Researching the efficacy of a reading intervention: An object lesson

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Abstract

Conducting classroom-based educational research trials is important for establishing the efficacy and effectiveness of specific instructional interventions. Such endeavours however, are challenging to implement. This was made evident during a recent independent evaluation of the efficacy of the MiniLit program, wherein various difficulties emerged relating to the dosage and fidelity of instruction, and the measures and analyses employed by the research team. As such, this served as an object lesson in what can, and frequently does, go wrong in even the best planned intervention research enterprises conducted in schools. The present paper is intended to capture the authors' experiences in implementing research trials in school contexts, with specific examples drawn from the independent evaluation of MiniLit. In particular, this study has reinforced the need to select assessment measures carefully, according to how well they represent targeted skills in the specific population of interest. In addition, it has highlighted the importance of planning program efficacy trials such that participants can receive enough exposure of the intervention to progress to a realistic extent.

Researching the efficacy of a reading intervention: An object lesson

Conducting research in an educational context is very challenging. As such, there are many pitfalls that a researcher in this setting may experience when conducting intervention trials. In this paper, we will discuss, in turn, the importance of using an accurate and reliable *measure*, implementing adequate instructional *dosage* and *fidelity*, and selecting the appropriate *analysis* for examining trial results. To illustrate these points, as an object lesson, we will draw on examples from a research trial with which we have recently had some experience (i.e., Quach et al., 2019). Importantly, our main intention in writing this paper is not to refute the findings that emerged from the trial. Rather, it is to communicate what we learned about conducting research in schools, with reference to the trial's planning, implementation and reporting.

An empirical evaluation of MiniLit

Between 2016 and 2019, a randomised control trial (RCT) of the MiniLit reading intervention program (MultiLit, 2011) was conducted by an independent research team in conjunction with Evidence for Learning (Quach et al., 2019). Alongside these parties, the stakeholder committee for the project also comprised representatives from the New South Wales Education Department and the MiniLit program publishers (MultiLit Pty Ltd, specifically current authors K. Wheldall and R. Wheldall). These stakeholders were consulted at various points before, during and after the trial's implementation, although only the independent research team themselves had direct control over how the study was conducted and reported on.

The overarching aim of the trial was, 'to determine whether the MiniLit intervention, offered to Year 1 students identified as being in the bottom 25 per cent of readers, improved their reading more than Usual Learning Support [or the control group], 12 months after randomisation' (Quach et al., 2019, p. 5). In 2019, the results were included in an evaluation

report published by Evidence for Learning (Quach et al.). Students were selected as suitable participants for the research trial based on their performance in the bottom quartile on the Wheldall Assessment of Reading Lists (WARL) (Wheldall et al., 2015) – a measure of word reading fluency. All children involved in the trial (n = 217) were subsequently tested on various reading measures before the intervention began, and again 6 months and 12 months after intervention began. Approximately half of the students were allocated to the 'MiniLit' group and half were allocated to the 'control' group.

The MiniLit program is designed for young struggling readers, and comprises lessons that target phonemic awareness, phonics (specifically, synthetic systematic phonics), fluency, vocabulary and text comprehension and was designed based on empirical research into what works to improve reading in young children (Reynolds et al.2010; 2011). Instructional components are delivered explicitly and systematically, ideally in small groups of 3 or 4 students, for one hour per day every day. More information about MiniLit is included as Appendix A.

In brief, several analyses were presented in the final report of the evaluation. In two of the four analyses, only the group who attended an acceptable number of MiniLit lessons (i.e., >80%) were included, while the other analyses included students who attended anywhere between 0 and 100% of lessons (Quach et al., 2019).

Although three assessments were used to measure the outcomes of the trial, one of these assessments, the York Assessment of Reading for Comprehension Passage Reading (YARC-PR; Snowling et al., 2012b), proved to be inappropriate for the cohort of young struggling readers in the study and yielded unreliable results. This was acknowledged by the independent evaluation team and Evidence for Learning (Quach et al., 2019, p. 39) and will be discussed later. Hence, this brief summary will focus on the results from the two other assessments, the Castles and Coltheart 2 (CC2; Castles et al.,

2009) and York Assessment of Reading for Comprehension Early Reading (YARC-ER; Snowling et al., 2012a).

