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Closing a Virtuous Circle: Reciprocal Influences Between Theory and Practice in Studies of Reading Intervention

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Abstract: The authors reflect on findings from three studies of different approaches to reading intervention (Al Otaiba et al., Denton et al., and Miller et al., all found in this issue). It is argued that the science of interventions for reading disorders is advanced and that these and other related studies provide a strong evidence base for guiding educational policy in this area. Reading interventions need to be based on theories of reading development and reading difficulty. Current causal models of reading development arguably have focused almost exclusively on the cognitive processes underlying reading development and how best to remediate deficiencies in such processes. Such models are typically silent on broader influences (motivational, attentional, and socio-cultural) on learning, however. It is concluded that future theories will need to be broadened in order to develop more effective interventions for children with a variety of reading and language learning difficulties.

Keywords: Reading intervention, causal models of reading, response to intervention

The science of reading is mature. Causal models of learning to read have been used to develop theoretically motivated interventions for children with reading difficulties (Fletcher, Lyon, Fuchs, & Barnes, 2007; Snowling & Hulme, 2011). In addition, the use of randomized controlled trials have provided rigorous evidence for “what works” for various groups of children with reading difficulties. This body of evidence arguably represents the most sophisticated evidence in the field of education.

The articles by Denton and Al Otaiba and their colleagues provide examples of robust trials and underline an important message for the field—interventions that are highly structured and directed toward componential reading skills can help at-risk readers, especially with the development of reading accuracy and decoding skills. However, the impact of such interventions on reading fluency and comprehension may be less positive. There is also an emerging consensus that “response to intervention” (RTI), an approach whereby students who do not respond to the first “tier” (or “wave”) of intervention are given increasingly intensive support (Tiers 2 and then 3), has not lived up to its early promise in that many poor readers are left with significant impairments after successive phases of assessment, monitoring, and intervention. Indeed there are an increasing number of mixed findings (with some component skills of reading improving and others not) and diminishing returns from Tier 3 interventions.

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These decreasing effect sizes reported for interventions may well relate to improvements in trial design and execution already identified as influences on trial outcomes in medicine. For example, Schulz, Chalmers, Hayes, and Altman (1995) used data from 33 meta-analyses relating to trials on pregnancy and childbirth to show that effect sizes were smaller from trials in which treatment allocation was adequately concealed, patients were not excluded from trials after randomization, and trials were double blind. Similar findings were reported from the meta-analysis reported by Savović et al. (2012), who also noted that effect sizes were smaller for objective than for subjective outcomes. The robust education trials reported here set the bar high by adhering strictly to the strategy of random allocation; moreover, although double-blinding is not practicable, analysis of effects is on objective measures of literacy (timed as well as untimed) and attrition and drop-out rates are examined. Thus, these rather small effects from highly rigorous intervention studies in reading may align with other findings from rigorous studies in other areas.

Denton et al. (this issue) present a good example of a controlled trial comparing two forms of reading intervention, Explicit Instruction and Guided Reading, against a baseline control group that received "typical classroom instruction." Whereas the majority of reading interventions focus on explicit sequential teaching to foster the subskills involved in reading development, the Guided Reading approach (Fountas & Pinnell, 1996) is often favored by teachers as an approach for improving reading fluency and comprehension, although evidence is lacking. This intervention approach involves extended reading practice, usually silently, with a focus on meaning during small-group lessons. Of importance, teachers use the findings from a running record of children's reading (Clay, 1985) to provide students with texts at the appropriate level of difficulty and to encourage them to use a variety of reading strategies as they progress through increasingly difficult texts (see also Hatcher, Hulme, & Ellis, 1994, for a similar approach).

In the Denton et al. study, trained and experienced teachers were hired by the research team to provide the two interventions and treatment fidelity was monitored rigorously. In the explicit instruction condition, children received between 35 and 50 min of instruction daily, and for the guided reading condition, four 45-min sessions per week in addition to normal classroom instruction. In addition, some schools provided supplementary reading instruction over and above that provided by the study (more than 100 hr in some cases), but there were no significant group differences in the amount of support received. Pretests and outcomes were measured objectively using tests of reading accuracy, fluency, and comprehension, and levels of attrition indicated that there were no significant differences on key variables between the participants who left the study and those remaining. The investigators also assessed whether there were differences in reading growth between the children in the intervention groups and those receiving typical classroom instruction.

