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Geoscientific Model Development Discussions
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Interactive comment on "δ^{18}O water isotope in the *i*LOVECLIM model (version 1.0) – Part 1: Implementation and verification" *by* D. M. Roche

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Below is my answer to the comments received by reviewer #1: 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 constructive comments lead to improvement of the manuscript and for his/her appreciation of our work.

This manuscript describes the implementation and basic verification of an 18O isotope scheme into the intermediate complexity Earth system model LOVECLIM. This is a

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very useful development, both for evaluation of the model's hydrological cycle and, given the model complexity, for use in long time-scale paleoclimate applications. Although the simplicity of the atmospheric model - in particular as the troposphere is modelled with a single layer - requires some significant simplifying assumptions, the resulting model is shown to behave well and reproduces most of the characteristics of the usual d18O-climate relationships. The major failing is in describing the isotopic composition of Antarctic precipitation.

Given the importance of the Antarctic d18O ice core signal in paleoclimate, this failure should perhaps be mentioned in the abstract?

We agree with the reviewer that this point should be mentioned in the abstract. It is done in the revised version of the manuscript with the following sentence added:

"The isotopic fields simulated are shown to reproduce most expected oxygen-18-climate relationships with the notable exception of the isotopic composition in Antarctica."

With one possible exception (1st para below) my comments are of a minor nature, relating primarily to the description of the formulation.

It is not obvious to me why convective precipitation is assumed to be at the toptroposphere temperature. Although the convective precipitation is derived as the (would-be) moisture flux through the tropopause, this seems to be a clever modelling construct rather than a reflection of the true source of the precipitation? This assumption appears potentially very important, both for the global average d18O of pptn and for

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its latitudinal dependence (as the latitudinal temperature gradient is presumably less pronounced in the upper troposphere?). The assumption should be discussed and, if necessary, sensitivity analysis performed to consider the results under the assumption of, say, mid-troposphere temperature. Similarly, why is the tropopause temperature assumed for snow and what effect does this assumption have on, in particular, d18O of high latitude pptn?

The assumption for large scale liquid precipitation to be in equilibrium with mid-troposphere temperature is indeed a strong one as pointed out by the reviewer. I believe that this assumption is reasonable for large spatial scales and for averages over several days (therefore excluding single rainout events whose isotopic composition may depart strongly from the mean). Two arguments are in support of that simple hypothesis. First is a physical reasoning based on the fact that liquid water falling in the atmosphere will partially re-equilibrate with its surrounding atmosphere. Thus, the δ^{18}O of precipitation that is measured at the earth's surface is not in equilibrium with the clouds in which it formed, but rather with water vapor at a lower altitude (cf. for example the discussion in Araguas-Araguas et al. (2000) comparing joint measurements of precipitation and water vapor). Since most clouds producing precipitation are within the troposhere and since we have only one single troposheric level, we only have the choice between above boundary layer, mid-troposphere and tropopause. Mid-troposphere thus comes as a natural choice. It is also correct, as pointed out by the reviewer, that the tropopause temperature variations are much smaller, and the absolute temperature also, yieling very homogeneous depleted values everywhere, at odd with IAEA measurements. Secondly, when plotting the altitude at which the δ^{18}O of precipitation is in equilibrium with the atmospheric column (in terms of temperature and δ^{18}O of vapor) in an isotopically enabled atmospheric General Circulation Model (HadCM3, Tindall pers. comm.), the value obtained is close to mid-troposphere values, thus justifying again our

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modelling choice for liquid precipitations.
The case of solid precipitation (snow) is different. Indeed, the isotopic equilibration time of a solid with its surrounding environnement is much longer than for the liquid. The simplest assumption is thus to assume that it is in equilibrium with higher altitude conditions, hence the choice for tropopause.
In both cases, the results are very reasonnable when compared to measurments, justifying our choices *a posteriori*.

To clarify that particular point, I have added the following discussion in the revised version of the manuscript:

```
The rationale behind this formulation is that the liquid
precipitation, while formed at high altitude, does
reequilibrate partially with the surrounding water vapor
during its fall (as shown in paired vapor / precipitation
measurements, (Araguas-Araguas et al., 2000)).  Hence, the
apparent fractionation equilibrium is not the altitude
of precipitation formation but somewhat lower in the
atmospheric column.  Since we do have only three layers, the
mid-troposphere is the most appropriate choice, as confirmed a
posteriori by our results.  In the case of solid precipitation
(snow), I consider it to be always in equilibrium with isotopic
moisture at the tropopause.  The case of solid precipitation
is different to that of the liquid precipitation since the
reequilibriation time is expected to be longer.
```

A few minor points, primarily on the description of the scheme.

i) Please define R18 in equation 3. Is the approximate value of n18/n intended here,

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or the form implied by rearranging eqs 1 & 2?

R^{18} in equation 3 is indeed the classical approximate form of $\frac{n^{18}}{n}$. This has been clarified in the manuscript after equation 3 as follow:

`[...] where` R^{18} `refers to the classical simplified form` $R^{18} = \frac{n^{18}}{n}$.

ii) Is the superscript 'i' intended in equations 4 and 5, or should this be 18? Please define Π.

The superscript 'i' was indeed a typo, corrected in the revised version as 18. Π **is intended as a generic replacement for the drag formulation classically used in models for evaporation equations. Formulated as in the manuscript,** Π **depends at least on wind. I do not provide the exact formulation for that coefficient, since the rest of the isotopic development does not depends on it, but only on the ratio of the isotopic version to the the non-isotopic version. The following was added to the revised version of the manuscript to clarify this point where the coefficient is first introduced:**

`[...] and` Π `the drag coefficient depending at least on wind speed.`

iii) Please define D in equation 9 and provide a source for this relationship. What value is assumed for n and on what basis was this value chosen?