After six months (or two school terms) of MiniLit intervention, students performed significantly better than students receiving usual literacy support on measures of nonword reading, regular word reading, letter-sound knowledge and phoneme awareness. The improvements were associated with medium and large effect sizes. The same pattern of results was sustained until the 12-month testing time point, which was six months *after* MiniLit intervention had ceased.

One conclusion to draw from these results is that children receiving MiniLit instruction consistently showed better letter-sound knowledge than children receiving their usual literacy instruction. These results, in addition to the consistent group differences in nonword reading accuracy, indicate that MiniLit students are better equipped to decode unfamiliar words.

The results from the RCT described above, and the methodological procedures undertaken to obtain these results, led us to several conclusions about what matters when conducting reading intervention research. The study will be used to illustrate what can go wrong in even the best-planned intervention enterprises by expert researchers conducted in schools. It is not within the scope of this paper to instruct on how exactly to conduct a research trial but, instead, to serve as a resource to inform readers on what missteps may occur and to suggest what might be done differently in future, according to reflections on our own experiences.

The measures

Making valid inferences from behavioural test data requires that the measures employed accurately represent the skills being targeted and discussed. This is more difficult to achieve when skills have multiple components. Reading comprehension, for example,

draws on a range of underlying cognitive and linguistic processes, the relative importance of which depends on the reader's age and level of experience (Castles et al., 2018; Nation, 2019). For skilled readers, who can automatically recognise printed words 'by sight', reading comprehension depends predominantly on how well they can understand the text content, using their broader language comprehension skills (Catts, Hogan & Adlof, 2005; Catts et al., 2015; Language & Reading Research Consortium, 2015). In contrast, the main task for an unskilled or beginning reader confronted with a passage of text is to decode the series of mysterious squiggles on the page (Tunmer & Hoover, 2018). Even where a child's reading is accurate but slow, their understanding of the written text depends significantly on the attention and cognitive effort they must devote to word recognition (Lonigan et al., 2018). For this reason, it is difficult to assess reading comprehension in children under approximately 8 years old, particularly when word reading for those children is inaccurate and not yet automatic (Nation, 2019).

A problem that was not anticipated at the start of the MiniLit (MultiLit, 2011) evaluation was the inadequacy of the YARC-PR test (Snowling et al., 2012b) as the primary outcome measure. As described above, reading comprehension is a skill that is difficult to define and measure – particularly in young readers whose word reading is inaccurate and not yet automatic. Practically speaking, this issue should have become obvious when the YARC-PR was administered to young struggling readers during the MiniLit evaluation. Many of the sample scored zero or well below the norms for the test at pre- and post-test. Moreover, the reading accuracy, rate and comprehension results that *were* included in the evaluation report were arguably rendered invalid, given that the testers failed to abide by the YARC-PR administration requirements. The test requires a minimum of two passages to be completed by students to obtain a score (Snowling et al., 2012b, p. 26); however, in the vast majority (97.5%) of cases, it was only possible to obtain results from one passage. As stated by the

research team themselves, 'In hindsight, it seems the YARC[-PR] measure was not an appropriate measure for the cohort of poor readers at baseline, due to the young age of the cohort and the level of difficulty of the task' (Quach et al., 2019, p. 39).

The challenges associated with measuring passage reading ability in unskilled readers are frankly acknowledged in the YARC-PR manual, where it is also recommended that the YARC-ER measure (Snowling et al., 2012a) is administered instead to 'children who exceed the maximum number of reading errors on the Beginner Level passage' (p. 26). As such, the lessons to be learned here are that, firstly, special care should be taken to select measures that are appropriate for the age and expected skill level of the target population. In this respect, the study stakeholders *as well as* the research team bear responsibility, as the measures were selected by the researchers in consultation with all parties. Secondly, researchers should try to respond flexibly to problems that only arise at the point of data collection. Where a measure is no longer reliably representing the skill it is intended to represent, a different measure should be employed where logistically possible. If the observed issues that emerge are too difficult to overcome due to, for example, time or resource constraints, interpretations of the data should, at the very least, be substantially moderated. Fortunately, in the case of the MiniLit evaluation, the YARC-ER was also administered as part of the test battery, and the results derived from this were not associated with similar methodological issues as the YARC-PR. Hence, these may be viewed as reliable.

