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REVIEW ARTICLE

Linguists play catchup with evolution

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Donald Loritz, *How the brain evolved language*. New York & Oxford: Oxford University Press, 1999. Pp. 227.

Lyle Jenkins, *Biolinguistics: exploring the biology of language*. Cambridge: Cambridge University Press, 2000. Pp. xiii + 264.

In the course of reviewing recent books on the evolution of language and communication (Dunbar 1996, Hauser 1996, Deacon 1997) I have had occasion to note that relatively few writers on these topics know much about linguistics, and to wish that more of them did. I should have remembered the old adage that one shouldn't wish for things – one might get them.

For more than a century, linguists honored the Linguistic Society of Paris's ban on all discussion of language evolution; other disciplines went ahead with it regardless. Now that the centrality of language evolution to any study of our species is becoming apparent, linguists are desperately trying to play catchup, and the two volumes reviewed here both appeared in the last couple of years. Both authors are linguists, albeit hyphenated ones. Donald Loritz teaches computational linguistics at Georgetown University; his doctorate was in psycholinguistics. Lyle Jenkins works in the Biolinguistics Institute in Cambridge, MA; however, his doctorate was in unhyphenated linguistics. It would be difficult to find two authors whose ideas were more diametrically opposed.

One hates to drag out the seven blind men of Hindustan and the elephant again, but I have yet to find a more appropriate metaphor for the field of language evolution. The ideal scholar in this field should combine a professional training in linguistics, paleoanthropology, evolutionary biology, neurology, psychology and primatology, at the very least. But human life is finite and all too short, so the best one can hope for is that researchers should combine a thorough grounding in at least one of these disciplines with a fairly extensive knowledge of, or at least respect for, all the others. Alas, few if any meet even this standard. And the problem is not so much a defect of intellect or knowledge as one of attitude.

The legendary Hindustanis could be excused their mistakes because these

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arose from an unalterable physical condition. Such an excuse is unavailable for language evolutionists. True, more is known about the relevant disciplines than ever before, but not, one would have thought, so much as to make the general outlines of this knowledge unlearnable by anyone willing to undertake the task. Yet over and over again one meets with work on language evolution where the most significant aspects of one or more of the relevant disciplines, or even entire disciplines along with their leading practitioners, are either totally ignored or, perhaps worse, trashed, for mainly doctrinal reasons. Anyone familiar with the early history of the Christian church must be struck by the similarities – battling bishops, unconstrained and wildly proliferating theories, accusations and counter-accusations of heresy. But like the blind Hindustanis, the church fathers had a good excuse: they had no objective, scientific standards against which their competing claims could be measured. For us, there's no such excuse.

What Loritz chooses to ignore consists of paleoanthropology, any evolutionary biology that relates to paleoanthropology, and surprisingly enough, linguistics, or at least the most significant work produced in that discipline over the last few decades. His is a strange book. Although its title refers explicitly to evolution, and although he states explicitly (16) that 'What is needed is a theory of how human language … arose as part of the same adaptive evolutionary process which led to *Homo loquens*', there's not a single subsequent mention of human evolution except the conjecture (140) that *Australopithecus africanus* could only say 'ta', or 'di', while *Homo habilis* could say 'tata' and 'didi', *Homo erectus* could say 'Tarzan' and 'Jane', early *Homo sapiens* could say 'Me Tarzan' and 'You Jane', and our own species can say 'I am Tarzan' and 'Thou art (sic) Jane'. And that's it. No justification, no explanation, no discussion of how meaning or structure crept in, or the social or ecological matrices in which language evolved. The author admits his conjecture is 'fanciful'; he inserts it because 'insofar as ontogeny recapitulates phylogeny, the evolutionary scheme is borne out' (140). 'Insofar as' – how far's that? And just what 'evolutionary scheme'? But Loritz is too eager to get to the next topic to elucidate such points.

I don't think I'm being unfair to the book when I say that what's in it and what's left out seem to be determined by wholly arbitrary criteria. For instance, the second chapter is devoted to a pretty basic, biology-textbook-type account of the evolution of brains up to the level of primitive fish. Then without apology or explanation, the next two chapters zoom straight into a synchronic description of the human brain, complete with diagrams of the Brodmann areas and the motor and sensory homunculi – again, pretty much what you'd find in an introductory neurology text. What relevance does this have to language evolution, or even to Loritz's own theories about how language works?

