**Climate
of the Past
Discussions**

Interactive
Comment

Interactive comment on "Simulating sub-Milankovitch climate variations associated with vegetation dynamics" *by* E. Tuenter et al.

Anonymous Referee #1

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The authors use the CLIMBER-2 model to investigate the response of the earth-system to orbital forcing over the time period 280-150kyrBP. They find that the system response includes significant components at sub-Milankovitch periods (10kyr, 5kyr) - periods which are also seen in the data record. They attribute this to changes in vegetation in their model, in particular grasses. As a consequence of the changes in grass fraction, they also find similar sub-Milankovitch variations in runoff. At the end of the paper, they hypothesize that these variations in runoff could have a significant effect on ocean circulation.

The paper is well-written, and includes some potentially very interesting and important results. However, I am currently worried about whether the main result, that the grass fraction displays a significant sub-Milankovitch response, is an (essentially numerical)

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artefact of the simple vegetation model used - this should ideally be tested by carrying out a simple sensitivity study with a more complex vegetation model, or at least some offline simulations using VECODE. I am also worried about the conclusions related to ocean circulation - again, this should be tested by carrying out a simple hosing sensitivity study.

General Comments

I have two major points about the paper.

(A) Firstly, I am very worried that the sub-Milankovitch response seen in the grass fraction may simply be a result of the relatively simple vegetation model which is being used, with 3 PFTs (trees, grass, bare soil), or to the artificial 'clipping' of the trees and bare soil fractions. For example, if one fraction (e.g, grass) is essentially what is left over once the tree and bare-soil fractions have been calculated, then maybe it is not surprising that there is a significant response in the grass fraction at half the forcing period, if the tree and bare soils have been 'clipped'. The authors need to discuss very clearly whether this is the case or not. Also, the authors should check that the tests for significance have been done correctly given that the 3 PFTs are dependent (must add up to 1). The validity of the simulations should be tested using a more complex model such as LPJ. A suitable sensitivity study could involve forcing the LPJ model at a single gridbox with the temperature and precipitation variations predicted by CLIMBER, and looking at the relative response of the different (more numerous) PFTs). This study would be simple to carry out - the LPJ model, including an example driver is available online, and runs fast enough offline to carry out multi-milennial simulations. At the very least, some offline simulations with VECODE should be carried out to attempt to characterise the response of the different PFTs in more detail (the explanation for why there is a half-period response in the grass fraction is currently inadequate).

(B) Secondly, I am not convinced by the hypothesis that the changes in runoff could significantly affect ocean circulation. It would be nice to quote the variations in runoff from

a particular river basin in Sverdrups, and relate this to previous work on water hosing, to find out if it could be significant. In any case, doesn't the runoff in CLIMBER actually end up in the ocean anyway, so if it is a significant effect then maybe some changes are seen in ocean circulation? However, these could be caused by related changes such as albedo, To isolate this effect, a further simulation should be carried out with CLIMBER where the variations in runoff from this study are applied as a time-varying hosing experiment to a pre-industrial or LGM simulation. This should be relatively quick as CLIMBER runs so fast, and similar hosing simulations have been carried out before with the model, so the code is essentially in place.

Specific comments

(1) Much more detail is needed in section 2 about the way that VECODE simulates grasses/tress/bare soil, as this is rather pivotal to the paper. The reference to Brovkin et al is not sufficient because (as far as I can tell) that paper only explains how the tree fraction is calculated. In the discussion (e.g. section 3, line 25), it is implied that the 'grass' fraction is simply what is left after the tree and bare-soil fraction have been calculated. If this is so, how is the 'bare-soil' fraction calculated? How realistic is this calculation (i.e. is it at all process-based)?

(2) Related to the previous point, we need to see a plot of the model's simulation of present-day vegetation, compared to observations (i.e. similar to the plot in Brovkin et al. but including grasses and bare soil as well as trees).

(3) If the sub-Milankovitch harmonics in the desert and tree fractions are a result of 'clipping' due to the non-negativity of the vegetation fractions, then is this a 'real' phenomenon?! You imply not in the text (using the word 'artefact'), but in the 'real world' these fractions can not be zero either. Clarification is needed as to why these are artificial. I expect that it is a consequence of the relatively simple partitioning of the vegetation into 2 PFTs + bare soil. Does this clipping influence the grass fraction as well, and to what extent is the 'significant' 10kyr response in the grasses an artefact of

this clipping?

(4) The explanation of why there is a half-period response in the grass fraction is not clear. Just because the trees and bare soil are in anti-phase, doesn't mean that grass should have half the period (I convinced myself of this by writing a short IDL code!).

(5) The explanation of the 5kyr response in the runoff in Asia is not very clear (section 3, lines 14-29). Perhaps a zoomed-in plot over one cycle could help clarify this? Again, is it related to the (artificial) 'clipping' of the grass fraction?

(6) It requires some effort on the part of the reader to relate the changes in orbital parameters to solar forcings. It would greatly aid the discussion if the top-of-the-atmosphere solar forcing variation (e.g june, december at 30oN, 65oN, 80oN) was shown, alongside or included within figure 2, and if this new figure were regularly referenced in the text.

Comments on figures

(7) The figures are all very difficult to read when printed at normal size. This would be improved by thickening the lines and making the axis labels and titles larger. Also, figures 3, 6 and 7 should be made larger (e.g. double the width and double the height.).

(8) The quality of figure 1 should be improved - it looks like it has been scanned in at low resolution. In fact, I am not convinced by the way the CLIMBER model land-sea mask is usually represented. Each fractional land gridcell is represented by a block, which (e.g. in the case of India) is shown in a position which gives an artificial impression of higher-resolution. In my opinion, each gridbox should be grey-scaled depending on its land-fraction.

(9) It would be much clearer if the different panels of each figure were labelled (a),(b), (c) etc. This would make the referencing from the text much clearer (e.g. section 3.1)

Minor Comments

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(10) references - Brostrom should come before Brovkin et al.

(11) section 4, line 12 - The grass fraction -> the grass fraction ?

(12) section 4, line 4 - Climber-2 -> CLIMBER-2 (also in the abstract).

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