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Interactive comment on "Constraining the Last Glacial Maximum climate by data-model (iLOVECLIM) comparison using oxygen stable isotopes" *by* T. Caley et al.

Anonymous Referee #1

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Review

This manuscript by T. Caley and co-workers examines the results of a Last Glacial Maximum (LGM) climate simulation using the iLOVECLIM Earth system model of intermediate complexity (EMIC), which has been enhanced by a module for stable water isotope diagnostics. The presented analyses comprise a comparison of the simulated LGM climate anomalies (with respect to a pre-industrial reference simulation) with isotope-related paleo-records from a variety of different paleoclimate archives: polar ice cores, speleothems, as well as planktic and benthic foraminifera data. On a global scale, model results of the isotopic composition of precipitation agree with the available isotope records. Most depleted LGM values are found at the high-latitudinal

polar regions, strong but spatially varying depletion is seen over the continents and very low depleted (or even slightly enriched) LGM anomalies are simulated for tropical precipitation. Data-model comparison for sea surface temperatures indicates that the iLOVECLIM results are in agreement with the latest proxy-based SST reconstructions. For the North Atlantic, the model results suggest a strong mean LGM annual cooling in agreement with foraminifera data. The data-model comparison also reveals large positive calcite O-18 anomalies in the Southern Ocean as well as a large positive (negative) O-18 anomaly in seawater for the north Indian Ocean (China Sea) between the LGM and present-day climate. According to the model results, LGM changes in d18O of deep ocean waters might have not been spatially homogenous.

Although several studies on the LGM isotope distribution in precipitation and ocean sea water using either atmospheric (AGCM) or ocean general circulation models (OGCM) have been published in the past, so far only very few fully coupled AOGCMs or EMIC simulations with isotope diagnostics exist. Regarding the LGM climate, to my knowledge this is the second study with an isotope-enabled atmosphere-ocean EMIC model and thus the findings are certainly of high interest and worth to be published. However, the current presentation of the model results as well as the performed data-model comparison falls too short in several aspects. Thus, I unfortunately cannot support a publication of the manuscript in its present form but rather must ask the authors for some major revisions of their study regarding the following aspects:

(1) Model description: A description of the iLOVECLIM model is completely missing in this article. Although this paper is already the fourth manuscript in a series of recent papers describing and evaluating the O-18 isotope diagnostics scheme in the iLOVECLIM model, the authors may not assume that everybody is familiar with their model. Thus they should add a description of the iLOVECLIM model in the "Material and methods" chapter (p. 109ff). They should give a brief overview of main model components, representation of key physical processes, implementation of O-18 isotope diagnostics as well as spatial and temporal model resolution.

(2) State of equilibrium: For many coupled AOGCM setups, it is a very challenging task to reach a defined state of equilibrium in paleoclimate simulations. For their LGM study the authors simply claim that "a new equilibrium was reached after 5000yr of integration" (p. 110, l. 4-5). The authors should address in more detail (i) how they define the LGM climate equilibrium in their study, and (ii) which trends and drifts, e.g. of temperature or O-18 in deep ocean waters, may still exist in their simulation. They should also explain in more detail how they have changed the modelled river routing in the LGM simulation as compared to the present-day experiment, as the redistribution of fresh water into the oceans might have a large impact on the simulated LGM equilibrium state.

(3) Data compilation - ice cores: For EPICA Dome C and Vostok, the authors have chosen the global meteoric water line given by Rozanski et al. (1993) to convert dD measurements into d18O values. However, for Antarctica a more appropriate local meteoric water line has been published by Masson et al. (2008). Present-day values of d18O in snow at the EPICA Dome C and Vostok drill sites also exist in this data compilation by Masson and co-workers. The authors should correct their values in Table 1, accordingly.

(4) Data compilation - speleothems: The authors need to explain in more detail their criteria for the selection of the speleothem d18O anomalies listed in Table 1. E.g., for China, why have the authors chosen to include data from the Kesang cave but not the well-known Hulu cave d18O record (Wang et al., 2001)?

(5) Data compilation - marine calcite data: The data-model comparison of the simulated d18O glacial changes in ocean waters and marine sediments is based on a new compilation of calcite d18O measurements from both planktic and benthic foraminifera. I am not an expert on marine isotope data and thus cannot evaluate the quality of the compiled new data set. As one of the co-authors of this manuscript is a well-known expert in this field and has participated in some previous key data compilation efforts, e.g. the MARGO project (2009), I assume that the compilation has been done in a

scientifically sound manner. Nevertheless, even as a non-expert I rate it as somewhat problematic that the compilation contains a number of unpublished data points, which have not undergone any careful peer-review, yet. I suggest that these data points might be either omitted or reviewed by somebody with profound expertise on d18O foraminifera measurements before publication.

(6) General model evaluation #1: The authors have decided to present all their results in this study as anomalies of the LGM minus the present climate, only. They state as their motivation: "The use of anomaly renders absolute values irrelevant; it concerns a purely relative change. We can therefore ignore complications such as species-specific climate variable relationships, vital effect offsets, and calibrations of values measured relative to the VPDB standard to values on the VSMOW scale" (p. 109, l. 8-11). This statement is erroneous in any case, where the conversion includes not only a constant offset as an addend term, but also a multiplicand term. For example, the general conversion between PDB and SMOW is: d18O(SMOW) = 1.03091 * d18O(PDB) + 30.91. Thus, any LGM-present delta anomaly has to be converted between the two standards as: d18O_anom(SMOW) = 1.03091 * d18O_anom (PDB).