The general pattern was for the explicit instruction group to gain more between pre- and posttest than the guided reading group, which in turn improved more than the typical classroom instruction group. Both intervention groups performed significantly better than controls in untimed reading, the effects being small except for the moderate effect on word attack for the explicit instruction group; in addition, growth in this measure was increased by some 54% compared to expectation for typical classroom instruction. More disappointing, the effects of intervention on reading fluency were small and nonsignificant, and for comprehension, effects were mixed with performance on one measure but not the other measure improving significantly. Finally, contrary to expectation, growth in reading comprehension was four times higher in the explicit instruction than the guided reading group.

In summary, the hypotheses, grounded in teacher practice, that the group receiving Guided Reading would outperform the explicit instruction group in reading comprehension and reading fluency yet gain less in basic word decoding skills were not upheld. Rather, both intervention groups gained higher scores for basic reading skills compared with typical reading instruction, but there was little difference between the two types of intervention. Notwithstanding this, the explicit instruction group showed the better performance in terms of both word attack and comprehension, suggesting that such instruction might yield greater all-around benefits.

In the study of Al Otaiba et al. (this issue), the focus was on the outcomes at the end of second and third grade of 419 students who participated in a controlled implementation of Response to Intervention in first grade. There were three comparison groups: students not at risk who required no more than the first Tier 1 intervention, easy-to-remediate students who responded to Tier 2 or Tier 3 instruction, and students who required sustained intervention throughout the school year. Teachers provided the Tier 1 interventions and research staff the Tiers 2 and 3 (embedded within the study was a comparison of dynamic vs. typical RTI, the details of which need not concern us here). The content of the programs reflected best practice, and there was a comprehensive set of activities. It is also worth noting the intensity of the programs; Tier 2 students received in total 1 hr of intervention per week in a group of four to seven, and the Tier 3 group received a total of 3 hr per week in groups of between one and three students.

To examine outcomes at the end of second and third grade, the authors analyzed standard scores where possible and controlled for demographic variables such as gender, free or reduced-cost lunch status, and racial background as well as literacy at the beginning of Grade 1 (the autoregressor in each case). It is worth reflecting on what outcomes might be hoped for. A positive message for education would be that the effects of intervention were maintained after Grade 1, yielding no significant difference between the typically developing (or no risk) and easy-to-remediate groups with both being ahead of the group with the most severe reading problems (the sustained remediation group). An even better outcome would be that children continue to make gains once the intervention ceases, in which case the group differences would be smaller (though it is always risky to predict null effects). In fact, at the end of second grade, the easy-to-remediate and sustained remediation groups scored less well than the no risk children in word reading efficiency, oral reading fluency, and letter-word identification, suggestive of some fall back on the part of the easy-to-remediate children (or that the not at risk group had accelerated their development). More positively, by the end of third grade, the easy-to-remediate group scored higher than the no risk group on all outcome measures except Passage reading; however, there was attrition from the already small easy-to-remediate group, which arguably makes this finding somewhat hard to interpret. Finally, the sustained remediation group showed less progress than the no risk group at the end of Grade 3 underlining their persistent difficulties.

In the second part of this article, the authors report the proportions of students with standard scores below 90 in each group. Broadly the results patterned with the way the groups were defined—for those not at risk fewer had scores below 90 than for those who were easy to remediate and those who needed sustained remediation.

A number of points can be made. First, relatively few children in any of these groups fell below the researchers' threshold for poor reading—mostly fewer than the 25% to be predicted based on the normal distribution except for measures of word reading efficiency, fluency, and comprehension for the sustained intervention group. Second, a point echoing the findings of Denton et al. (this issue), it would seem optimistic to expect that RTI can improve reading substantially for children who are by definition “hard to remediate” given

the modest levels of input which can be provided in school settings (here between 2 and 3 hours per week similar to that in the Denton study).

