Anonymous Referee #1

General comments

The paper introduces a model for "upscaling evaporation and other hydrological processes from grid cell to a catchment level". The manuscript is well written, well structured, and comprehensive, including parameter sensitivity analyses and evaluation against different sites and hydrological flows.

The motivation for the study is stated in the introduction as: (1) "to improve evaporation description by more physiologically-based approach", and (2) to take full advantage of open spatial data. In relation to this, the only substantial revisions I would like to propose is to (1) compare the simulation results from the "physiologically-based approach" with an "empirical and simple approach" and (2) compare the simulation results from a model run that makes use of the detailed GIS data and one that does not.

I think such analyses would help the reader better relate the results to the motivations stated, and better understand the added-value of the presented "improved approach".

Response

We are grateful for the thorough review, excellent comments and concrete suggestions how to further improve the manuscript.

Regarding to general comment (1) on comparing the proposed 3-source ET-model to empirical and simple approach our response is as follows:

The level of detail evapotranspiration is necessary to describe must depend on the goal and application. For predicting ET at a single site, or to get reasonable partitioning of long-term water balance at catchment scale, in principle any method embedding the main drivers (radiation, temperature and air humidity) could be used with proper calibration / parameterization. The main aim of the study is, however, seek for alternative approach where physical and physiological arguments are coupled as a relatively simple scheme then shown to be applicable at range of forest/peatland sites across latitudinal climate gradient in Finland/Sweden. Thus, we expect the proposed model can reasonably predict the spatial heterogeneity of catchment / landscape ET, as it can utilize open spatial data available. This is crucial for the second motivation of the study. To summarize, we see the point of the reviewer but in our opinion extending the paper towards model comparison would distract the reader from the main message of the study. To make the above argument more clear, we check the Introduction and Discussion for this regard.

The general comment (2) on studying the importance of spatial heterogeneity on simulation results is a good point, and one of the main motivations of developing the proposed model framework. We, however, opted against adding specific analysis of the role of different sources of spatial heterogeneity to keep the manuscript length tolerable. It was not specifically mentioned that which model outputs would be required to analyze more deeply but regarding to water budget components and soil moisture variability these are already partly included in:

- Fig. 9 showing the role of spatial heterogeneity of vegetation, soil type and topographic wetness index on long-term evapotranspiration and its partitioning;
- Fig 8. Showing how root zone moisture varies spatially at two extreme cased (moist / dry)
- Fig 7: Temporal evolution of mean soil moisture and its variance, and discussion around.

Interpreting these figures with the spatial data layers (Fig. 2) will allow interested reader to gain some insights on the role of spatial heterogeneities on respective model outputs. Further, the role of vegetation, TWI and

The importance of spatial landscape heterogeneity on catchment water balance partitioning at annual, and on specific discharge at daily timescale could be further studied by setting up a factorial model experiment e.g. at catchment C3 used for illustrations. One can consider four spatial layers (LAI, deciduous fraction, soil type, TWI) as independent variables, each with two levels (0 = constant, 1 = spatially variable) yielding to 2^4 = 16 combinations. This analysis could be added into Supplementary material but we would prefer a separate study.

Specific comments (C) and our responses (R)

C: Symbols: Please consult the HESS manuscript guidelines Symbols and equations. E.g., (b) “Multi-letter variables should be avoided. Instead use single-letter variables with subscript (e.g. ERMS instead of RMSE. . . “

R: Will be revised to follow HESS manuscript guidelines

C: Terminology: Please consider using evaporation for total evaporation, instead of evapotranspiration (Savenije, 2004).

R: We are aware of Savenije (2004) - paper and share its opinion on importance of distinguishing between different components of evaporative water fluxes as they have partially different controls. This is where the proposed scheme is beneficial compared to most of the previous models. Being aware that terminology often differs between disciplines, evapotranspiration is well-recognized and well-defined term at least in ecohydrology, atmospheric sciences etc. so we prefer to keep the original terminology.

C: Names of sites: Please consider using a naming system that is easier to remember and relate to for the reader. E.g., FIHy-Pine instead of just FIHy.

R: We acknowledge the comment but as the main point of the manuscript is to show general applicability of the model across sites, specific sites are of less importance. Therefore, no changes are made.

Figures: Please consider adding letters (and sometimes subtitles) to subplots for easier identification. Please add legend to the figures and not only explain in caption (e.g., Fig 7).

R: We aim to easier interpretation when preparing the final figures and will consult HESS manuscript guidelines. Please note that Fig 6 and 7 are changed which improved the clarity.

Title: Please consider adding “boreal” to the title to be more precise, i.e., “Modeling boreal forest. . ..”

R: We considered the comment and well understand the point. However, as the model approach is not limited to boreal forests (except for some of the specific parameters) we prefer the original title.

C: Please consider providing a more balanced review of the different types of models that recognize more merits and disadvantages of the different model types. The reference (Reed et al., 2004) cited, for example found lumped models to have a "better overall performance than distributed models" and (Winsemius et al., 2006). It would also be worth adding a coupled of sentenced on the models that integrate combine lumped features with distributed physically based ones (Gao et al., 2013; Khakbaz et al., 2012), particularly since the study itself presents that lumped approaches are not relevant for ungauged basins, while for example (Hrachowitz et al., 2013; Hundecha et al., 2008; Winsemius et al., 2009) do not dismiss their relevance in such contexts. (Hrachowitz et al., 2013) for example states: "During the PUB Decade, an increasing understanding of the importance of openness towards different approaches, and the willingness to communicate and search for opportunities developed."

