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# Compellingness in reading research

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## *Preamble*

The article that follows this preamble is a contribution to the *Reading Research Quarterly* Conversations series and has as its topic the concept of compellingness in reading research. The authors, both of whom have been editors of reading research journals, were excited to have been given the opportunity to submit their thoughts on this topic and to have been offered the freedom to decide how to represent a conversation within *Reading Research Quarterly*.

In many respects it is impossible to have a conversation in print. The term *conversation* implies many discourse features that cannot easily be imported to a research journal. Let us review some of them briefly.

- The topic of a conversation, and its importance, are decided by the participants and not by those who are listening in. It would be surprising to hear someone saying of a conversation: “Engaging! I recommend acceptance!” or “What a pointless conversation! I’d reject it.”
- Conversations often deal with topics that are too delicate to commit to printed form and often have greater personal and emotional content than do journal articles.
- A conversation takes place in turns, with no opportunity for revision. The utterance is a first-and-final draft.
- A conversation makes more assumptions about shared world views and knowledge than is usual in a journal, so there is more ellipsis. Humour

and irony are possible and even welcome, whereas in a journal the assumptions about cultural knowledge that are made in a conversation might be marginalising and unacceptable.

- Conversations are rhetorically as well as grammatically messy. Utterances are not *crafted* in the manner of a written article.

For all these reasons, the conversation that follows is risky:

- It is on a topic that would not normally be raised in a research journal, namely the conditions under which we come to believe what an author writes.
- The conversation is on a topic that both authors believe has for them personal as well as professional significance. It is precisely because they want to address the personal as well as the professional implications of the topic that they found the discourse framework of a conversation appealing.
- The discourse structure of this conversation is not that of a research paper: Each utterance was sent to the other participant in the dialogue as a first-and-final draft. The recipient did not then converse with the other, except by sending the next utterance, again as a first-and-final draft.
- This conversation ranges more widely than is usual in *Reading Research Quarterly* articles. In order to make their points, the authors draw upon their reading in logic, literary theory, mathematics, and the philosophy of science, as

well as reading research. There is even a culturally marginalising reference, which requires an understanding of American soft drink sales wars. This use of wider academic and cultural reference within a *Reading Research Quarterly* article is part of our purpose, namely to reflect some of the personal and idiosyncratic aspects of interpretation and response.

- The authors have naturally tried to express their thoughts as clearly as they are able, but they have avoided redrafting, since to have done so would have defeated their larger purpose, which was to capture some of the immediacy of an argument that is conducted as a conversation.

We hope that you enjoy listening in on our conversation.

**Colin Harrison:** In the reading field we are now some years into the process of beginning to come to terms with a proliferation of research paradigms. Editors of research journals have issued invitations to authors to submit papers representing a range of methodologies, and, as readers, most of us try to suppress our methodological or paradigmatic prejudices and attempt to judge each article fairly and to reach a decision about whether or not the author's claims are justified. But what makes us believe an author's claims? What is it that makes a research article compelling, and is the answer to this question changing as journals become more eclectic in terms of the range of paradigms that the editors accept? I would submit that compellingness is a neglected but important concept, because no matter how timely, brilliantly conceived, carefully constructed, or well-written an article is, if we do not find it compelling, the article will fail in its most important goal, namely to change our view of how things are.

Clearly, this conversation could continue indefinitely if we were to consider in detail the legitimisation crisis in science, the relationship between knowledge and belief, the concept of truth, the relationship between art and life, and so on. But one of the good things about conversations is that they make no claim to completeness, and omissions are therefore not culpable but to be treated simply as roads not taken.

Compellingness, as I'm using the term, is the property or attribute of a research article, but I'm not wanting to suggest that compellingness exists independently of the reader. Compellingness is the product of an interaction between the text and the reader, a psychological phenomenon and, of course, a social phenomenon. Compellingness is also about personal belief, and I want to relate the concept to the writing of three thinkers who in very different ways got me interested in the relationship between

knowledge and belief. Let me say at once, and in only the very briefest way, something that each of the three said that relates to the notion of compellingness.

