Reply to "Does Radioactivity Correlate with the Annual Orbit of Earth around Sun?" by G. W. Bruhn

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Initially, I had indeed been wondering what an analysis of this 20-years-old experimental data by a professional mathematician could possibly reveal. Now, I'm wondering whether or not it will ever be possible to put sequences of words in such a way that nobody will be able to misinterpret them.

This experiment was, as stated in my original paper as well as by G.W. Bruhn, one with several unknown parameters (the exact decay of tritium and the aging and degeneration of the various aspects of the recording device), but with only one set of experimental data. I can understand that some mathematicians may feel uneasy about that. But in physics, such situations are not so rare. For example, the famous redshift of the spectrum of light from distant galaxies is interpreted by most mainstream physicists as "flight" of these galaxies, *assuming* that light does not loose any energy while travelling long distances. In general, physicists are trying to make *reasonable* assumptions in such cases in order to progress any further.

Bruhn apparently *assumes* that not only the decay of tritium, but also the aging and degeneration of the recording device are strictly exponential. Since the experimental data reveals something else, in fact it reveals a slight and smooth "bend" in the curve, he *consequently assumes* that the exponential aging and degeneration rate of the recording device must have changed more or less abruptly by some mysterious physical impact in the middle of the observation interval. Mathematically, this might look attractive. But from a physical point of view, this is one of the most *unreasonable* assumptions I can think of. Firstly, as I pointed out in my paper, the apparatus was constructed explicitly to avoid any seasonal or other changes in the conditions. Secondly, if some external event had disturbed the apparatus around that time, I would have immediately stopped the experiment and hardly published anything about it, since I would have considered the observation interval (approx. 9 months) as much too short to derive anything meaningful from it. And thirdly, if indeed some mysterious physical impact should have occurred, the curve would probably show some "jump" rather than a slight and smooth "bend."

It is trivial that in cases like that, if you *tune* your assumptions without considering their reasonability, but with the goal to "prove" a null result, you will eventually reach that goal. Bruhn's paper is a good example of this.

One of Bruhn's misconceptions clearly shows up in his discussion of a comment by one of the reviewers (section 4). Bruhn apparently assumes that for a meaningful data analysis, all you need is a sufficiently large number of measurements. This is true in simple cases only, where for example variations of measured values are due to noise, but obviously not in cases such as the present one, where additional periodic variations might occur, and where consequently

the number of periods fitting into the observation interval is relevant too.

In contrast to Bruhn's analysis, I made assumptions which are physically much more probable and reasonable. My first assumption was that the overall aging and degeneration of the recording device, according to its construction, did not correlate significantly with the seasons, and was aperiodic. My second assumption was that this aging and degeneration was non-exponential, but nevertheless smooth. Aging processes may in some cases be exponential, but are certainly not so in general. There are numerous examples of non-exponential, yet smooth aging processes. I tried several non-exponential, aperiodic and smooth aging approximations with quite similar results. The reason that they are similar is that the observation interval was relatively short (regrettably due to personal, non-technical reasons only approx. 18 months). I finally picked the one which resulted in the smallest periodic deviation. In that way, I got a *minimum estimate* of the periodic deviation.

I will not comment in any detail about Bruhn´s discussion on "true" *vs*. "optimal" solutions (section 3), since it is completely out of tune with my paper. I did not use the terms 'true' or 'prove' in the context of the experimental data or of my analysis, rather I spoke of an 'educated guess'. I suspect that Bruhn was misinterpreting some of my statements and erroneously classifying my epistemic position as "naïve-realistic."

What remains to be said is that, if anyone with an open mind finds the perspectives of this kind of experiment promising, and has furthermore the facilities to perform high-precision, high-stability, long-term experiments (for at least two years), he or she should go ahead.