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Geoscientific
Model Development
Discussions

Interactive comment on "Stable water isotopes in the coupled atmosphere–land surface model ECHAM5-JSBACH" *by* B. Haese et al.

Anonymous Referee #1

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Review of Werner et al

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1 General comments

This paper describes the implementation of water isotopes in the JSBACH land surface model and evaluates the simulation of the isotopic composition of the precipitation when coupled to the ECHAM5 model. The isotopic version of ECHAM5 was already documented in Werner et al. (2011).

This paper is a significant contribution because papers documenting water isotopes in land surface models are not so many.

I felt however a bit frustrated because I couldn't understand everything, and in absence of further explanations, not everything makes sense. So I think that the paper could be improved in several ways as described in the specific comments.

2 Specific comments

2.1 Better describe the JSBACH model

To better understand the results and to better justify your hypotheses regarding isotopic implementation, it would be useful for the reader to know more about the JSBACH model.

- What kind of model is it for the soil? How many layers?
- What kind of model is it for the snow?
- In section 3.2.1 it is suggested that bare soil evaporation and transpiration are the only sources of total evapo-transpiration. What about canopy-interception? Snow sublimation? How are they counted in fig 7?
- Do you have any lakes or floodplains that provide additional sources of evapo-transpiration? If not is it a problem for the isotopic simulation in some regions?
- Can you summarize briefly the main differences between JSBACH and the default soil model of ECHAM5? We learn progressively some differences along the results section, but it would be good to have a synthetic summary in the model description. For example, this could be a synthetic table.
- We discover on section 3.1 that the maximal soil depth varies spatially and is different between stand-alone ECHAM5 and JSBACH. Can you explain very briefly how the maximal soil depth is calculated in each model? This could go in the synthetic table suggested above.
- How are surface runoff and drainage treated? What are their relative contribution to total runoff? This point is crucial for isotopic fractionation, see below.

The model description section could be cut into a basic description and then an isotopic description.

2.2 Better describe the isotopic implementation and justify the underlying hypotheses

- Why don't you take into account fractionation during evaporation of plant-intercepted water? Does the model simulate any throughfall? If all the water evaporates from the plants within a few time steps but at the same time some of this enriched water drips to the ground, then fractionation needs to be taken into account. For example, this source of fractionated water was suggested as a possible contribution to the d-excess increase in the Amazon (Gat and Matsui (1991)).
- Can you give the formulation of the kinetic fractionation coefficients that you use in the different simulations? Which one leads to the strongest fractionation in terms of $\delta^{18}O$? Which one leads to the strongest fractionation in terms of d-excess? Can you remind the hypotheses and limitations underlying these 2 formulations?
- If the soil model is multi-layer, what kind of scheme do you use for water transport and diffusion into the soil?
- p 3390 l 3-9: Sachse et al. (2012) is cited suggesting the existence of some fractionation during transpiration. I think this paper has been mis-interpreted. Fractionation from leaf water to evapo-transpiration is well known, but it doesn't imply that there is fractionation from soil water to evapo-transpiration. What's important for your implementation is the fractionation from soil water to transpiration. At time scales larger than daily, a steady state is reached in which all the soil water taken up by the roots is transpired. So what's important is the fractionation during soil water uptake by the roots. And numerous studies have shown

there is no fractionation during root uptake, or at most very small fractionation for specific species (Washburn and Smith (1934); Wershaw et al. (1966); Zimmermann et al. (1967); Ziegler et al. (1976); White et al. (1985); Barnes and Allison (1988); Dawson and Ehleringer (1991); Walker and Richardson (1991); Dawson and Ehleringer (1993); PJ et al. (1993); Ellsworth and Williams (2007)). Even Sachse et al. (2012) argues there is no fractionation during root uptake. Some papers document the effect of fractionation at leaf level on the transpiration flux composition, but this plays at sub-daily time scales (Lai et al. (2006); Lee et al. (2007)). This is not what you are looking at. The paragraph suggests that previous studies neglecting fractionation during transpiration were wrong, but actually all citations I know suggest they were right.

- The fractionation during transpiration is difficult to justify physically. This experiment is rather an idealized experiment exploring an extreme case. This should be stated explicitly. Later on, the paper compares the model-data agreement of the noF, FE and FET simulations. It is very interesting to compare these 3 simulations, but please clarify that nothing can be concluded about the realism of these simulations. If the FE simulation is not the best, it probably reflects error compensations.
- How do you treat surface runoff and drainage? If the rain runoffs at the surface, then this runoff has the composition of the rain. In this case, due to water balance, the composition of total evapo-transpiration has the composition of the total water inputs into the soil. In contrast, if the soil drains, this drainage has the composition of the soil water which is enriched. In this case, due to the water balance constrain, the total evapo-transpiration is more depleted than the total water input into the soil.
- What do you do for snow melt?
- How does you implementation of water isotopes compare with other models (e.g.

(e.g. Melayah et al. (1996); Riley et al. (2002); Braud et al. (2005); Yoshimura et al. (2006))? What is the degree of complexity compared to these models? For example, do you neglect exchanges with the vapor phase in the soil? The processes that you neglect should be stated more clearly.

