

Review of Extier et al

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This article presents the implementation of new isotopic tracers in the iLOVECLIM intermediate complexity model. Up to now only $\delta^{18}O$ was implemented in iLOVECLIM. Now δ^2H and $\delta^{17}O$ are also implemented. This allows to simulate some Deuterium-based paleoclimate proxies, such as the δD of leaf waxes, and the second order isotopic parameters d-excess and ^{17}O -excess. This article is very important because it opens the door to the use of iLOVECLIM to interpret past variations of a larger range of isotopic proxies.

The article is generally well written. I have however several major comments that I think would need to be addressed to give more confidence that iLOVECLIM is working properly and is a safe model to use for some paleoclimate applications (to be precised).

1 Major comments

1.1 More quantitative and honest assessment about the skills of iLOVECLIM

First of all, using intermediate complexity models to interpret paleoclimate proxies has advantages relative to using GCMs. I think these advantages could be better emphasized, e.g. in introduction.

These advantages come at the expense of a less realistic representation of the climate and isotopic composition. It is expected and there is no shame about it. As a reader, my main motivation for reading this article was to address the following question: For what kind of paleoclimate applications is using iLOVECLIM relevant and safe? For what aspects of isotopic variations is iLOVECLIM realistic enough? I think this article could do a better job at addressing these questions.

The article already presents comparisons between the skills of iLOVECLIM and GCMs. So all the material is already there to address these questions. It just needs to be integrated into a more quantitative comparison.

For example:

- Calculate some skill metrics for iLOVECLIM and other GCMs, e.g. model-observation correlations, root-mean-square errors, for δD , d-excess, ^{17}O -excess, possibly in different regions (e.g. entire globe or tropics). The metrics could be summarized in a table or in Taylor plot diagrams for example.
- In addition to maps, it would be very helpful to assess to what extent iLOVECLIM can simulate the main isotopic effects relative to observations: amount effect (scatter plot of δD over tropical islands), temperature effect (scatter plot of δD as a function of temperature), continental effect (e.g. box and whisker plots of δD within 20°S-20°N over land and over ocean). The simulation of these effects could be compared between iLOVECLIM and other GCMs.

I think that evaluating the isotopic effects is essential for a model that is supposed to be used for paleoclimate applications in the future, because isotope effects are an essential ingredient of paleoclimate variations in isotopes.

Based on this more quantitative comparison, clear and honest statements could be formulated about in which region, for which variables and for which isotope effects iLOVECLIM is realistic and could be used for paleoclimate applications.

1.2 Suspected problem in the treatment of land evapo-transpiration

- l 155-163 needs to be clarified. l 161 writes that “In the same way, evapo-transpiration occurs from the soil bucket water with fractionation”: so what was equation 10 about? Evapo-transpiration represents both evaporation from soils and standing water and transpiration from plants. I cannot think of any water flux between the land and the atmosphere that is not evapo-transpiration.

- Why assuming that there is fractionation during evapo-transpiration? Evapo-transpiration is dominated by transpiration (e.g. [Jasechko et al., 2013]) which does not fractionate. Transpiration does not fractionate because no fractionation is associated with root uptake [Washburn and Smith, 1934], the water is transported by the xylem to the leaves without any fractionation, and then the water reservoir in leaves is smaller than the evaporation flux during a day. In all GCMs that are coupled to simple bucket models, evapo-transpiration is assumed not to fractionate (e.g. [Hoffmann et al., 1998, Risi et al., 2010]).
- When coupled to more sophisticated land surface models, transpiration is still assumed not to fractionate (e.g. [Haese et al., 2013, Risi et al., 2016]). The bare soil evaporation is assumed to fractionate, but never at equilibrium like equation 10. Rather, the [Craig and Gordon, 1965] equation is assumed, with specific kinetic fractionations for the soil (e.g. [Mathieu and Bariac, 1996, Barnes and Allison, 1988, Haese et al., 2013, Risi et al., 2016]).
- The fraction of bare soil evaporation (fractionating) and transpiration (non-fractionating) impacts the isotopic composition of the precipitation over land regions [Haese et al., 2013, Risi et al., 2016]. The non-fractionating transpiration is known to be essential for determining the isotopic gradients over the Amazon, Congo basin and Eastern Africa [Salati et al., 1979, Levin et al., 2009, Worden et al., 2021, Shi et al., 2022] and might also play a role in isotopic changes during past climates [Pierrehumbert, 1999]. I suspect that the big depletion bias simulate over tropical land (Fig 1a) could be partially due to the assumed fractionation during evapo-transpiration. I would recommend to re-run a new simulation without any fractionation during evapo-transpiration. This might help to improve the simulation.

1.3 Suspected problem in the simulation of ^{17}O -excess

When looking at Fig 1d or Fig 6b, I'm very worried about the ^{17}O -excess simulation. Those extreme values from -150 to 150 permeg look very strange. The spatial pattern also looks strange. What would cause such a strange pattern? The LMDZ simulation of ^{17}O -excess, for example, was much smoother and didn't show this spatial pattern at all.

Why is ^{17}O -excess so noisy in Antarctica and Southern Ocean? Is it a problem with large inter-annual variations and a too short simulation period? Or simply a bug? The traverse data from [Pang et al., 2015] shows much smoother variations. Given the scarcity of ^{17}O -excess observations, the data from [Pang et al., 2015] deserves to be used and cited in this paper.

