Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-283-RC2, 2019

Geoscientific Model Development Discussions
Open Access

Interactive comment on "Paleo calendar-effect adjustments in time-slice and transient climate-model simulations (PaleoCalAdjust v1.0): impact and strategies for data analysis" *by* Patrick J. Bartlein and Sarah L. Shafer

Anonymous Referee #2

Received and published: 8 January 2019

This manuscript discusses the problem associated with the usual definition of seasons in climate models, when simulating climates under different orbital (precessional) configurations, a situation rather common in paleoclimatology. The subject is important, sometimes even critical, and too often it is overlooked. So this discussion and more importantly the availability of some useful routines is a significant and valuable contribution. Unfortunately, I have serious critical comments, both on the manuscript itself, but also on the methodology used in the mathematical routines. With these shortcomings I cannot recommend publication of the manuscript in its current state.

Printer-friendly version

Discussion paper

Major comments

1 – Solving the astronomical problem (§4.2 – lines 350 to 377) The question of the length of seasons is a very classical astronomical problem, since at least the antiquity. It was solved, in the two-body approximation, by Kepler (1609) with the famous first and second laws. The mathematical problem is called "Kepler's equation": M = E – e sinE where M is the mean anomaly (basically time), e is the eccentricity and E is the eccentric anomaly (basically the orbital position). The direct problem (ie. computing M or time for given orbital positions E like equinoxes or solstice for the seasons) is trivial. Solving the inverse problem (computing position E as a function of time M) is likely to be part of the climate model code (usually in a routine called "solar"... but see below), and numerous algorithms have been proposed during the last four centuries to solve it exactly. In this context, it is extremely disturbing to see a scientific paper based on a very crude approximation of Kepler's equation (manuscript equations 1 and 2) based on a first order expansion in e. To be more explicit (see standard textbooks...): M =(2π/T) t E = 2 Arctan[sqrt((1-e)/(1+e)) tan(v/2)] v = λ – ÏŰ + π (T = 1 year, t = time from perihelion, v = angular orbital position from perihelion, λ = angular orbital position from some reference, usually spring equinox, ÏŰ = climatic precession, or angular orbital position of perihelion versus a reference, usually vernal point, π = 180° = spring equinox vs. vernal point) So, for given orbital parameters (precession ÏŰ ; eccentricity e) it is rather simple to compute time t as a function of λ using Kepler's equation. I do not understand the need for a first order approximation in e when equipped with a computer... Where does it come from ? Kutzbach and Gallimore (1988); citing Symon (1964)... probably citing someone who had no pocket calculator and found it useful to write 1st order approximations. This is no more relevant and, I think, not acceptable today : Arctan, tan, Sqrt are usually available in most computer languages...

2 – The same kind of unacceptable "pedestrian" procedure is used (line 374) for solving the equation sin(x)=-1 Using standard mathematics, the solution is x = -π/2 The corresponding algorithm described in the manuscript appears, to say the least, a bit

awkward: Âń Select φp that minimizes {-1-sin((2π/360)(φ - φp))} Âż I prefer to write it more simply as Âń φp = φ + π/2 (or in degrees φp = φ + 90°) Âż and would not use a computer minimization routine for that. . .

Overall, I do not understand why the whole procedure is so complex, and divided in five computer procedures (Step 1 to 5). It is based on unnecessary approximations and on very awkward procedures like ". . .advancing along the orbit at 0.001-day increments. . .". As a result, I have some severe reservations on the relevance or validity of the code. As explained above, the time between 2 orbital positions is just a simple application of Kepler's equation: t2 - t1 =(T/2π)(M2 - M1) = (T/2π)(E2 – e sinE2 – E1 + e sinE1) = ... with (E1, E2) functions of orbital positions (λ1, λ2) as explained above. This should stand in 2 or 3 lines of computer code, no more.

3 – A large part of the manuscript is about describing the "impact of the calendar effect" (§3 – lines 133 to 300). Most of this has been already described and discussed in numerous papers. I believe this part is far too long and far too descriptive. Again, most of this is known since quite a long time: geologists in the XIXth and early XXth centuries were discussing the astronomical forcing not in radiative terms (W/m2) but in terms of season duration (number of days of winter versus summer). This "calendar effect" is in fact at the heart of the precessional forcing. The key point is not so much its existence or its relevance, but how to deal with it in order to interpret model results in seasonal terms.

4 - Lines 320-348. I do not understand why the "mean-preserving" interpolation does not preserve exactly the mean. The fact that the error is small (but can be as big as -0.12 mm/day) is not reassuring. Where does the error come from? If there is no other mathematical treatment that the "mean-preserving interpolation", then I expect the mean to be exactly preserved, not approximately.

Minor comments

line 30-31 : Âń larger number of days Âż. . .. Should be the opposite.

Please state somewhere in the introduction that a fixed angular month is precisely 30°. Stating that a “fixed-angular month corresponds to a fixed number of degrees” is not sufficient. It should also be stated that all “fixed-angular month” are equal, and therefore equivalent to 360°/12. Without such a clear statement, the definition of a “fixed-angular month” is very ambiguous.

line 360 : Âń To calculate the orbital parameters . . . we adapted a set of programs . . . https://data.giss.nasa.gov/ar5/solar.html Âż This link is broken. (I am pretty sure this solves Kepler’s equation. . .)

line 369 : equation (2) there is a missing index p in the second cosine (see the original ref).

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-283, 2018.