Hydrol. Earth Syst. Sci. Discuss., 12, C3086–C3089, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C3086/2015/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Quantifying energy and water fluxes in dry dune ecosystems of the Netherlands" *by* B. R. Voortman et al.

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Received and published: 14 August 2015

Reviewer 1

This paper follows up with a 2014 paper which presented unsaturated hydraulic properties of mosses. The present paper provided datasets on lysimeter-measured ET and ET-related fluxes, compared the measurements with Penman-Monteith equation whose parameters were derived from the measurement datasets. Through the comparison they showed the change of surface resistance before and after drought and they showed behaviors of different moss species. I found this paper to be a nice contribution to HESS. The ecosystem and the hydrologic processes studied (ET and groundwater recharge in inland dunes) are obviously of societal importance. The data was carefully

collected and the calculations were meticulously carried out. Moss ecohydrology is a knowledge gap and I believe the datasets presented in this paper will be a great value for improving our understanding and representation of moss in earth system models. I believe the paper can be published with moderate revision.

Some main suggestions: 1. The finding of higher surface resistance after drought deserves some more discussion. Most land surface or hydrologic models have a root resistance term linked to soil moisture (e.g, see CLM and its use by hydrologic models [Oleson et al., 2013; Shen et al., 2013] and the body vegetation water stress literature [Lai and Katul, 2000; Braud et al., 2005]). Since the rs in this paper was backcalculated, it might have lumped the soil moisture limitation into rs. If this were the case, it is still valuable data but hardly novel. On the other hand, if the effect of soil moisture cannot explain all the increase in rs, this is more interestingâËŸAËĞ Tit might be related to some ecophysiological traits of the mosses after drought, perhaps due to cavitation. If soil moisture measurements had been collected, this is a good opportunity to add a moisture limitation term into the ET equation to resolve the different effects. If not, the authors need to be more careful about their conclusions and the claim of novelty. Reply 1. The back-calculated rs before and after the drought event were based on moist days for which we assume that moisture limitation doesn't affect the surface resistance (lines 346 to 352). Thereby we assume that the back calculated rs doesn't include the moisture limitation and is only the effect of plant desiccation (a reduction of the transpiring leaf area). We noted in the abstract that we attribute the change in rs to the desiccation of leaves, however this was not clear from the manuscript. We added a sentence in the discussion section to emphasize that the change in surface resistance is caused by a smaller transpiring leaf area (line 620). It should be noted that this was only done for the vascular plants (grass and heather). Mosses of these habitats are desiccation tolerant and do not show such a pronounced response of the green leaf area to drought. We added a sentence in the method section to emphasize that we only back-calculated the surface resistance for the vascular plants (lines 370 to 372).

2. I found the discussion of moss ecophysiology to be interesting (behave just like soil). How can we model moss in a global or basin-scale setting? How should it be fundamentally differed from grass or trees? Some discussion will be welcome. Reply 2. Mosses can be parameterized as a soil layer in hydrological models that numerically solve Richards equation (e.g. Hydrus or SWAP), using van Genuchten parameters to describe the capacity of the moss layer to conduct water. For global or basin scale models such detailed mathematical simulations are usually not feasible (limited by computing power).To our opinion, any discussion on the global or basin scale simulations of moss evaporation requires extra simulations and validation of the proposed approach, diverting from the subject of the manuscript. Therefore we didn't add a discussion section on this subject to the manuscript. We are cautious to discuss this subject because the procedure to simulate moss evaporation on a global or basin scale is not strictly limited to mimicking moss as a soil layer. The same hydraulic behavior, although physically unsound, could be mimicked by treating moss as a vascular plant, e.g. by using a Feddes function to imitate that the surface becomes too dry to deliver the potential rate. So, technically, there are multiple options that need an extensive review before suggesting the proper/best approach.

3. The authors need to acknowledge that the method they used to estimate ET is very data-intensive, and unlikely to be available on large scales. Therefore, the limitation of the net longwave radiation model should be discussed. Reply 3.The meteorological data requirements to simulate ET are not different form conventional methods to simulate evaporation with the Penman Monteith equation. However, the amount of parameters are indeed large, especially for the net longwave radiation model. We added a section to emphasize that the Rnl model is parameter intensive (lines 515 to 520).

4. Just a suggestion, if the authors could include site-gathered pictures of different species discussed in the paper, it will be much more intuitive. It will also be nice to have some general background information of the climate conditions of the study region (annual precip, ETp and their distributions). Reply 4.We added site-gathered pictures

of the studied vegetation types and added information on the annual precipitation and reference ETp (lines 119 to 121, and Fig. 2).

Some detailed comments 1. The authors kept referring to moss carpets "tempering" ET, but this word is very vague and unclear. I had to go to the cited paper to understand what they meant – "reducing the magnitude of". Please considering switching this word to sth else. Reply 5. We changed the formulation to "reducing" ET.

2. In the abstract, "due to the desiccation of leaves", how do we know it is the desiccation of leaves? Why not stems/rhizoids? It could be because I am not familiar with moss, but this part deserves more clarification. Reply 6. The change in surface resistance was only quantified for the heather and grass surface (see reply 1).

3. Abstract page Line 22âËŸAËĞ T"feedback on the parameters of Penman Monteith equation"!should be more specific "influence on the surface resistance" (but please see main comment pt 1 above) Reply 7.We adjusted the formulation

4. Section 3.2, again, please see above comments about soil moisture limitation. Reply 8. See reply 1.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C3086/2015/hessd-12-C3086-2015-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 4541, 2015.