IS IT JUST READING? COMMENTS ON THE PAPERS BY MANN, MORRISON, AND WOLFORD AND FOWLER

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My comments on the stimulating papers by Mann (in press), Morrison (in press), and Wolford and Fowler (in press) come under four headings. First, I identify their differences with respect to the organizing theme. Second, I discuss the central difficulty, for theories of reading disability, posed by the high correlation between reading and IQ, and ways of dealing with this difficulty. In the third and fourth sections, I comment on the individual papers and summarize what I think are the main lessons to be learned from this collection.

How the Papers Differ

One crucial question posed in these papers is whether the disability shown by poor readers is more general or less general than the process of reading itself. If one thinks the problem with disabled readers lies with letter perception, then one has implied the problem is less general than reading; if one thinks the problem is in low IQ, then one has implied it is more general. Of the three participants, Mann (in press) has identified herself and her colleagues at Haskins Laboratories with the "less general" point of view. Their position is that it is a subcomponent of reading that holds back the typical poor reader--his or her inability to achieve and maintain a phonetic code for short-term memory. This is not to say that the defective phonetic coding does not compromise other processes than reading; in research that I shall mention again below, Brady, Shankweiler, and Mann (in press) have shown that phonetic perception in the auditory mode is also differentially impaired in poor readers.

Morrison (in press) and Wolford and Fowler (in press) think the typical problem with reading disability is more general than the reading skill itself. The former attributes the problem to difficulty in the learning of irregular rule systems, of which the especially relevant example is the set of correspondences between graphemes and sounds in English. Wolford and Fowler (in press) attribute the problem to difficulty in generating a response on the basis of partial information. These two mechanisms are quite obviously more abstract than a specific, phonetic-coding deficit.

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+Also Yale University.
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A second dimension of variation among these three papers is whether they assign the reading deficit to a process that is specifically linguistic or not. There is no reason that our three authors should have assorted themselves in the same way on these issues as on the first but they do: Mann's endorsement of phonetic coding as the major problem puts her quite clearly in the linguistic-deficit camp, whereas Morrison and Wolford and Fowler have chosen more abstract cognitive deficits.

A third dimension of variation is on the matter of who exactly constitutes the impaired-reading population we are concerned about. Mainly, the question is whether or not to consider IQ differences as an inherent part of reading disability. I would have expected more discussion of this very important point than I found in these papers. Wolford and Fowler alone come out and face the issue head-on, in a refreshing survey of IQ differences between groups of good and poor readers "matched" on IQ. Even with deliberate matching to remove this confounding, the vast majority of studies do show an IQ advantage for the good readers; Wolford and Fowler conclude that the association is an inescapable one. In the opening paragraphs of his contribution, Morrison assumes the opposite position. So does Mann, by virtue of the effort she and her colleagues have made to exclude IQ differences from good/poor reader comparisons. This issue sets the stage for the next section of my own paper:

What to Do About IQ Differences

As an impressionable teenager, I learned from the instructor in my undergraduate tests-and-measurements course a powerful law of psychology: "All good things go together." The correlation between reading performance and IQ ranges from around .50 to .60, in unselected lower school populations, to over .60 in high school (Sternberg, Note 1). In view of this correlation between reading and IQ, nothing could be less interesting than to select children on the basis of high and low reading ability alone and to show them different on one's favorite information-processing measure. At the very least, the skills used in reading are only a tiny subset of the skills that contribute to IQ scores. Since all the skills will tend to go together in unselected populations, it should not be surprising that one predicts the other.