First is Wittgenstein, whose *Remarks on the Foundations of Mathematics* (1956) contains ideas on compellingness in proof in mathematics which I found fascinating when I read them as a student in the 1960s. He wrote, "And how does it come out that the proof compels me? Well, in the fact that once I have got it I go ahead in such-and-such a way, and refuse any other path" (p. 50). For me this remark makes two points: first, that compellingness is not inevitable; and second, that compellingness is not simply an attribute of the proof. Compellingness in the proof has an effect upon me, upon the path I go on to take. Compellingness changes how I view the world.

Wittgenstein, formal logician though he was, did not view the world in simple terms of *true* and *false*. In his final writings, published as *On Certainty* (1969, p. 84e), Wittgenstein used the concept of *certainty-value* to describe a sentence, rather than truth-value, in certain circumstances. Students of formal logic are familiar with the concept of the *truth-value* of a proposition, which was at one time viewed as dichotomous—a proposition is either true or false. Modal logics extended this notion to include functions such as *possibly* or *necessarily*, but Wittgenstein's point went further than this. I would suggest that he was arguing that statements about the depth of one's conviction were as relevant as truth-value functions.

Another aspect of compellingness to which Wittgenstein often referred was an unanticipated change in knowledge state, in other words a surprise.

You are astonished at what the proof shews [*sic*] you. But are you astonished at its having been possible to draw these lines? No. You are astonished when you tell yourself that two bits like this yield this shape. When, that is, you think yourself into the situation of seeing the result after having expected something different. (1956, p. 20)

So it's not simply the proof that wins us over; it's the sense of astonishment, of wonder, of surprise. What I'm arguing here is that one of the greatest logicians of the century reminds us that in science, very personal factors—one's world view, one's feelings of certainty or uncertainty, one's sense of wonder—all interact to influence what we might otherwise choose to interpret as compelling.

The other two authors are Paul Feyerabend and Tom Wolfe. Feyerabend's *Against Method* (1975) and Wolfe's *The New Journalism* (1973) were required reading in some innovative courses on curriculum evaluation in the early 1980s in England, and both were read as cri-

tiques or even manuals of research method. Feyerabend made the anarchic claim that progress in science was possible only when the canons of traditional scientific rigour were ignored or violated. Just about all the really important advances in science, argued Feyerabend—the Copernican Revolution, the wave theory of light, the quantum theory, for example—occurred because scientists decided not to be bound by (or unwittingly broke) all the rules of scientific reasoning and methodology then current. One corollary of this, declared Feyerabend, is that theory often runs ahead of the capacity of science to prove its case, for what we call *facts* are no more than propositions belonging to the domain of the about-to-be-refuted ideologies. Feyerabend argued, for example, that the views of Galileo prevailed not because of solid evidence from optics, but because of good techniques of persuasion, because Galileo wrote in Italian rather than Latin, and because he appealed to people who were temperamentally opposed to the old ideas. However, it is important to note that Feyerabend was not against science; his stance was similar to that of William Blake, who was passionately opposed to the church though an ardent Christian.

If Feyerabend invited one to see science as hegemonic myth, the 60-page essay that opens Tom Wolfe's *The New Journalism* offers a sometimes surprising perspective on the relationship between fiction and reality. At the same time postmodernists were encouraging us to see science as fiction, Wolfe was delineating with great skill just what it was that made fiction compelling as a depiction of reality. For Wolfe, there were four principles that made for realism, particularly social realism in writing, and these, he felt, applied just as fully to Charles Dickens as to Norman Mailer. These were (a) recording events in a scene-by-scene construction, rather than through historical narrative; (b) recording dialogue in full (Wolfe makes the point that Dickens often gave very brief descriptions of his characters; he established his characters through attention to dialogue); (c) making use of a series of *I was there* perspectives, presented through the eyes of particular characters; and, (d) recording symbolic details of an event or scene in order to evoke entire patterns of life (specifically what Wolfe called *status life*) of the personalities and participants in a narrative. I would want to suggest that Wolfe's four principles are often what make for compellingness in case studies and ethnographies in reading research.

So here are three very personal perspectives on compellingness: (a) compellingness in mathematics as a well-formed proof that contains a *surprise*, (b) compellingness in scientific enquiry as an anarchic paradigm shift presented with bravura, and (c) compellingness in a

narrative as a symbolically evocative reconstruction of events that makes extensive use of dialogue.

