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Comment on egusphere-2022-569

Anonymous Referee #2

Referee comment on "Celestial Mechanics and Estimating the Termination of the Holocene" by John Abele Parmentola, EGUsphere, https://doi.org/10.5194/egusphere-2022-569-RC2, 2022

The first issue here is terminological, but is indicative of major problems throughout the paper:

In mathematics and physics including geophysics, generally, "convolution" and "deconvolution" refer to a specific mathematical operation, equivalent to multiplying and dividing fourier transforms. The author seems to think a "deconvolution" model means an "explanatory" model, but if there is any deconvolution or convolution going on here, I've completely missed it. The "model" is a quasi-instantaneous linear forcing represented through the simple Eq. 3 with no reference to the physics or chemistry of the climate system.

More generally, this manuscript seems to represent a big step backwards: among other

issues, the author implies, without ever saying so, that the climate system on these time

scales is linear. But even the original Hayes et al. paper referred to Milankovich "pacing" of

glacial intervals, recognizing that the relationship between forcing and response was not

that of a linear system. In the intervening time interval, a number of papers (see for example,

Tziperman et al., 2006, Paleoceanography, or various papers by Peter Ditlevsen) have dealt explicitly with the anticipated results of forcing a nonlinear system with quasi-periodic deterministic driving. None of this is even mentioned.

The paper is really a collection of wiggly lines. But a huge literature exists on the response of linear and nonlinear systems to deterministic narrow-band (in the Fourier sense) forcing. (Studies of ordinary ocean tides are one much discussed, analogous, application dating back to the 19th Century.) The author repeatedly invokes "correlation" between two wiggly lines---all based on some perceived visual resemblance. But the human eye is notoriously poor at separating apparent patterns from real ones (a branch of psychology known as pareidolia), and for that reason, specific statistical tools have been developed to obtain objective tests. It is a truism of data analysis that even two unrelated records will show a correlation, both numerical and visual, and hence a level of no significance is *always* necessary. In most cases here, one wonders what would have led the author to reject his own hypothesis of apparent correlation? The model being used is fully deterministic and the distribution e.g., of zero-crossing times, is also deterministic unless the system is nonlinear and chaotic. Deviations from claimed correlations are explained away by arm-waving stories (lines 517+ are one example). The use of half-cycles, rather than the usual use of frequencies, here would imply some kind of obscure rectification mechanism at work. Furthermore, the paper takes no account of any kind of noise in the ice core records, including issues of dating.

Smaller issues:

the 65degree N dependence is invoked without any discussion of its relevance, particularly to an individual ice core.

No error bars are placed on any of the ice core timings (e.g, Fig. 1)

Several figures refer to curve fits to data points (e.g., line 150), but with no specification

of what constituted those curves.

Why don't the author's hypotheses operate in the interval before 1MY BP, when the

glacial-interglacial intervals are widely believed to have occurred only at ~40KY?

Was the precession index ever defined? Is A truly time-independent?

Line 97 and elsewhere, what the author means by aperiodic would be the simple beating of two or more nearby frequencies--each individually periodic.

Line 299. Why is it surprising?

Line 413. What is meant by "judiciously"?