

***Interactive comment on* “Simulating hydrology with an isotopic land surface model in western Siberia: what do we learn from water isotopes?” by F. Guglielmo et al.**

Anonymous Referee #1

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The manuscript “Simulating hydrology with an isotopic land surface model in western Siberia” by Guglielmo et al. addresses a critical limitation in many land surface and hydrological model: the frequently inadequate partitioning of evaporative water fluxes due to evaporation and transpiration. Given the importance of a suitable representation of these processes not only as input into GCMs, the objective of this manuscript is to test in how far isotope data can help in meaningfully separating and constraining these processes, which comes timely and may in principle of interest to many in the community. While the manuscript is well structured and clearly written, I do have some serious reservations about the relevance and validity of the results as I am far from

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convinced that the methods applied reflect the state-of-the-art in our understanding of how soils store and release water. Much of the model itself as well as the interpretation of the results are, at least it seems to me, based on the assumption that drainage and evaporative (i.e. evaporation and transpiration) water originate from the same pool of water. This is in stark contradiction to the growing experimental and modelling evidence on a range of spatial scales that this is not the case. In particular, a significant body of recent literature supports the hypothesis that water that is at least transiently stored in the soil matrix (and that thus is available for transpiration) is becoming older relative to the water being released by drainage the wetter the soil and the lower the soil moisture deficit. This is at this point thought to be the case as during dry conditions, high negative pore pressure “sucks” water into the smaller pores where it is rather tightly bound. As the system wets up, the suction reduces, less water can be stored in the small pores and thus remains in larger pore spaces where flow velocities are higher, contact times and contact surfaces with resident water are lower. Water entering the system under wet conditions will therefore experience less exchange (“mixing” or “dispersion”) with resident water as it effectively starts to bypass the matrix through preferential flow paths (see recent experimental and modelling work by e.g. Brooks et al., 2010, Nature Geoscience; Botter et al., 2011; Hrachowitz et al., 2013, HESS; Evaristo et al., 2015, Nature; Benettin et al., 2015; WRR; Harman, 2015, WRR). The overly simplistic conceptualization of these transport processes in the model used in this study therefore makes me wonder, in how far the model processes do actually represent real-world processes. Following from that, and as the remaining analysis is largely based on a parameter sensitivity analysis of the model, I am not sure that the reported results/interpretation do have any significant physical meaning or if they merely illustrate the sensitivity of a “wrong” model.

The authors themselves point out that the modelled isotopic profiles do not exhibit good match with the observed profiles. This is in particular true for Deuterium but also for the O-18 peaks at depth, which are attributed to seasonal effects. The authors list a variety of (speculative) reasons for this mismatch, but go on as treating their model as a “valid”

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(sorry cannot think of a better word now) representation of reality. In general there is always a risk involved in virtual experiment approaches, where models are used to learn about processes, as is done here. Therefore, it is a widely acknowledged requirement for such approaches that the models used need to *at least* achieve a good fit with the observed data, so that they can be considered at least partially reasonable representations of reality. Only from such a starting point (as imperfect it is) further virtual experiments (e.g. analysis of which parameters and thus which processes control the response) can result in meaningful interpretations. From that perspective I am not convinced that the presented model fulfils these requirements and can be used to infer meaningful insights on real system behaviour.

Apart from that, minor and major points include:

(1) In general I found that much of the more recent literature in the general topic was ignored and it may be beneficial for the paper to consider the latest developments in a bit more detail (e.g. Brooks et al., 2010; Nature Geoscience; Sutanto et al., 2012, HESS; Hrachowitz et al., 2013; HESS; Jasechko et al., 2013, Nature; Klaus et al., 2013; HESS; Coenders-Gerrits et al., 2014, Nature; Sutanto et al., 2014; HESS; Wang-Erlandsson et al., 2014, Earth Syst. Dyn.; Evaristo et al., 2015, Nature; Harman, 2015, WRR; Rinaldo et al., 2015; WRR)

(2) P.9396, l.1: replace "confirm" by "test"

(3) P.9397, Eq.3: should the equation not read $d\text{-excess} = \delta D - (8 \cdot \delta^{18}O + 10)$??

(4) P.9397, l.18-22: maybe link explicitly to fractionation here

(5) P.9398, l.10: precipitation is ALWAYS a flux (even if annual volumes are reported), i.e. needs to have the dimension L/T (or here specifically mm/yr)

(6) P.9398, l.11: what is a short growing season? Please be more specific and give the months

(7) P.9398, l.12/Fig.1: please indicate the exact positions of the stations with a symbol.

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(8) P.9399, l.5-14: I have never worked with vacuum distillation. I was wondering how much of the water stored in a soil sample can be extracted with that method? 100%? If it is less, one has to be careful about the interpretation of the isotopic signature: while more mobile water will be extracted, water bound more tightly (with a potentially different isotopic signature) will be extracted to a lesser degree. It will then be difficult to interpret which pool of water the sample represents.

(9) P.9400, l.9ff: more detail about the model is necessary in order to understand what was done and how. For the convenience of the reader I would also suggest to provide the relevant set of equations in the supplementary material, even if it was already published elsewhere before.

(10) P.9400, l.15: the parameters used in the model are provided without any further justification and seem rather arbitrary. In particular, where do the 300mm water holding capacity come from? What is the reasoning for using the same value for all 4 sites? As I understand (and as it is used in most hydrological models), the water holding capacity is the storage capacity in the root zone between field capacity and wilting point. Below the root zone, the water content of the unsaturated zone will be on average at around field capacity (no water can be extracted by roots!). In the root zone, the storage capacity will be controlled by vegetation (i.e. the water volume accessible to roots; Gao et al., 2014; GRL) and soils and will thus vary from site to site. Similarly, a static runoff partitioning (95% drainage, 5% fast runoff) cannot be seen as a plausible representation of reality, as it goes against any type of experimental evidence and the concept of hydrological connectivity. In a nutshell, this is an experimentally well established positive feedback process: the wetter a system, the higher its connectivity, the higher the proportion of fast/preferential/surface runoff. It remains also unclear how isotopic fractionation due to soil evaporation was implemented. More detail is needed here to assess the model.

(11) P.9401, l.3ff: this section remains rather vague. How was the isotopic composition in each layer estimated? By vertically and routing each individual input signal and

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summing all partial signals in each layer up at each time step?

(12) P.9402, l.2: what is τ ?

(13) P.9404, l.14ff: for which time period was the model run? Which parameters other than the ones discussed had to be selected (“tuning”) and how was this done? How meaningful is it to run a model on a 6-hourly basis(!), if the available isotopic input data have a temporal resolution of one month(!)???

(14) P.9405, l.24-26: speculative, can be removed if no further evidence is presented.

(15) P.9406, l.6: this is vertical *dispersion*, really, because much of the movement is advective

(16) P.9406, l.14-24: possible, but again rather speculative in the absence of data supporting this.

(17) P.9408, l.28: no, the entire conceptualization of how water is stored and released is over simplified (See above).

(18) P.9409, l.1.13: difficult to assess as no details are given of how e.g. fractionation is handled in the model.

(19) P.9409, l.14: with a hindsight it may have been more instructive to take isotope profiles at different points throughout the year and test if the model can reproduce the temporal changes in isotopic signatures integrated over the entire respective soil profiles.

(20) P.9414: quite frankly, I have not quite understood the reasoning behind this part of the analysis and I can therefore not comment on its feasibility.

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