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Climate of the Past Discussions

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Interactive comment on "Deciphering the spatio-temporal complexity of climate change of the last deglaciation: a model analysis" *by* D. M. Roche et al.

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We reply to the individual comments raised by the reviewer below.

Individual comments: Abstract: 1 Introduction: The introduction explains the sequence and factors of deglaciation. However, some studies suggest that a glacial termination materializes because Earth's climate system passes a critical threshold (S. Barker et al., Nature 457, 1097 (2009); F. Lamy et al., Earth Planet. Sci. Lett. 259, 400 (2007); E. W. Wolff, H. Fischer, R. Rothlisberger, Nat. Geosci. 2,206 (2009)). Also consider that it is theoretically possible that a particular set of boundary conditions may not give rise to a unique climate state (Lorentz, 1968, 1970, 1976). Please include this in the introduction.

Barker, Lamy and Wolff are now cited in the introduction. The discussion of the determinism of climate change in a theoretical sense (Lorenz, 68, 76) seems a bit remote from the introduction as it stands. Model experiments as we are doing them somehow assumes that the system is deterministic, at least to a certain extent.

Lines 41-46 should be rephrased, to introduce the concept that this is study of simulation model results, not a modeling study. A modeling study would involve implementing either new mathematical descriptions or new numerical models for the physical processes they describe. DONE. The sentence now reads : "[...] by performing and analysing results from a model simulation to assess, within the physical processes contained in our climate model,"

2.1 Model description Please expand on the differences compared to other intermediate complexity ES models. Stress what are the strong points and the weak points in comparison to these other models. DONE

2.2 Deglaciation forcing

Lines 86-99: As the authors are using ice5gv1.2 ice sheet evaluation, the exact method how this is incorporated, how is the interpolation done etc. should be mentioned. How is the difference between ice thicknesses given at two consecutive heights assumed to melt? As the authors state, it is not obvious how the changes should be taken into account to conserve mass, momentum and salinity. The text has been modified to explain in more details the forcing procedure, as also requested by reviewer nb. 2.

Lines 117-118: Please rephrase “theoretical framework “. This study does not bring forth new propositions nor new hypothesis, but to test the reliability and validity of the warming signal in simulation model results. DONE. The sentence including "Theoretical framework" has been changed to "in climate in a more abstract framework."

Lines 119-120: Even if the aim of the study is not in a detailed data-model comparison,

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it should be considered as the simulation models challenge the question of reliability. We need to know why it is possible to relay on the results derived from the simulations. So at least mention at what data sets one should look at for a comparisons. A recently published wrok as made progress in the compilation of datasets that could be a modelling target for future studies (Shakun, JD & Carlson AE, QSR 2010, 29, 1801-1816). The reference is now cited in the text but in the conclusion where this aspects are discussed more logically.

3 Analysis Method

Lines 125-126: Please define what the authors mean by first significant warming in reality? Is it a synchronous or asynchronous event? How should this be incorporated in the modeling results testing context?

We have detailed this aspect as suggested. The following sentences were addedÂă: "The first significant warming is, a priori, an asynchronous event in each grid cell of the model, though some regional patterns are expected to emerge. In reality this first significant warming would be the first detectable warming in the temperature recorded by any method."

In lines 126-129: Mention that the control run (constant conditions) is the null hypothesis compared to transient run.

We added the following sentence in the revised versionÂă: "From a statistical point of view, the reference LGM period is thus the null hypothesis compared to the deglacial sample considered."

Lines 140-142: As the analysis method is about looking at local starts, how to conceptually get from local starts to global start? Can the study bring more that global average? We do not discuss global start as it is not evident from the nature of climate change what a global start is. Even when considering the sea-level change, which is a quite global variable, one has to discuss the melting of specific areas of the ice-sheets,

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thus local start. Therefore, there is no such thing as a global start in climate change, except perhaps in the energy balance of the Earth. Even the insolation at the top of the atmosphere, hence the Milankovitch forcing, is not global but local (geographic and seasonal).

How about also including/introducing uncertainty analysis, inference about the simulation model output given uncertainty in simulation model input? Quantitative knowledge of these limits is an important prerequisite for designing the diagnostic procedures and for interpreting results adequately. The probability of detection could be defined as the chance for identifying, from one pair of model simulations, a prescribed change. This quantity is equivalent to the power of the statistical test and it depends on the magnitude of the change, the length of the model integrations and the rarity of events under consideration. See for example Frei, C., 2003: Statistical limitations for diagnosing changes in extremes from climate model simulations. There seems to be a misconception on the forcings used in our study. All changes considered (pCO2, ice-sheets and insolation) are very smooth. This smoothness is clearly shown in Figures 1 and 2. There is thus no uncertainty in the variability of the forcing factors. The changes prescribed that lead to the climate changes we analyse are anyhow big relative to the inherent uncertainty of data generation. A detailed uncertainty analysis is beyond the scope of this paper.

4.1 Results Annual mean

The results analysis pinpoints several interesting responses. Why not underline the time periods when slow forcing was the main driving force? What can be deduced from those time periods? It would be really interesting to do the simulation results – proxy records comparison for these time periods. The simulations we presented only include slow forcing. Thus, it is always the main driving force. In the real system, there are time periods like the begining of the deglaciation that are mainly driven by slow forcing (cf. Figure 3), some others when fast response to an unknown forcing (e.g. Bolling-Allerod) is the main driver. As noted in the text (cf. conclusion in the revised version), the model-data comparison will be the second step to consider, first should be the confirmation of the results in another climate model for the same forcings and same period of time.