But where should the field of reading look for explanations when findings of scientifically rigorous trials are disappointing? There seem to us to be a number of possibilities. First, the intensity of interventions may simply be insufficient. Providing between 35 and 50 min per day of small-group support to children who may be multiply disadvantaged is arguably a “drop in the ocean” compared to the 30 min or so a day of one-to-one input that might be observed in the homes of middle-class children of educated parents. More speculatively, increasing intensity of interventions in school may have impact only if they are coupled with increasing time spent reading outside the classroom, including in the home. We believe that one important next step for intervention studies is to take seriously issues to do with pupil motivation. For interventions to be truly effective, it is likely that they will need to increase students’ enjoyment of reading. If this can be achieved, steps then also need to be taken to provide pupils with reading resources that they will use in their own time outside the classroom to practice reading. This may involve web-based, app, and other resources that are suited to different students’ levels of literacy skills and different interests. In short, causal models of reading development that ignore environmental or cultural factors including the home literacy environment may be insufficient for developing optimal interventions. It is important that the sometimes disappointing outcomes from reading interventions studies are used to inform theory in order to form a virtuous circle between theory and practice.

In addition, current causal models of reading development may fail to specify all the within-child factors that are important for reading success. The article by Miller et al. (this issue) suggests that one such factor is attention. Starting from the premise that poor attention is a major cause of underachievement, this study investigated the predictive role of teacher ratings of children’s attention on growth in sight word reading and reading comprehension (listening comprehension was also investigated but is not discussed here). The participants were children who had received instruction in decoding and reading fluency following a well-structured and systematic program—one arm of a randomized trial—and the analysis examined the importance of attention as a predictor of RTI, in relation to other cognitive variables.

The authors report a path model in which there are three first-grade predictors of growth in sight word reading during Grade 1: sound matching, attention as measured by a teacher questionnaire, and vocabulary.¹ A range of other possible predictors including sight word reading at the outset, phonemic decoding (which was at floor), hyperactivity, backward recall, matrix reasoning, and listening comprehension were not significant predictors of the growth of sight word reading at Grade 1. Sight word reading in Grade 3 was, in turn, predicted by growth in sight word reading in Grade 1, and there was an additional direct path from sight word efficiency in Grade 1. In summary, the impact of attention on Grade 3 sight word reading was mediated by growth in sight word reading in Grade 1. Its contribution to reading comprehension in Grade 3 was also indirect, mediated by sight word reading in Grade 3.

A possible limitation of this study was that the measure of attention was based solely on teacher ratings; these were strongly correlated with first-grade cognitive measures as well as with reading itself. It is plausible that teacher ratings of attention were influenced by their observation of children’s other skills and notably their reading, rendering the causal relations among attention and other cognitive skills equivocal. Nevertheless, an

¹Somewhat puzzlingly, the standardized path weight for this predictor on sight word reading was negative (–.24).

increasing number of studies now point to the importance of executive attention (Kegel & Bus, 2013) and self-regulation (McClelland et al., 2007) as possible causal influences on the development of reading. One implication is that interventions for reading problems should be designed to incorporate strategies for improving attention, or at least to minimize the negative effects of inattention, especially when children are taught in small groups.

Another factor missing from causal models is the mismatch between the language of children and their teachers. It was Bernstein (1971) and Labov (1972) who perhaps first alerted us to the differences between the language code of the home and that of the school, and how this may affect educational achievement. More recently, Seidenberg (2013) reignited the debate by considering the mismatch in dialect between children from minority groups and their teachers; in particular, Seidenberg argued that differences in dialect could affect the ability to learn to read, a process that requires mappings between speech sounds (that vary between dialects) and spelling patterns that are fixed but not consistent. Together with other aspects of language variation, such differences could, Seidenberg argued, account for an increasing achievement gap between children from different social and cultural groups. It is notable that in the two intervention trials reported in this issue, levels of social disadvantage were high: More than 50% of the participants in the AI Otaiba study were African Americans (likely speaking nonstandard dialect), and some 68% were eligible for free lunch.