2.3 Better explain the simulated results

This is my major comment. Almost everywhere, it is written that when there is more fractionation at evapo-transpiration, $\delta^{18}O_p$ becomes more enriched: examples: p 3390 l 17, p 3391 l 6, p 3396, l 13. I would expect the contrary. The explanation given in p 3390 l26-29 is not convincing.

If there is more fractionation at evapo-transpiration, then the evapo-transpiration should become more depleted. As a consequence, the resulting vapor and precipitation should be more depleted. At the continental scale for example, to first order the water vapor advected from the ocean equals the precipitation over the continent plus the total runoff going back to the ocean. If there is some fractionation during evapo-transpiration, then the soil becomes more enriched and so the runoff going back to the ocean becomes more enriched. If the composition of the water vapor advected from the ocean doesn't change, then it means that the continental precipitation needs to become more depleted. This is just mass conservation. If you get the contrary, it suggests a mass conservation problem.

The fact that the soil water gets more enriched when there is fractionation cannot explain that the subsequent evapo-transpiration becomes more enriched. The subsequent evapo-transpiration will always be more depleted. Even in the extreme case of no drainage, whatever how strong the fractionation is, the total evapo-transpiration composition will equal the total precipitation composition.

If the authors are sure there is no conservation problem in their model, then they need to explain better their results. Some mass balance considerations are necessary. It would be good to plot somewhere the composition of the runoff, or at least to give the global average and compare it to evapo-transpiration and precipitation.

2.4 Miscellaneous

- l 3377 l23: among other land surface models with isotopes, you can cite Riley et al. (2002); Cuntz et al. (2003); Braud et al. (2005). Among land surface-atmosphere models with isotopes, you can cite Risi et al. (2010)
- p 3383 l5: what are your motivations for comparing the 2 resolutions? I understand comparing resolutions for an atmospheric model is important and this was done nicely in Werner et al. (2011). But why doing it again here? What are the added value of re-doing it when coupling with a land surface model? For example, maybe with higher resolution you can better resolve spatial patterns in vegetation distribution?
- p 3387 l 25: “decrease in the amount of precipitation or an increase in soil moisture”: I understand that according to the amount effect, a decrease in precipitation leads to an increase in $\delta^{18}O_p$. But how does soil moisture affect $\delta^{18}O_p$? The rationale needs to be better explained in terms of processes. Same p 3388 l 5.
- p 3396 l 1-2: why? with a one layer model with vertically uniform isotopic composition, the surface soil water will be too depleted and the deeper soil water will be too enriched. So the transpiration will be too enriched but the bare soil evaporation will be too depleted.
- Can you give more information about the GNIP stations: how many years are recorded on each stations? What is their longitude and latitude, or is it possible to show their location on a map?

- section 3.2.2: is it possible to give an idea of the basic performance of the model regarding soil water, evapo-transpiration and $\delta^{18}O_{ws}$? If not, can you highlight the difficulty of evaluating these variables? In perspective, can you mention some national or international efforts to establish a network of $\delta^{18}O_{ws}$ measurements? e.g. MIBA (http://www-naweb.iaea.org/napc/ih/IHS_resources_miba.html), BASIN (http://basin.yolasite.com/).
- You say the noF simulation agrees best with observations. However, from a physical point of view, we know that the most realistic should be the FE simulation. This suggests that they are error compensations. Probably the noF simulation agrees best for the wrong reason. Can you elaborate on that a bit more? This comment is related to my previous comment: to identify the compensation of errors, it would be great to evaluate $\delta^{18}O_{ws}$.

3 Technical corrections

- p 3377 l9: “straigtforward”
- p 3388 l 18: “Additionally ... analyzed” is a repetition from line 7-8.
- p 3391 l6: “stronger depleted” -> “more depleted”. Same in many other places: e.g.: p 3391 l 15, p 3395 l 2. “relative stronger depleted” -> “relatively more depleted”.
- p 3391 l 10: “similar pattern like” -> “similar pattern to”. Same l20.
- p 3392 l 18: “the similarly” -> “similarly”
- p 3393 l 7: “amplitude of the temperature” -> “amplitude of the temperature variations”?

- p 3393 l 26: “So is the calculated amplitude ... for Vienna approx. 15 cm, for Ottawa approx. 7 cm ...” -> “The calculated amplitudes ... are approximately 15 cm for Vienna, 7 cm for Ottawa ... respectively.”. In English the verb follows the subject.
- p 3394 l 11-12: words are not in the correct order, maybe try: “The difference between the calculated $\delta^{18}O_{ws}$ in noF and FE (respectively FET) is -4 (respectively +2)”.
- p 3395 l 22: “implementation” -> “implemented”
- p 3396 l 4: “evaporation” -> precise bare soil evaporation or evapo-transpiration.
- p 3397 l 21: “comparable” -> “comparably”
- p 3401 l 14: The journal is wrong, it is Ann rev of Earth and Planet Sci.

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