Geosci. Model Dev. Discuss., 5, C1035–C1037, 2012
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Interactive comment on "Stable water isotopes in the coupled atmosphere–land surface model ECHAM5-JSBACH" *by* B. Haese et al.

Anonymous Referee #2

Received and published: 14 December 2012

Review result for Haese et al titled "stable water isotopes in the coupled atmosphere-land surface model ECHAM5-JSBACH" submitted to GMDD.

This paper presents a set of sensitivity tests for an isotope AGCM with and without a detailed land surface isotopic parameterization. The authors incorporated prognostic isotopic reservoir (soil moisture) and corresponding fractionation process associated with evaporation from soil and plant into JSBACH, and quantify the impact of adding these processes. The work is not new in the community (there are a few similar studies with different climate models), but in this particular model, it is the first report. However, I strongly suspect that there are significant errors which would largely influence their final conclusion. Without fix the problems, I do not think the paper is worth to be judged.

The first error is in Equation (3). The author multiplies Rˆx_res to the whole right hand

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side, but this does not make sense at all (e.g., vapor isotope, qˆx_vap is be multiplied by soil moisture isotopic ratio Rˆx_res). In the correct form, Rˆx_res should be multiplied with the second term in the parenthesis (alphaˆx(T)*h*qsat). All relevant studies use this way. If this way is wrong, the authors need to justify their way more clearly. Because of this error, the isotopic ratio of evaporation is over estimated. Therefore the precipitation isotope ratio is enriched in many places.

Secondly, in Equation (3) again, the dew should not be physically represented with this form. There is no physical link between dew formation and the soil moisture. Dew is simply the over saturation of water vapor, so that equilibrium fractionation should be used. This error also has negligible impact to the conclusion.

Third, in Equation (4). The authors misunderstand the meaning of fractionation with transpiration. Yoshimura et al. (2006) implemented fractionation in transpiration, but against the leaf water, not to the soil moisture. Because the leaf water is very small pool, isotopic enrichment in the leaf water would occur instead of isotopic depletion of transpirated vapor. Moreover, Rˆx_ws must not be placed in this position anyway (same as the first error).

With these three critical errors, I believe that their results became totally meaningless. For example, the soil moisture isotopic ratio became depleted at high latitude by having evaporative fractionation (Fig 9) is totally unreasonable.

This time I hesitate to comment about all the results since they will be dramatically changed with the right formulations. Here I don't mean that the previous formulations are "right", but at least if new formulation is proposed, more supportive evidence should be shown.

Final point, I really like the sensitivity test with different kinetic fractionation parameterizations. However, the author did not write detailed specification of the tests. Please describe both kinetic fractionations with equations. Then the results would be relevant to the readership.

I feel sorry to give such negative comments, but please be encouraged to reformulate the code and rerun the model. This would give much nicer results, I believe.

Interactive comment on Geosci. Model Dev. Discuss., 5, 3375, 2012.

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