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D is the molecular diffusivity of water. This form is generic and used for example in Mathieu and Bariac (1996). We chose the value of n so as to match the fractionation values measured in Merlivat (1978) as already stated in the manuscript. The text of the revised version of the manuscript now reads:

[...] D denoting the molecular diffusivity of water, D^{18} the one of the respective isotope and n a coefficient that varies with turbulence and evaporative surface (Brutsaert, 1975; Mathieu and Bariac, 1996).

iv) Please state the units of T and λ (eqs 14 to 17). What value is taken for the tunable parameter λ?

The unit of T (C) and the value of λ ($4\ 10^{-3}$) are specified in the revised version of the manuscript.

v) although it does no harm, I do not see much value in the two page description of the formulation of Rayleigh distillation. Does this differ from a textbook derivation?

I am unsure whether it is similar to textbook derivations, but it is a rather common development indeed, found already in the Craig & Gordon, though updated in the manuscript to our conventions. I always find helpful to have the complete developement where possible. I thus propose to move the derivation of the Rayleigh distillation in appendix, so as not to distract the fast reader. This is done in the revised version of the manuscript.

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vi) Figure 4 and section 3.3 is included as a verification of the atmospheric component, but no observations are presented for comparison. Although the results appear quite plausible, this verification seems of limited value without such a comparison.

I fully agree with that comment. In the revised version of the manuscript, I have added the amplitude data computed from the GNIP database. The text has been revised to take into account the new information added:

Comparing the modelled results with the amplitude derived from the GNIP database (IAEA, 2006), there is an overall good agreement between the two. Both observational data and simulation show an enhanced seasonal cycle in $\delta^{18}O$ of precipitation over the continents and a dampened seasonal cycle over the oceans. There is also a continentality gradient observed both in model and observations, with a tendency to higher seasonal amplitude for higher continentality. The model even obtain a good representation of the minimum values over the ocean (around 0.9 per mil over the tropical oceans) and a high amplitude for ice covered regions like Antarctica.

References

Araguas-Araguas, L., Froehlich, K., and Rozanski, K.: Deuterium and oxygen-18 isotope composition of precipitation and atmospheric moisture, Hydrological Processes, 14, 1341–1355, 2000.

Brutsaert, W. A.: Theory for local evaporation (or heat transfer) from rough and smooth surfaces at ground level, Water Resources Research, 11, 543–550, 1975.

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6, C1118–C1126, 2013

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IAEA: Isotope Hydrology Information System. The ISOHIS Database., http://www.iaea.org/water, 2006.
Mathieu, R. and Bariac, T.: A numerical model for the simulation of stable isotope profiles in drying soils, Journal of Geophysical Research, 101, 12 685–12 696, 1996.
Merlivat, L.: The dependence of bulk evaporation coefficients on Air-Water interfacial conditions as determined by the isotopic method, Journal of Geophysical Research, 83, 2977–2980, 1978.

Interactive comment on Geosci. Model Dev. Discuss., 6, 1467, 2013.

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Interactive Comment

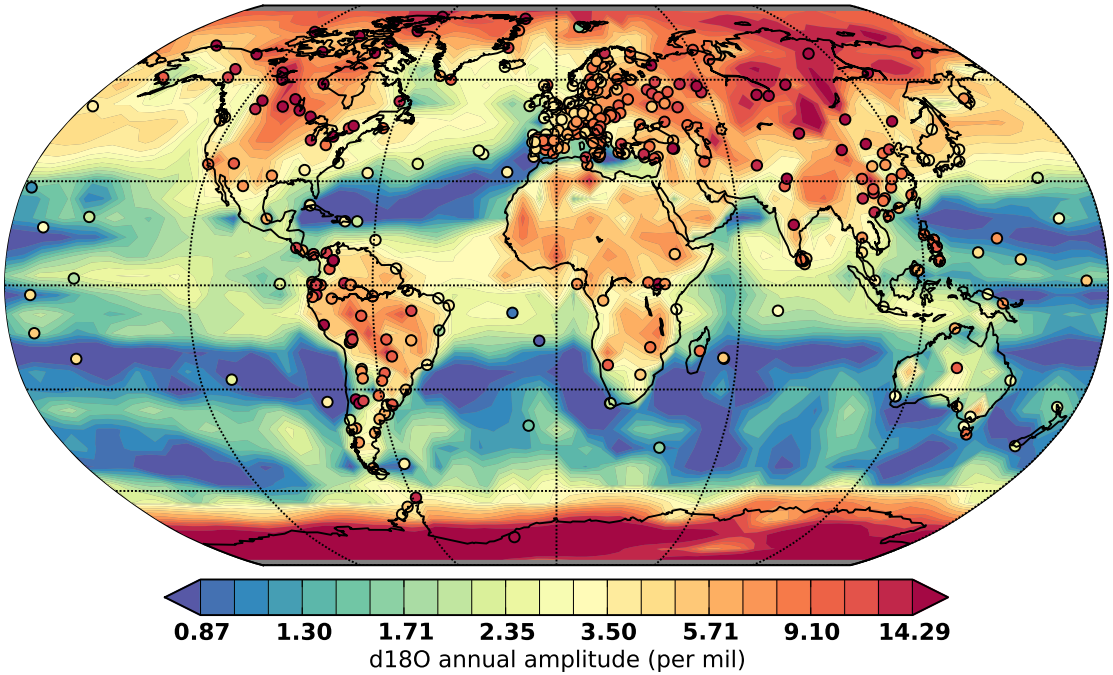

Fig. 1.

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