

REFeree'S REPORT on Morée et al., *essd-2019-79*

Title: A Last Glacial Maximum forcing dataset for ocean modelling

In this paper, Morée et al. present a new standard dataset for forcing of ocean-ice model simulations of the Last Glacial Maximum (LGM, ~21ka BP). This kind of forcing dataset is already available for contemporary climate conditions but has so far been lacking for LGM climate. The availability of this type of dataset provides a possibility for standardized model intercomparison for the response of ocean-ice models to prescribed LGM conditions, and for moving from interglacial to glacial conditions. Such a dataset is also highly applicable when ocean-ice model simulations are preferable in the interest of reducing computational cost and run-times, as is clearly described in the paper. In previous reviews of this manuscript, reviewers raised major concerns about

1. the adequacy of using PMIP3 model averages, and about the fact that using such a model mean will be inconsistent with atmospheric dynamics,
2. re-referencing of 2 m to 10 m quantities,
3. the potential for using 3D model output, and
4. the lack of river runoff flux anomalies.

In the response to reviewers in the previous round, the authors have provided adequate responses to each of these concerns. First, they have clarified the manuscript according to their intentions for point 1. Next, they have re-evaluated points 2 and 3 and based on this evaluation decided to leave out the re-referencing step and found that the surface salinity anomaly can be improved by using 3D model output. For other fields, they explain why 3D data is not a viable/useful addition. Finally, for point 4, they have added the river runoff flux anomalies to their dataset. As these major concerns have been addressed, and I have no further major issues with the presented scientific approach, I recommend this manuscript for publication after minor revisions.

General comments

Text: The manuscript is well structured, and the text is generally easy to read, though some concepts and methods are introduced without proper explanation (see Specific comments). Therefore, it would be helpful if some clarifications were added. I have also given suggestions to smaller changes of the text that I find would improve the reading experience.

Figures: When showing anomaly figures, it is always easier for the reader to understand the fields resulting from these anomalies if the original fields (in this case, PMIP3 piControl) are also shown. I therefore suggest adding figures of the piControl fields for each of the variables to the supplementary material. I have also suggested to add model spread of the

sea-ice anomaly to supplementary material, as model spread in other variables is assumed to be based on the spread in modelled sea-ice.

Data availability: To simplify for the user, it would be great to add a download link which allows download of the entire dataset at once. I found it difficult to know if I had downloaded all files in the containers and sub-containers. Once downloaded, the dataset appears to be easy to handle and well documented, though I have not yet had the opportunity to apply it to simulations.

Specific comments

Abstract

Page 1, line 11: “*ocean-sea-ice*” – Though ‘ocean-sea-ice’ is adequate in the sense that the model does not simulate land-ice, it does not read well. I would suggest changing to ‘ocean-and sea-ice’. The term ocean-sea-ice-atmosphere model is used later in the paper (P 2, L 8), and by clearly stating ocean and sea-ice here, the second contraction of these words becomes clearer.

Page 1, lines 12-14: “*The data presented here are derived from fully coupled paleoclimate simulations [...]*” – The previous two sentences (lines 8-12) seem to be about justifying the use of ocean-ice only models. Here, you very suddenly switch to talking about data from fully coupled simulations, which got me confused. Consider making this transition smoother by first introducing the problem addressed by your dataset (now on lines 21-22) rather than the dataset itself.

Page 1, line 19: “*pre-industrial times*” – The anomaly fields presented in this paper are based on PMIP3-simulations and are thus LGM-PI anomalies. However, it is also stated that the fields are optimized for use with the CORE forcing fields, which are not explicitly pre-industrial, but which have rather been evaluated against contemporary data. This should be clarified in the text (see also page 2, lines 32-33)

1 Introduction

Page 1, lines 26-28: “*[...] the LGM is extensively studied in modelling frameworks (Menviel et al., 2017; Brady et al., 2012; Otto-Bliesner et al., 2007).*” – For a topic that has been studied as extensively as the LGM in modelling frameworks, only citing three studies seems too little. I would suggest adding a few more relevant references and adding an e.g. before the given references.

Page 2, lines 3-5: “*Complex fully coupled models can typically not be run into full equilibrium (which requires hundreds to thousands of years of integration) due to computational*

costs (Eyring et al., 2016). Therefore, the PMIP3 models exhibit model drift (especially in the deep ocean, e.g. Marzocchi and Jansen, 2017).” – It would be valuable for the reader if you explained/exemplified, in one sentence, why equilibrium states are desirable and/or what kind of problems you get when analyzing a model that drifts.

Page 2, lines 8-9: “We refer to a forced ocean model as a model of the ocean-sea-ice-atmosphere system in which the atmosphere is represented by prescribed 2-D forcing fields.” – It would be helpful to add a few references to widely used examples of such models, or papers that make intercomparisons.

Page 2, lines 22-27: starting with “The description of the procedure [...]” – I find it a little odd to present Section 3 before you present Section 2. In my opinion, it disturbs the fluidity in reading the text, and it is not clear to me why you have chosen to do so.

2 General description of the dataset

Page 2, line 29: “pre-industrial state” – What is the definition of pre-industrial in PMIP3?

Page 2, lines 32-33: “and are optimized for use in combination with Coordinated Ocean-ice Reference Experiments (CORE) forcing fields (Griffies et al., 2009).” – It should be clarified that these fields are in fact not strictly pre-industrial but rather corresponding to contemporary forcing. This was pointed out in a previous version of the manuscript, but unfortunately, that information has been removed.

