

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

Anonymous Referee #2

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I thank the reviewers for their reply to my main concerns about the manuscript. I still have some questions with regard to the relationship between the views of the authors with regard to the relationship between Greenland, North American and Northern Hemisphere Glaciation, and one remark with regard to the lack of albedo feedbacks in L08. I will present them in the following. In order to clarify the relationship between FCPD and L08, the authors explain the differences in the setup of the simulations, and state that FCPD deals only with Northern Hemisphere, while L08 deals only with Greenland glaciation. However, an explanation of the connection of both with Northern Hemisphere glaciation is still missing. In my view, this explanation should include a clear differentiation between the processes involved in ephemeral glaciations during the Miocene and the permanent glaciation at $\sim 3\text{Ma}$ (or “intensification” of glaciation, as the authors call it). The authors provide figure R1, but did not provide an explanation of

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the figure. Could you please describe the figure? In L08, the authors aim to explain the “intensification” of glaciation at ~ 3 Ma, as opposed to the ephemeral glaciations between 38 to 4 million years ago. They conclude that Late Pliocene Greenland glaciation was controlled by a decline in atmospheric CO₂ levels (title of the manuscript). This glaciation is a non-ephemeral glaciation. However, with figure R1, the authors suggest that Greenland was glaciated before during the Miocene, given sufficiently low concentrations of CO₂. Shall we conclude from this that L08 aimed to explain permanent glaciation of Greenland at ~ 3 Ma, but only explained ephemeral glaciation of Greenland? What was the environmental factor that made glaciation at ~ 3 Ma permanent and ephemeral during the Miocene? The authors seem to rule out CO₂ as this one in the introduction of FCPD when they suggest that low levels of CO₂ were reached already during the Miocene. With regard to the lack of albedo feedbacks in FCPD, I agree with the authors in that what we see is a “minimum response”. As the ice sheet grows, the atmosphere does not see a change of albedo because both systems are not fully (or “bi-directionally”, if you prefer) coupled. However, when, in their response, the authors explain the modelling of albedo feedbacks in L08, they mix two different processes: the role of albedo feedbacks for the inception of the Greenland ice sheet, (that is, for its formation, or “first” appearance) and for its growth. Yes, their ice sheet of the CO₂ simulation from the ice sheet model grows more than the one they prescribed as initial condition in the atmospheric model, and they state that, if the atmosphere could see these area increase, it would cool more and make the ice sheet grow more. But with my comment about ice sheet feedbacks I was referring to the process of inception: the authors are prescribing an ice sheet in the atmospheric model as initial condition, and the modelled regional climate permits the growth of the ice sheet. Would the Greenland ice sheet form at all or would it be as big if the authors do not prescribe its existence “a priori”? If the authors were investigating the inception of the ice sheet, why did they prescribe it already as initial condition? Perhaps the authors’ choice of model set-up is conditioning one of the conclusions of L08, last paragraph: “It [this study] does, however, support the assertion that some ice did exist on Greenland

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before the onset of extensive glaciation”?

Also, I have an additional question with regard to FCPD only. In the introduction and the methodology section, the authors state that they present new modelling results with a fully coupled ocean-atmosphere GCM. However, in “Results” they tell us that the limited spin up time means that only changes in the atmospheric circulation and ocean mixed layer can be examined. Does this mean that, in this case, the model is equivalent to an AGCM coupled with a slab ocean model? The reader should be cautioned before that the AOGCM is not being used to its full potential and therefore ocean circulation is not modeled in this study.

I hope this comments will stimulate further discussion and aid to the clarification of the authors’ view on the problem of glaciation at ~3Ma. Thanks!

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

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