

## ***Interactive comment on “Modelling Maastrichtian climate: investigating the role of geography, atmospheric and vegetation” by S. J. Hunter et al.***

### **Anonymous Referee #1**

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The contribution by Hunter et al. deals with the modelling of the Maastrichtian climate using a coupled ocean atmosphere GCM, and a dynamic vegetation model. My overall feeling is that this paper does not really bring new scientific informations about the late Cretaceous climate. Although the tool used is particularly complex, I do not really see what is new compared to previous studies, including Otto-Bliesner et al. (2002). I think the authors do not fully exploit their results. This is why I suggest that this paper could be published only after accounting for the following very major comments.

1) The introduction presents a review of the previous modelling works, pointing at the main difficulties encountered up to now. I think several important recent contributions have been forgotten. This includes the paper by Donnadieu et al. (2006, EPSL 248, p 426-437) and Roche et al. (2006, Paleoceanography, 21, 2, art. PA2023). I also think

that a discussion about the reality of the flat Cretaceous gradient should be included in the introduction. Indeed it has been recently demonstrated that latitudinal temperature gradients might have been not so flat during the Cretaceous (Puceat et al., 2007). Furthermore, the objective of this study is not clearly defined in the introduction. Why are you running the coupled model ? What are the processes tested here which were never tested before ? What is clearly the scientific question behind the study ? After reading the whole paper, this question is still not obvious. Only the title is clear relative to the objectives, but the text is not.

2) the contribution aims at evaluating the role of geography, CO<sub>2</sub> and vegetation on the Cretaceous climate as claimed in the title. I think that this study mainly explores the sensitivity to CO<sub>2</sub>. The sensitivity to geography is not really studied here: the authors mentioned that the Cretaceous simulation at 1x differs from the present day one, and that this is not only due to geographical change since other parameters are changing including the solar constant. It should not be difficult to maintain the solar constant at its present day value so that the sensitivity to the geography will be emphasized. Also regarding sensitivity to vegetation, the reason why a couple atmosphere-vegetation-ocean simulation differs from and ocean-atmosphere simulation is eventually simply mentioned, and not really explored. This is a main flaw of this contribution: the reason why the model works sometimes, and why it does not work sometimes, is not truly investigated. See for instance page 993, line 7 'could be a consequence'; page 994, line 13 'appears to be driven'; page 995, line 23 'these data-model mismatches are difficult to reconcile'; and several other sentences.

3) why is the vegetation warming effect much lower in this work compared to Otto-Bliesner and Upchurch (1997) ? Is it related to the boundary conditions of the numerical test or is it really a physical effect ?

3) An extensive study of the role of the paleogeography has been recently performed by Donnadieu et al. (2006, EPSL 248, p 426-437) and I think that the author should have a look at it. This work is not cited here.

4) The discussion section does not contain new informations or suggestions: it simply lists the main results obtained, still without explaining them.

5) The model of Hunter et al includes a deep ocean module. As mentioned by the authors, 200 yr simulations are not long enough to reach the thermodynamic and dynamic steady-state of the deep ocean. I'm not sure this has no impact on the calculated climate, as argued by the authors. The thermohaline circulation has indeed an impact on the present day climate by warming the northern latitudes. Maybe this point should be discussed.

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Interactive comment on Clim. Past Discuss., 4, 981, 2008.

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