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Interactive Comment

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

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1 Summary

The paper presents the results of four simulations with the FOAM climate model, with a focus on snow accumulation at high latitudes. One pair of experiments studies the effect of a fairly classical change in obliquity. The second pair of experiments "isolates" the effect of the change in annual mean insolation caused by the change in obliquity.

Although not quite formulated that way, the motivating question (p. 517) is whether the effect of insolation on ice volume is exerted *locally* (which the authors call linear, although the cited examples do not necessarily call on a linear measure of insolation), or through a change in *planetary gradients* of temperature and/or insolation (a decrease in obliquity enhances the polar transport of latent heat, and hence promotes glacial inception) [1]. [Loutre et al.(2004)] put special emphasis on the fact that obliquity controls the latitudinal *gradient* of *annual mean* insolation and this appears to be the starting point of this paper : to what extent is this gradient instrumental in driving glacial-interglacial cycles? Lee and Poulsen have therefore opted for an experiment design in which only an annual mean change in insolation, consistent with that caused by a decrease in obliquity, is taken into account (the experiment pair is called AM), to compare it with a pair of experiments where all the consequences of obliquity on top-of-the-atmosphere insolation are properly taken into account (experiment TOTAL).

2 Overall recommendation

At this stage the article does not reach the level of significance required for publication because of insufficient critical analysis of the experimental design and still unconvincing interpretation of the results and of their implications to our understanding of long-term climate dynamics.

[1] The latter was indeed suggested by most of the authors quoted by Lee and Poulsen (although not in [Crucifix and Loutre(2002)]), but as pointed out by [Raymo and Nisancioglu(2003)] the possible relevance of insolation gradient to our understanding of glacial ages was proposed as early as in the seventies [Berger(1976)] and in the eighties [Young and Bradley(1984)].

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3 Main comments

There are several caveats associated with this methodology :

1. First, the 'AM' insolation (Fig 1b) cannot possibly be obtained on a spherical earth. It is therefore fundamentally unphysical and makes the results impossible to be verified experimentally (arguably, this is often the case in climate feedback analysis and I would not be the one casting the first stone)

2. The design requires arbitrary decision about polar-night region. Here, the annual mean change has been redistributed within the daylight season, which is getting shorter and shorter as one approaches the poles. Consequently, *the resulting decrease in summer insolation* in AM is about half of that in TOTAL, which roughly corresponds to the ratios of summer snowfall change seen on Figure 2. Therefore, it is not clear that the effect of insolation on snowfall is not purely local, at least in summer. On the other hand, both experimental setups lead to latitudinal gradients (obvious on Fig 1). Consequently, it is hard to argue that either experiment picks up the “seasonal” effect or the “gradient” effects, or vice-versa.

On the other hand, model results are not always convincingly examined:

3. The mechanisms of snowfall in this model — and the corresponding performance in present-day conditions — should be better discussed. That summer snowfall systematically increases in response to summer cooling is interesting but, honestly, this is also a bit suspicious because in the present-day continental snowfall is not immediately related to temperature. One explanation is that rainfall becomes snowfall but in that case the relevance for interpreting the sediment record is not straightforward.

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4. The explanation for winter snow-fall increase in TOTAL is very puzzling. According to the authors, the increased insolation causes an increase in evaporation, thus increased low-clouds, which *reduce* incoming radiation and then temperature (presumably North of 60° N, since the one between 30 and 60^o N needs to increase to keep the feedback loop stable and thus plausible), which eventually increases continental snowfall. How and why ?

5. The reported effects of surface temperature changes on relative humidity tend to be confusing: p. 522, one reads that *the greater NH winter solar heating and evaporation increases the relative humidity*, but p. 523, we are being told that *the condensation due to SAT change does not change the local relative humidity*, in spite of the fact that, p. 524, *a decrease in surface temperature might also reduce humidity due to a decrease in saturation vapor pressure in a cold climate*. Although the confusion probably arises from lacking specifications of relative vs specific humidity, a clarification would be highly welcomed.

More generally,

6. This article only focuses on snow accumulation, with virtually no consideration to snow ablation. It is correct that general circulation models not coupled to a high-resolution ice-sheet model are not really appropriate to correctly quantify changes in ablation. However, the dominant paradigm of glacial-interglacial cycles inherited from [Milankovitch(1998)] and others (Murphy (1869) seems to have been one of the firsts) remains that ice volume changes are primarily controlled by ablation, even if some of the authors quoted above, to which one should add [Gildor and Tziperman(2001)] make a fairly convincing case that accumulation must have played a role. At the very minimum, the authors should more clearly put their study into this context.

7. The penultimate paragraph is redundant with the introduction and does not really add much.

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4 Minor comments

There is a number of mis-quotations and imprecisions. Crucifix and Loutre did not find, in their experiments with an admittedly quite simple and limited climate model, a very important role of obliquity. The high-latitude temperature changes induced by obliquity throughout the Eemian in their model is about half of those caused by precession, thus a third of the total. Furthermore, they did not observe strong effects due to changes in atmospheric latent heat transport.

Some historical hints : Milankovitch did not use 65° N insolation, although that curve is systematically used in reference to his ideas about the importance of summer insolation. Huybers is correctly quoted (apart from the missing S) although it is good to remember that the question of the effect of season length was already well present in the controversy between Murphy and Croll in the XIXth century, and that very question was discussed in length by Milankovitch himself.

Finally, a few clarifications are needed : what is meant by *internal climate oscillation* (p. 518). Gradients (p. 519) are expressed in W/m^2 : but gradients between what and what ? Shouldn't they be expressed in W/m^2 per degree of latitude (or per kilometer ?).

References

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