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Climate of the Past Discussions

Interactive comment on "Mountain uplift and the threshold for sustained Northern Hemisphere Glaciation" *by* G. L. Foster et al.

Anonymous Referee #3

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Review of "Mountain uplift and the threshold for sustained Northern Hemisphere Glaciation" Author(s): G.L. Foster et al. MS No.: cp-2009-63 Submitted to Climate of the Past

This paper examines the hypothesis that the low elevation of the North American Cordillera in the early-middle Miocene may have contributed to a mild climate, despite a low level of atmospheric pCO2, as suggested by proxy data. The authors present results from a GCM as well as from an offline ice sheet model that support the idea that uplift of the North American Cordillera in the Late Miocene was likely responsible for global cooling during that time period. This study provides a more detailed analysis of the role of topographic height in lowering the pCO2 threshold required for glaciation than previous studies by focusing on the role the North American coastal range on con-

tinental climate. The results are consistent with previous work (DeConto et al., 2008; Kutzbach et al., 1989), and clearly support our understanding of the role of topography on the CO2 threshold for glaciation. The fact that uplift enhances the likelihood of glaciation is not a new result. The strength of this paper lies in the finer detail analysis.

The authors provide a nice overview of regional topographic evolution and relevant work in this area and this provides sufficient justification for the topographic boundary conditions (used in GCM and ice sheet modeling). However, a couple of points regarding the experimental setup should be clarified (see 'Specific Comments' below), particularly in regard to the use of pre-industrial conditions. As the authors are interested in climate shifts through the Miocene, I wonder how appropriate it is to use a pre-industrial 'background' state? I think the conclusions of the paper regarding the role of topography on North American climate are well supported by the results, but I wonder why the authors have not also extended their discussion to include effects of changing topography on glacial initiation in Europe and Asia? If this is beyond the scope of the paper, then I think the paper should state as much, and be reworded to emphasize the focus on 'North American' glacial initiation, rather than 'Northern Hemisphere' glaciation. The inability to model feedbacks of a growing ice sheet also presents a challenge in the interpretation of results. Nevertheless, I think the authors have adequately addressed this in the text. Overall, this paper represents a step toward furthering our understanding of the role of topography in climate and stresses the importance of further studies examining topographic evolution throughout Earth's history. The paper is well written in general, and should be published with some minor corrections and clarifications as outlined below.

Specific Comments: (1)What is the background pCO2 level used in the GCM experiments? I am assuming 280ppm (pre-industrial)? (2) If this is a pre-industrial simulation, is there a fixed ice sheet on Greenland? What feedback does this have on climate? (3) In the 'Results' section, you mention that there is a limited model spin-up – This is the first mention of this that I can find. What is the spin-up time on the GCM experi-

ments? Are you convinced that the atmosphere and surface-ocean are equilibrated? There needs to be some discussion of this. (4) Is the 'low-45N' ice sheet simulation also 'cold-orbit' (as in the control)? This wasn't clear in the text. Is there a difference in the climate driving these ice-sheet two simulations? (I am assuming that there isn't, but this needs to be clarified).

Interactive comment on Clim. Past Discuss., 5, 2439, 2009.