

## ***Interactive comment on “Transient simulations of the present and the last interglacial climate using a coupled general circulation model: effects of orbital acceleration” by V. Varma et al.***

**Anonymous Referee #2**

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In this paper the authors intend to study the impact of an acceleration technique on long transient climate simulations performed with a CGCM. Similar studies were already performed with Earth System Model of Intermediate Complexity. However, this one is amongst the first to do it for CGCMs. They identified two time slices (the present interglacial and the last interglacial) and they compared the climate simulated with and without an acceleration technique. All the other ‘parameters’ remain the same. This is a perfectly sound technique and the description of the results is clearly presented.

Main comments:

I am slightly disappointed that there is not a word on the acceleration technique itself,

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although I am sure that it is described elsewhere.

All the analysis remains mostly qualitative. There is few quantification of the difference between the results obtained with and without the acceleration technique. Along the same line, location of disagreements often remains vague (the high latitudes, the sites of deep water formation). The authors do not justify their choice of the variables displayed. Although surface temperature seems to be an obvious starting point, the choice of the wind field and the deep ocean temperature should be explained. Other variables, such as sea ice extent or temperature at different levels in the atmosphere, could have been chosen as well.

I have an uncomfortable mixed feeling with the paper. I have the impression that throughout the paper, the authors tend to show that the acceleration technique is doing a very good job and that the differences between accelerated and non-accelerated simulations are minor. However, the conclusion is doing the reverse, insisting on the major discrepancies. For example, I read in the Discussion that ‘Except for some high-latitude regions, in particular the Southern Ocean, the acceleration technique does neither ham- per model intercomparison nor model-data comparison studies’ but in the Conclusion it is stated that ‘the acceleration technique may compromise transient climate simulations over large regions in the Southern Hemisphere’. Even if the reservation of ‘some high latitudes of the Southern Hemisphere’ appears in the first sentences, the ideas conveyed are not exactly along the same line. At last, I remain with a strong question. Is it useful or not to use an acceleration technique?

The authors decided to test the method on interglacial periods. Do they think that the choice a warm periods influence their results? Which results do they anticipate for climatically different times slices (for example, glacial periods or terminations)?

Minor comments:

The PIG-accelerated simulation is the only one to use an average of three members. I urge the authors to consider only one member, along the line used for the three

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other simulations. Alternatively, they could use three members for each of the four simulations.

How is the PIG accelerated simulation initialised? Does it use the same initial and boundary conditions as the non-accelerated PIG simulation?

The authors indicated that “For the analyses of the model results decadal means (referring to model years) have been used from all the transient simulations “. If I understand well, this mean that an average over 10 ‘real’ years in the case of the non-accelerated simulations is compared to an average over 100 ‘real’ years. Could the authors consider using an average over the same number of ‘real’ years. Moreover, Working with average of model years means that there are ten times less data points in the accelerated simulations than in the non-accelerated ones. I am therefore wondering how the authors computed a difference between the two types of simulations (with a different number of data points).

P5625 – line 5 “zonally averaged surface temperature”. I assume that the authors mean ‘anomalies from the beginning of the simulation’. Please clarify.

P5626 – the EOF. As a non-specialist of the EOF, I do not understand the figures, how they are produced and what they exactly represent. I can only figure out what they should be. Would it be possible to provide more information?

P5626 – lines 22-26: Doesn’t this conclusion confirm that the comparison should be performed on time series averaged on the same number of real years (and not model years)?

P5626 : although the potential role of the sea ice is described, neither the evolution of the sea ice during the simulation, nor its difference between the accelerated and the non-accelerated simulations is discussed.

P5627 – line 27 : “Rather surprisingly”. Could the authors explain why they were surprised? What were they expecting?

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P 5628 – lines 5-7 : It is the first time in the paper that a link between surface temperature, upwelling and deep ocean temperature is presented. A more elaborated discussion would be welcome. Moreover, I invite the authors to provide a more precise identification of the region(s) under concern.

P 5628 – line 24-25 : “In these regions, inappropriate deep-ocean initial conditions may severely compromise accelerated runs, strongly determining the climate trajectories”. This idea would be worth to be included in the conclusion. Or is it only a conclusion from Lunt et al., and Timm and Timmermann?

Although I did not checked the references in details, I couldn’t find the reference ‘Govin et al., 2014’. ”

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