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Interactive comment on “Hindcasting the continuum of Dansgaard–Oeschger variability: mechanisms, patterns and timing” by L. Menviel et al.

Anonymous Referee #2

Received and published: 20 October 2013

The paper provides new information about dynamics underlying millennial scale variability using transient global simulation with an intermediate complexity model for the period 50 ka to 30 ka BP. The main results of the paper are two fold: to assess the validity of this kind of model to hindcast climate changes with glacial boundary conditions, and therefore to examine the different response characteristics of various climate variable at regional scale to AMOC changes based on a close comparison with paleo-data. A schematic view is tentatively proposed to explain the effect of the Northern Hemisphere ice sheet instabilities on global climate at millennial time scale. In general the paper is well written and contain all elements for a relevant discussion. To my opinion this study constitutes a very interesting approach and is suitable for publi-

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cation in Climate of the Past. However the Take Home Message of the paper should be strengthened, in particular some of the questions listed in the introduction are not clearly addressed in the discussion part. Taken together, parts of the introduction and discussion need to be clarified. This concerns two major points and minor points that need to be taken into account before publication and that I develop below.

-Point 1: Climate mechanisms This is an important issue of the paper that could be more emphasized, in particular in the Introduction part. In the present version, three “key” questions are listed in the Introduction part, for which (at least for the last 2 questions) I do not see any clear answer in the discussion (presented as a long conclusion). To clarify the message of the paper I would suggest to revise the important issues of the paper in order to provide a more balanced discussion to highlight the ability of the model to capture the dominant modes of HE and DO variability and the overall good data/model agreement. Following this point, part of the discussion (rather than in the conclusion part) should be devoted to a more detailed discussion of the dynamical processes based on the physics of the model and the data/model comparison; for example, what causes -enhanced tropical wind during HE/stadials, - a decrease in Asian monsoon activity during HE, -the muted temperature response in central Europe compared to the western Mediterranean Sea. To my opinion the discussion needs also to mention processes that are not fully captured by the models and that still remain unsolved: for example, results in Naafs et al., (2013) paper report a warming in the North Atlantic during HE, involving a northward expansion of the subtropical gyre. The last point related to climate mechanisms concerns the freshwater forcing which has been prescribed in order to obtain a match between modelled and observed SST on the Iberic margin. A comment is missing about the magnitude of the freshwater flux used in this study compared to other studies simulating HE or DO type events.

-Point 2: Model / data comparison One of the strength of the paper is to show some comparison between modelled results and paleo-data (SST, d18O, SSS, reflectance, ...). While the modelled temperature and precipitation captures very well temperature

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and precipitation proxy variability, it should be mentioned that some of the proxy are not direct indicators of these parameters particularly for d18O which can be controlled by remote signals rather than local (water vapor source for example, see Legrande et al., 2010). In the second part of the conclusion, SSS reconstructions in the Nordic seas are considered as representative of the salinity of the North Atlantic. I would introduce this SSS reconstruction more carefully since the Nordic Sea (Irminger basin) reveal a highly variable environment also shown in Elliott et al., (1999). While the Irminger basin reveals a synchronous response with the central north Atlantic during Heinrich events, it is highly influenced by coastal ice sheet and ice shelves from the Nordic area at millennial scale. I am not sure if introducing the term “Heinrich event 3.2” is really relevant for this paper. This cold event is well expressed in SST reconstructions but does not appear very clearly in North Atlantic IRD records. Therefore a more detailed discussion should be necessary to look at the spatial distribution of this event over the whole North Atlantic, but as already said it does not add value to the paper.

-Minor points -Concerning the EOF analysis results, I was wondering if it is worth to discuss for precipitation the first EOF, which explains 16% of the variance? -Is it possible to merge fig. 2 and 3 since same parameters appear (Med. SST) ? -The discussion in the section “Timing and duration of events” reveals differences between HE timing and timing of Heinrich layers in the sediments of about 3ky. Could you shortly comment on this point? -Fig. 10 is not very clear: why is there a one way arrow for temperature and precipitation boxes? In the discussion related to figure 10, it is stated that “instabilities from the Laurentide ice sheet were associated with much larger iceberg” . . leading to the HE. Say it more carefully since HE3 is mainly related to the Fennoscandian ice sheets (see Grousset et al.). -Why is the considered time interval changing in figure 11?

Interactive comment on Clim. Past Discuss., 9, 4771, 2013.

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