If one takes seriously the definition of reading that distinguishes it from language comprehension over the oral-auditory channel (auding), then reading skills are not only a tiny, but also a very specific subset of all the skills that are measured on the major IQ tests. That is, when a reading test measures comprehension, we would not want to say that a low-scoring individual is a "poor reader" unless we know that his or her comprehension in reading is poor in relation to his or her comprehension of the same material in auding. With tests of reading that mix in ability to comprehend language--written or spoken--it is indeed a thorny problem whether the IQ test is fundamentally different from the reading test at all, or just a larger set of cognitive skills. If the proper distinctions among reading, auding, and comprehension are made, however, these tests would not properly be used to identify poor readers.
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The issue that lies behind these commonplace observations is not an easy one: If our definition of reading disability is to exclude intelligence as a factor, then does this mean that children of low IQ are ineligible to have reading disabilities? What are we to do with the fact that tests of IQ in some cases make use of reading skills, and vice versa? What of the fact that the mixture of skills tapped by both reading and IQ tests changes as one goes from age six to sixteen? These matters are the subject of several searching analyses in the opening section of the edited collection by Benton and Pearl (1978). For the moment, we can agree that one's research strategy should differ depending on whether, like Mann and the Haskins group, one considers IQ a potentially troublesome covariate of reading or, like Wolford and Fowler, one considers reading to measure a fundamental component of IQ.

What can be done if one wants to investigate reading disability with IQ held constant? I think there are four solutions, and variants on them, that have been used:

1. One can take pains to match good and poor readers on IQ. This is the most popular control method and the most worthless. For one thing, Wolford and Fowler (in press) have demonstrated that the "matching" doesn't work—22 out of 23 studies they inspected showed that the good readers were smarter, on the average, than the poor readers. The size of the numerical IQ difference between groups is irrelevant, as is the fact that the difference is typically nonsignificant. The nonsignificant difference is to be expected if some group IQ measure with low reliability is used, or if there are few subjects, either or both of which circumstances are often the case. The size of the obtained group difference in IQ is not relevant in view of the potential regression artifacts that exist. This regression artifact is the really telling argument against matching. The problem is of course that tests of IQ are less than perfectly reliable. This means that some of the children scoring high are really high by accident and would score lower on another round of testing; likewise, some of the children scoring low are really closer to normal than their score indicates, and would get a higher score on another round of testing. If, instead of administering the IQ test again, we administer a test of something that is correlated with IQ, such as a reading subskill, we would expect the children who were "accidentally too high" and "accidentally too low" to move back towards the overall population mean. In order to match groups of readers on IQ, it is necessary to take good readers who have low IQ's for their group and poor readers who have high IQ's for their group. (This is because the traits are so highly correlated in the general population.) What matching does is virtually to guarantee that the scores of the good readers will improve on any measure that is correlated with IQ and that the scores of the poor readers will go down for statistical reasons alone. Thus, one can go through life testing good and poor readers on information-processing skills and, as long as these skills are related to IQ, one will always find good readers doing better than poor readers.

2. Another remedy for the IQ-reading correlation is to use a control task of some kind and show that good and poor readers do not differ on it. This method is referred to as convergent-discriminant validation in testing circles. The presumption behind this strategy is that this control task does not tap into the reading skill but that it does correlate with IQ. In such a case, the contribution of IQ could be discounted as responsible for the
differences observed in the two reading groups. Just anything cannot be used as a control task, of course: If, for example, the control task is so easy as to produce a ceiling effect, so difficult as to produce a floor effect, or so unreliable as to be insensitive to anything, then it is no good as a control task.

Brady et al. (in press) have given us a good example of the control-task strategy that avoids these pitfalls. They were interested in the possibility that reading ability is related to the ability to achieve a phonetic code from speech, as well as from print. They found that identification of phonetic segments was equal for good and poor readers when the intelligibility of speech was high; however, when masking noise was added, the poor readers suffered a significant impairment relative to the good readers. The special strength of this experiment was in a control task in which the sounds to be identified were naturalistic, non-linguistic sounds. The addition of noise to these sounds reduced performance to the same level it had for speech sounds; however, the amount of this reduction was the same for good and poor readers. Thus, we may conclude that it is the processing of linguistic segments that discriminates good and poor readers, not just general auditory identification.