(7) General model evaluation #2: Furthermore, I do not agree with the authors that their consequent use of anomalies in their performed model-data comparison "renders the absolute values as irrelevant". For a profound evaluation of the model results, the overall performance of the model regarding different climate variables and regions of interest in terms of absolute values is of highly importance. Just to give a (slightly exaggerated) example: If a model overestimates present-day mean surface temperatures in a specific region by +20°C as compared to observations, but simulates a LGM-present temperature cooling of -2°C in agreement with some proxy data, such model results can certainly not be rated as a good model-data agreement! Only if one can demonstrate that a present-day bias between observational data and model results is rather small and most likely a climate-independent constant term related to some intrinsic model deficits, one might focus further model analyses on LGM anomaly values,

only. Thus, I rate it as an absolute necessity that the authors present and discuss their key findings in much greater detail with respect to the related model performance of iLOVECLIM for the present-day climate. I am aware that some of these present-day iLOVECLIM results have already been published in previous articles of the authors (Roche, 2013; Roche and Caley, 2013; Caley and Roche, 2013) but I don't think that any knowledge of these older articles should be an essential prerequisite for a thorough understanding of the results presented in this new manuscript.

(8) General model evaluation #3: The authors consider their study as the first attempt of a coupled atmosphere-ocean EMIC simulation of the Last Glacial Maximum (p. 108, l.7-9). Apparently, the authors are not aware of the study by Brennan et al. (2012) who have already published a very similar study with the UVic model. A detailed comparison and discussion of their model results to this previous study needs to be included in a revised version of this manuscript.

(9) Comparison to Antarctic ice core data: I am surprised to see a comparison of model results with Antarctic ice core data in this study. In Roche (2013) it has been clearly shown that iLOVECLIM fails to reproduce modern d18O values in precipitation over the Antarctic continent. The "absurd d18O values in precipitation" (quote from Roche, 2013, p. 1487) have been explained by a deficit of the advection module of iLOVECLIM, which is apparently not able to numerically conserve very low humidity content and related isotope values over Antarctica. This deficit is not mentioned at all in this new manuscript. Why not? Has the problematic advection scheme been replaced by a better one? If so, how? If not, why do the authors now rate their results for the LGM climate (with even lower LGM atmospheric moisture content and precipitation rates as compared to present-day) as more trustworthy than the results presented in Roche (2013)?

(10) Comparison to speleothem data: Again, I am a little bit astonished about the author's choice of including a comparison of LGM-present speleothem isotope anomalies in this study. In Caley and Roche (2013) it was stated for the iLOVECLIM model that

"limitation of the model together with the processes operating in the atmosphere, soil zone, epikarst and cave system hamper a good quantitative data– model comparison for the calcite-d18O signal." Fig. 2 in Caley and Roche (2013) clearly illustrates this model deficit with an insignificant correlation (R2 = 0.1) between measured late-Holocene calcite-d18O values and model results. Now the authors argue: "The better agreement between data and model results in term of annual mean anomaly suggests that this approach allows us to reduce complications with the atmospheric, soil and cave processes and that the model is capable to reproduce the right amplitude of changes." (p. 115, l. 20ff). In line with my general model evaluation comment #2 (see above) I think that the authors have to present a much more profound speleothem data-model comparison for a convincing line of arguments. E.g., they should add a comparison and discussion of simulated present-day d18O values versus measured late-Hololcene calcite-δ18O for the identical speleothem data set as given in Table 1. Furthermore they should quantify any model-data agreement by calculating not only a correlation coefficient, but also root mean square errors as well as uncertainty and statistical significance of the found data-model consistency.

(11) Comparison to marine isotope data: In Roche et al. (2007) it has been shown that the deep ocean circulation of an LGM simulation with the LOVECLIM model is at odds with the general accepted proxy-based view of LGM circulation. The model produces a stronger LGM overturning than for the Late Holocene, opposite to the expected weaker glacial overturning in the Atlantic Ocean. I am puzzled that this important aspect of the simulated LOVECLIM LGM climate is not discussed at all in this manuscript. Why not? In contrast to the study presented by Roche et al. (2007), the authors have now the unique opportunity to directly check if the simulated Atlantic overturning and its simulated isotopic signature is in agreement with available marine isotope data. Such analysis could lead to some new key insights about possible states of glacial overturning and its imprint in marine oxygen isotope data. It should obviously be included in a revised manuscript version.

(12) Figs. 2,3,4,6,8,11,12,13: The chosen asymmetric, highly non-linear colour code of most contour map plots makes it unnecessary difficult to determine absolute values of difference between individual isotope measurements and related simulation results. I highly recommend that the authors change the colour scale of the contour maps to a more conventional linear (or logarithmic, if necessary) one.

As I ask for major revisions before publication, I omit any minor issues and corrections of the manuscript at this stage of the review process.

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