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Interactive comment on "δ^{18}O water isotope in the *i*LOVECLIM model (version 1.0) – Part 2: Evaluation of model results against observed δ^{18}O in water samples" *by* D. M. Roche and T. Caley

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Below is my answer to the comments received by reviewer #2: the initial comments are *in italic*, my response **in bold** and the subsequent changes to the text `in typewriter` where necessary.

We thank the reviewer for his/her comments.

This manuscript describes the validation of the d18O isotope enabled model

iLOVECLIM with respect to present day observations of the isotopic composition of precipitation and ocean. Given the limited complexity of the atmospheric model (a single moist layer), generally good model-data agreement is observed. The evaluation is very thorough, and model-data mismatches are clearly identified and, where possible, explained. Although acceptance should of course be conditional on acceptance of Part I, the following comments are of a minor nature:

Page 1500 line18. If I understand correctly, snow fractionation assumes the tropopause temperature (Part I). Does the temperature at the tropopause depend upon the surface orography? I'm not clear how the isotopic composition of Greenland precipitation can be a function of altitude in the model?
Yes it does, to a certain extent. The temperature of the tropopause is not fixed in the model, being computed from the advection within the model. Thus, the presence of orography is modifying the vertical pressure structure as well as the wave pattern in the atmosphere. The Greenland tropopause temperature is thus not exactly the same as the one over the neighbouring oceans. In our case, the anomaly is of about one degree C. It is thus a weak dependence. Second, since the air mass is drying out rapidly over the orography, it also impacts the isotopic content of the precipitation, the major effect in our model, as is stated on line 19 of the same page: "This production of isotopically depleted precipitation is physically related to the Rayleigh distillation that takes place when an air parcel, lifted uphill, condenses".

Figure 3 illustrates that precipitation weighting gives Antarctic d18O in the range -10 to -50 per mil. (Note that the y-axis is labeled "mean annual d18O" – am I correct in understanding that this data is precipitation-weighted (page 1502 line 10)? If so, please re-label the axis.) The range of Antarctic d18O values is broadly consistent with observations (Masson-Delmottte et al 2008). To what extent are the Antarctic *observations better representative of pptn-weighted d18O than annual averaged data? (e.g. from MD 2008: "Surface snow-sampling procedures differ significantly from one site to another. In some cases, shallow snow cores or pits, typically 1 m deep, were sampled and one or several measurements were performed."). Could this explain some of the model-data mismatch? I understand that the failure was explained in Part I as probably arising from a numerical artifact, so perhaps the authors feel any such statements would be meaningless? How does the isotopic composition of Greenland and Antarctic snow (which integrates the pptn-weighted signal) compare with observations?*

- **On the figure: we found out thanks to the comment of the reviewer that the script generating the figure 3 was incorrect in the averaging. The amplitude of the d18O annual mean is now the same as in the figure 2 – as it should be. The range is thus now down to -38 per mil. We modified the text accordingly to the re-computed correlation coefficients.**
  `Action: old figure 3 is replaced in the revised by the figure attached to this response.`

- **On the y-axis label: "mean annual d18O" is indeed always precipitation weighted, not only in figure 3, but in all figures. Indeed the mean of the d18O is not the same as the precipitation weighted d18O mean, if the precipitation is not homogeneous over the course of the year or over several years. We thus did not change the labelling in figure 2 and 3 since it is coherent with what is done.**
  `Action: none needed.`

- **On the similarity with Antarctic measurements: the snow measured in**

Antarctica is fallen over one or several years. Thus, the result is mean annual accumulation-weighted d18O of snow. Since this is precisely what we compute in the model, there is thus no difference between what is measured and what is modeled. We do not see, as the reviewer seems to imply, that our methodology could be different in a way to observations and hence induce a bias in the comparison. As stated before, an annual mean of d18O that would not be precipitation (or accumulation) weighted would be meaningless, since it would implicitly infer a constant accumulation over the year or overall several years (at the sampling step).
`Action: none needed.`

P 1496 line 20: H218O repeated
This has been corrected in the revised version.

P1502 line 1: refer reader to fig 4
This has been done accordingly.

P1505: refer reader to appropriate figures.
This has been done as per suggestion.

P1505: line 27 latter, not later
Corrected as suggested.

Fig 5 caption: for clarity, restate that all data are normalised about their annual average.
This has been done as per suggestion.

Interactive comment on Geosci. Model Dev. Discuss., 6, 1495, 2013.

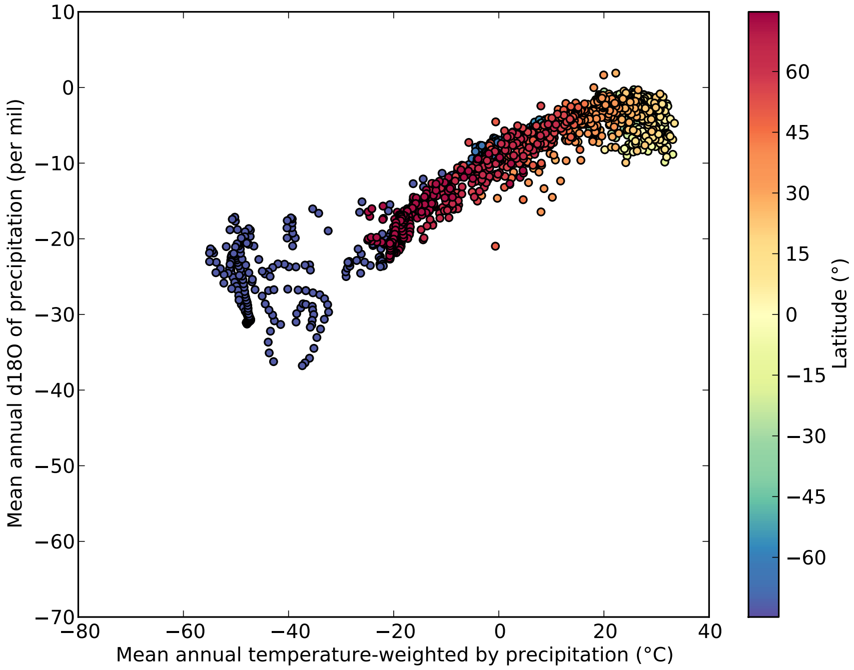

Fig. 1.