

Advocacy of thinking differently: flame on!

Being within a week of my 58th birthday, and having long ago given up thoughts that unorthodox thinking if politely presented may positively affect my future, I here present some thoughts about physics, physicists, and the shortcomings of same. Some of what is written below may be considered impolitic, impolite venting.

I begin with one of my favorite guesses about the nature of our universe. Beneath our bosonic pseudo-orthogonal geometry, however many dimensions of space and time there be, lies a fermionic symplectic geometry from which our observed geometry arises. I came to this conclusion when I extended the definition of a Clifford algebra to encompass both pseudo-orthogonal and symplectic bilinear forms. Peter van Nieuwenhuizen also once entertained a similar notion back in the 1970's (I have a copy of the paper somewhere, quite possibly never published), feeling uncomfortable in the early days of supersymmetry with the idea of tacking onto a bosonic geometry fermionic Grassmann variables. I personally have felt from the outset that this was a thoroughly loony idea, and ugly in the way trying to stick a picture on a wall with wet chewing gum is ugly. You know those little voices that whisper in your ear when something is not a good idea? Well, physicists should listen more closely to them, and less to the unctuous mutual congratulatory voices of their peers. This, by the way, is true even if the idea stated above is pure drivel.

If not drivel, attempts to unify quantum mechanics (QM) with general relativity (GR) are in that case doomed, because GR is based upon geometric ideas that are secondary, not primary. In any case, QM and GR should not be unified, they should be derived.

This is very unlikely to happen, because physicists are in general too embarrassed to ask and/or think about the kinds of questions the answers to which are so far out of sight we can not within extant paradigms begin to formulate an approach to their resolution. And mathematicians spend way too much time refining the tools used to study mathematics, and too little listening to the message of mathematics.

String theory will never succeed. It is founded on ideas several levels of profundity below where it needs to be.

Unfortunately string theory may also never fail. (Note to Greene and Kaku, and other media spokesmen for ST: earnestness, even when backed up by a saccharine musical score and script, while able to sway the weak-willed, does not equal truth. And to all the other ST apologists: proclaiming an idea that is failing better than the alternatives is no excuse not to look for one, although it is frequently presented as such. For cryin' out loud, get some gumption.*)

Smolin is correct in almost everything he said in *The Trouble With Physics: The Rise of String Theory, the Fall of a Science, and What Comes Next*. However, in that all the ideas he cited worthy of more attention were his own, and/or those of friends, it seems unlikely theoretical physics will ever achieve the kind of openness he so lucidly espoused, if even that revolutionary tome should contain such a noticeable thread of the us-vs-them mentality he was deriding.

Penrose's book, *The Road to Reality: A Complete Guide to the Laws of the Universe*, contained an even stronger thread, but its intention was a more blatant advocacy of his own ideas at the expense of alternatives, be they mainstream or drifting in the theoretical limbo. Because my own work made it into Penrose's bibliography I was congratulated a couple of times. This is like congratulating a friend who makes it onto the evening news because he was mugged. My work exploits all the division algebras. Penrose is infatuated with the complex numbers alone. My work made it into his bibliography in his effort to diminish the need for anything beyond the complex numbers. While I am grateful for the inclusion even under these circumstances, he needn't have bothered. Unless he knows something I don't, my work is not a threat. Were it not for google and the internet it would be nearly invisible.

Well well, were I in their shoes (and I am, although the shoes are smaller), I can't say I'd behave very much differently. In fact, my 1994 book, *Division Algebras: Octonions, Quaternions, Complex Numbers and the Algebraic Design of Physics*, begins with a similar self-serving critique of the theoretical ruling class.

The main tenet of that book is something that I believed then, believe even more now, and am convinced is essential to true advancement in theoretical physics. It is a principle, and has formed the foundation of all my work over the last 30 years - actually 40, since I harbored the same basic belief in my late teens, and it directed everything I did in the decades that followed. It's that kind of principle, one that can

direct thinking, but did not initially provide a foundation - like the unity of space and time - upon which to build a theory. This principle begins by acknowledging that in mathematics there are resonances, mathematical objects that burst with richness compared to similar objects in the same class, and that these resonant mathematical structures are intimately linked to the design of our physical reality (although linked is far too weak a word to describe what I really mean by this).

Smolin's notion of what constitutes a valid founding principle is more Einsteinian. One observes things as they are and postulates relationships that, if true, not only unify previously disparate notions, but lead beyond them to predictions of things that may be true but were unknown. The resonant mathematics principle I have discussed in much of my work is of a different sort, and resides further out. Let me explain.

There is a question that one sometimes - in fact, very rarely - hears when physicists gather to discuss their work. This question is never asked seriously, but only broached to make the point that our ignorance is vaster than our arrogance.

Why is there anything?

This question is very far out. Whatever successes may ultimately come from the competing approaches to unification, to the best of my knowledge none of them claims that an answer to this question will be part of the result. Even more, the very idea - the question itself - is not taken seriously, and not so much because it is not considered a valid question, but it is deemed so far beyond the reach of our science that effort expended in search of an answer is considered effort wasted. I don't disagree. I can't even completely convince myself that we are capable of understanding the answer - be there one - if handed to us on a silver platter.

