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title: Review of "Future changes in water availability: Insights from a long-term
monitoring of soil moisture under two tree species" by Nikol Zelíková et al.
author: Reviewer
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General Comments

(overall quality of the preprint)

Scientific significance:
The manuscript represent a substantial contribution to scientific progress within the scope of Hydrology and Earth System Sciences. It analyses long term soil moisture measurements from a spruce and a beech forest. The data were used to fit a water balance model to the sites. Based on the model results the authors investigate the sensibility of the water balance terms on short and long term changes in the environmental conditions, in particular precipitation (water and snow). Finally, they draw conclusions about the differences in the water balance of spruce and a beech forests. This topic is of great importance because the proportion of these two tree species in Europe is changing over large areas due to climate change.

Scientific quality:
The long term measurements are without doubt a big asset. In the comprehensible scientific approach, presumably all available measurements were used to calculate the terms oft he water balance. Nevertheless, the delineated approach includes mayor uncertainties that are not satisfyingly regarded. The measured quantities do not allow a direct partitioning of precipitation in evapotranspiration and groundwater recharge or runoff. Therefore, the partitioning is done by empirical models that can not clearly validated, i.e. the partitioning and the discussed individual water balance of the two forest stands is partly based on model assumptions and parameters that are derived on other sites.

Even so, I assume the authors used all measured data and information of the site and the results are convincing. So, the uncertainties just need to be more clearly highlighted and, if possible, quantified.

A weak point of the study is the approach used to calculate evapotranspiration. It does not explicitly regard the differences between spruce and beech. In my opinion the Penman-Montheith equation is state of the art and it allows the differentiation via canopy and aerodynamic resistance. A method to reduce the uncertainty are direct measurements of evaporation and transpiration. The authors already named sapflow measurements within the paper. Here I would like to point out that scaling to the forest stand is critical and that an underestimation of transpiration usually occurs.

Another uncertainty is the high spatial heterogeneity of soil moisture due to canopy and soil structure. The authors mentioned up to five measurement profiles. A description of the variability between the profiles with respect to canopy cover would help to establish confidence in the representativeness of these measurements.

As the authors stated, a big advantage of the long term measurements is the possibility to investigate trends in the time series (see line 63, henceforth the shorting L63 is used). However, I missed a discussion of whether or not changes can be observed over time. Subsection 3.1 shows the inter-annual variation of air temperature and precipitation but not of the soil water content and the other terms of the water balance. Also, "3.3 Climate-induced soil water regime and soil water fluxes" covers more seasonal changes at the site than changes induced by climate variability or change (long term changes).

As a distinct feature of the tree type specific water budget the authors discuss the inner-annual variation of the terms. It would be nice to have a visualisation of a typical annual cycle of soil moisture, evapotranspiration and drainage (something like a climograph).

Presentation quality:
In general, the scientific results and conclusions are presented in a well-structured way. The number and quality of figures/tables is adequate (except for Fig. 5, where the font sizes are too small for a printout). The English is comprehensible and generally good, but there are some sentences that lack clarity and conciseness and need to be revised.

Specific Comments

In the following, I will switch to direct address to create a dialog.

As mentioned above the question arises: **Are the measurements representative for the sites?** Did you compare other measurements on the same patches? On L119, you write "One to four tensiometers were available for each measuring depth at each site, and the single value for a particular depth was taken as their average." First, what is meant by "single value", second could you illustrate the positioning of the sensors with respect to canopy cover, and third could you show the variability of the soil moisture for both sites?

Concerning the "**Vertical distribution of pressure heads**": Long term mean values over different seasons and conditions (Fig. 3) are difficult to interpret, as the differences between the measurement levels are small compared to the variability of the pressure head. I am wondering whether there is a significant deviation of the pressure head in a certain depth from the other levels, especially for beech. The categorisation according to precipitation is a good approach, however, Figure 4 shows that there is still a large degree of variation when considering a whole year. It would be interesting to see what the differences between levels and sites in the time domain look like (similar to flood statistics, i.e., what is the return interval of pressure heads below a certain value and how long do they persist).

In Figure 5 and the text, you use four soil moisture categories. Unfortunately, I couldn't find a clear definition.

L199: "Dry and wet years were identified by analysing the soil moisture regime in terms of the vertical distribution of pressure heads". Typical time series were given in Figure 5. Could you give a clear definition? Please explain the method or give a reference.

Model calibration: "The **entire period** of available data was used for model calibration". Validation of the model is therefore only partially possible at best. The given RMSE of the pressure heads are just an assessments of the quality of the fitting procedure (Btw: What method was used to optimise the parameters?). Usually, one part of the data is used to calibrate the model, and the other part is used for validation.

The model is calibrated with respect to the soil water content. The long-term means of the drainage fits well to the measured runoff. Although, it is assumed that beech and spruce stands experience the same drainage in the long term, that might not be realistic (see your discussion starting at L437). Could assess the error in S(t) and D(t)?
You write "However, the modelled high transpiration rates at the beech sites mostly follow from fitting to the high-resolution time series of measured local soil moisture data, which show lower values during the summer season compared to spruce, and simultaneous observations of no change in groundwater levels." (L440). However, this is no justification for the assumption that the ground water recharge below the beech is the same as below the spruce.

Please, explain (and discuss) the choice of your method for the **calculation of the evapotranspiration** ET. As far as I understood, you have all variables for the calculation of the actual ET available at your site. Why do you calculate the PET following a reduced approach (Oudin et al. 2005) and estimate the actual ET for the two sites from that?