Not until chapter 5 does Loritz get to the point. His crucial concept is 'adaptive resonance'. Just as there are gated dipoles in the human visual

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system (so that when one stares at the red globe of the setting sun, for example, one occasionally sees the 'rebound' of the binary red-green opposition in terms of the legendary 'green flash'), so there are similar gated dipoles in every neural system, and hence in the language that some of those neural systems produce. The poles of these neural dipoles resonate with one another in ways that are 'adaptive' (quite how is never explicitly spelled out), leading not just to the rhythmic regularities of phonology, but also (via Case Grammar) to the construction of complex sentences.

Let's leave aside for a moment whether any such theory could have a reasonable foundation, and see how it impinges on the central issues of language evolution. To me it seems that explaining how language evolved turns on the answers to two crucial scientific questions. The first is, how and why one particular primate species, or one primate line of descent, came to develop a system of communication that allowed the transfer of (potentially) unlimited factual information, and the basic principles of which differed from those of all previous systems of communication. The second is how such a system acquired the very specific characteristics that the syntax of modern human languages exhibits. If one abbreviates these questions to 'How did meaningful units (words or signs) evolve?' and 'How did syntax evolve?', little is lost.

There are, needless to add, a number of ancillary questions. Is language continuous or discontinuous with other communication systems? Did language evolve gradually, abruptly, or in discrete stages? What if any were the linguistic capacities of antecedent species? What role if any did the social organization of humans play in either major development? However, the acid test of any work that deals with language evolution is first, whether it even deals with the two major issues, and second, how far it goes towards resolving them. The first issue Loritz doesn't address at all: his chapter on meaning (chapter 11) starts with a superficial discussion of William James and C. S. Peirce, and then hops across Kuhn, Magellan, Jakob Grimm, language teaching, the Enigma machine, machine translation, semantic networks and perceptrons, to the stunning conclusion: 'In this chapter I have described meaningful linguistic theories as a social state of *Homo sapiens*. Thought and meaning are both only stable when neurons or people are in a state of communicative resonance' (170). The second he does address, if only very obliquely, by his own idiosyncratic assessment of the basic properties of syntax.

He starts on the wrong foot by supposing that generative grammarians derive sentences like 'Behaviorism was slain by spoonerisms' (his example 1.10) from 'Spoonerisms slew behaviorism' (1.9) by means of a movement rule. That may have been true in the 1960s, but not since. And no-one, anywhere, ever even dreamed of claiming that 'in transforming 1.9 into 1.10, some *behaviorism*-cell and some *spoonerism*-cell actually change places in the brain' (20).

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Throughout chapter 10, which deals exclusively with syntax, Loritz is under the illusion that if he can suggest ways other than movement by which such sentence-pairs can be generated, then movement forms no part of grammar and he has 'dispensed with the need for generative linguistics' (160) in the course of a mere 18 pages. He totally fails to see that the issue is not whether there is movement or not, or whether movement is 'real' or not (whatever that means). The issue is that in a sentence such as *Who did John see__, who* bears some kind of linking relationship with the gap after *see* (otherwise the sentence would be baffling) and that the relation between gaps and their antecedents is often far less transparent than it is here. Take the following:

(1) (a) How did John want to fix the car?
    (b) How did John know to fix the car?
    (c) How did John tell you to fix the car?
    (d) How did John say where to fix the car?
(2) By tightening the fan-belt.

What has to be explained (among countless other things) is how we know that (2) is a possible answer to (1a) and (1c), but not to (1b) or (1d), while answers as disparate as *Because I told him it wasn't running well* or *In a rather rude and peremptory manner* are possible answers to (1b) and (1d) respectively, but not to (1a), although the second of these might also answer (1c). The generative solution – that there is some sort of principled relationship between *how* and some invisible gaps ('empty categories') in these sentences, such that there is a legitimate gap only in the lower clause in (1a), only in the higher clause in (1b) and (1d) but in both clauses in (1c) – may not be the whole story, or even the right one, but at worst it provides a coherent and self-consistent account of the differences.