The control-task methodology can be useful when wisely applied, but it is no panacea. There remains the danger that the control task chosen, even if it is of comparable difficulty to the ostensibly reading-related task, is not sharing much variance with IQ. In the Brady-Shankweiler-Mann study, for example, the reasons why adding noise to speech damaged speech-perception performance might not be the reasons why adding noise to naturalistic sounds damaged performance on them. By way of an analogy, to include tying of shoelaces as a control task in reading research might be an empty experimental gesture even though there can be not the slightest doubt it is correlated at least with mental age.

3. A third way of dealing with the IQ-reading correlation is to accept the confounding of good and poor reading-group differences with IQ, at face value, but to show that it could not be responsible for the obtained results. Say a particular pattern of data is obtained when subjects are split into groups on the basis of reading ability; perhaps the good readers show phonetic confusions but not the poor readers. The danger is that IQ could somehow be responsible for this pattern. The remedy suggested here is then to split the entire group of subjects by IQ, pooling together the good and poor readers. If IQ is responsible for the reading-group difference, then the same pattern should appear in this second analysis. That is, the high IQ subjects would, in this case, show the evidence for phonetic coding. If that is not the result, however, if the IQ split produces no differences in phonetic coding, then we may be assured that our original observation should not be rubbed off an IQ-regression artifact. Mark, Shankweiler, Liberman, and Fowler (1977) have used just this technique in one of their experiments.

There are cautions that go with this method, of course. If we select for low and high reading performance, we almost guarantee that a subsequent split on the basis of IQ will produce a relatively restricted range (again because of regression). If the resulting partition of subjects on IQ produces a weak but nonsignificant copy of the reading split, there is no protection at all.
4. The best means of dealing with the IQ-reading association is probably statistical control. One simple solution is to use IQ as a covariate in assessing the influence of reading ability. Mann and her colleagues report several uses of this in her (Mann, in press) paper. Fancier techniques are possible: With adequate prior measures of IQ, reading performance, and other predictors, as well as criterion measures on the information-processing task of interest—all including reliabilities—one is in a position to tease out the operating relationships with multiple- and partial-correlation methods. Good examples of this approach are beginning to appear (Jackson & McClelland, 1979). A recent paper by Perfetti, Beck, and Hughes (Note 2) carries this type of analysis still further: These three investigators employed the logic of causal analysis, through time lag correlations, to face the issue of which component skills "enable" (their term) the later reading skill. This kind of approach means testing more subjects, for picking extreme groups allows one to eliminate intermediate cases. But the extra cost of testing more subjects is small compared to the cost of turning in results that cannot be interpreted.

The Mann Paper

In her paper, Mann (in press) continues the careful effort by investigators at Haskins Laboratories to associate reading disability with processes at the phonetic level of the spoken language. As she herself states, and others have increasingly concluded (see Crowder, 1982, Chapter 9), it is dubious that the speech-based process in reading has much to do with lexical access. Rather, the interest is in a phonetic short-term memory system that would hold verbatim information pending higher-level linguistic processing. Readers are thus hypothesized to use speech in "...reading situations where sentence structure is at stake...when their task involves recovering the meaning of written sentences and not simply words alone..." (Mann, in press). My comments on Mann's paper concentrate on this hypothesis from two points of view—whether sentence-level comprehension really does depend on a verbatim short-term memory and how we should interpret the association of this short-term memory with reading disability.

First, however, I want to acknowledge the sensitivity shown by Mann and Haskins workers to the IQ issue, which I just finished discussing. In most of their recent studies, Mann and her colleagues have applied either an appropriate statistical adjustment (covariance analysis) to rule out an IQ interpretation of the advantage shown by good readers, or have shown that an IQ split of the subjects does not produce the pattern of interest (solution number 3, above). It is to be hoped that the work-in-progress done in collaboration with Shankweiler and Smith (Mann, in press) will receive the same thorough treatment.

