Response to Charles Hulme's critique of: ***Early Reading Instruction*** (MIT Press, 2004), and ***Language Development and Learning to Read*** (MIT Press, 2005).

By the author: Diane McGuinness

I must take exception to the critique of my two books by Professor Hulme (THES, 24th March, 2006) due to the extraordinary number of inaccuracies which range from misrepresenting the dates of publication and sequence of these books to unfounded remarks about what these books do and do not contain.

I am well able to take criticism of my work (and used to it), especially when the points are well argued, but when a review presents such gross distortions of the facts, I cannot let this pass without comment.

Professor Hulme's review characterizes me as having "extreme and intemperate views" to such an extent that "much of what she writes appears to display a contempt for other people's research--." Though, he goes on to say that I do not have extreme and intemperate views about *his* research – "My own research gets off relatively lightly here" –. This he seems to put down to some intentional scheme on my part of trying to avoid annoying *him* in particular: "Better not to have another bad citation in this case, I feel." It is somewhat ironic that he didn't prefer a more benign (and accurate) interpretation, that I have favorable views about his research!

From the outset, Hulme misrepresents my books: "These two long books are published simultaneously." No they are not. They are published sequentially, one year apart. He says he will review the first book "*Language Development and Learning to Read*" first. But this is the second book, and was published in 2005. The first book, *Early Reading Instruction*, was published in 2004.

He feels these two books, one dealing with the applied research on reading (studies on reading instruction per se), and the other dealing with correlational research on reading predictors, could, with some editing, have been published as one book! I can assure Professor Hulme that had my editors believed this was possible, it surely would have been done. As it was, the book was cut by some 300,000 words to get it into two volumes.

Unfortunately, Hulme appears not to have read these two books sufficiently well to know what is in them, other than to take exception to a few comments, and then skim or ignore the extended arguments which support them. The remarks he objects to are generally conclusions based on an inductive analysis of the research evidence. All this evidence, he simply ignores. (Not so, Professor Pumfrey, who reviewed these books for "*The Psychology of Education Review*" March 2005), and who understood the inductive approach completely.)

Hulme's first stumbling block was my refutation of a highly popular view of children's language development, in which it is argued that as children get older, they develop a sensitivity to finer and finer increments of the speech input, from larger units (words, syllables) to smaller ones: phonemes. I referred to this as “The Dogma,” because this view has become pervasive, has absolutely no research support, and won’t go away. Unfortunately, it appears Hulme also holds this view. Thus his strong belief seems to make it impossible for him to read and assimilate the ***five chapters*** of research data that follow this assessment, which prove unequivocally that phoneme awareness is present a birth, that 9 month-olds can detect and isolate whole words from the speech stream on the basis of phoneme cues, that 3 year olds can blend isolated phonemes into words and point to the correct picture with nearly 100% accuracy, plus many other examples.

All this Hulme simply tosses aside: “But the infant speech perception studies to which she is referring here assess only very basic speech sound discrimination abilities with restricted sets of isolated syllables.” No, the research absolutely does not mean this! However, this does not mean that infants and young children are *conscious* observers of what they can do -- that they are “are aware of being aware.” And this is the critical point. It is for this reason that the phoneme level of speech has to be “taught” or “elicited” when children (or a non-reader at any age) learn an alphabetic writing system.

Even the scientists who initiated the research that led to “the dogma” *were well aware of the infant research on phoneme analysis*, and had to modify their position, labeling the automatic (unconscious) aptitude “implicit,” and the ability to carry out certain phoneme sequencing or isolation tasks as “explicit” (with conscious awareness). This distinction was emphasized a number of times in the early chapters. By failing to understand or consider this distinction, Hulme has utterly misrepresented what I wrote.

Furthermore, it is clear that implicit phoneme analysis STAYS below the radar screen of consciousness unless one has to learn a phonetic alphabet. And it is for this reason, and this reason alone, that phoneme analysis *must be taught*. It is not that poor readers have a deficit or disability, but rather that teachers must make children aware that our alphabet marks the smallest units of our speech, the phonemes, *and no other unit*.

Misunderstanding all this, Hulme misrepresents my position once again later in his review: “Perhaps more contentiously, McGuinness concludes that there is no evidence that teaching phoneme skills is useful in helping children learn to read.” This is quite preposterous. This statement arises from points in my other book (*Early Reading Instruction*), in which I show that the research conclusively proves there is no benefit to “phoneme-only training programmes” as opposed to instruction using a good synthetic phonics programme from the outset, one which teaches segmenting and blending using letter symbols and lots of writing practice. Phoneme analysis sufficient to be able *to decode* is acquired much more rapidly in the context of print than in isolation.

