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Climate of the Past Discussions

Interactive comment on "Amplification of obliquity forcing through mean-annual and seasonal atmospheric feedbacks" *by* S.-Y. Lee and C. J. Poulsen

Anonymous Referee #3

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1 Summary and recommendation

This paper presents a set of sensitivity experiments with a fast coupled atmosphere-ocean climate model (FOAM) in order to better understand the relative role of annual mean insolation changes, versus seasonal changes, both associated with variations in the Earth obliquity. Indeed, many authors have suggested that obliquity variations probably had a significant role in setting the pace of glacial-interglacial changes, or at least had important impacts on the climate system, that are often not fairly accounted for by the traditionnal presentation of Milankovitch theory. In particular, several different mechanisms have been proposed, either involving seasonal changes, or involving

annual mean changes, in particular in atmospheric meridional transports linked to different insolation gradients. This study is indeed the first, to my knowledge, to attempt to decipher the influence of these two obliquity effects on climate. It is therefore an important piece of information and a welcome addition to the debate. I would recommend its publication, provided the authors address the comments listed below.

2 Main comments

2.1. A major limitation of the manuscript, as acknowledged by the authors, is to analyse the model outputs in terms of snow accumulation only, whereas the ice-sheet mass balance is also (and mostly) dependent on ice ablation. Though I understand that it is beyond the scope of this paper to use a complete ice-sheet model, it is traditionnal to force ice sheets using a positive degree days (PDD) parameterization. This quantity could have been easily obtained from the authors' model, over some prescribed canadian area, and it would have nicely complemented the discussion on seasonnal versus annual changes, but also in terms of ablation changes. Such an analysis was, for instance, performed in Jackson and Broccoli (2003) concerning obliquity versus precessional changes. If the corresponding daily model outputs are still available to the authors, I would urge them to perform such an analysis.

2.2. The authors are also mentioning a set of 2 additional sensitivity experiments in order to quantify precessional effects. They find that the maximum precessional effect on snowfall represents 85% of the maximum obliquity effect. It would have been interesting to have a bit more details on these simulations and on the corresponding results. There is no corresponding figure. Besides, several papers have already investigated the question of obliquity versus precessional changes using coupled climate models, and some comparing with the litterature would be beneficial (Jackson et al. 2003; Vettoretti et al., 2004; Tuenter et al., 2005). Again, the manuscript does not seem to fully take advantage of the numerical simulations performed.

3 Minor comments

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3.1. p517 line 6: "non-linear climate feedbacks". It is said that von Neumann once referred to "nonlinear theory" as a theory of "non-elephants", emphasizing that this terminology is basically meaningless (except for very specific linear problems...). Actually, almost everything is non-linear, and the Milankovitch theory certainly is. I guess the authors are here refering to indirect (or non local) effects of insolation, like changes in transports. "Non-linear" is certainly a word that should be avoided by climate scientists (and I am afraid that "feedback" is likely another one).

3.2. The red and black curves on Figure 3 are described in the caption as DeltaTOTAL (solid) and DeltaMA (dashed). The reading of the text suggest that DeltaTOTAL is actually the black one (DeltaMA the red one). But this is the just opposite on Figure 4. Please, be consistent.

3.3. p.526, line 21. "power variance is an exponential function of the absolute variance". I don't understand this sentence, that I suspect meaningless. First, what is "power variance" ? Is it simply power (or power density), but then (when integrated over frequency) it is simply EQUAL to variance. But what is "absolute variance"? And where does the exponential function come from ? The authors should use standard terminology...

3.4. The model set-up is mainly designed around 4 simulations that are interpreted in relative terms using 2 difference fields (DeltaTOTAL and DeltaMA) which represent the total influence of tilt change, and the annual mean associated with the same change. Results are actually mostly discuused in terms on DeltaMA versus DeltaSEA = DeltaTOTAL - DeltaMA. Why not plotting directly DeltaSEA?

3.5. FOAM was integrated 200 years, which is indeed often considered enough for surface results. Still, this might be quite optimistic, in particular in the North Atlantic were oceanic convection sites are prone to changes, that may be caused by small long-term drifts. Are there any sizeable climatic drift in the ocean in these areas ?

References:

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Jackson et al. Orbital forcing of Arctic climate: mechanisms of climate response and implications for continental glaciation. Clim Dyn (2003) vol. 21 (7-8) pp. 539-557.

Vettoretti et al. Sensitivity of glacial inception to orbital and greenhouse gas climate forcing. Quat. Sci. Rev. (2004) vol. 23 (3-4) pp. 499-519

Tuenter et al. Simulation of climate phase lags in response to precession and obliquity forcing and the role of vegetation. Clim Dyn (2005) vol. 24 (3) pp. 279-295

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