Loritz's 'adaptive grammar' provides no account at all, and it is impossible to see how it could, even in principle. Only sentences without empty categories are discussed, indeed the term seems not to have entered Loritz's vocabulary. Sentences are supposed to be generated (155) by 'alternately outputting elements from the nominal topic gradient and the verbal relation gradient' (those dipoles again!), the latter being determined by the former and the former by discourse salience and/or new versus old information (try that on (1) and (2) above). When even the few linguistic facts adduced fail to fit this pattern, astonishing grammaticality judgements are made. In discussing how children learn that *Irv poured the glass with water* is ungrammatical (188–189), Loritz actually claims that this is because 'water' is 'hardly the sort of thing one would explicitly present as new information' but that more novel referents can take final position, as in the (fully grammatical, he claims) sentences of (3) (his 12.15 and 12.16).

(3) (a) Irv poured the glass with ice water.
    (b) Irv poured the glass with Chateau Petrus 1961.

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If he accepts these, he would surely accept the yet more novel (4):

(4) (a) The demolition crew poured the holes with gelignite.
(b) The Russians poured Chechnia with troops.
(c) Mary poured Bill with scorn.

As for the overall process of language acquisition and its rapid acceleration around age two, 'What triggers this sudden burst of language', he asks, 'if not walking?' (184). After all, walking contains 'a rhythmic dipole'. Then, remembering that paraplegic children also talk, he quickly adds that 'phylogenetically the bilateral dipole extends back to the fishes'. So why can't I hold conversations with a carp (though voiceless, it might still sign with its fins)?

Further quotation should be superfluous; the contribution this book makes to our understanding of how language evolved is effectively zero, and the fact that a distinguished university press saw fit to publish it signals something defective in that press's review process.

Jenkins' book, though far more substantive, is a little odd too, if in a very different way. While Loritz rejects the claims of generative grammar, Jenkins embraces them with an uncritical, indeed evangelistic fervor. Some measure of this can be taken by looking at the index entries under Chomsky. Chomsky is cited on 143 out of the 232 pages of text, a stunning 62%; of the first 33 pages, only two fail to mention or quote him, and on a number of them (15 through 17, for example) there is more text by Chomsky than by Jenkins. Outside of genres such as biography or biblical exegesis, such proportions are rare indeed. Indeed, at no time does Jenkins question or even mildly dissent from an opinion of Chomsky's, any more than a commentator on the Epistles of Paul would dissent from or question the Apostle – an uncritical acceptance he is far from extending to other authors.

Again, as with Loritz's book, there is a problem with the title. What exactly is biolinguistics? Although Jenkins claims that the term goes back as far as 1950 (1, fn. 1), it cannot have had the same sense then as he now endows it with, and the only other user he cites (John Locke) is ideologically far removed from him. Elsewhere he states that, 'We use the term "biolinguistics" for roughly the study of the five questions posed in the introduction,[1] but where the context is clear, we often use the shorter form, linguistics' (19). So the definition seems to be, 'Linguistics the way Jenkins (and Chomsky) think it ought to be done'. I'm not saying that isn't the right way; I just wonder why Jenkins thinks we need the additional term.

[1] Those questions are:
1) What constitutes knowledge of language?
2) How is this knowledge acquired?
3) How is this knowledge put to use?
4) What are the relevant brain mechanisms?
5) How does this knowledge evolve (in the species)?

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I suspect that the term is some kind of pre-emptive strike. Much of Jenkins' introduction, and the whole of his first chapter, is concerned with 'the unification problem' – that is, how to integrate linguistics with other sciences, in particular with biology. If the banns have already been read and have gone unchallenged, the marriage is all the likelier to take place. And if 'the syntactic computations of the language faculty are the biological evidence' (3), then all that is left is for biology to search out the mechanisms that underlie those computations and through which they came into existence.