The dosage

Before discussing dosage (and fidelity) of instruction in a research setting, it is perhaps important to first make a distinction between *effectiveness* and *efficacy*. According to Singal et al. (2014):

> *Efficacy can be defined as the performance of an intervention under ideal and controlled circumstances, whereas effectiveness refers to its performance under*

> *'real-world' conditions. However, the distinction between the two types of trial is a continuum rather than a dichotomy, as it is likely impossible to perform a pure efficacy study or pure effectiveness study.* (p. 1)

That efficacy and effectiveness exist along a continuum is important to note, as factors influencing trial outcomes can only be controlled to variable degrees. Nevertheless, so-called efficacy trials are typically conducted under highly controlled conditions, with interventions delivered in a standardised way (Singal et al., 2014). As such, they are implemented in order to answer whether the intervention works under ideal circumstances. Effectiveness trials, in contrast, are implemented to determine whether the intervention has external validity and works under real-world conditions. In trials involving educational interventions, dosage is defined as, 'how much of the intended intervention has been delivered and/or received' (Humphrey et al., 2016, p. 6). Thus, in effectiveness trials especially, dosage may be affected by factors like student attendance, student behaviour, teachers' familiarity with the intervention, and systemic or curricular priorities outside of the subject the intervention is targeting. In practical research terms, there may also be pragmatic and funding exigencies that impact on the dosage of educational intervention provided.

In the context of the MiniLit (MultiLit, 2011) evaluation, only 54.6% of students in the 'MiniLit' group received more than 80% (i.e., four out of five days per week) of the full MiniLit program (Quach et al., 2019). This figure of 80% was recommended a priori by the program developers as the minimum proportion of lessons that students in the 'MiniLit' instructional group should receive. Indeed, at the very outset of the study, the recommendation was 90%, as stated in the published study protocol (Quach et al., 2016), though this was reduced between publication of the protocol and collection of the post-test data. According to the research team, the finding that only half of the students received a sufficient proportion of the program may have been due to either limited staff resources at

schools, or their implementation of the program on fewer than the recommended four days per week (Quach et al., 2019). In addition, the research funding and time demands meant that the RCT needed to be completed in two school terms (each term comprising 10-11 weeks). Combined, these factors likely resulted in many schools not being able to complete all MiniLit lessons in the given timeframe.

The finding that schools struggled to condense the program into two terms is, in itself, an important take-away from the study: in the real world. Beyond this observation however, the results obtained for the full MiniLit group, which includes those students who received between 0% and 100% of the program, cannot be said to represent implementation of MiniLit under ideal conditions, since almost half of the students attended fewer than 80% of lessons. It is unsurprising therefore that the intervention group showed considerably more gains when only those students who received more than 80% of lessons were included in analyses. Indeed, an RCT had already shown that two terms of MiniLit instruction elicited fewer gains than three terms (Buckingham et al., 2012). With respect to dosage then, it may be noted that this most recent MiniLit RCT, was closer to an effectiveness trial than an efficacy trial, although this was not stated by the research team (Quach et al., 2019).

In summary, the dosage of an educational intervention is a key consideration when drawing or reporting interpretations from the findings of a research trial. In the planning stages of a trial, our own experiences have led us to consider how the time constraints that limit instructional dosage might best be managed. Where efficacy of an intervention is being investigated (and the intervention is therefore to be delivered under ideal conditions), the scheduled duration between pre- and post-test time points should be long enough to allow students to receive adequate instructional dosage.

The fidelity

In an educational research setting, 'fidelity' may be defined as the quality with which the intervention, as conceptualised by the program developers, is actually carried out during the trial (Humphrey et al., 2016). As with dosage, some research trials may be conducted with highly controlled interventional fidelity, in which case they may be said to more closely resemble efficacy trials than effectiveness trials. Where fidelity is more loosely controlled, other factors – associated not with the intervention itself, but with its implementation – may affect students' outcomes (e.g., lack of administrative and organisational support; Stockard, 2020).

