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Interactive comment on "Earth Orbit v2.1: a 3-D visualization and analysis model of Earth's orbit, Milankovitch cycles and insolation" *by* T. S. Kostadinov and R. Gilb

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Note: Some responses here refer to our responses to Reviewer #1. Please also see the figure attached to those comments. The revised model files are included as supplement to our response to reviewer #1 as well.

Responses to Anonymous Referee #2

This paper presents a very useful tool for visualising and analysing the Earth's orbit. The Earth Orbit Model will have wide applications for both science research and edu-

cation. The paper is presented in two parts; initially an overview of orbital fluctuations or the Milankovitch cycles is given. The software is then explained in detail and a brief section on model validation is included. In general the paper is well written and provides enough detail for the end user to appropriately use the model.

Specific comments on the manuscript Comment: 1. There are inconsistencies in the usage of the terms Ky and yr that should be addressed.

Response: These inconsistencies have been addressed and kiloyear (kyr) is used everywhere to mean one thousand years.

Comment: 2. pp 5950 (8): The authors mention the 100 Ky problem briefly. The text would benefit from a little more explanation as to the nature of the problem and its significance.

Response: We now have expanded the discussion of the 100-kyr/mid-Pleistocene transition problem, including stating the problem and briefly summarizing the currently proposed solutions.

Comment: 3. Although it is interesting to see the EPICA/deuterium data included in the model, there are climate records that span greater intervals of Earth history that would also be useful to compare to the orbital parameters. For example, going back 5 Ma, the benthic oxygen isotope curve of Lisiecki and Raymo (2005) could be added. Or to go back even further in time, one of the Zachos compilations would be a useful comparison. It might also be better to allow the user the option to display the data-model comparison or not, as looking further back in time than the EPICA record, one currently ends up with an empty plot at the bottom of the screen.

Response: See our response to the Reviewer #1 comment "P5964 – l2-6." Thank you for suggesting the Lisiecki and Raymo(2005) and the Zachos(2001) data sets, which we have now included as a separate optional figure in the model.

Comment: 4. True anomaly is first mentioned on pp 5955 (9 & 22 & 25), however the term is only described on pp 5957. I think the reader would benefit from description/definition of the true anomaly in the first instance that the term is used.

Response: We now define true anomaly in Sect 2.1 and mean anomaly in Sect. 2.2, where their definitions belong best. These terms are still mentioned a paragraph earlier when we discuss the solar constant, because we believe it's important to point out that averaging over angle vs. over time yields different results. The definitions of these terms do not belong there, however, so we clearly point the reader to the immediately following definitions.

Comment: 5. pp 5961 (7-18) – The authors discuss the effect of the varying length of the seasons over geologic time scales. I am not sure if I have understood correctly, but is this essentially the same problem as has been identified within the palaeoclimate community as pertaining to the definition of the calendar (i.e. fixed-day/angular or classical)? If this is the same problem, it might be useful to the reader to have a few sentences relating the author's description of the problem to other published efforts to understand the effect of this problem, when considering different orbits over time (e.g. Jousamme and Braconnot, 1997; Chen et al., 2010 – Climate Dynamics)

Response: Yes, this is the same problem. We have edited and expanded this section significantly and the fixed-day calendar vs. the fixed-angular calendar issue is now discussed at length in the context of Joussaume and Braconnot, 1997 and Chen et al. (2010), using consistent terminology.

Comment: 6. Validation of insolation output. It is great to see the authors validate their independent calculations in such a way. The authors state: "Validation is excellent; all test cases result in differences less than 1Wm-2 (Fig. 5)". It might be easier for the reader to determine the effectiveness of the insolation solution by comparing the magnitude of the disparity to the kind of magnitude in discrepancy that one might expect using alternative astronomical solutions for that time (e.g. BL78, BL92 and Laskar, 2010). In other words, the different astronomical solutions may lead to an insolation

discrepancy of X Wm-2 at certain points in time, with the differences becoming larger in the further back in time (on the whole). How do the differences shown here compare to the inter-solution differences one might expect?