Is a phonetic (verbatim) short-term memory really necessary for understanding what sentences mean? The rational argument for this hypothesis is compelling: The language has many distributed forms, for example, auxiliaries separated from their main verbs by considerable distances; it seems preposterous that each word could be processed "all the way up" as it is encountered in the stream of print or speech. This consideration is so compelling I still believe it, deep down, despite recent evidence that it may be wrong!
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Many of us have taken it for granted that the short-term memory that serves language comprehension in this way would be phonetic, that is to say, capable of holding the words themselves at the segmental level for later analysis. Levy (1978) reports research that is highly troublesome for this assumption: Her technique was to present an articulatory distractor (counting) along with the visual presentation of three sentences. The measure was subsequent discrimination of these sentences from other sentences with semantic or lexical modifications. The basic finding was that recognition of the sentences was reduced considerably by the simultaneous articulatory distractor, a result that suggested that the distractor task incapacitated the phonetic short-term memory system that is necessary for reading. The problem comes in another study, in which the memory measure was discrimination of true and false paraphrases of the presentation sentences. Here, verbatim information was not worth anything because the words tested were not those originally presented. Of course, retaining the meaning of the sentence remained crucially important. In this paraphrase task, performance with the distractor was no worse than in the control condition, where phonetic processing was left free. Thus, it seems from this result that reading for meaning does not depend on a verbatim short-term memory system, otherwise articulatory distraction would have harmed memory for meaning. Therefore, we might conclude, if a short-term retention system is important in reading, it is not a phonetic short-term retention system.

Hitch and Baddeley (reported in Baddeley, 1979) have reported a similar outcome: They gave subjects sentences expressing simple propositions that were either true or false (BEES HAVE WINGS) and had subjects either carrying a simultaneous digit-memory load or performing a concurrent articulatory-distractor task. The finding was that keeping the articulatory (phonetic) system occupied with the distractor task had no effect on true/false reaction time. However, the digit load did interfere. Again, comprehension seemed not to depend on an intact speech system, as the hypothesis of Mann and of many of the rest of us would predict.

Carpenter and Dahaneman (1981) have offered a different kind of evidence that suggests comprehension does not ordinarily wait long enough for a process of phonetic analysis and short-term storage. In their "garden path" materials, subjects read sentences with words such as BASS in the context of text about fishing. The word immediately following BASS was, however, GUITARIST, which undermines the first interpretation that would have been applied to BASS (that it rhymed with PASS). The measurement of interest was in visual fixation times, word by word. As would be expected, nothing special happened up to and including the word BASS. However, fixation times were reliably longer on the word GUITARIST in the garden path sentences than in appropriate controls. This means that during the time of a normal fixation, typically a quarter second, analysis of that word had gone on to a level that responded to semantic anomaly.

Frazier and Rayner (1982) have shown much the same thing with syntactic anomaly. Their subjects read sentences such as WHILE SHE WAS SEWING THE SLEEVE FELL INTO HER LAP. Here, it is the word FELL that receives the longer-than-normal fixation. The fact that people extend their normal fixation period of around 250 milliseconds, on this word, means they must have detected its anomalous role in the parsing solution that they had been constructing up
to that point. If the syntactic/semantic analysis that supports this had been awaiting the formation of a phonetic string in short-term memory, it would be a slower process. I certainly had not previously dreamed that parsing and analysis of meaning occurred while the person is still looking at the word in question. Certainly, a phonetic short-term memory representation of a word would be hard to set up within the first 250 milliseconds that the subject laid eyes on it. If the trailing phonetic process referred to by Mann and the Haskins group were comparable to the eye-voice span of oral reading, we should have expected the "cognitive alarm" to have sounded only some three or four words after the eyes first rested on the troublesome word FELL.

Thus, we have two discouraging results for the Haskins argument that a phonetically based short-term retention system is a necessary supporting process for reading sentences. First, we find that comprehension of the meaning for sentences is unimpaired by eliminating the phonetic system through articulatory distraction. Second, we find that high-level comprehension processes can occur within the quarter-second or so that the eyes are still fixated on a word, too fast for a trailing phonetic process.