Please also note that distributed models can have conceptual components, and are not always necessarily "physically-based" (as implied by the sentence formulation at P2L10).

Please also consider placing the models used in the study in context with other model types reviewed in the introduction.

R: Thanks for pointing out these biases and unintended messages given in the section. The excellent suggestions of the reviewer will be embedded into the revised Introduction.

C:P2L33 "Penman-Monteith equation": Please consider citing the original reference (Monteith, 1965).

R: References provided discuss some of the current methods used for ET in hydrological models, and were not meant refer to Penman-Monteith equation. We will improve the clarity of the sentence.

C: P3L9 "to derive a model": I am not sure I understand the word choice "derive" here, do you simply mean "used in"?

R: "to derive" changed to "to develop"

C: P5L16: Please explain the threshold parameter and refer to where its values can be found.

R: It was thought role of threshold parameter in linear eq. 6 is self-explanatory; however as this was also pointed out by Reviewer 2 we explain its meaning in text and refer to Table 1. The value used here is based on Lagergren and Lindroth (2002) sap-flow study on Scots pine and Norway spruce, as cited in P5L14.

C: P5L18 Eq 7: Please consider adding reference to the equation. Is it (van Genuchten, 1980)?

R: The concept of plant available water or relatively extractable water (eq. 7) has been widely used in the literature; we will provide original reference if possible.

C: P5L20: Please write out the equation (?). In general, please consider referring to Table 3, each first time the parameters listed are mentioned.

R: We provide equation and refer to Table 1 for parameter values and their sources. Please note that Table 3 provides parameter ranges for sensitivity analysis.

C: P10L24: Please consider putting Fig S1 in the main manuscript.

R: Improved version of Fig S1 will be added to the main manuscript as suggested.

C: P1213: Please consider referring back to the methods section.

R: The remark will be considered when finalizing the revised manuscript.

C: P16L12-13: Please note that methods for determining root zone storage capacity using satellite based information has been developed e.g., (Gao et al., 2014; Wang-Erlandsson et al., 2016).

R: Thanks for pointing out these excellent references; remark to possibility of using remote-sensing methods to determine root zone storage capacity is added.

C: P.19L1- : Please consider discussing a bit more in detail in what way the model is advantageous for the applications listed.

R: The comment was considered but to avoid repetition from earlier discussion we decide not to expand this section.

C: P31 Caption: How many days are included/excluded? How much of the mean annual evaporation is taken into account when considering only the dry canopy conditions?

R: The main message of Fig. 4 is to show the model, with single parameter set, can reasonably well describe the mean and variability of daily evaporation across boreal sites in snow-free growing season, in absence of recent precipitation. As data from different sites does not represent same period (and length of dataset is different), adding diagnostics of dry vs. wet day distribution will add little to the present study.

The partitioning of ET into transpiration, evaporation from the ground and intercepted rainfall / sublimation of show on the canopy depends strongly on LAI, weather conditions (potential evaporation, distribution and intensity of rainfall) and time period considered. To elaborate this point in more detail, the ET data in Fig. 9 is in plotted against annual maximum LAI. By definition, focusing to dry-canopy conditions only would eliminate evaporation E and consequently 10 – 40% of annual evapotranspiration. The contribution is likely smaller during growing season but strong dependence on precipitation frequency and intensity is expected.

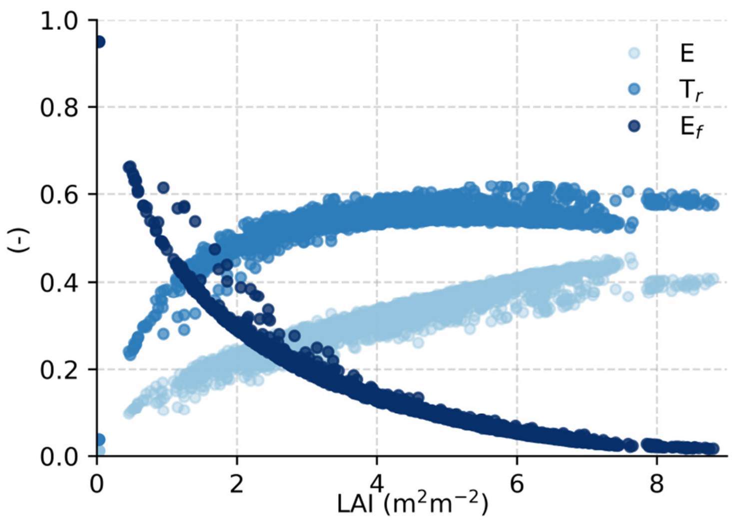

Fig: Partitioning of long-term (2006-2016) evapotranspiration at C3 into components: Tr – transpiration; Ef – evaporation from the ground; E – evaporation / sublimation from wet canopy.

C: P34 Fig 7: Please spell out IQR. Should the right axis of subplot b be blue? Please add units.

R: Changed as suggested

C: P33Fig 6: Please consider including a measure of the runoff performance? And present the measure for all catchments?

R: The value of Willmott's index of agreement (eq. 22) was added to caption. Please note that same measure is given for all catchments in Supplementary Table 2.

C: Fig 35: Please state the catchment name considered.

R: Changed as suggested

C: P38: Please provide justification, explanation or reference for of all values selected.

R: Changed as suggested