

## ***Interactive comment on “Impacts of ditch cleaning on hydrological processes in a drained peatland forest” by H. Koivusalo et al.***

### **Anonymous Referee #3**

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#### General Comments

Overall, the manuscript focuses on an important and relevant topic - the hydrology and the wise use of peatlands - within the scope of HESS. Furthermore, the manuscript is based on good data, especially taking into account the harsh environment of Northern Finland. However, having said this, the manuscript cannot be recommended for publication without undergoing a major revision:

A) The authors aim at modelling the effects of ditch cleaning. The experimental setup for this goal is with four test catchments (two with cleaned ditches, two without cleaning) rather good, especially as there is long-term data available before and after these cleaning measures were taken. However, before applying or adapting any model, this

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data - both the groundwater levels and the discharges - should be analysed systematically! During the discussion of the model performance, some effects have described, but this is placed nearly at the end of this section (page 163/ lines 9ff).

B) The modelling methodology, especially the calibration strategy, remains rather unclear. For example, only three out of 39 sites were used for calibration, calibration parameter values, calibration methods (manual?) and calibration periods were not given and the reasons behind the unsatisfying model efficiencies during validation were not discussed. Furthermore, only the results of two of the catchments are presented. I would suggest that 1) the parameterisation strategy should be clarified and 2) that the model should be calibrated for two catchments (one with cleaned ditches, one without) and validated using data of the other two catchments. From this, it should be possible to assess whether the model is capable of simulating changes of the hydrological conditions. 3) If this assessment is successful, the catchments where ditches were cleaned could be simulated with "uncleaned" ditches and vice versa. Finally, the discussion should include some considerations on the uncertainty of simulating internal fluxes using evapotranspiration and soil parameters mainly derived from literature.

## Specific Comments

### Introduction:

The introduction introduces the problem and the objectives well, but it could be shortened. Overall, the sections "Introduction", "Site description" and "Methods" account for around 12 of the 18 pages, which is slightly unproportional.

### Methods:

This section is very detailed, but not very easy to follow. It would be easier for the reader if the section was structured into several subsections, perhaps following the outline given on page 153/ lines 24ff.

Page 156/ lines 18/19: "The water level in the ditch is set equal to the elevation of the

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ditch bottom". Why was that done? Later on (page 162/ lines 23ff.), it is stated that sites with a coarse subsoil react relatively sensitive to a change in the ditch depths. Thus, I think that the above assumption is not valid and will introduce (unnecessary) errors.

Page 156/ lines 23ff. Why are you sure that there is no recharge to the deeper groundwater, especially in areas with a sandy subsoil? Did you calculate a water balance (from measurements) for the single catchments?

Page 157/ lines 13ff. How is the channel flow calculated? If there is some kind of roughness coefficient involved, wouldn't it be influenced by the ditch cleaning, and wouldn't be the water level in the ditch (and thus, in the forest) be influenced by this?

Page 159/ lines 2ff. In this paragraph, the parameterisation of the soil properties is described. Several issues should be clarified: - Where did you get the values for the hydraulic conductivity from? Calibration ranges of all calibration parameters should be given as a table. - The water retention curves were derived from the bulk density and from literature. Was the bulk density measured at the different sites? - Why wasn't the topsoil/subsoil boundary determined "independently" from the GPR measurements? - Why was the calibration carried out with only three of 39 sites, and why was measurement site 7 chosen for calibration together with the discharge of catchment C3 although site 7 is within catchment C1?

Results & Discussion:

Page 161/ lines 23 ff The model was applied to "shed light on the causes behind varying measurements". To me, this sounds like a circle conclusion: First, the reaction of the water table was analysed to assign soil parameters, and then different soil properties were found to be responsible for the different reactions of the water table at different sites.

Page 162/ lines 3 ff The model efficiency during validation is not really good. This

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should motivate the authors to critically re-assess and discuss their model. In this paragraph, however, after showing a successful and a less successful example, just the calibrated values of the hydraulic conductivity are given, but the relevance of this is not discussed. Does it, for example, mean that the ks values for till are less well estimated than for sand, or that the till sites are more heterogeneous than the sand sites?

Page 163/ lines 14 ff. "The simulated effect of the ditch cleaning on the water table is small". What about the measured effect? And isn't it rather obvious that the drawdown effect of a lowered water table in the ditches (as it is set to the ditch bottom in the model) is stronger for sites with a higher hydraulic conductivity?

### Technical Corrections

Generally, the language is fluent, precise and without any major errors. The figures are well prepared and the references are numerous and up to date.

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