My second reflection on the Mann (in press) paper concerns the direction of effect that connects a deficit in phonetic processing and a deficit in reading. Morais, Cary, Alegria, and Bertelson (1979) demonstrated that learning to read, in illiterate Portuguese adults, has the consequence of dramatically improving performance in a phonetic segmentation task similar to those used with children by the Haskins group. The linguistic maturity that goes with reading thus seems to depend not only on age but on specific training in only the reading skill itself. I think this is different from the conclusion Mann (in press) wishes to reach in the concluding section of her paper, about how linguistic skill may presage reading success. The argument that the former presages the latter comes from the circumstances that the two skills were measured in kindergarten and a year later, in first grade, respectively.

To make a causal argument, however, more is required: The time-lagged correlation technique, for example, measures the predictor and criterion both, at each of two times. The telling outcome is when the predictor at Time 1 correlates better with the criterion at Time 2 than the criterion at Time 1 with the predictor at Time 2. (This would be true if smoking at age 20 correlated with lung cancer at age 50 more highly than cancer at age 20 correlated with smoking at age 50.) Perfetti et al. (Note 2) have begun to take this logic seriously in their investigations. (It is interesting that people shy away from the word "cause" in this field; Mann and her associates talk of "presaging" and Perfetti et al. talk of "enabling.")

The danger is of course that the kindergartners who did well in Mann's segmentation task are those who had already learned to read, and they performed well in segmentation precisely because they had learned to read. The linguistic awareness that allows segmentation would then be a consequence of reading acquisition and not a precondition for it. At a different level, with second-language learning, I can testify that it was only when hit with Latin that I began to gain awareness of grammar in my own language. Thus, it may be a general rule that "linguistic awareness" is a consequence of formal instruction rather than a precondition for it. Would learning to read result
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in children's relying more on a phonetic short-term memory code than before? The objection I am making is less attractive in this instance but the need for something like causal analysis is no less pressing.

In conclusion, I want to be clear that the value of Mann's contribution, and that of her colleagues at Haskins Laboratories, is not weakened by our ignorance of which way the causality goes. It is important that reading seems specifically to track phonetic skills in children, even when IQ is removed. That association has received impressive documentation by the Haskins group. By comparison, there is only a loose set of suggestions that other cognitive factors play a central role. Anyone wishing to advance one of these suggestions seriously faces an enormous task. The remaining two papers in this group have tried to establish just that, and so it is with narrowed eyes that I turn to them.

The Morrison Paper

According to Morrison (in press), the controlling deficit with disadvantaged readers is their difficulty with irregular rule systems, such as grapheme-to-phoneme correspondences in English. One question that needs to be raised, in connection with the Morrison paper, is to what extent the problem lies in one particular irregular rule system--spelling-to-sound correspondences in English--as opposed to a general deficit with all irregular rule systems. If it is "knowledge about words and how they are pronounced" (Morrison, in press) that is to blame, then the question becomes how this hypothesis is any different from that of the Haskins group or from one of the "processing deficit" hypotheses that Morrison wishes to reject. It sounds to me as if the failure to translate letters into their corresponding sounds is none other than a failure to achieve phonetic coding.

It is not clear, either, whether the irregularity of English spelling rules, by itself, even contributes to the difficulty that some American schoolchildren have learning to read: If the irregularity were to blame, then in languages such as Spanish, there should be little or no difficulty; the same would be true of different writing systems, such as Japan's, which do not use the alphabetic principle. However, recent evidence indicates such language communities do indeed see reading disability among their children (Stevenson, Stigler, Luckner, Lee, Hsu, & Kitamura, in press) previous claims to the contrary notwithstanding. (I thank Robert Sternberg for bringing this article to my attention.)

If, on the other hand, Morrison wants to suggest that disabled readers are poor at mastering any irregular rule system, then another two questions emerge:

The first of these is whether it is only because irregular rule systems are more difficult than regular rule systems that poor readers seem to have particular trouble with them. The sad fact is that easy tasks seldom produce large differences between normals and disabled populations, whereas difficult tasks do. This is true whether one is looking at normal and disabled readers, normal and amnesic adults, or at young and elderly populations. I have spelled out this problem in some detail in Crowder (1980) for the case of aging and memory capacity. What it means is that we should be particularly
suspicious when group differences emerge only, or especially, in the most difficult of the tasks or conditions under study. Morrison (in press) acknowledges that there may be ceiling effects in the data of his Figures 1, 2, and 3, in which the difficulty of individual letter-sound rules is shown separately for normal and disabled readers. That admirable candor still does not alert us to how insidious the problem is. For example, among the individual conditions shown in those figures, I calculated the Pearson correlation between the difference between normal and disabled readers and the overall performance level of the normal readers. This correlation was -.60. Furthermore, a look at the figures shows that even within letter classes, this correlation was substantial.

