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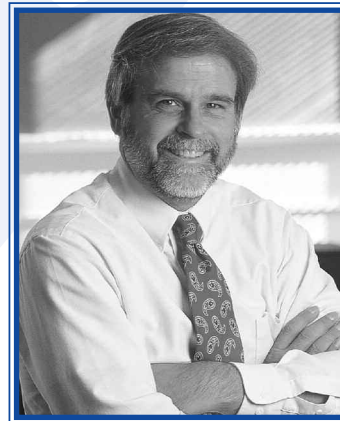
and avoiding Poison Oak

By H. Lee Swanson

In the New Times for DLD newsletter, December 2009, Linda Siegel presented a counter argument in response to my article in the DLD newsletter September 2009. In my article, I discussed evidence based on outcomes from various meta-analyses of the research literature on the relevance of IQ to the field of LD. Prior to my response to her observations, there are a number of issues Linda and I agree upon. First, the link between IQ and teaching is obscure (to some it may be nonexistent). I agree with Linda in that I do not think IQ scores are particularly helpful as they relate to directly informing teachers about what to teach. As I stated, “I would not argue that IQ scores tell you what to teach (perhaps low scores on vocabulary would be suggestive), no more than composite scores of reading and/or math tell you what to teach”. The essence of my argument was that when experimental intervention studies are used as the unit of analysis, studies with aggregated IQ and reading scores that are both in low range (< 25th percentile or a standard score of 90) yield higher effect sizes related to intervention outcomes than studies with reading scores in the low range (< 25th percentile) but with high IQ scores (e.g., > 100). I prefer to argue across studies rather than relying on a selective study.

Second, correlational data is not the same as experimental data. I could not agree more with Linda. IQ correlates with a host of variables, including reading, homework, etc. The essence of my argument is that when the magnitude of the correlations between reading and IQ, reading and naming speed, reading and phonological awareness, reading and orthographic processing, etc, are considered across studies (and corrections are made related to range restrictions), the magnitude of the coefficients are not statistically different from one another (Swanson, Trainin, Nechoechea, & Hammill, 2003). Thus, I would suggest some processes have been overstated in discussions related to predictions of reading. (Incidentally the term “prediction” is used in correlational studies, even when the time differential is not there. The fact that I discuss post-test outcomes related to pretest sample characteristics across studies would qualify as predictive based on

What about IQ again



H. Lee Swanson

her definition). Before I move to some other observations, I would like to suggest that no causal data has been published linking discrepancy and nondiscrepancy groups to treatment outcomes. There are no studies to my knowledge that have randomly assigned nondiscrepancy groups (e.g., IQ and reading scores below a standard score of 90) and discrepancy groups (e.g., IQ > 100 and reading score < 90) to an experimental condition and evaluated treatment and control condition outcomes. Although this comparison would not be a true experimental design (it would be at best quasi-experimental design), the majority of studies cited as causal evidence rely on a post-hoc sorting of high-risk groups of poor readers as a function of treatment outcomes. These are not hypothesis driven or predictive studies.

No doubt, there is a conundrum related to IQ and learning disabilities when one focuses on relevance. By definition, IQ scores are *irrelevant* to LD. That is, poor reading performance in these children after intense instruction is “unexpected” given that an IQ score is in the average range. IQ scores predict reading in average reading samples, but are weakly correlated in LD samples. This discordance (to use the term as used in Ferrer, Shaywitz, Holahan, Marchione, & Shaywitz, 2010) or disassociation is perhaps the hallmark of being LD. So indirectly, one could make the argument that to define LD, there needs to be “by definition” a weak (some arbitrary cut-off) link or disconnect between IQ and reading. So the reader may question why I think IQ has relevance.

First, IQ plays a moderating role in the strength and direction of processing differences and treatment outcomes. I would not argue that IQ “mediates” reading performance. Our meta-analyses data suggests, however, there is significant moderating effect

(continued on page 2)

(continued from page 1)

related to verbal IQ when determining the magnitude of differences in effect sizes between discrepancy and nondiscrepancy studies (e.g., Hoskyn & Swanson, 2000; Swanson & Ching-Ju, 2010). Moderator variables specify the conditions (high vs. low phonological skills, high vs. low verbal IQ) under which the relationship between an independent variable (RD vs. non RD) and dependent variable (phonological processing) takes on a different direction or strength (Baron & Kenny, 1986). In contrast, mediators specify the mechanism by which the independent variable influences the dependent variable. It is possible, of course that the two processes may combine so that the moderator variable may be mediated by another variable (e.g., variations in phonological training). But this is beyond any existing data set I'm aware of. Regardless, we have found a moderating effect for verbal IQ in regression models predicting effect sizes when classification information is partialled out in the analysis (Hoskyn & Swanson, 2000; Swanson & Hsieh, 2009).