Jenkins supports this last claim by a comparison with Mendelian genetics; Mendel's theories remained unappreciated for so long because he had only computational evidence and the underlying mechanisms that yielded the phenomena he discovered were still unknown. But the analogy is shaky. Mendel's proportional statistics were derived from physical experiments that yielded consistent results; the structures of syntax are derived (unavoidably) from introspective processes and theory-internal arguments that have yielded widely differing proposals over the forty-odd-year history of generative grammar. There can be little doubt that those proposals demonstrate a steady chronological progress towards understanding the fundamental principles of human language; there should be equally little doubt that saying we thoroughly understand those principles is, at best, premature. We have x number of theories of grammar, but there can be only one way in which the brain does language. We would have to be surer than we can yet be that we have correctly identified the syntactic computations the brain actually uses before we can claim those computations as biological evidence. Even then, they are not all the biological evidence.

Under all this (and much more clearly expressed in Jenkins' penultimate chapter on language evolution) is an idea that is absolutely necessary for every researcher in the field to understand and take to heart, although very few so far have shown even an awareness of its existence. That is, that no theory, whether social or cognitive, continuist or discontinuist, pan-adaptationist or neo-neo-Darwinian, can pretend to give an adequate account of language evolution unless it explains how and why syntax evolved so as to have the particular set of properties it possesses, rather than some other set.

If Jenkins had simply expounded this idea, then gone through the burgeoning literature and shown how almost every theory of language evolution is vitiated by the lack of it, he would have written a finer, if very different, book. Instead, he opts for a polemical approach, again with unnerving parallels to early Christian history. Just as many Christian theologians would handle pagans quite mildly, reserving their sharpest diatribes for those co-religionists who differed from them doctrinally, so Jenkins lets off empiricists and connectionists relatively lightly, but unleashes his assaults primarily on those who share his overall nativist position but

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who differ from Chomsky on one point or another. For instance, no non-nativist is accused of 'irrationality, incoherence, and vacuity' (189) as Steven Pinker, leading popularizer of nativist linguistics (Pinker 1994), is.

Once his indignation has been aroused, Jenkins is all too prone to wilful misunderstanding. For instance, on page 28, he cites Pinker as saying 'Chomsky's arguments for the innateness of language are based on technical analyses of word and sentence structure … I think converging evidence is crucial' (Pinker 1997a) and going on to list such 'converging evidence' as child development, genetic disorders and the like. Pinker's meaning is quite obvious: that such studies converge on the more technical linguistic studies and hopefully provide support for them. Jenkins, however (28–29), takes him to mean that these other lines of inquiry CONVERGE ON EACH OTHER, and that according to Pinker, Chomsky's linguistic analyses do not constitute comparable evidence at all.

At other times, a quick substitution of words enables Jenkins to attack statements that he should otherwise find unexceptionable. The writer of this review is cited as stating that 'there may be FEATURES IN THE INNATE LANGUAGE CAPACITY that do not necessarily surface in all human languages' (Bickerton 1984: 26, fn. 3, emphasis added).[2] Two lines after citing this (81), Jenkins redesigns it as 'the absurd conclusion that ALL LANGUAGE FEATURES surface in all human languages' (emphasis added).

However, Jenkins' primary bone of contention is natural selection. The greater part of his chapter on language evolution (chapter 5, over a third of the book) is devoted to an attack on what he describes as 'ultra-Darwinism' – the position that all evolutionary developments fall under natural selection. Against it, he proposes a 'neo-neo-Darwinist' explanation of evolution. Let's compare the two (emphasis throughout is mine). Here's Jenkins (169):

> Chomsky notes that no-one assumes that "every trait is specifically selected." Rather an organ might be developed for one purpose, and then be refined for a different purpose. He cites the standard case of THE ORIGIN OF INSECT WINGS AS THERMOREGULATORS … "In some cases it seems that organs develop to serve one purpose and, when they have reached a certain form in the evolutionary process, become available for different purposes, AT WHICH POINT THE PROCESSES OF NATURAL SELECTION MAY REFINE THEM FURTHER FOR THESE PURPOSES" (Chomsky 1988: 167).

And again (227): 'Chomsky has proposed that certain design features of human language could have arisen through "SHIFT OF FUNCTION" and subsequently come under selectional pressure.'

[2] The pagination Jenkins uses here is of a pre-print version, rather than the published article.