How does all this relate to the *Reading Research Quarterly*? Very directly, since I would want to suggest that compellingness, although personal, has a profound impact on how readers read, how referees adjudicate, and how editors edit. I feel that it is good to discuss this—it's about the reasons why papers come not only to be published, but to be believed, and to change things. I'm not wanting to say that editors or readers ultimately pay little attention to such important issues as scientific rigour, but rather that if there are other factors that operate when we judge the worth of a paper, it is appropriate to explore these. I would also want to go a little further and suggest that, while the perspectives I have offered on the concept of compellingness are my own, it was certainly the case that, in their very different ways, Wittgenstein, Feyerabend, and Wolfe felt that their notions of what I have clustered together under the term compellingness were pretty universal, and permeate at the deepest levels the internalised conceptions we all possess of the ways in which narratives, whether scientific or fictional, can effectively represent the world.

**Philip Gough:** Colin, I find your argument compelling. You (along with Wittgenstein, Feyerabend, and Wolfe) persuade me that compellingness is an important topic. You could add a Nobel laureate to your distinguished trio: my colleague (I've never met him) Steven Weinberg (1995) recently wrote (with apologies to Duke Ellington), "It don't mean a thing if it ain't got that zing" (p. 10).

As you note, we are confronted with a variety of narratives, or myths (to use Feyerabend's term), for describing the world. I agree with you that compellingness can be found anywhere on this epistemic spectrum.

As Wittgenstein notes, a mathematical proof can prove compelling. I well remember being blown away by Cantor's diagonal argument (that the cardinality of the rational numbers must exceed that of the integers) in a mathematical linguistics course.

Fiction can be equally compelling. Many Americans' attitudes toward slavery were compelled by reading Harriet Beecher Stowe's *Uncle Tom's Cabin*. Samuel (1995) credits A.J. Cronin's novels for Labour's landslide victory in the 1945 British election. Solzhenitsyn was exiled from the USSR in 1974 no doubt because of his government's fear of the beliefs his novels might compel.

Journalism can certainly be compelling. Our withdrawal from Vietnam is widely assumed to have been influenced by the journalistic accounts of what was happening there. More trivially, Tom Wolfe's *The Painted Word* (1975) had a profound impact on my view of modern art.

Qualitative research, too, can be compelling. For example, I found Shirley Brice Heath's *Ways with Words* (1983) poignant and convincing.

And, of course, a scientific paper can be compelling.

But what is being compelled? I would argue that it is belief, not knowledge. Thus I fear that we cannot use it as the only basis for evaluating manuscripts.

I would maintain that what each of us finds compelling rests upon what we already believe. How else can you explain why a paper that is compelling to some can be abhorrent to others? Take, for example, Denny Taylor's (1994) venomous attack on West, Stanovich, and Mitchell (1993), a paper I found persuasive. Taylor was clearly not compelled; she was, it would seem, even disgusted. But on what basis? I would contend that she disagreed with the paper because of her prior beliefs.

So I fear that we cannot ascribe compellingness to a text. Instead, as you put it, it is "the product of an interaction between the text and the reader." The problem with this is that human beings are too ready to believe. Human beings are born believers, and they tend to believe anything. Teigen (1986) took 24 well-known proverbs and transformed each of them into its opposite, gave both to undergraduates, and asked them to rate their originality and truthfulness. He found no recognizable difference between the set of eternal truths and their exact opposites. My colleague Dan Gilbert (I do know him) has shown that we even tend to believe propositions that have been denied (Gilbert, 1991)!

But why should we accept our beliefs? We know that they can be false: Our ancestors believed that the world was flat; some of our contemporaries believe that Elvis lives. Clearly, we need some way to decide which of our beliefs is true. We need some way of distinguishing knowledge from belief.

Some of our colleagues would like to blur, if not erase, this distinction. They hold that what we count as true (i.e., what we know) is socially constructed. On this view, there are many different ways of knowing. My colleagues in education here at Texas were (and are) intrigued by Belenky, Clinchy, Goldberger, and Tarule's *Women's Ways of Knowing* (1986); so was I. But I believe it was mistitled. It should have been *Women's Ways of Believing*.