The second question raised by the assertion that there is a general problem with irregular rule systems, in disabled readers, is what confirming evidence there is from outside the realm of reading. In the absence of hard evidence that disabled readers are systematically poor with irregular rule systems of any kind, the Morrison (in press) hypothesis would have to be taken on faith. The fact is, there are several pieces of evidence that rule regularity is not a relevant dimension to reading disability: (1) Mann (in press), in her Figure 1, has shown that the failure of poor readers to use a phonetic code in short term memory extends to spoken sequences, as well as written. It cannot be claimed that spelling-to-sound rules are to blame when there is nothing written in the experimental procedure. (2) There is the Brady, Shankweiler, and Mann (in press) experiment, showing that poor readers are at a disadvantage in perceiving phonetic segments through speech (but not naturalistic sounds). Again, when there is no writing, we cannot talk of a spelling-to-sound conversion problem. Finally, (3) there is evidence that poor readers are in trouble with rule systems that are completely regular. Supramaniam and Audley (Note 5) have examined reading in seventh-through-ninth graders in relation to the Test of Primary Mental Abilities. They found a correlation of .72 between the numerical-arithmetic subscale of this test and word recognition, the highest association in their data. This last result supports the claims of Morrison and of Wolford that reading disability is more general than just a reading problem. But it extends this claim in just the wrong direction for Morrison's hypothesis, arithmetic being perhaps the most well-behaved rule system we have!

The Wolford and Fowler Paper

Wolford and Fowler (in press) have presented an important new observation about the difference between good and poor readers: The latter are systematically unable or disinclined to make use of partial information to select a correct alternative. They noted that the apparently greater use of phonetic information by good readers than by poor readers is inferred, by the Haskins group, from the relative prevalence of errors that preserve one phonetic aspect of the correct item. In a spirit of magnificent skepticism, they observed that poor readers' failure to use partial information is an alternative explanation for the same data.

The question was then why the good readers don't also use partial visual information and, in so doing, commit errors of visual confusion. Wolford and Fowler responded correctly that nobody makes visual confusions in the short-term verbal memory task that Conrad (1967) and others have used, neither young
subjects nor adults. To offer a fair opportunity for good readers to make use of partial visual information, then, we need a task where it is plausible to expect visual factors to be more important than they are in short-term memory. Such a task is the so-called whole-report procedure. In this task, the same number of letters is presented for report (four) as in the short-term memory task. However, they are presented simultaneously for only 117 milliseconds, rather than successively at a rate of 600 milliseconds apiece. Furthermore, recall of the letters is immediate in the whole report task, not delayed by numerical distraction as in the short-term memory task. It is likely before the fact, therefore, that the limiting factor should be memory in the short-term memory task and visual acuity in the whole-report task. Sure enough, adults make primarily visual errors in the latter, not phonetic errors (Wolford & Fowler, in press).

The striking new result turned in by Wolford and Fowler (in press) is that on the whole report task, good readers make significantly more visual confusions than the poor readers, who do not differ from chance. There were not any appreciable phonetic confusions for either group in whole report. With the same subjects, and comparable stimulus materials, the Haskins-Conrad result was replicated for short-term memory; there, the confusions were all phonetic and good readers made more of them than poor readers. The force of this pattern of results is to produce an enormous leap in the generality of the confusion-error result: As Wolford and Fowler say, the more general, and therefore preferable, conclusion is that the good readers are better able than the poor readers to deal with stimuli analytically, and to use partial information to select a response choice. This conclusion is greatly enhanced by the two other experiments Wolford and Fowler (in press) report. I shall not describe them here, but both generalize the partial-information hypothesis in tasks that are satisfactorily different from the letter-string tasks described above (and from each other).