The dataset from [Uemura et al., 2010] deserves to be used and cited as well. I understand that is was in the vapor and iLOVECLIM does not allow for a model-data comparison of the vapor. Yet this dataset provides useful information: it shows that over the ocean, the ^{17}O -excess varies very smoothly and is mainly controlled by the surface relative humidity. This observation makes the simulation by iLOVECLIM all the more suspect.

I understand that ^{17}O -excess is very difficult to simulate in models. I don't think that a proper simulation of ^{17}O -excess is a necessary condition to publish this paper. However, I do think that honest statements about the failure of iLOVECLIM to simulate ^{17}O -excess, and speculation on the causes for this failure, would be very valuable.

For example, l 320-321: "could get closer": could also get further away... I don't think it is very honest to pretend that the model-data disagreement is due to uncertainties. Given the completely different ranges of ^{17}O -excess values for observations and iLOVECLIM showed in Fig 6b, and given the smooth variations that have been reported in all ^{17}O -excess observations so far, I think the authors can state with a high degree of confidence that iLOVECLIM fails to properly simulate ^{17}O -excess.

2 Minor comments

- l 11: remove "and numerical models": we don't need isotopes to infer hydrological changes in numerical models, these can be directly diagnosed by outputting all necessary variables.
- l 24: compare -> compared
- l 24: And? The reader here expects a sentence assessing the skill of iLOVECLIM for ^{17}O -excess. This is a key aspect of the paper and it needs to be in the abstract.
- l 53: "new method ... 2006... 2008." I don't think methods published more than 15 years ago can still be called "new".

Generally, I found that many references in this paper were quite old, maybe some bibliographic update could be useful.

- l 90: “500 and 200hPa ... dry layers correspond to the stratosphere”: Does it make any sense that the stratosphere is so low in altitude? Don’t these levels simply represent the free troposphere?
- l 110-115: Equation 4 is simply the [Craig and Gordon, 1965] equation. This is the equation used in all isotope-enabled model, including all GCMs. I don’t know why the authors introduce it in such a complicated way, and why Cappa et al 2003 and Roche 2013 need to be cited for this. Rather, [Craig and Gordon, 1965] should be cited.
“The evaporation term...”: Really? If you write the bulk evaporation equation for humidity, and the same for isotopes, you take the ratio, and you find the [Craig and Gordon, 1965]. So I don’t understand the problem.
- R_a , h_a : what do they represent? Does it represent the isotopic composition of the 800hPa layer? Physically, does it represent the “free atmosphere”, or the boundary layer? If this really represents the “free atmosphere”, does this lead to a systematic bias, with too depleted R_a ? Is there a correction to account for this?
“free atmosphere”: should rather be “free troposphere”?
- l 128: “tropopause, mid-troposphere”: what do these levels correspond to? Do these correspond to 200hPa and 500hPa respectively? It would be clearer to refer to the levels in hPa rather.
- l 150: is there any representation of evaporation of rain as it falls? Rain evaporation is known to be essential for simulating the amount effect, e.g. [Field et al., 2010, Risi et al., 2021]. If there is no rain evaporation, does it mean that the iLOVECLIM cannot represent the amount effect? Does it mean that any use of iLOVECLIM to interpret paleoclimate proxies in tropical regions is problematic?
It would be very helpful to show to what extent iLOVECLIM is able to simulate the amount effect, see major comment 1. From Fig 1 it looks like it is not, but it’s hard to see on a map.
- l 174: what is the time step of the model?
- l 205: “Risi et al 2012” can be used as a reference for SWING2, but for LMDZ4, replace by [Risi et al., 2010].
- l 205: are all these simulations part of the SWING2 database? If so, write it.
- l 210: “better reproduce isotopic change above 80°N than in the other models”: I cannot see this in Fig 2. There isn’t any observation above 80°N in this Fig.
- Fig 1: I don’t think the map for $\delta^{17}O$ is useful, since it shows exactly the same as for δ^2H . The added value of $\delta^{17}O$ relative to other isotopes is already well summarized by ^{17}O -excess in Fig 1d.
In contrast, I think that it would be worth to show the model-observation comparison for temperature and precipitation, because these variables can help interpret some of the model biases for isotopes.
- Fig 2: were the model outputs co-located with the measurements? For a more rigorous comparison, it might be useful to do so.
- Fig 2: it would be useful to have the same for d-excess and ^{17}O -excess. More generally, it looks like there is a new figure style for each isotopic variable. It would help the reader to have more coherent figures between the different variables. e.g. zonal mean for δ^2H , d-excess and ^{17}O -excess, same style of model-obs scatter plot for δ^2H , d-excess and ^{17}O -excess, etc...
- If there are too many figures, I think Fig 3 is not so useful. The MWL is not a stringent test on the simulations.
- l 285-290, 301-310: maybe these paragraphs could be summarized by just noticing that the spatial pattern of $\delta^{17}O$ looks almost exactly the same as $\delta^{18}O$? The ^{17}O -excess parameter is what bears the added value.
- l 311: “proxy” -> “variable”. For present day, $\delta^{17}O$ is directly measured.
- Fig 9: same for d-excess?

- l 424: “relatively similar close to zero values” -> values close to 0‰.
Same problem l200
- l 443: remove “a better agreement... at least”, because only the second part of the sentence is correct.
- Fig A1: I think this figure should replace Fig 4 in the text, and the appendix text can be merged in the main text. Everything that could be seen in Fig 4 can be seen in A1.
- Please check the reference list. Some articles cited in the text are missing, e.g. Werner et al 2011.

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