There are many ways to come to a belief, an idea about how the world is. They include reading a novel, a news report, or a case study; we might add quiet reflection, or even (as the anecdote has it) smelling a rotten apple in a drawer. But we must somehow evaluate those beliefs; we must decide which are true and which are false. We do so spontaneously, all the time. In mathematics, we distinguish a faulty or incomplete proof from

a complete and correct one. We distinguish yellow journalism from good journalism. And while we call fiction fiction (to distinguish it from fact), we distinguish realistic fiction from unrealistic fiction. (I submit that anyone who has read both would agree that John Le Carre is truer than Ian Fleming.)

What is it we are trying to get at here? It is the matter of truth. We may be compelled, convinced, totally believe something. But when our beliefs differ, how should we decide among them?

One way is to leave it up to each of us (and our friends); we can each have our own truths. That seems to me the basic tenet of social constructivism. But my most fundamental belief is that there is a world out there, and our task is to see which belief fits that world.

We could leave it up to an authority, like a church or a government. If they differ, they can settle their difference by war. More peaceably, we might just vote on it. But our species has been laboring for 2,500 years to find an apolitical way to settle the matter.

What is this way? Sir Francis Bacon (1620/1994) outlined it (in the *Novum Organum*) nearly 400 years ago. If we want to know whether X is related to Y we must vary X, hold everything else constant, and see if and how Y changes. We can vary X ourselves, as in an experiment, or we can let nature do it for us (as in a correlational study).

Is there any other way? Perhaps. Qualitative researchers certainly think so. But I am yet to be convinced. I would contend (and I know I'll offend) that qualitative research is closer to good journalism than to science. I don't think that it's as good as research using traditional methods.

Why don't I think so? Why do I have such faith in the old way? Because it works. It has yielded us a stupendous amount of knowledge about the physical world. And it has done so democratically; anyone can use it.

Has the scientific method yielded absolute knowledge, the final truth? Of course not. All of our beliefs must be tentative. The postmodernist takes this to mean that all ideas are equal. I find this amusing. Deconstructionists may make fine literary critics, but I wouldn't want one for my doctor or my engineer.

The great thing about science is that if it is wrong, it will ultimately fix itself. Science is self-correcting. This, to me, is the heart of the matter. Along with Karl Popper (1934/1959) (I wish you'd been assigned to read him instead of Feyerabend, whom Regis, 1995, called science's worst enemy), I believe that it is disconfirmation that brings us closer to truth.

Theories predict phenomena. If they do so correctly, we can go on believing them. That does not make

them true; it makes them only tentatively true. But disconfirmation does render them false.

Those of us who would like to think of ourselves as scientists do so because our basic belief is in this method. We may hold beliefs (we would call them hypotheses) about (say) whether phonological awareness is the key to learning to read an alphabetic orthography, or whether there are one or two routes to the lexicon, or whether reading can be decomposed into skills. But if we are wrong, the scientific method will prove it so.

This is a strange time in our field. Never have we known so much about the reading process or its acquisition. But the scientific paradigm that yielded this knowledge is under attack from all sides. Part of the reason, it seems to me, is that many in our field have more faith, greater belief, in other things (e.g., an instructional philosophy, or the belief that our society is unjust) than in this method. I find this understandable but regrettable, for what they have offered us methodologically is no substitute for the real thing. I believe that there's no shortcut to knowledge.

**Colin Harrison:** Phil, you sum up some of the differences between our positions admirably. You see compellingness as essentially a matter of personal belief, and you want to distance it from notions of knowledge and truth, which you characterise, if not as absolutes, then at least as approximations towards absolutes. So, you argue that although we can only get to approximations of truth, a principled basis for striving towards it can be found through scientific method, which offers us the security of a mechanism for testing theories and for accepting or rejecting them.

I, by contrast, while recognising that compellingness is about personal belief, see the concept of compellingness as a kind of unacknowledged superordinate whose effects need to be considered carefully, since they will influence our notions of knowledge and truth, whatever the paradigm within which we are working. Thus I fear that we may not be able to avoid using compellingness as a part, and sometimes a major part, of the basis for (say) evaluating manuscripts.