Although they practice the artifact-prone matching technique of dealing with IQ (Number 1 in the list given earlier in the paper), Wolford and Fowler place themselves among those who consider the skills in reading—especially, using partial information—inherent in the very definition of intelligence. The problem would then become to set out the individual skills measured in IQ tests and see which of them load most heavily on the partial-information factor. It may well be that Wolford and Fowler themselves have stated the crucial process a bit too narrowly and that, as they suggest in the closing sentences of their paper, the really pivotal skill is the capacity for analysis; without analytical capacity, using partial information and a great many more things are difficult. The experiments Wolford and Fowler offer are not really capable of distinguishing the capacity for analysis of parts within a whole from using those parts for response selection. It is to be hoped that yet more converging investigations can distinguish these possibilities.

So, perhaps disabled readers are less intelligent than normal readers with respect to analytic skills. I expect this hypothesis will be a valuable one with regard to "garden variety" poor readers. I reserve the right to suggest that there may be a special class of disabled readers, sometimes called dyslexics, for which this analysis is insufficient (see Crowler, 1982, Chapter 11). These are the individuals whose auding is perfectly normal and grossly discrepant from their reading, those who form a bump at the low end of
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the reading-skill distribution, those with a familial history of reading problems, and those for whom the ratio of boys-to-girls affected approaches 4/1. I take it this symposium does not aim specifically at this very special population and so I shall not continue in this vein; however, I personally would rather we reserved terms like "reading disability" for these children and adults.

Lessons to Take Home

There is no shortage of theories and hypotheses in the area of reading disability. What we need more of are facts that fit together. The hypotheses will surely come and go, even as they have in the most advanced sciences, but the facts, if generated by clean experimental or quasi-experimental logic, will endure. On these terms, I believe we can carry away two very solid new pieces of factual information from this set of papers.

1. Good readers make visual confusions more than poor readers in a whole report task. I have just finished reviewing this finding of Wolford and Fowler (in press) and so I won't harp on it more now. I think it puts in a more general light the "special relationship" between phonetic processing and reading established by the Haskins group.

2. Brady, Shankweiler, and Mann (in press) have shown that good and poor readers differ in phonetic perception under noisy stimulus conditions but not in identification of naturalistic sounds.

These two new facts may be rationalized together by the assumption that when noise is added to speech it results in fragmented stimuli, similar to those postulated by Wolford and Fowler to be especially hard for poor readers to use.

In answer to Morrison's (in press) challenge then--why reading?--the weight of the new evidence points in the direction of a general answer. It is not just reading that suffers in poor readers; they are subject to deficits elsewhere in cognitive functioning. We have seen the poor readers at a disadvantage listening to speech, remembering "meaningless" Chinese characters, and, in the work of Supramaniam and Audley (Note 3), performing poorly in numerical-arithmetic skills. Thus, if Morrison meant "Why reading and not other skills as well?"--we can answer that the other skills are, after all, affected. For future investigators, a big priority for the agenda is then to see which "other skills" are the ones that go with reading. On this matter, the present papers have formed a promising beginning.

REFERENCE NOTES

REFERENCES


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FOOTNOTE

1A reviewer has pointed out, quite correctly, that we should not be glib in assuming that "strictly auditory tasks" would not be affected by literacy. Knowing the orthography may well influence lexical representation and organization. For example, Seidenberg and Tanenhaus (1979) demonstrated orthographic effects in rhyme monitoring with only auditory stimuli. On the other hand,
not all speech perception tasks would likely be subject to orthographic influences. Highly analytic tasks, like the rhyme monitoring of Seidenberg and Tanenhaus, would be expected to show such effects while direct speech perception would likely not. With the nonsense syllables used in the Brady et al. study (in press), there is no orthographic representation waiting in the lexicon, of course.