The second reason for considering IQ in the study of LD is that we need to understand how such children get average IQs in the first place. Very little research has been directed to explain how poor readers attain average intelligence. Throwing IQ out of equation will not advanced our ability to find answers to this research question. Assuming a study controls for SES, educational program and a host of demographic variables, why is it that these children attain normal intelligence? Why is there very little disruption in their overall cognitive ability given their dismal reading performance over time?

Finally, I'm not sure RTI (Response to intervention) as a means to identify LD (those children who are non responders) will be enhanced by dropping IQ out of the equation. RTI has been embraced by some as a valid means to identify children at risk for LD. I agree with this position. I might indicate in our recent attempts to perform a meta-analysis on published RTI studies, however, less than 15 of these studies report pretest data on nonresponders and only one study provided IQ scores (Loan, Sanchez, Arrellano, & Swanson, manuscript under review). This is not a strong argument for IQ irrelevance. I might also indicate that the designation of a "nonresponder" as a potential indicator for determining at risk for LD may not be a good indicator of LD. The typical effect size for children with LD in the experimental treatment condition when compared to the control condition is about .79 (Swanson & Hoskyn, 1998, p. 288). Further, intervention studies inflate the magnitude of the outcomes (effect sizes) when studies leave out both IQ and reading psychometric scores in defining the sample (see Simmerman & Swanson, 2001; Swanson & Hoskyn, 1998). So at the very least, the reporting of IQ and reading together have a calming or stabilizing effect when interpreting treatment outcomes.

Although Linda suggests that a focus should be placed on how children respond to intervention (RTI), I think a discussion of some caveats in this research is in order. Although we are making progress on several fronts, I will briefly outline my concerns (See Swanson, 2009, for further discussion of these points). At the

present time, RTI as an assessment approach to define LD has a weak experimental base. There have been no controlled studies randomly assigning children seriously at risk for LD to assessment and/or delivery models [(e.g., tiered instruction vs. special education (resource room placement)] that have measured outcomes on key variables (e.g., over identification, stability of classification, academic and cognitive growth in response to treatment). The few studies that compare RTI with other assessment models (e.g., discrepancy based or low achievement based models) involve post hoc assessments of children divided at post-test within the same sample. In addition, different states and school districts have variations in their interpretations on how RTI should be implemented, thereby weakening any uniformity linking the science of instruction to assessing children at risk for LD. Thus, although there is enthusiasm for RTI as a means to provide a contextual (or more ecologically valid) assessment of children at risk for LD when compared to other models (e.g., models based on inferences from behavioral data about internal processing), the use of RTI as a scientific means to identify children at risk for LD has several obstacles to overcome. The first obstacle is that in contrast to standardized formats of testing and assessment, there are no standardized applications of evidence-based instruction. A second obstacle is that teacher effects cannot always be controlled. The teacher variable plays a key role in mediating treatment outcomes for children. Further, this variance cannot be accounted for by merely increasing treatment fidelity. Procedures that control for treatment fidelity in applying evidence based treatments account for a very small amount of variance in student outcomes (see Simmerman & Swanson, 2001, for discussion). Although the role of teacher effects can be controlled to some degree, there is no "expert teaching model" that has been operationalized and implemented for instructional delivery in evidence-based practices. Another obstacle is that even under the best instructional conditions, individual differences in achievement in some cases will increase. In fact, we find that evidence based intervention procedures at best account for only 21% of variance when predicting the effect size intervention outcomes between treatment and control conditions for children with LD (Swanson, 1999). There will be some instructional conditions that vastly improve achievement in both average achievers and children at risk for LD, but these robust instructional procedures will in some situations increase the performance gap between children. Thus, significant performance differences will remain for some children at risk for LD when compared to their average achieving counterparts even under the most intensive treatment conditions. Perhaps even more fundamental than these major obstacles, is the lack of consensus about what "responsiveness" entails and how it should be uniformly measured.

So in summary, I agree with Linda that jumping into the Poison Ivy patch is not a good thing. However, like most complex issues, throwing out some variables because they don't provide information directly related to teaching or group separation may create a Poison Oak Patch for the science of LD in the future.

(continued on page 3)

(continued from page 2)

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