And you are right—while I have no wish to deny that Popper's *scientific* paradigm has a place in reading research, I would want to suggest that there are areas in which it has little utility. I certainly feel that reading research would be the poorer if it were predicated wholly on a Popperian model. Reading researchers have taken the Popper Challenge and are no longer convinced that there is just one "real thing."

As you are aware, Popper's approach separates (what you were too polite to call) *pseudoscience* from science by subjecting theories to the test of refutability.

Popper used the examples of astrology and astronomy to illustrate the difference between the two; in his view, the theories of astrology are in principle irrefutable and are therefore not scientific. I would contend that if the Popperian view of science is under threat this is not because it is inherently wrongheaded, but rather because it has a tendency to claim more than it can deliver, to be unhelpfully exclusive, and to be unwilling to recognise that it is just as susceptible to the influences of bias and subjectivity as qualitative research.

Is the hypothesis "phonological awareness is the key to learning to read an alphabetic orthography" provable? Is it, in any final way, refutable? Of course, the research on this topic that you and your colleagues have conducted has been and will continue to be of great importance, but is the hypothesis one that can be settled by *science*? I'm not at all sure that it can, at least not in our lifetimes. As Ericsson and Simon (1993) put it, "Contrary to the doctrine of the Popperians, there is no 'sudden death' of theories" (p. 285). Cases of the unequivocally triumphant overthrow of a theory in the reading field are rare; in order for us to make both publishing and pedagogic decisions, we operate on other principles. I'm arguing that it might be helpful for us to look a little more squarely at how these principles operate.

I would suggest that in the field of reading research, most of the decisions we reach are not determined by science in your strict sense—through the processes of testing and rejection of hypotheses—but rather by a potpourri of processes involving principled enquiry, myth, narrative, rhetoric, and hegemony. Approaches using what you would accept as scientific method have a place in this, but to suggest that research using the Popperian paradigm is somehow the real thing seems gratuitously dismissive of other avenues of enquiry. What is more, it seems to claim a direct line (using a sort of Platonic cellular phone) to the truth.

In reading research, whatever the paradigm we are using, the variables with which we are dealing are so manifold that truth may be permanently elusive, and compellingness becomes correspondingly more significant. (I even have a half-serious Law of Compellingness: where  $\tau$  is the probability of establishing the truth, and  $C$  is the relative importance of compellingness over truth,  $C = (1/\tau) - 1$  so as  $\tau$  varies from unity to zero,  $C$  varies from zero to infinity.) I want to suggest that compellingness regularly takes precedence over truth but that we hesitate to acknowledge the fact because to do so would be to foul our own nest.

Take, for example, your recent book on reading acquisition (Gough, Ehri, & Treiman, 1992). Is it not the case that, between every line of this excellent book, the strong feelings and personal belief systems of the au-

thors are written in invisible ink? Are not the skills in authorship and rhetoric of the authors as much a part of the compellingness of the book as the evidence adduced? Is not the fine name of the book's publisher a further part of what makes its message compelling?

Take another very important current question in reading research: Are Reading Recovery programmes effective? You might argue that my question here is poorly formed or naive, but the five-word form is the one I have a professional responsibility to address since I work as a consultant to one of the many evaluations of Reading Recovery currently being undertaken. Some distinguished colleagues in our field (among them Center, Wheldall, Freeman, Outhred, & McNaught, 1995; Clay, 1987; Glynn, Crooks, Bethune, Ballard, & Smith, 1989; Hiebert, 1994; Pinnell, Lyons, Deford, Bryk, & Seltzer, 1994) have contributed to this debate, but a close reading of the available evidence can leave one feeling rather like a layperson trying to decide whether or not Fermat's last theorem has indeed been proved. In my view, factors such as Pinnell's having in her team of authors the statistician who wrote the equation for calculating student gains add to her paper's compellingness. Equally, Hiebert's outstanding reputation as a scholar adds to her paper's compellingness, but so does a more personal factor such as my having heard her deliver a fine research review on assessment.

