

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

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This paper describes a set of six simulations (using the term “ensemble” in the paper is fallacious) of the Maastrichtian climate in which atmospheric CO₂ levels and the spatial distribution of the vegetation are tested as free parameters. The authors try to compare their modelling results with previous modelling studies and with data constraints in a way rather exhaustive but they reach no firm conclusions and leave the reader unsatisfied. They just invoke the usual processes when data-model mismatches are found, i.e. vegetation distribution, stratospheric clouds or cyclonic activity. When comparing with previous model results, they do not shed light on new feedbacks or physical processes and they also make basic errors.

1) They simply do not recognize that their vegetation sensitivity experiment has a

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smaller temperature effect than the one of Bliesner and Upchurch (0.2°C compared to 2.3°C) because the latter study compares a bare-earth model alongside one with prescribed vegetation based upon a geological best estimate while the former uses either shrub everywhere or a simulated vegetation cover. It is obvious that the effect in terms of temperature will be larger when using a bare-earth model than when using shrub everywhere. Indeed, the difference between the albedo's value of bare-land and the albedo's value of other PFTs is larger than the difference between the albedo of shrub and other PFT.

2) Authors write several times in the paper : “ It is not possible to associate this to a single driving factor, as the differences in the two experiments are numerous ... ”. It is a really surprising comment ! Models are the tools that allow to separate the impact of the various factors at play in the real climate system. For example, if authors want to determine the impact of the geography, they just have to run two simulations with the only changing parameter being the geography.

3) I think it is an oversight to this paper that they do not explain why the thermal seasonality increases in their model. Many authors have shown and explained why the seasonality was weaker during the Cretaceous (see Poulsen et al., 1999; Donnadieu et al., 2006; Valdes, Sellwood and Price, 1996). Most of them argue on the presence of numerous seaway in the northern hemisphere and go into details by analysing the radiative terms and the heat transport. Once again, the authors should give more details concerning processes occurring in their model and should not just describe the results in terms of temperature and precipitation.

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