4.2 Seasonal means

Lines 198- 199: the authors state that they are doing the confirmation of previous results. The proponents of simulations as theory-based inferential processes ignore, to some extent the iterative process of reliability. This has been suggested by Boumans (2004) (Boumans, M.: The reliability of an Instrument. Social Epistemology 18(2-3):215-246. The value of model predictions is undermined by their uncertainty, which arises primarily from the fact that our models of complex natural systems are always open (see Oreskes, 2000). Models can never fully specify the systems that they describe, and therefore their predictions are always subject to uncertainties that we cannot fully specify. Moreover, the attempt to make models capture the complexities of natural systems leads to a paradox: the more we strive for realism by incorporating as many as possible of the different processes and parameters that we believe to be operating in the system, the more difficult it is for us to know if our tests of the model are meaningful. Here, one cannot confirm simulation model results inherently: both are products of the same simulation run.

This it true, but inherent to the process of model simulation. In our case, all model runs with the same model are consistent with each other. Thus we may confirm the model results of Timmermann et al., as the same model is used.

4.3 Precipitation evolution

Lines 216-217: explain why the change in precipitation is the most likely? Because the glacial to interglacial changes in those areas are dominated by precipitation changes, not temperature changes, as seen in data and model (cf. MARGO and Braconnot, PMIP-2, Roche, CP, LGM). The wording has been modified in the text to account for this comment.

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Line 225: Define ITCZ. InterTropical Convergence Zone. This is now added in the revised version.

4.4 Impact of interannual variability

How about looking autocorrelations?

Autocorrelations are used to estimate the long term memory of the system. In our case, we are looking at the surface temperature on at least 25 years timescale. The model as no memory on such scales in the surface temperature fields. The model has some interannual variability but no marked centennial variability with a precise frequency. cf. Friedrich et al., GMD, 2010 for the type of frequency present in the LOVECLIM model with different forcings.

5. Discussion Lines 310-313: Natural climate is of course one trajectory of many possible solutions, but our reconstruction of the trajectory is uncertain. Simulation model is a heuristic tools to facilitate the study complex phenomena. Even the best models of natural phenomena do not depict it completely. The more complex the model gets, the more difficult it is to test the model. I would drop this analogy statement. It was not our intention to claim that our model could depict climate completely. We have now clarified that we intended to point to the anology between the analysis of only one run (instead of an ensemble that would capture the range of trajectories) and the climate as registered in proxy data. The sentence now reads: "Analysing a single simulation is therefore close to what is recorded by proxy data, albeit that we have a perfect recording of our simulated climate within our idealized "model world" as opposed to the imperfect recording of the Earth's climate in proxy data."

Lines 304-309: Ensemble runs could really open new study possibilities. Even if they would have needed unattainable computer power, what could they reveal? How about replacing temporal samples with ensemble samples? What size? Discuss computational constraints => what was the constraint? How to get past it?

We fully agree with the reviewer on the point that ensemble runs would be a nice addition. Running ~100 members (cf. comment to reviewer nb.1) of the deglaciation would require 100 processors during one month, not unnattainable but quite costly. In the manuscript as it stands this would be quite beyond the scope of the study. Furthermore, this would not affect significantly the timing of climatic changes, since they are much slower (>1 kyr) than the internal variability of the system (< 100yr).

Lines 328-332: "the readers should not forget" – please do not underestimate the readers, better to either a) compare to other model results or b) state what is plausible/not plausible in the model context. Do we see seasonal trends? Discuss the importance of slow forcing factors, are they visible or are abrupt events needed to induce noticeable changes? Discuss how the missing ice sheet component affects comparisons with results and records?

In the whole study we have discussed the importance of slow forcing factors. Yes, they are noticeable as they are the only forcings we include, and we notice the response (see results). The comparison to another model with the same forcings will be performed in the future within the frame of the ORMEN project. As stated in the manuscript, this is but a starting point. The missing ice-sheet component does not affect the comparison to records in itself as we prescribed an ice-sheet from reconstructions. What necessarily affect the comparison is the lack of freshwater fluxes from melting ice-sheets as discussed at length in the manuscript.

6 CONCLUSIONS

Please clarify based on comment above, what can really be concluded from the simulation results and what cannot? Be specific on uncertainties.

The main conclusions could be substantiated with: a) First, looking at the areas to react: point out proxies where to look. This could really be an interesting next study!

This is a important point that we discussed only little in the previous version of the

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manuscript. We want indeed to go for a model data comparison, but, in our mind, this shall be built on two steps: 1) substantiate our own modeling results with another model as the results may be model dependent. Even more so as the model do not include freshwater forcings. 2) go for a model – data comparison in the spirit of what has been done on a global scale by Shakun et al., 2010 (QSR). The text of the present version has been extended on these aspects.

b) Secondly, consider the passive areas: is there reasonable possibility to deduct lag? "We think you mean this, then not!" The paragraph referred to by the reviewer indeed presented some inconsistencies in the wording. The text as been revised as follow: "Therefore caution on the spatial structure or robustness is needed when trying to infer leads and lags from existing deglaciation records or model results in these regions before any physical interpretation can be drawn."

c) Questioning the climate change is here is irrelevant – it should be done in introduction and in analysis methods. There the authors discuss first significant warming, without definition. Define it there, and here state if the definition was good/restrictive/... d) The valuable and strong point of this work is defining the sample size. In conclusions, do not give self-evident truths ("has to be long enough to be detected against background noise"). State your results, and against what proxies these could be validated.

c) & d) Reviewer #2 has the opposite opinion on the subject. He states that "A last important finding for both model and proxy data, is that the definition of the time frame of climate change (as a time long enough to permit detection against background noise) will vary depending on the time series length and the spatio-temporal resolution, and hence must be seen in relation to what is studied." It might be that this inference is a self-evident truth for some, but not for others. In which case, it does not hurt to state it in the manuscript nonetheless.

Interactive comment on Clim. Past Discuss., 6, 2593, 2010.