You referred earlier in this conversation to Denny Taylor's "venomous" (I would have said "passionate") attack on the West, Stanovich, and Mitchell paper in the *Quarterly*. For me, that exchange served as a powerful and uncomfortable reminder that we all bring values, beliefs, and strong feelings to the office with us; we can't leave them at home. It's just considered professionally inappropriate to print them. I'm not suggesting that it would be helpful to have more intemperate research papers and commentaries published, but rather that it might be valuable for us as reading researchers with a common purpose to look more closely than we have done at how we read and by what routes we come to value the research we find compelling. My expectation is that to do this would not be to jettison the actual and potential gains of traditional research, but instead would permit us to gain a fuller understanding of how the widening range of research paradigms in reading gain their effects.

**Philip Gough:** Colin, I agree with much of what you say. But I fear I come to the same old hegemonic conclusion.

First, I agree that compellingness is an important concept that influences our evaluation of manuscripts (and other texts) and we should explore it further. (I

wish we had more space to do it here and now!) But I still maintain that it is belief which is what you referred to earlier as the "unacknowledged superordinate whose effects influence our notions of knowledge and truth." It is our beliefs that determine what we think we know and what we think is true. In consequence, they influence what we find compelling. But I see this is a problem, not a solution.

I must concede that a paper might be so compelling that it would overturn our beliefs, but I submit that this seldom happens. The problem is that our beliefs are too often too strong.

Our beliefs certainly influence how we evaluate arguments and evidence. Denny Taylor's beliefs influenced her assessment of West, Stanovich, and Mitchell; in turn, our beliefs influenced our evaluation of her commentary. Our choice of words to describe her undoubtedly reflects those beliefs. I called her attack venomous, you called it passionate. It certainly was passionate. But what was that passion? Perhaps I was unkind to call it venomous; I wish now that I'd simply called it angry. I was, as you put it, bringing my own "values, beliefs, and strong feelings to the office."

The central issue between us (as between Taylor and her targets) would seem to be the value of the traditional scientific method. You concede that it has value. But you contend that there are areas in which it has little utility. I would dispute this. I would agree that it is very difficult to apply the scientific method to some questions. One is the Great Debate. Many people have tried to use the method with (I think) some success. These efforts have not compelled reading educators, however, because (I think) they believe so strongly in their kind of reading instruction that scientific evidence is largely irrelevant to them. If that evidence does not support their position, they simply reject it (and seek other methodologies that might). But I don't think that this means that the scientific method has no utility here, or that we should turn to other methods.

We could settle the Great Debate scientifically. It would involve a great (and cooperative) effort, and that is unlikely to be achieved in our time. I personally don't think it would be worth the effort, because I seriously doubt that either phonics or whole language has the right answer. Phonics is dull, boring, and meaningless; whole language is none of these, but it threw out the baby (the mastery of what I call the orthographic cipher) with the bath water (tempting me to call it hole language). I dream that we can find a way to teach every child to read independently in a semester. Since neither phonics nor whole language comes close to doing that, I think it would be better to expend our efforts searching for a method that would.

In the meantime, teachers, like physicians, can't wait for the final scientific answer—they must teach their children now. Undoubtedly you are right in saying that many things will influence their choice of method. But I think that scientific evidence should be one of them.

You say that Popperians will not admit that they are “just as susceptible to the influence of bias and subjectivity as qualitative researchers.” I don't think that's true. We know quite well that we are susceptible; there is a large literature in psychology addressing this matter. But we think that the scientific method is the best way humanity has discovered of reducing the influence of those biases. For one thing, science teaches us to be skeptical, to challenge our beliefs, to seek to falsify them. (My favorite journals include not only the *Reading Research Quarterly* but *The Skeptical Inquirer*.)

We should, I concede, be skeptical of our most fundamental belief, our belief in our methods. But I fear that if we let other beliefs count for more than that, we will not be able to see that our beliefs about that world are false. You know that the Russian geneticist Lysenko believed in the genetic inheritance of acquired characteristics. That hypothesis accorded so well with Stalin's other beliefs that he made Lysenkoism part of the doctrine of Soviet science. In consequence, genetics in the USSR fell 20 years behind the rest of the world.