Response: See the response to Reviewer #1 Comment to Figure 5, as well as the attached figure in our response to reviewer #1, showing the much more extensive validation, as well as the inter-solution comparison you have suggested.

Comment: 7. pp 5965 (7) – I find the term +10 000 yr since present difficult to understand, is this 10 000 years in the future? Perhaps rephrase.

Response: This text has now been replaced. This usage is avoided in the text and clearly explained in the GUI.

Comment: 8. 5965(23) – Define UT

Response: UT (Universal Time) is now defined in the Introduction.

Comment: 9. Figure 2. It might be useful for the reader to have a little more description as to what the lines on the orbital configuration mean – give details of the black and the red lines. Maybe additionally show a Palaeo case and describe what it means when you are looking at July and it falls in the wrong season with regards to present day. Also, at what latitude is figure 2a representing for the 16 September and using which calendar start date. It would be good for the figure caption to contain enough information that the GUI user could reproduce it with ease. The text on the figure in this case is a little small and difficult to read.

Response: We have significantly expanded the caption of Figure 2 to explain all the elements of the plot. An additional real case for 10 kyr in the future has been shown (roughly equivalent in terms of precession to 10 kyr in the past), and then the demo exaggerated case is shown. These are discussed in more detail in the main text as well.

Comment: 10. Figure 5. Could the authors comment as to why the error bars are so much larger at 80_S 10,000 years ago than for any of the other cases?

Response: The validation has now been replaced with a much more extensive one – See the response to Reviewer #1 Comment to Figure 5, as well as the attached figure. The large number in question here is 1) not large compared to some others in the new validation figure, 2) not large at all in terms of percentage difference(not shown). Note that this particular case is no longer a part of the validation, but to address your specific question, insolation at polar latitudes can be very sensitive to the exact orbital parameters, because at low solar altitudes above the horizon, the derivative of the cosine of solar zenith angle is large. Day length itself becomes a value of large uncertainty in some cases. So larger differences are expected in this case due to the exact treatment of these issue and numerical procedures used. The above does not necessarily explain why the case for 10kyr in the future exhibits larger differences than the present, but it demonstrates that the exact values of insolation are highly sensitive there.

Comment: 11. Figure 6. It is not clear from the figure caption or the text what causes the discontinuity in the green and black lines in figure 6. Is this related to the leap year considered? Could the authors elaborate on this please?

Response: See our answer to the Figure 6 comment of reviewer #1. We have now added a brief discussion in the text addressing these discontinuities. They are due indeed to the treatment of Feb 29 in the leap year and the nature of March 19 (the day before equinox) in the model year (which uses the sidereal orbital period, and is neither leap nor common).

Specific comments pertaining to the GUI Comment: 1. In the Milankovitch Orbital Parameters section the input requirements for the choice of year is in years since J2000, however, in the Time Series and Insolation Plotting Options section, the input should be in thousands of years since J2000. This is slightly confusing and it should be considered whether the inputs should both be in the same units. Additionally an example

alongside the input box of what numbers (+/-) would be necessary input values to calculate insolation properties for e.g. 20,000 ka BP or 2 Ma ago, would be beneficial to the user.

Response: The units have been changed consistently to kyr everywhere where relevant in the GUI and text, and an example input is stated in the GUI just above the input boxes for the Laskar or the Berger solutions.

Comment: 2. The authors refer to the Astronomical Unit in Table 1 as a constant/variable model input parameters. From the GUI the AU changes with every solution requested – that AU varies might not be obvious to the reader. Perhaps therefore, the authors should list in the table which values are constant assumptions and which vary depending upon the GUI inputs.

Response: The semi-major axis is a prescribed model constant and it does not change with the different solutions or in the demo mode. It is almost exactly equal to 1 AU. This has now shown in a less ambiguous way in the GUI. The Table 1 values were slightly different from the GUI, which has now been fixed. The table also clearly lists which values are constants. The text (Sect.2, paragraph 1) also explains that the semi-major axis determines the orbital sidereal period, which is also a prescribed constant, i.e. Kepler's 3rd law is implemented implicitly by using these consistent constants.

Interactive comment on Geosci. Model Dev. Discuss., 6, 5947, 2013.