Hulme objects to my more favorable view of the basic research on language development and to my statement: “Unlike reading research, mainstream research on language development has continued apace with no setbacks and is one of the great success stories of the behavioral sciences.” He finds this statement “absurd,” and tries to show how mixed up I am, by citing the fact that both “reading research” and “research on language (speech) development," were both ongoing at the same laboratory (the Haskins Laboratory in Connecticut). I guess this means he thinks these studies were connected when they were not. I am well aware of who is who in this regard. And it isn't true that the people he lists were/are primarily at the Haskins Lab.

To set the record straight. Until the mid 1970s, the Haskins Lab was essentially a research institute devoted to the study of speech perception and voice recognition devices. Al Liberman, a psychophysicist doing research on speech perception, was a primary (full-time) member of this research team. His wife, Isabelle Liberman was trained as an educational/clinical psychologist and worked initially as a guidance counselor with a special interest in learning disabilities. She subsequently joined the education faculty at the University of Connecticut, where she began a collaboration with Donald Shankweiler. Not long after this, space (and perhaps funding) became available for this work to spill over into the Haskins Lab. Since that time, this area of research has become greatly enlarged and emphasized. However, the primary effort still remains speech perception and speech recognition.

I. Liberman is to be credited for the initial push to promote auditory processing as critical to learning to read rather than the visual hypothesis that was currently extant. My position is that her basic premise was correct (auditory perception is far more central to learning an alphabetic writing system), but her specific developmental model is not. It is perfectly acceptable in science for a model to be proven false. What is not acceptable, is that when such proof becomes available, the original model persists! At this point, the older model becomes "dogma." The fact that this model is still alive and well took me by surprise when I began working on this material. And I was taken aback when I discovered that the phonological developmental model had never been properly tested. Yet there was abundant evidence available (which I review) that this model is false.

I. Liberman's work is tangential to the late Al Liberman's work, and that of his many colleagues at Haskins, including Michael Studdert-Kennedy, Susan Nittrouer (his student) and others who do exemplary research on the development of speech perception in children. I cite this evidence extensively, but Hulme completely fails to mention it. Instead, he writes this: "This group's work is dismissed as, in essence, worthless and misguided by McGuinness, but these same researchers were (and some still are) at the forefront of work to uncover the basis of human speech and language mechanisms." This demonstrates a very shallow reading or shallow understanding of what I wrote.

Hulme goes on to complain that my book is "neither comprehensive nor balanced in terms of the research that is reviewed." If this is the case, why would he suggest that I shorten these two volumes (900 pages) into one book!

He writes that I say nothing about comprehension. Yet the whole of the second section of Language Development is about natural language development which includes every aspect of comprehension, including syntax, semantics, prosody, and so forth. It also includes the research of Beitchman, and of Bishop, who looked in detail at reading comprehension test scores longitudinally in language-delayed children. Likewise, in

Early Reading Instruction, I cover the National Literacy Panel's study of reading comprehension, with an entire chapter on this topic. Furthermore, I refer in both volumes to Hart and Risley's amazing longitudinal study in which verbal and reading comprehension were taken into account.

This unprofessional, pot-shot type of reporting continues throughout and is uncalled for.

"McGuinness consistently fails to pay any attention to individual differences," he writes. This tells me that Hulme never even skimmed the third and final section of *Language Development*, in which every single study is scrutinized for whether the individual differences of age, sex, memory, etc. were controlled for and reported. As my early research was on the topic of sex differences, I am the last person who would leave individual differences on the side lines.

In addition, all the research on late-talkers (reviewed in *Language Development),* reflects the struggle of researchers to locate markers (individual differences) that would predict these language delays. One of the major markers is sex, and sex continues to be a strong marker across the age span. Indeed this point was made many times.

Finally, Professor Hulme appears to believe that I know next to nothing about statistics. I can assure him this is not the case, having taught statistics and research design for over 20 years, and am currently writing a book on the topic. As he put it: "A little knowledge can be a dangerous thing. A prominent feature of both books is their frequent digression into issues about statistics and research design. For most readers, I imagine these passages will be baffling, but perhaps persuasive, since they are expressed with great bravura - "

What Hulme objects to is my criticism that much of the reading research fails to adhere to proper research design which can then be tied to appropriate statistical tests. I refer here to studies in which there is no random selection of subjects into a data pool. Because statistics is based upon the mathematics of probability, which in turn is based on the world of random or chance events, statistical tests are only valid when certain criteria are upheld. One of these criteria is to take the world as you find it and don't divide your subject populations to suit your objective. This argument is spelled out in great detail in my book and is irrefutable, despite what Hulme thinks.

