

Interactive comment on “Effect of the Ordovician paleogeography on the (in)stability of the climate” by A. Pohl et al.

Anonymous Referee #2

Received and published: 10 September 2014

I should note that I have not read the other review of this paper, so my review is independent.

This paper presents a set of climate model simulations designed to test the sensitivity of climate to CO₂ forcing under two different Ordovician palaeogeographies. The model displays non-linearity in its response to CO₂, with a step-change in temperature at a CO₂ threshold which is found to depend on the underlying palaeogeography. This non-linearity is not found with a slab model, and so the authors infer that ocean circulation is playing a role in the non-linearity. The different thresholds for the different palaeogeographies are attributed partly to the presence of more land in the Northern Hemisphere in one palaeogeography, the presence of which precludes a coherent circumglobal circulation.

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The paper is fairly straightforward and I have no major problems with the findings. I think that it could be published with the analysis carried out so far, but that some more analysis (listed as ‘Main Comments’ below) will uplift this paper from being fairly pedestrian to being very good.

Main comments:

The proposed mechanism appears to rely fairly heavily on the seaice physics and dynamics. So, does the model simulate sea ice well in the modern climate?

The slab model is only run for the 450 Ma palaeogeography. To have confidence that the non-linearity is related to the full ocean, it would be helpful to show that the slab model at 470 is also linear.

The paper several times refers to a “tipping point”. However, a tipping point implies some form of hysteresis. All that is shown here is that the model displays a non-linearity. The authors should either formally show that the model displays ‘tipping point’ behaviour (i.e. carry out some transient simulations in which CO₂ is ramped up and down slowly), or cut all references to ‘tipping points’ and ‘bifurcation’ and instead use ‘non-linearity’.

The model does not include a land-ice component. With the inclusion of a land-ice component, the main results of the paper (e.g. a lower CO₂ threshold for 450 Ma compared with 470Ma) may well change, and even reverse. This should at least be commented on. The Donnadieu modelling group does regularly use a land-ice model, so it is possible that they could incorporate this into their model setup.

The authors postulate that the presence of a proto-Siberia in the NH is responsible for the difference between the two paleogeographies. This could be tested more robustly by carrying out a further simulation in which just this one aspect of the palaeogeography is changed.

Changes to seaice are postulated as being responsible for several aspects of these

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simulations. However, this is not proven, just inferred from the fact that there are changes in seaice. To properly nail this, the authors would need to carry out a further simulation in which seaice was held constant whilst other aspects of climate were allowed to change.

At the moment, much is made of the changes seen between 450 and 470 Ma in the Northern Hemisphere. However, the changes in the Southern hemisphere are just as large, but of opposite sign, and deserve some more attention.

Specific comments:

P2769, Line 26 – not sufficient based on what? A quick calculation of radiative forcing would be useful here.

Figure 1 is not totally clear – e.g what are the blue bars in the timescale?

The word “ice” and “ice cap” is used several times without it being clear if sea-ice or land-ice is being discussed – please clarify in all cases.

P2773, line 4: “The solar luminosity was set 3.5% below present according to the Faint Young Sun paradox (Gough, 1981)”. This is not according to the FYSP but according to a model of solar physics.

Figure 3 – make seaice and land a different colour. Also, the map projection is not very helpful – why not centre on the pole?.

P2776. line 16 – “more stable” is not clear. ‘has a higher CO2 threshold’ is more accurate.

Technical comments:

The paper should be professionally proof-read as there are numerous instances of unusual phraseology and vocabulary.

Abstract: “quasi-oceanic Northern Hemisphere” does not make sense until the paper

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is read – please rephrase.

Abstract: “The peculiar shape of the oceanic heat transport” is unclear – I am not sure that ocean heat transport can have a ‘shape’.

P2769, Line 1: “reflects” is unclear.

P2769, Line 1: give dates of Ordovician.

P2769, Line 14 “drawdown of icesheets” should be “reduction of icesheets”?

P2769, Line 19 “remains yet unusual” should be rephrased.

P2774, line 1 – “well sensitive” should be “very sensitive”?

Figure 4 – state in caption that these are results from the coupled model, not slab model. Also for other figures where appropriate.

P2778 occurs later -> occurs at higher co2.

P2780 line 27 – “slaved”? And 2782 line 1.

P2783 – “peculiar” is peculiar! Also elsewhere.

P2783 – “till” should be “up to”.

P2784 – “brutal”?

Interactive comment on Clim. Past Discuss., 10, 2767, 2014.

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