Page 2, lines 33-34: “The use of an anomaly forcing implies the assumption that no changes in temporal or spatial variability occurred between the lgm and piControl states beyond changes in the mean.” – What are the implications of this assumption? It does not seem to be discussed anywhere in the paper.

Page 3, lines 2-3: “A discussion on the limitations of our dataset is provided in Sect. 4.” – Why not mention this in the introduction, where the rest of the structure of the paper is presented? It seems unnecessary that the reader needs to search for this information.

Page 3, lines 13-14: “[...] proxy-based reconstructions are available for some of the variables (e.g., temperature)” – ‘some of’ feels unnecessarily vague. You only have seven variables. You could say specifically for which variables proxy-based reconstructions are (currently) available and include relevant references.

Page 3, line 23: “[...] which have been extensively used in the ocean modelling community (e.g. Griffies et al., 2009; Schwinger et al., 2016).” – If they have been extensively used, it would be advisable to add a few more references here.

Page 3, lines 30-31: *“This choice ensures that the anomaly forcing data can be used with any pre-industrial land-sea mask.”* – Is this true in any forced-model resolution? (I think here of models with lower-than-average horizontal resolution.)

3 The variables

Page 4, line 4: *“[...] a 6-hour time resolution”* – There is no description in the text of why some variables are time interpolated to 6-hour time resolution and some to a daily time resolution. It would be helpful for the reader if this was outlined in Section 2.

Page 4, line 8: *“without any strong spatial pattern”* – Here, I disagree with the authors. I do see a clear spatial pattern, with more spread in the Northern Hemisphere and particularly in the western boundary current regions/close to the major Northern ice sheets. I would therefore like to know what the authors base this statement on.

Page 5, lines 18-20: *“The inter-model spread ($\sim 1-3 \text{ m s}^{-1}$) has little structure except for the $\sim 4-5 \text{ m s}^{-1}$ disagreement in the Southern Ocean south of $\sim 40^\circ\text{S}$, and the $\sim 3-5 \text{ m s}^{-1}$ disagreement in the North Atlantic (Fig. 6).”* – These disagreement zones are indeed quite pronounced. It gets me wondering what the likely explanation for the disagreements in each of these two zones is. Later, in the Discussion (page 8, lines 2-3), you state that these explanations are beyond the scope of this study, though in this section you do provide explanations for some of the other variables. I would argue that if these explanations have been provided in other PMIP3-studies, it would not be a major effort for the authors to include them. For the North Atlantic, this is not evident, but for the Southern Ocean, is it possible that the explanation is found in the differences in the jet position relative to the sea-ice edge, as described by Sime et al., 2016? (Sime, L. C., Hodgson, D., Bracegirdle, T. J., Allen, C., Perren, B., Roberts, S., de Boer, A. M., 2016: Sea ice led to poleward-shifted winds at the Last Glacial Maximum: the influence of state dependency on CMIP5 and PMIP3 models, *Clim. Past*, 12, 22412253, doi: /10.5194/cp-12-2241-2016)

Page 6, lines 24-26: *“Regarding the dynamical inconsistencies, it is important to note that the CORE forcing itself is a mixture of reanalysis and 25 observational data products and as such not dynamically consistent.”* – This sentence is phrased in a way that makes me feel like the CORE forcing has been previously discussed in the paragraph, which is not the case. Consider rephrasing or adding a sentence ‘Our dataset has been adapted to be an extension of the CORE forcing.’ (or similar) before this sentence.

Page 8, lines 2-3: *“The attribution of the model spread to specific processes is beyond the scope of this article [...]”* – Yet the authors do attempt to provide these explanations for several variables. For a user of the dataset, I think it would be useful to know where the model spread originates from, and while reading the article, I found it somewhat frustrating that some of these explanations were missing (see my comment for page 5, lines 18-20). I

feel that, in those cases where that information is available from other PMIP3 research, it is something that the authors could provide without additional analysis. For those variables that explanations for the model spread are given, there are generally no motivations for these explanations, for example in the form of a cited paper or a confirming supplementary figure (see e.g. page 4, lines 14-15; and page 5, lines 25-26, which could both be confirmed with a figure of model spread in sea-ice cover). Based on the fact that some explanations are indeed already given, I would advise to rephrase this sentence to ‘The complete attribution of the model spread [...]’, whether or not any further explanations can be added.

Page 8, line 5: “*Finally, there is no other way [...]*” – I would suggest expressing this a bit more cautiously, by changing to *Finally, there is currently no other way [...]*

Technical corrections

Page 3, line 9: “*yearly*” – annual

Page 3, line 19: “*The SSS fields is [...]*” – The SSS fields are [...]

Page 3, line 35: “*under the assumption of an unchanged spatial and temporal variability*” – remove an

Page 4, lines 6 and 8: “*2-3 kg kg⁻¹” and “1-2 kg kg⁻¹”* – According to the colourbar in Fig. 1, the magnitude of the specific humidity anomaly and spread is 10-3 kg kg⁻¹.

Page 4, line 23: “*symmetrically*” – symmetrical, i.e. a symmetrical spatial pattern, though it is not entirely clear what symmetrically refers back to here. Consider either changing to symmetrical or rephrasing the sentence.

Page 5, line 24: The temperature anomalies of 10K and ~2.5K are presumably supposed to be negative.