The importance of instructional fidelity was demonstrated in results from the MiniLit (MultiLit, 2011) RCT (Quach et al., 2019). Better fidelity (as measured by the frequency with which tutors followed the program scripts and instructions) was associated with better student outcomes in the areas of letter-sound knowledge, phoneme awareness, word reading and nonword reading. At a broad level, fidelity was controlled in that all tutors received two days of workshop training, as well as subsequent phone and email support and up to two follow-up observation sessions. In addition, fidelity may have been compromised by the fact that none of the classroom teachers who implemented MiniLit were, previous to the study, implementing a standardised literacy program. Indeed, they reported emphasising a 'whole-word reading' or 'reading for meaning' approach to literacy instruction (Quach et al., p. 67). Given that this does not align with the pedagogical approach of MiniLit, this made for a non-optimal intervention environment (Quach et al.). Specifically, such an environment may have influenced teachers' attitudes towards – and familiarity with – the systematic synthetic phonics approach employed in MiniLit, which may in turn have influenced the fidelity with which they delivered the intervention.

Relatedly, there were no published data from the study that pertained to the type or fidelity of literacy instruction in the *control* group. According to the researchers, the form of learning support provided to the control group students was restricted only insofar as it could not be MiniLit (Quach et al., p. 28). We would therefore posit that, had it been logistically possible, it may have been useful to more closely examine what was offered to students in the control group, given that this was the gauge against which MiniLit students' progress was measured.

More broadly, for any research trial in which an intervention program is being implemented by those outside of the program development team, fidelity is likely to vary. Measuring its impact on student outcomes is important, and such deep analysis may also provide insight into what barriers exist to teachers implementing the program.

The analysis

After the educational intervention has been implemented, its effect on student outcomes can then be quantified through statistical analyses of the data. For confirmatory quantitative research, the hypotheses to be tested should be decided upon ahead of data collection. Such a protocol limits the influence of 'hindsight bias', whereby an observer generates an explanation based on already-known data, while simultaneously believing they would have anticipated that explanation in advance (Nosek et al., 2018). To this end, 'preregistration' of studies is becoming increasingly popular as a method of deciding on, and transparently communicating, research plans in advance of observing outcomes (Nosek et al.).

Before any data for the MiniLit (MultiLit, 2011) RCT were collected, the research team published an evaluation protocol, in which the plan for the trial – including what analyses would be conducted – was described (Quach et al., 2016). Like a preregistration report, this document provided the research team with a way of explicitly outlining their aims

and intended methods to address those aims. Such noble ends do not, however, need to be achieved at the expense of remaining flexible in response to unexpected pitfalls. As described at the beginning of this paper, the YARC-PR proved inappropriate as a test measure to administer with young, struggling readers. Nevertheless, in what we would consider a methodological misstep, the results from this measure were included as the 'primary outcome variable' throughout analyses. As has been recommended for preregistered studies in which there are changes to the procedure (Nosek et al., 2018), an alternative course of action would have been to document the rationale for removing or replacing the YARC-PR as the primary measure, at the point of pre-intervention data collection when the administration difficulties became apparent. Given how early in the trial the issue emerged, most of the study design would have been preserved and subsequent group comparisons would not have been undermined by potential hindsight bias.

In addition to the analyses themselves, there were also lessons to be learned with respect to how the results from those analyses were reported. For background, there were four main analyses conducted by Quach et al. (2019) to test for significant differences in skill gains between the MiniLit and control groups:

1. Outcomes for full groups (p. 89);
2. Outcomes for full groups, adjusting for a priori baseline confounders (i.e., student age, sex, family socio-economic status [SES], rapid automatised naming [RAN] score and phonological memory score) (p. 90);
3. Outcomes for students who attended >80% MiniLit lessons (and matched control group) (p. 91);
4. Outcomes for students who attended >80% MiniLit lessons (and matched control group), adjusting for a priori baseline confounders (i.e., student age, sex, family SES, RAN score and phonological memory score) (p. 92).

