysis and Report Writing (UCL and BIT) UCL will lead on the data analysis following the data analysis plan outlined in this trial protocol. |
| October 2020 | KS2 Outcomes Available for Follow-Up Analysis (UCL) |

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Appendix 1 Overarching IPE similarities of projects

Similarities across projects

The logic models from the 5 cultural evaluations were compared to understand their similarities and differences. From this, an amalgamated flow chart was designed to show the general route that all the programmes can take (Figure 2).

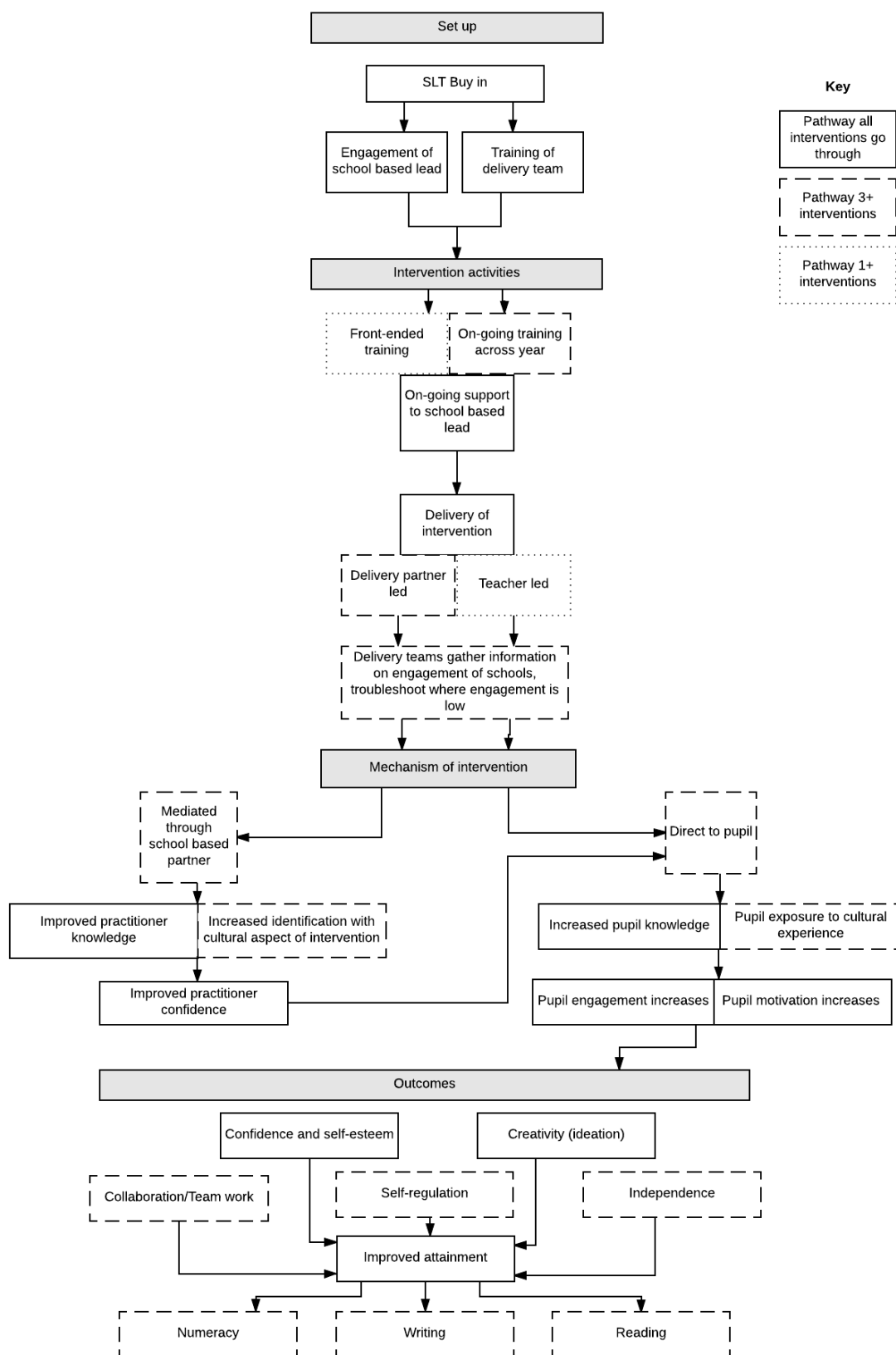


Figure 2. Amalgamated logic model of the 5 interventions

Implementation Similarities

From Figure 2, we can see that the following are standard across all 5 interventions:

1. Senior leadership buy-in
2. On-going (yet varied) support from delivery team staff - relationship with school, and teachers or teaching assistants.
3. Training days for teachers or teaching assistants
4. Delivery teams gather information which helps them understand how the schools are engaging in the intervention - to what extent it can we use this to gather fidelity information?

When considering the differences in implementation there are two possibilities which all of the five interventions take: 1. The intervention is mediated through school-based partners, or 2. The intervention is delivered direct to pupils. These two possibilities should be measured in a standardised fashion as they may have implications for how arts-based programmes are designed in the future. These 'options' are outlined below:

- 1) Training model - front-end loaded and/or on-going across the year
- 2) Direct delivery of intervention - via a member of school staff or via a delivery partner
- 3) Mechanisms of change - mediated through a member of school staff or delivered directly to pupils
- 4) For writing orientated interventions, the extent the practices reflect robust evidence of what works?

Moderating factors

Across the 5 interventions, several common moderators emerged from the logic model IDEA workshops. We will aim to capture these systematically when drawing up the MOU with the schools. Of all the 29 different moderators outlined, we will systematically capture those referenced by 4 or more of the projects. These are as follows:

- 1) School Ofsted rating
- 2) Current activities relevant to the intervention
- 3) Pupil SEND/EAL
- 4) Teacher/TA experience (years)
- 5) Teacher/TA background knowledge in arts-related programmes

Mediating factors

There was generally much less overlap between projects overall in relation to mediating factors, and the 43 mediating mechanisms listed (although many between-project similarities). The only ones which were relevant for 4 or more of the projects were broad, and the first is being captured in some of the projects already. The second, creativity, will also be captured as part of the overarching Ideation measure.

- 1) Improved pupil self-efficacy
- 2) Improved creativity