More generally there is abundant evidence that learning to read builds on a foundation in spoken language (Hulme & Snowling, 2013); indeed the status of a child's language system at the point of beginning reading instruction predicts their eventual success (Bishop & Snowling, 2004; Justice, Bowles, Pence, Khara, & Skibbe, 2009). This latter finding suggests the possibility of a critical age or sensitive phase for reading acquisition—thus, if reading instruction is started too early, this could have detrimental effects. This important issue has been the subject of very little scientific research, though there is much dogma to be had. Computational models of reading have suggested that there is an optimal stage of readiness for creating mappings between letter or symbols and their sounds (e.g., Harm & Seidenberg, 1999). Arguably there is a need to take this evidence seriously and to ensure that as many children as possible begin formal reading instruction with a solid foundation in oral language. Without this grounding, the development of an efficient orthographic system is likely to be compromised in the longer term.

Relatedly, genes via their effects on the developing brain may place biological constraints on the development of reading (and language). The findings of intervention studies, and particularly RTI studies, emphasize that it is important to think of reading disorder as a neurodevelopmental disorder with early onset, which is persistent through development and frequently comorbid with other disorders, such as language impairment or deficits in the executive control of attention. Indeed, within the framework of multiple-risk models of dyslexia (Pennington, 2006; Snowling & Hulme, 2012), reading disorder is the outcome of multiple risk factors that accumulate toward a threshold for “diagnosis.” For many of children with significant difficulties in reading and language, the challenge of developing reading fluency may be a bridge too. We may have to accept that, for some children, reading accuracy and not necessarily fluency needs to be the major goal of reading intervention.

From a more prosaic stance, it is unclear whether small effects of interventions on objective measures are really as disappointing as they seem to be at first pass. If outcomes are measured in terms of age-standardized scores, then one needs to ask whether it is realistic to accelerate development above the normal rate—an increase that is needed to demonstrate that an intervention is effective. Finally, we might speculate that “typical” or “routine” classroom practice has genuinely improved as the result of reading research,

making it more difficult to demonstrate positive impact over and above “business as usual” (Duff, Hulme, Grainger, Hardwick, & Snowling, in press).

SUMMARY AND ISSUES FOR FUTURE RESEARCH

The articles in this issue demonstrate the theoretical and practical importance of reading intervention research, and attest to the methodological sophistication of the field. We would like to argue for an even closer link between theory and intervention research however. We need well-developed models of reading development in order to plan effective interventions for children with reading difficulties. Arguably, we have relatively well-developed causal models that specify the cognitive mechanisms that are critical for the development of reading accuracy/decoding (e.g., Hulme & Snowling, 2013). As we have pointed out, however, such models are typically silent on issues to do with motivational effects on learning, the role of attention, and on the role of a range of sociocultural influences on learning. It is to be hoped that such models can be expanded to include a broader range on influences, which may in turn help to guide more effective approaches to intervention. In contrast, we have much less well-developed models of reading fluency. It is clear that reading fluency depends to some degree on mechanisms that overlap with those responsible for the development of reading accuracy. But it is also clear that fluency depends on additional factors as well (possibly including information-processing speed; Kail, 1993) and that for children with decoding difficulties it may be very hard to remediate their reading fluency problems even when reading accuracy has improved substantially.

From an applied perspective, we believe it is critical to get funding agencies to fund truly long-term intervention studies. We now know that reading problems in many children tend to be persistent, and we really need interventions that are delivered over periods of years (say, from 6 to 9 years of age) with appropriate long-term follow-up to evaluate the effectiveness of such sustained interventions. We also believe that intervention research needs ultimately to be extended beyond the classroom. Parents, and peers, may have potentially valuable contributions to make to invention programmes that aim to improve children’s literacy skills. Finally, we now understand the critical importance of broader oral language skills for the development of both decoding and reading comprehension skills, and we believe an important issue is to explore further the role of oral language interventions as methods of potentially improving reading comprehension skills (Fricke, Bowyer-Crane, Haley, Hulme, & Snowling, 2013).

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