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Comment on egusphere-2022-569

Anonymous Referee #1

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-RC1, 2022

Review of "Celestial Mechanics and Estimating the Termination of the Holocene"

Summary

The article claims (in the abstract) to address several issues regarding the Milankovitch theory and "its relationship to palaeoclimate data over the last 800,000 years". The author observes that a substantial number of papers have connected astronomical parameters with "palaeoclimate data" but still fail to completely account for the interglacial and glacial durations, and the "timing" of prominent excursion in palaeoclimate data. He also remarks that some form of "pattern recognition" has enabled "quantitative estimates of the Holocene termination" but implies that this pattern recognition has not been successfully been applied to other terminations.

The originality of the approach presented here is to examine the percentage change between successive mean-daily-insolation maxima and minima at 65N. This metric is, in principle defined only at successive insolation maxima, but it is being interpolated and then partitioned into its precession and obliquity components, which themselves are characterised by cycle durations (e.g. his Figures 4, 5, 6).

Figure 12 suggests a match between the precession component of the variations in the metric, and the duration of glacial-interglacial cycles. The match is admittedly intriguing (if not perfect) but the article does not provide a physical interpretation. A second result (Figures 13 and 14) suggests a (less clear) match between the obliquity component of the metric and the amplitude of interglacial maxima. Timings match at the cost of a 10,000 year temporal shift, which the author attributes to timing inaccuracies.

The author then comments on the timing of the next glacial inception.

Comments

Position with respect to the state of the art

Quite a number of phenomenological models connecting astronomical forcing have been reasonably convincing at predicting the timing of glacial-interglacial transitions (many of these models take the form a dynamical system) but it is fair that the duration of interglacials, and the interglacial/glacial transitions may have received a bit less attention than the timing of terminations. The argument about "pattern recognition" brought about by the author is not obvious to me because, precisely, such rules are derived by examining all interglacials, see, e.g.,

Tzedakis P. C., J. E. T. Channell, D. A. Hodell, H. F. Kleiven and L. C. Skinner (2012), Determining the natural length of the current~interglacial, Nature Geoscience, (5) 138--141 doi:10.1038/ngeo1358 and Tzedakis P. C., E. W. Wolff, L. C. Skinner, V. Brovkin, D. A. Hodell, J. F. McManus and D. Raynaud (2012), Can we predict the duration of an interglacial?, Climate of the Past, (8) 1473–1485 doi:10.5194/cp-8-1473-2012

No physical modelling

It should be made clear that the current contribution focuses entirely on the one hand, on insolation at 65N and astronomical components, and, on the other hand, the EPICA Dome C Deuterium curve. There are no explicitly physical assumption linking both. When the author uses the word 'model', it is to name the "deconvolution model" that decomposes the metric into its obliquity and precession components. One decomposition is based on arithmetic developments (eqs 1 to 10) and compared with a numerical deconvolution.

I could not read a justification for using 'percent change between two successive maxima' as a reference metric. There is no argument about its physical relevance, and there is no empirical evidence (here or in the literature) that using this metric provides superior results to other, more standard, metrics. For example, the obliquity imprint on the EDC curve would be more straightforwardly explained as an influence of annual mean insolation (controlled by obliquity) on Southern Ocean sea-surface temperatures.

Lack of method description

Numerical deconvolution and estimates of instantaneous amplitude and frequencies have become fairly standard in astronomical theory of palaeoclimates and cyclostratigraphy. The author does not give much detail about the numerical methods used here. He explains that he uses "Laskar's tool", which is a reference to the IMCCE website. As far as I could tell the website generates insolation for user-supplied latitudes and seasons but (again as far as I could tell) does not make demodulation.

Questions about equations

- equation (1) featuring a product between obliquity and precession is not justified. Insolation anomalies would have come much more naturally as as sum of obliquity and precession components, not a product.
- I suspect indices a and p are swapped in equations (4) and (6)

Lose semantics

- the author says that between maxima the curve "should not be trusted for numerical precision", but given that the percent change in successive maxima is only defined at maximum points, the meaning to be given to "numerical precision" is not clear
- some sentences are clumsy. E.g. l. 136: "According to the Milankovitch hypothesis, their determination [I understand, of the behaviours of astronomical parameters] provides a consistent temporal calibration that should correlate insolation changes with features of palaeoclimate data. Things could have been stated more clearly. The discussion about "ameliorating timing differences" through "constructive and destructive interference" is somewhat obscure. l. 214: "While the time scale of the precession index contribution to insolation is affected by eccentricity, its short-term half-cycle is primarily due to precession" remained impenetrable I am afraid. l. 547: "both MIS 18d and 13c represent deep ice cores". With some good will one can see what the author is referring to, but semantics are inaccurate.
- aperiodic and quasiperiodic seem to be used interchangeably, while they should not.

Conclusions about the "catastrophic consequences to future of civilization from another ice age" really need to be recast and put in context given that the catastrophe we are all facing is definitely not that of a coming ice age.

Relevance of Appendix A is arguable and seem to be mostly standard material about insolation but I would leave it to the editor.

Conclusion

A lot of work has been put in this article and I feel sorry to come with a recommendation that sounds negative. Given the countless possibilities given by playing with insolation curves and data, it seems to me that new metrics have to be introduced with care, and the possible link with the palaeoclimate proxy for climate, which they are supposed to explain, have to be physically justified. This is not the case here.