Still, between this question, and those that motivate the core of current mainstream research, there is a vast sea of other profound questions, mostly unformulated, but needing some day to be addressed (or at least formulated). In my opinion, out there in that sea of unformulated questions are associated answers that will not unify GR and QM, for example, but will derive and/or supplant both with some more profoundly fundamental core of ideas. Neither GR or QM are completely satisfying theories, and the futile decades of research to link the two are filled with so many ugly and slapdash notions that it should be embarrassing. So many times over the last 30 years have I attended talks by exalted theorists who claim that idea X is not only correct, but will be shown by experiment within months to be so. As I believe I wondered in the introduction to my book, suppose any of the idea X's had been shown to be correct: how then would you explain why it was inevitable that it be so? None of them had that feeling of inevitability that should accompany a truly good idea. Of course, in most instances these grotesquely exaggerated espousals of mediocre ideas arise out of a need to maintain funding for those working in the field. As Smolin eloquently demonstrates, string techniques (not a theory) have achieved a kind of apotheosis in this regard: great mediocrity and even greater enthusiasm for same.

We are in a quagmire, but in truth that is our natural state. Were it not for intrusive observations of reality and the occasional persuasive iconoclast periodically prodding us out of our dogmatic torpidity, we as a species all too easily settle into comfortably familiar territory filled with increasingly complex sterile discussions of too well done arcana. Any intellectual and/or artistic human endeavor that reaches a point where the obstacles to advancement become nearly insurmountable will devolve into a pseudo-religious lethargy.

I have almost no faith that anything truly exciting will come out of the community of theoretical physicists in my life-time. Like Smolin, I saw the spark fizzle out over 25 years ago. Some time during the 1970's it became less and less interesting for me to attend Boston area colloquia. I haven't been to one in years.

Most researchers who harbor thoughts outside the confines of mainstream dogma generally present their ideas far too carefully, fearing career crushing ridicule. Even Smolin, despite the inflammatory title of his book, minced his words and message considerably within its pages. I suspect in the short run this will serve him well. But I gave up on a career in this science long ago, and I am now nearly 60. Why the hell at this point in my life should I mince?

In truth, however, I don't have much more to say. My work has amply demonstrated the power of the mathematical resonance principle. It derives the Standard Model of quarks, leptons, and their interactions, and makes predictions that go beyond this. The resonances exploited to this end were the real normed division algebras: the reals **R**, the complex numbers **C**, the quaternions **H**, and the octonions **O**. This

success led me to investigate a collection of resonant laminated lattices: A_2 , E_8 , and Λ_{24} (Leech lattice). In my last publication (JMP, 10.2004) I used these three

resonant lattices to help motivate a new kind of spinor space for the three families of leptons and quarks ($\mathbf{T}^6 = \mathbf{C}^1 \otimes \mathbf{H}^2 \otimes \mathbf{O}^3$). This connected with some of my more purely mathematical work, and to its possible connection with physics. It gave rise to something that is not quite a conjecture, but merely an idea that would not surprise me at all were it shown someday to be true.

If part of what we presently treat as a continuum turns out to be discrete, then part of that will be this fermionic space, achieved by replacing \mathbf{T}^6 with its discrete version, namely $A_2 \times E_8 \times \Lambda_{24}$.

Every bit of my work may be wrong, as well as everything I conjecture or suspect - unlikely, but possible. What is not wrong is that we shall almost certainly never know. Forget my work. There are many others out there in the intellectual hinterlands, and the farther out they are, the worse their predicament. The timidity the most talented of mainstream researchers inject into their work in an effort to maintain careers and reputations will forever preclude advancement into the heady realms we need to investigate to understand our universe. And, of course, who has the time to wallow in the ideas of dreamers?

Some 30 years ago theoretical physics began to grow decrepit and, quite frankly, boring. Some 30 years from now I will be dead (or, if not, at least decrepit and boring). So I am nearly, but not quite yet, beyond caring.

Geoffrey Dixon
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* On the subject of gumption, I have never encountered a history of any science that was not replete with stories of how some idea presently accepted as fundamental was greeted at the time of its inception with dismissive ridicule from an entrenched majority seeking to retain its power and influence, and otherwise psychologically incapable of surrendering long held beliefs.

In the words of Mark Twain: "Whenever you find yourself on the side of the majority, it's time to pause and reflect."

The problem with being on the side of the majority is that once there the supportive voices of this majority corrode our ability to think critically and independently, so pausing and reflecting is not something most of us are any longer capable of. Ask yourself a couple of questions:

Was I herded into working in my present field?

If no one else on the planet was working in this area, would I feel compelled by the promise of its ideas to carry on alone, despite the potential stigma of being labeled a crackpot?

If your answer to either of these questions is 42, then you most certainly are a crackpot.

Geoffrey Dixon "The past can intellectually enrich or pollute the present."
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