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Interactive Comment

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

Anonymous Referee #2

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The manuscript by Hunter et al. describes several sensitivity experiments of Maastrichtian climate using the HadCM3L ocean-atmosphere GCM. For the most part, the experiments and experimental design are sound. However, the manuscript itself is very poorly conceived, and offers no new insights into Late Cretaceous climate or the working of the GCM. In my opinion, the manuscript needs a major overhaul and requires significantly more analysis of the model results.

The construction of the manuscript is curious. First, it's not at all clear what the point of the paper is. The title indicates that the study investigates geography, atmospheric pCO2, and vegetation. One of three of these (CO2) might be appropriately listed. There is some comparison of the Cretaceous experiments with a pre-industrial experiment, but geography isn't the only difference between these experiments and so the

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comparison is appropriately judged inadequate. (p. 992, "It is not possible to associate this warmth with a single driving factor, as the differences in the two experiments are numerous (geography, land-surface scheme, reduced solar constant.") There is some comparison between experiments with prescribed and simulated vegetation. However, there isn't a single figure showing predicted vegetation nor a single explanation of how vegetation affects climate.

The manuscript includes information that is completely extraneous. For example, on p. 983, "...the Maastrichtian stage...precedes the terminal mass-extinction event..." Okay, but this paper doesn't focus on the mass extinction event. The next paragraph is a description of the Maastrichtian geography. Why? Fig. 1 and 2 illustrate the geography. The final three paragraphs of the Discussion discuss the possibility of Maastrichtian glaciation. Interesting, but completely irrelevant to the study at hand.

The review of Cretaceous modeling in Section 2 suffers from the same lack of purpose. It is not clear what the point it. In addition, the review is incomplete and misses many important Cretaceous modeling studies (Brady et al., 1998; Poulsen et al., 1998, 1999, 2001, 2003, 2006; Hotinski and Toggweiler, 2003; Donnadieu et al., 2006; Puceat et al., 2005; Fluteau et al., 2007; and more). Many of these papers should be cited at other points in the paper as well. For example, the discussion of flow in Tethys in Section 5.6 should reference Poulsen et al. (1998) and Hotinski and Toggweiler (2003); discussion of surface ocean climatology and stability should reference Brady et al. (1998) and Poulsen et al. (2001); discussion of the hydrologic cycle should reference Poulsen et al. (1999; 2007); discussions of Maastrichtian proxy data should include Frank and Arthur (1999), D'Hondt and Arthur (1995, 1996), Puceat et al. (2003, 2007), MacLeod and Huber (2001), and many more. The point is that the paper is not well referenced.

The experimental design is mostly fine. There are a couple of issues. First, it is not clear why present-day trace gas concentrations (N_2O, CH_4) were used, and what present-day means (1990?). This becomes problematic when comparing the Cretaceous experiments to a pre-industrial (i.e. pre-industrial trace gas concentrations) sim-

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ulation. Second, there is no discussion of how river runoff was treated in the model. Is there a river-routing scheme? Third, the integration time is much too short for these experiments to be in equilibrium. Perhaps this is okay, if the study focuses on surface conditions, but it is incumbent upon the authors to prove that it is okay. Is there still a trend in upper ocean temperatures? Is the atmosphere in radiative balance? Fourth, the vegetation model includes grasses, which had apparently not evolved yet. In many past climate studies, it is typical to either remove the grass category or to replace it. Hunter et al. decided to do nothing. Why? Does it matter? The authors should show with another sensitivity experiment the influence of grasses on their simulation.

The real problems for this paper arise in the Results. There are a number of issues. First, the results are too descriptive. Much of the content could be summarized in tables. For example, the description of temperature differences between experiments, including the temperature differences at various latitudinal bands and comparisons with previous studies, should all be contained in a table. The other sections of the Results should be treated similarly.

Second, the results are very superficial, and purely descriptive. The paper basically describes the surface climatology with no analyses of the cause of the differences between experiments. There is no insight provided into any of the climate differences. For example, p. 994, "This increase appears to be driven by the differences between PreInd and Maas1 experiments..." What differences exactly? (It is likely due to differences in ocean-land fraction.) The authors describe changes between their Maas4CON and Maas4VEG experiments, yet never state how the vegetation differs and what about the vegetation causes the climate (temperature, precipitation change). For example, p. 994, "...we observe an increase of 4% in the global average and 9% in the average land precipitation." Why? (It is likely linked to the simulation of forests, and their ability to trap latent heat.) These are two obvious examples, but the problem persists throughout the Results. The advantage to using a climate model is that all of the changes can be understood with enough work, either through analyses or through

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additional experiments. The authors have not done this work. They make claims (e.g., p. 993, "...this apparent improvement...could be a consequence of the poor representation of these regions within the GCM...") without providing any validation.

Third, the results are not sufficiently supported in the paper. For example, Section 5.2, focuses on thermal seasonality. Yet, there is not a single figure showing a seasonal result, let alone the seasonal cycle at a location. The manuscript discusses the results from a vegetation model, yet there is not a single plot of vegetation!

Fourth, the comparisons with other modeling results contribute little to the study. Comparison for comparison sake is not useful. Why compare results with Bush and Philander (1997) and Otto-Bliesner et al. (2002) if these comparisons provide no insight into the cause of the differences? It is also not clear why these studies were chosen for comparison, and not some other. This should be stated.

Fifth, the comparison with proxy data is insufficient, and again superficial. At the very least, the proxy data and model results should be plotted together. Beyond this, there needs to be discussion and analysis of proxy limitations. There have been a number of papers that have critically examined model-data comparisons, and that examined the limitations of the proxies. The authors should look to these for guidance.

Sixth, most of the results, explanations, and discussion of the results can already be found in the literature. This paper presents nothing new.

Some minor points: p. 990, reference to Kump et al. (1999). This is a fine text book, but it is not an appropriate reference for the evolution of solar luminosity. An appropriate reference is Gough (1991).

The use of "ensemble" to describe this series of sensitivity runs is confusing and inappropriate. An "ensemble" usually refers to a series of runs in which the initial conditions have been altered.

p. 995, "Molluscs" and "Planktonic foraminfera" are not proper names and should not

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be capitalized.

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