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And here's Pinker, arch-ultra-Darwinist (1997b: 170–171):

> THE THEORY OF THE EVOLUTION OF INSECT WINGS … far from being a refutation of adaptationism, is one of its finest moments … [P]roto-wings too small for flight are effective heat exchangers. The larger they grow, the more efficient they become at heat regulation, though they reach a point of diminishing returns. That point is in the range of sizes in which the panels could serve as effective wings. Beyond that point, they become more and more useful for flying as they grow larger and larger, up to their present size. NATURAL SELECTION COULD HAVE PUSHED FOR BIGGER WINGS THROUGHOUT THE RANGE from no wings to current wings, with a GRADUAL CHANGE OF FUNCTION in the middle sizes.

So what's all the barney about? My guess is, relatively minor differences of emphasis and rhetoric plus the good old primate desire to be alpha male, or at least, alpha male's top henchman. As for the rest of Jenkins' neo-neo-Darwinian approach, it is a strange mixture of Thompsonian Laws of Form, Fibonacci numbers, self-assembly and symmetry-breaking. Prior to the publication of Calvin & Bickerton (2000), an MIT Press referee asked why we hadn't discussed neo-neo-Darwinism. I replied that when neo-neo-Darwinism developed a coherent theory with predictions that could be tested, either logically or empirically, I would be happy to discuss it. That time had not arrived then, and Jenkins does nothing to repair the deficiency.

Indeed, he does not even need to. What has been variously described as 'preadaptation', 'exaptation', or 'change/shift of function' is in itself an adequate mechanism. The trouble lies not so much in the means proposed as in the way the problem is stated.

Everyone (Loritz and Jenkins included – almost the only thing they have in common) talks about the evolution of language. But, as noted earlier, the problem of language evolution is really two quite separate problems: how did meaningful units evolve, and how did syntax evolve? It would have been perfectly possible for a species to develop one without developing the other, and if the reasoning of Bickerton (1990) is correct, our ancestral species may have done just that for as long as two million years. If we merge or confuse the two issues, we will get nowhere; the seeming inability of the field to progress much beyond Darwin's speculations, despite our vastly increased knowledge in a dozen areas that bear on the problem, is in large part a consequence of this mistake.

The first problem does not require any shift of function: all that is needed for a structureless protolanguage is a species with the cognition of any ape, plus motivation and a viable modality (Bickerton 1990). The second does, because syntax is an even more radical innovation than meaningful symbols. It is the second problem, of course, that Jenkins is concerned with, but he is so busy defending Chomsky against Pinker (not to mention philosopher Daniel Dennett, biologist John Maynard Smith and a host of others) that he

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fails to see this clearly and accordingly fails to take on Pinker where he is weakest – his belief that syntax must have developed gradually (Pinker & Bloom 1990). Indeed, with a couple of exceptions (discussed below) he does not seriously engage with any substantive work bearing on language evolution. He is too busy showing that other sciences have had problems similar to those of linguistics when theoretical advances outran knowledge of underlying mechanisms (oblivious to his own admission that 'an analogy never proves anything', 68). He is too busy trying to garner evidence that, contra the opinion of many, Chomsky really is interested in the evolution of language, and really has done substantive work towards the understanding of it, despite Chomsky's own remark (which Jenkins himself cites on page 13) that both the evolution of language and the neural mechanisms supporting it 'appear to be beyond serious inquiry at the moment' (Chomsky & Lasnik 1993: 509).

If Chomsky had been seriously interested in the evolution of language, he would have developed his ideas by first trying to identify a plausible candidate for a function-changing faculty, and then testing his choice by determining whether that faculty could plausibly have given rise to some of the core principles of syntax. This not only could be done, it has been done. Bickerton (2000) proposes the derivation of syntax from a kind of social calculus designed by primates to make reciprocal altruism work more efficiently, which yields argument structure; Calvin & Bickerton (2000) shows that the addition of argument structure to a formless protolanguage can, if the capacities of an enlarged brain are added, yield the basic structural principles of syntax. Whether this proposal is correct it is, of course, far too early to tell; however, it is concrete enough to test (the appendix to Calvin & Bickerton 2000 seeks all too briefly to test it against the findings of contemporary linguistics). Were Jenkins right about Chomsky's interest in evolution, Chomsky could have had some alternative proposal up and running a decade or more ago.