In reading research, it is common for children to be given a reading test battery, and then selected into the study solely on the basis of their reading test scores, as 'good' versus 'poor' readers. They are then given tests known to be correlated to, or likely to be correlated to reading test scores, and voila, a significant result appears. This practice voids the conditions for which the mathematics of probability can be applied, because it violates the principle of "random selection." (among other things) . I christened this type of design an "isolated groups design" because of the way it looks when you graph the distributions of the data from the two groups. It will never be found in any textbook of statistics, nor in any account of how statistics works by the geniuses who invented these tests, such as Karl Pearson, William Gosset, and Ronald Fisher, because ***no statistical tests can be applied to it.***

Hulme hasn't the faintest idea what I am talking about, and uses a spurious counter-example taken from medical research to refute my position. (Of course, one first has to believe that medical research is valid, when much of it is not. Interested readers should look at "The Limits of Biological Treatments for Psychological Distress," Fisher and Greenberg, 1989, for a revealing assessment on how difficult it is to do medical research properly.)

Hulme argues that

"Her objections to these studies are based on a profound misunderstanding of basic statistics. Such designs (case-control studies) are widely used in medicine and epidemiology, and there is a whole raft of thorough statistical work on their design and interpretation. Based on McGuinness's view in chapter nine, we could throw out all studies that have ever compared a clinical group with a control group." He uses examples of people who have had heart attacks versus people who haven't, or people who are depressed versus those who are not."

Yes, I certainly would throw these studies out. What on earth would a design like this prove? One needs to know the connection between past behaviors and some medical outcome. The mind boggles as to which type of behaviors and conditions might become candidates here. Diet? Exercise? Family history? Job stress? Financial stress? - or more than one of these factors, or all 5, or perhaps something we haven't thought of yet. Furthermore, Hulme's example is nothing more than a correlational study in disguise, and correlations can never prove causality, proving a point I was making in my book. And if this is retrospective, how can any of this be verified?

Now, one might want to take a group of people all prone to heart attacks due to family history of heart attacks, and ***randomly assign them to treatment conditions***, such as a particular diet, or exercise regime, and then see what happens over the long term. This is a bona fide research design, and, providing the patients stick to the regime, the data can be treated statistically by a number of valid tests. In the example for reading research which I outline below, they cannot.

In reading research, the researcher typically identifies a random population, roughly of the same age, and gives every child a reading test. The test is given IN ORDER TO split the children into two groups: One of these groups does extremely badly on the test, and the other scores in the normal range. Next, take these two extreme groups, give them some more tests (tests of memory, visual and auditory perception, etc.). Then compare the two groups statistically on each of these skills, using the statistical tests designed for "independent groups" or "random groups." Run the math, and you will discover the "poor readers" scored "significantly" below the normal readers on most these tests. As the tests themselves are chosen because they are related to reading in some way, it is not surprising what the results show. The outcome is almost a forgone conclusion, because the research design dictates the outcome. As I wrote in my book, this is the only research design I am aware of in which a null result (no significant differences between groups) is far more unlikely (improbable) than a significant result.

Finally, Hulme writes: “None of the work dealing with the neurobiological basis of reading is even mentioned.” There is no neurobiological basis of reading, so what on earth can this statement mean? Reading requires quite ordinary skills of visual perception, auditory perception, paired-associate memory for linking sound and symbol, and the ability to carry out what brain researchers call “flexible noticing order” a process performed admirably by the frontal lobes. If reading required some amazingly complex brain apparatus, then we would indeed be in trouble. However, as 100% of the population in countries with a transparent alphabet can acquire the basic-code (knowledge of which letter symbol(s) goes with which phoneme in the language (Spain, Italy, Sweden, Finland, Austria, Germany, etc.), what kind of “neurobiological basis of reading” has any relevance? The recent studies using fMRI, convincingly show what everybody who knows anything about the brain can tell you, that when someone can’t read, images of his brain taken while he is trying to read will look different to someone who can read. Furthermore, when the poor reader is taught to read, the patterns of his brain metabolism will look identical to someone who can! Does this obvious result warrant any special comment? I hardly think so. As someone who has spent 10 years working in a brain lab where brain function in perceptual and cognitive tasks was our major focus, I am perfectly aware of which “neurobiological bases” are meaningful and which are not.