Take an easier matter (than that of the Great Debate), the effectiveness of Reading Recovery (RR). I wonder if you would agree that the real question about RR is its *relative* effectiveness; I would be surprised if you were to tell me there is someone who doubts that it is effective. The question must be whether it works better than other, less expensive methods of remediation or instruction. What better way could there be to settle this question than a controlled experiment? I can't think of any. While I, too, might be persuaded by Freddy Hiebert, or the credentials of some other author, I would be far more compelled by a well-designed, adequately controlled, experimental study.

Do we traditional researchers claim more than we have delivered? I wish I knew what you had in mind here. Perhaps you see my hypothesis that phonemic awareness is the key to learning to read an alphabetic orthography as too bold a claim—at least you ask if it is provable.

I think that our research has firmly established that few preschoolers have phonemic awareness, that it is a better predictor of subsequent reading achievement than socioeconomic status or IQ, that instilling it in those who don't have it will foster their subsequent reading achievement, and that those who have difficulty learning to read lack it. I probably overstated its case in calling it the key; a more careful statement would have been to

call it the foundation, for while I know that a child may have phonemic awareness and yet not read well, I would maintain that a child cannot become a skilled reader without it. Phonemic awareness is necessary, but not sufficient, for reading achievement.

Is that hypothesis provable? Not in any final sense, though all the evidence that I know of is consistent with it. But it would be very easy to disprove it, to falsify it; all you have to do is find some successful readers who lack it.

You suggest that I am “gratuitously dismissive of other avenues of enquiry.” I disagree. I don't dismiss other avenues of enquiry; I welcome them, for truth is hard to come by. I avidly read many other myths (including fiction) looking for hypotheses about reading and its correlates. But I admit I am seldom compelled by them; I admit to having a methodological prejudice. As I said earlier, no qualitative researcher has ever shown me how you can conclude that X is related to Y without controlling potentially confounding variables (Bacon's rule). Until one does, I will continue to be skeptical of their claims.

But I would maintain that my decision to do this is not gratuitous; it is principled. The rule of our game is very simple, and anyone can play it. If you won't follow that rule, then you are not playing the game.

Is our method a direct line to truth? Certainly not. Truth *is* permanently elusive. But using this method, we have demonstrated that we are closer to it than we used to be, and not only in the reading field. Your (U.K.'s) new Chief Scientific Adviser, Robert May (1995), recently editorialized that “life in developed countries today differs strikingly from life 100 years ago, and scientific advances are the cause” (p. 1199). Earlier, you reminded us that Feyerabend argued that these advances came about because, as you put it, “scientists decided not to be bound by (or unwittingly broke) all the rules of scientific reasoning and methodology then current.” But I don't believe that any scientist has ever wittingly broken Bacon's rule.

In the end, though, I must agree that we should consider more than scientific rigor in evaluating manuscripts. For me, rigor is necessary; without it, I'll never be convinced that X is connected to Y. But it is far from sufficient; compellingness involves much more than that. Every issue of every scientific journal I pick up contains articles that fail to move me at all. The scientific method can certainly be applied foolishly, like the students at Rice University who are currently reporting their studies of Twinkies on the Internet. There is more to research (and life) than the scientific method. But as Steven Weinberg (1995) concludes,

I think we scientists need make no apologies. It seems to me that our science is a good model for intellectual activity. We believe in an objective truth that can be known, and at the same time we are always willing to reconsider, as we may be forced to, what we have previously accepted. This would not be a bad ideal for intellectual life of all sorts. (p. 13)

### Concluding statement

**Colin Harrison and Philip Gough:** This has been a conversation. Even though it was conducted electronically, it followed (at least some of) the discourse rules of a conversation, and not those of a research paper. Thus, the four conversational turns have not been reworked, and they have local rather than global rhetorical structures. The themes we reintroduced at the beginning are not brought together in a neat conclusion. Rather, as in an ordinary conversation, the conversation stopped because it was interrupted (by constraints of time and space, not to mention a remote electronic cattle goad), rather than because we had finished what each of us wanted to say.

*Editors' note:* We see this as the beginning of a Conversation. We hope you agree and join the discussion.

JER  
DMB

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