But perhaps the real reason why Jenkins, along with Loritz, fails to really get to grips with language-evolutionary issues is due to the defect noted at the beginning of this review: drawing evidence from only a subset of the relevant disciplines. Jenkins, like Loritz, totally ignores paleoanthropology. True, the fossil and archaeological records are slender, and have often been abused to yield outrageous just-so stories. But they are not too slender to yield plausible inferences as to the levels of cognitive and linguistic capacity attained by our ancestors at different periods, and new evidence arrives almost daily. Even more importantly, they put the whole issue in the context of evolutionary biology, and by imposing a set of constraints quite different from those of linguistics or neurology, help to shrink the problem space and reduce the cancerous proliferation of flawed or incomplete theories. Language evolution is the story both of a faculty and of the species that developed that faculty;

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it makes no sense to ignore either of the two, although all too many scholars in the field do just that.

Having had some negative reactions to Jenkins' book, I am glad to be able to record some more positive ones. His discussions of ape language experiments (208–218) and the work of Terence Deacon (215–227) overlap and merge into one another, but both strike to the core of their subject-matter and show a level of insight one wishes he had deployed throughout the rest of the book.

Jenkins skillfully deflates the grossly exaggerated claims for Kanzi in particular and bonobos in general, shrewdly juxtaposes these with the onslaught on Ebonics – people willing to grant language to apes seemed equally willing to deny it to blacks – and correctly describes the purely ideological nature of both. He goes on to make a great many eminently sensible remarks about the scientific debate on ape 'language', showing among other things that to claim apes have a language capacity comparable to that of human children at some particular age is meaningless unless we know the neural capacities that underlie each species' communicative behavior (and that allow, in our case but not theirs, a potentially infinite number of sentences to be produced).

Deacon (1997) is another of those overly impressed by Kanzi. Jenkins points out the vacuity of his claim that both Kanzi and human children acquire language because of their immaturity, and pours well-merited scorn on Deacon's quasi-mystical notion that languages somehow exist in their own right, outside the human brain, which they merely parasitize like some kind of virus. He distinguishes (as Deacon and far too many others do not) between language change and language evolution; it is of course nonsense to speak of the evolution of English, or Indo-European, or any other variety of human language, since only language with a big L has evolved. He correctly attributes the errors committed by Deacon (a fine writer, as long as he sticks to his own specialty, the structure of the brain) to the latter's ignorance or lack of understanding of both language and linguistic theory.

There are other good things in this book – Jenkins' demolition of the false 'experience-dependent'/'experience-independent' dichotomy (92–93), for instance – but they are widely scattered amongst unconvincing analogies, repetitive and often nit-picking polemics, and capsule summaries of complex issues too brief to be accurate, let alone revealing (developmental dyspraxia, half a page; developmental dysphasia, two pages; Williams syndrome, half a page; and so on). This is a great pity, since the book could have given some salutary and desperately needed lessons to all those (and there are far too many of them) who write about language evolution in blissful ignorance of what it is that has evolved.

Few of these, I fear, will listen, and fewer still take the lessons to heart. Jenkins' uncritical acceptance of his party's party line and his refusal to see how any rational person could fail to be convinced by it are calculated to

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irritate and repel precisely those who would most have benefited from his work. His book, though far more thoughtful than Loritz's, has equally helped to show me what was wrong with my belief that the state of the art would be improved if more books about language evolution were written by people who knew some linguistics.

That's not to say that we can dispense with such knowledge – very far from it! – but rather that, vital though it is, it is not in itself enough. The field of language evolution lies at a point in cognitive space where a number of diverse disciplines and their equally diverse cultures and belief-systems intersect. Scholars who want to obtain worthwhile results there have to be aware of this and realize that if they ignore relevant results from any of these disciplines, they do so at their peril. They must realize too that more than mere knowledge is required. An interdisciplinary field demands that those who enter it do so with some degree of openness to unfamiliar or even superficially repellant modes of thought. While we should continue to censure theories that are demonstrably untenable in light of current knowledge, we should tread very carefully when on unfamiliar terrain, and particularly when any reputable body of scholars in another discipline holds opinions orthogonal or contrary to our own. They COULD all be fools, but it's likelier that they have their reasons and that we would benefit from learning what those reasons are. We ourselves would discover new things, and we might even learn how to convince them that we are right, after all.

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