The results afforded most emphasis in the evaluation report were those from the first analysis listed, despite there being a borderline-significant (p = .05) difference between groups at baseline on the a priori measure of phonological memory. This is arguably an ill-considered choice, since the a priori confounders (which were accounted for in the second and fourth analyses) were selected at baseline on the basis that they were 'predicted to explain variance in the main outcome measure' (Quach et al., 2019, p. 30). Indeed, on some measures (e.g., phonological awareness, nonword reading), the statistical significance of group comparison analyses did depend on whether the statistical models were adjusted for the confounders.

Furthermore, the results pertaining to students in the MiniLit group who attended more than 80% of lessons arguably received less attention in the report. This decision is understandable because it aligns with the intention-to-treat approach employed from the outset of the study (Quach et al., 2016). Yet, it also appears to reflect the researchers' adherence to protocol with little allowance for deviations to the original plan. Due to time constraints, almost half of the students in the MiniLit group received 80% or less of the entire program, which represents a substantial reduction in the 'ideal conditions' required for program delivery in efficacy trials. Accordingly, what was reported in the main analyses may be considered to reflect results from something closer to an effectiveness trial than an efficacy trial.

Detailing the plan for a research trial ahead of knowing the actual outcomes does not eliminate the potential for bias or unethical research practice, but it does provide a high degree of scientific transparency. In this regard, the preparation of such a document aligns with good research practice. However, a protocol should not rigidly dictate the progression of the project without allowing for unexpected deviations (e.g., a measure proving inadequate, or time constraints reducing dosage of instruction) – particularly when those deviations

emerge when the study outcomes have not yet been observed. Of course, there are logistical limitations (e.g., funding, ethical approval, timing) that limit the flexibility with which researchers can respond to unexpected hurdles. Nevertheless, and as evidenced by a review of the analyses conducted in the MiniLit evaluation, contingency plans in any project are essential, as are frank acknowledgements of the factors that affect how readers should interpret results.

Conclusion

It is with the power (and perhaps bias) of hindsight that we have, in this paper, described some potential missteps associated with conducting educational research trials. The intention here is not to set an unreasonably high standard for independent and rigorous evaluations of classroom-based interventions. On the contrary, our experience of being a stakeholder in the MiniLit evaluation was beneficial, as it allowed us to learn some important lessons about selecting appropriate measures and statistical analyses during the planning stages of the trial. Additionally, it has shown the importance of remaining flexible in response to unexpected difficulties. If challenges arise that impact assessment validity or instructional fidelity, those challenges should be acknowledged, accounted for when making interpretations, and mitigated wherever possible. Although time-consuming, challenging and costly, trials such as the one referred to throughout this paper are critical in ensuring our students receive instruction that is of educational value.

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Conflicts of interest

The authors hereby declare a financial interest in the outcomes of this study. All authors work for MultiLit Pty Ltd, which publishes the MiniLit program.

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Appendix A

The following description of the MiniLit Program is edited and abstracted from the details provided on the website of MultiLit Pty Ltd at http://www.multilit.com/programs/minilit-program/.

MiniLit targets students in the bottom 25% in reading skills and is specifically aimed at struggling Year 1 readers, but may also be appropriate for 'at risk' kindergarten, and some struggling Year 2 students. It is a Tier 2 school-based, small group program (up to four students per group) within a response-to-intervention framework.

MiniLit is an integrated and balanced program of 80 carefully structured lessons, divided into two levels of 40 lessons each:

- Level 1: Teaching the basics of letter/sound knowledge and decoding skills for CVC words.
- Level 2: Extending word attack knowledge by teaching commonly used digraphs and longer words.

The program takes around 20 weeks to complete, with four lessons (each up to 60 min) per week, and includes regular curriculum-based measures to monitor the progress of the students. The entry point into the program is flexible and, based on students' assessment scores, can be anywhere within the 80 lessons. Each lesson comprises three main components:

- Sounds and Words Activities;
- Text Reading; and
- Story Book Reading.

Well-trained teachers or paraprofessionals with teacher support can deliver the program.