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MORE FROM THE MAINSTREAM

by Miles Mathis

Yesterday [I published a partial set of emails](#) I got from a major university several years ago. Today I will share with you some information from a more recent set of emails, this time from the private sector. These two papers were written not only to answer my critics, but to give my regular readers a better idea of what is really going on behind the scenes. Even many of my best readers seem to think I am more marginalized than I am. They know there is a lot of chatter about me on the internet, but because my critics tend to be so vociferous and annoying—and since they are not above spreading false information—some readers get the impression that my ideas, though interesting, are going nowhere. And because I haven't taken time to do much “normal” PR, this adds to the confusion. For the most part, I just publish my papers and let the rest take care of itself. In a world that is used to videos and lectures and handshaking and ubiquitous testimonials by the rich and famous, my public relations can only seem a bit anemic, not to say backwards.

Of course this isn't a proper way to judge the popularity of a theory—supposing you were interested in things like popularity. It would be like judging a youtube video based on the most outrageous negative comments by the most annoying trolls, rather than on the view total or on the positive comments by sensible people. Which is why I am sharing what some sensible people have been saying. Up to now, you have only gotten that from my book blurbs, and I have kept the bulk to myself. The blurbs have been glowing in the extreme, that is to say, but they have been very limited. Three books, less than 15 blurbs.

As with my last paper, this paper will share only one set of emails, and a partial set at that. All information will be anonymized, to protect my source. It will also serve to protect his research. These emails are from an experimental physicist working for a large private company. This was his initial email:

I read most of your papers and I am not asking you to read any of mine. I am an empiricist, and I am looking for some novel input about the influence of Nuclear Magnetic Resonance on the stability of various nuclei for my future experiments. Can you relate this issue to your unique understanding of matter in the charge field?

I told him I could, and asked if he had read all the nuclear papers, including the most recent on Iron.

Yes, I read your recent [paper on period 4](#). The collisions with neutrons and outer spin reversals make sense. The cloud chamber track erasing hypothesis raised many eyebrows [he is talking about my paper [quark.html](#), where I redefine beta decay as a real collision].

I am considering whether Nuclear Magnetic Resonance precession is capable of exposing those inner neutrons more than in "normal" conditions. There is a large body of evidence showing that a static magnetic field does not affect the rate of beta decay. A static field might affect the direction of beta decay

(see β NMR) or paths of charged beta decay products via Lorentz deflection, but it does not affect the rate.

For some reason, flipping the nuclei 90deg. on their sides by NMR makes a difference and increases this rate of β decay. All of my colleagues tell me that this is impossible, but they grow silent when I show them the GM tube counts.

I then told him how and why this was happening, using my diagrams to illustrate the actual paths. I also suggested that he try a different element than the one he was accustomed to, since according to my predictions, this new element should be easier to knock neutrons out of. He did try it, and I turned out to be correct. In fact, I was *so* correct that his energy out was too much for his configuration.

I just tried a Xelement disk with a static magnetic field of X Gauss and a perpendicular alternating field of XkHz. Beta particles generated in this experiment are very energetic (well over XMeV) which gave me problems containing them in a disk of that radius because it required a static magnetic field over X Gauss for circular confinement, which I was not prepared to generate.

Upon discovering my ability to answer his questions with ease, he replied,

I am quite satisfied that there is a model for these interactions after all. Legacy science leaves me completely out in the cold when I do these experiments. For example, I can easily find the "decay energies" for the unstable element X, see here....but I cannot find anywhere the expected "decay energies" for element X2 (the most abundant stable isotope) under NMR excitation, because it is...well ..."stable". Without knowing the speeds of these beta particles it is very difficult to calculate the expected cyclotron radius in a given magnetic field. Does your model allow for the calculation of beta particles coming out of the element X nucleus?

Also, I am still struggling to understand, really understand mechanically, why the polarization of nuclear spin axes with static magnetic field does not affect nuclear beta "decay" rates, but the addition of a perpendicular high frequency alternating field at the Larmor frequency, does.

I sent him more nuclear diagrams to show exactly why the direction of input was important, and informed him that I could also calculate the energy of the beta particles.

Miles,

Thanks, for your input. Since last time I wrote my experiment became more complicated. I was able to confirm a double-extrema function for the intensity of stimulated beta decay vs. NMR excitation pulse length.

The existence of two maximums suggests that you were correct in predicting that maximum stimulation does not occur exactly at 90deg nuclear spin precession, but at some angles a little below and a little above 90deg....

That's highly interesting, wouldn't you say? What it means is that all the mainstream stuffed shirts telling each other my theories have no experimental confirmation are dead wrong. They have always been wrong, and my papers have shown experimental evidence for my theories that goes back to Maxwell and before. But since mainstream physicists now dismiss any experiment that wasn't done this decade as ancient history—unless it is one of the legacy experiments they spin to confirm their

own theories—they always find a way to ignore that. How will they ignore this? I predict they will say this guy hasn't published results of his experiment in a peer-reviewed journal, so we have no confirmation. Two problems with that: one, this is not a university physicist who gets his jollies from peer-review publications. This is a working physicist who is doing research for the private sector. He doesn't wish to publish, he wishes to keep the information to himself. Two, supposing he did submit his research for peer review and publication, what are the odds it would get published if he mentioned my name? Zero? The paper would never be accepted, and if it were, they would demand he conform his theory to mainstream theory. He would have to explain the new evidence using the standard model. The fact that I used my own nuclear diagrams, making predictions that turned out to be true, would mean nothing to them. Physics isn't about confirming straightforward mechanical predictions with experiments anymore, it is about forcefitting all experiment into existing theory, to add to the prestige of the existing big dogs. Unless the editors can find some way to do that, they won't risk publication. You know that as well as I do.

This means that real physics can now get done only outside of academia. We saw that clearly in my paper from yesterday, where we saw the mainstream university and government physicist admitting that he couldn't hope to do what I had done from the inside. To do any real physics, he saw that he would have to quit. Since questioning any major tenet of mainstream physics is now seen as insubordinate and dangerous, a person like me wouldn't last five minutes in academia. For all the talk we hear of academic freedom, an open dialog, and so on, we find that is just talk. In the real world, you shut up and calculate, no matter whether you are a graduate student or a tenured professor. If you start telling any inconvenient truth, not even tenure will protect you. Ask Ward Churchill.

Which means that my future either lies on the path I am already on, or it lies in the private sector. As we have seen above, the private sector requires real results, not fudged equations and a load of PR. “Legacy physics” has been useless to the working physicist above, so useless that he ended up coming to me, the pariah of the mainstream. The infidel. The iconoclast. Curiously, what the vaunted mainstream could not tell him in a decade, I could tell him in a matter of minutes.

I also recommend you catalog what he said about **The cloud chamber track erasing hypothesis raised many eyebrows**. *Many* eyebrows, not just his. I hear stuff like that all the time, and what it means is that many of my theories are making the rounds, not just on the internet but in the universities and private companies. My new solutions to old problems are being talked about and seriously considered by working physicists. Do you know any other “internet crank” that can say that? I don't.

Some readers will wonder why I didn't include my end of the emails, and why I blocked all the juiciest information, like the real numbers. Well, because the purpose of this paper was not to give away all my specific information for free. It was to suggest that the information is good information, based on correct theory, and that real physicists recognize that fact. If you want the real numbers applied to specific experiments, I guess you will have to hire me.