What is the reason for calculating the Net longwave radiation (L155)? It seems not necessary, neither in your described model nor in the PET approach of Oudin et al. (2005). However, you cite Kofroňová et al. (2019), who used the Penman-Montheith equation to calculate the potential evapotranspiration (which is actually not correct, since the Penman-Montheith equation calculates the actual evapotranspiration). Which approach did you use Oudin et al. (2005), or the Penman-Montheith equation?

Concerning S(t) (L160): How is the influence of tree type regarded?

Looking for correlation between the terms of the water balance and environmental quantities (L326), why do you use the snow cover duration and not the precipitation during the winter season (water equivalent of the snow). I am not surprised by the weak correlation between snow cover duration and soil moisture, there can be long cold winters with snow cover but little precipitation and vice versa for warm winters. The usual argument, snow cover enhances infiltration, is not applicable at your site, as you wrote on L131: "surface runoff is not generated in the experimental catchment and all water directly infiltrates into the soil".

Results from literature and own observations get sometimes mixed up in the argumentation (see L382 ff. and L442: "The comparatively high transpiration rates of beech during the summer season were separately validated by measured sap flow (Brinkmann et al., 2016; Gebhardt et al., 2023)"). Please make clear what is your observation and what can you conclude from that, and finally compare it to literature.

Technical Corrections

(Minor errors and comments)

In the following I listed critical points (mostly about language) and gave some suggestions after the "==>".

General points

- At various points the text is not as precise as it should be for a scientific publication, please revise the manuscript carfully, e.g.
    - L20: "While long-term column averaged pressure head indicated drier soil at the spruce site overall, this was driven by the wettest years in the dataset."
    - L364: "The partitioning of the water fluxes in both stands was driven by different rates of interception (higher at the spruce site) and transpiration and soil evaporation (higher

under beech).": The partitioning of the incoming precipitation is driven by vegetation type, soil conditions and other environmental conditions ...
    - On L454 you conclude "This was because the transpiration flux was the governing mechanism of the forest water balance in dry summer seasons.", However, the transpiration was not measured, thus this could not be a conclusion, it is more an assumption.
- When listing quantities, please use “and” rather than a forward slash “/”, as the latter is mathematically equivalent to division.
- Figures: writing of the units in squared brackets might be common but is wrong. The axis label represents the numbers. So I ask whether “pressure head [cm] = -800” is a correct statement? In comparison, “pressure head / cm = -800” or “pressure head in cm = -800” would be correct. I would recommend the later phrasing.

Specific points

L20: "While long-term column averaged pressure head indicated drier soil at the spruce site overall." ==> article missing or use plural, i.e. "the pressure head" or "pressure heads"

L22: "... drove complex but robust differences in flow partitioning between the forest types." ==> "... drove complex but robust differences between the forest types in regard to flow partitioning"

L25: "Estimated summer recharge" ==> "The estimated ground water recharge in summer"

L53: "greater"?: "Schume et al. (2004) and Šípek et al. (2020) reported greater soil profile drying during the vegetation season at beech sites." ==> "Schume et al. (2004) and Šípek et al. (2020) reported a stronger drying of the soil profile during the growing season at beech sites."

L60: "they provide only a partial view of the role of individual water fluxes" ==> "They only provide a partial insight into the role of individual water flows."

L123-125: Please clarify and shorten if possible

L135: Define Θ (it's probably the change in soil moisture in the topmost layer)

Eq. 7: $\Theta_{(t-1)}$ is not defined. Is it the measured Θ?

L168: Please regard the unit conversion: K_s is given in mm h^{-1} , whereas $D(t)$ is in mm d^{-1}

L188: "additional requirements" ==> "boundary conditions"

L200: check grammar

L232 ff.: "attained" ==> "reached"

L236: "vegetation seasons" ==> "vegetation periods" or "growing season"

L239: Please define the "four soil wetness categories" more precisely

L243 "the spruce site attained lower pressure head values than did the beech site" ==> "the spruce site reached lower pressure head values than the beech site"

Fig. 5: nice overview, but graphs are to small for printout, zooming reveals poor quality, it would be nice to see PET also

L270: To assess the RMSE values the reader should first be introduced to the values of the snow water balance.

L275: What do you mean with "SWBM efficiencies". I recommend deletion of "SWBM efficiencies in terms of the"

L278: "VWL" is not defined up to now.

"3.2.2 Simulated Water balance" contain several redundancies and could be shortened

L284: Please define "actual evapotranspiration (AET)". Is it equal to $S(t) + P_{int}(t)$?

L285: "transpiration/soil evaporation (T+E)", Isn't that the same as S? (L134: "S(t) is the actual evapotranspiration rate (mm day−1)") ==> "transpiration plus soil evaporation S"

L287: "Beech reaches almost 100 mm higher T+E, and similarly, spruce reaches this level in the case of interception." ==> "The beech stand reaches almost 100 mm more S than the spruce stand, on the other hand, the evaporation from the interception storage in the spruce stand exceeds that of the beech stand to the same extent."

L348: "Seasonal precipitation also had a major influence on the differences in particular water fluxes between beech and spruce sites" ==> are there water fluxes between the beech and the spruce site? ;-)

L361: "measured pressure heads" ==> "measured soil water potentials"; The analysis also uses other measurements, doesn't it?

L429: "the tensiometer measurement limit" ==> "the tensiometer limit" or exactly "the measuring range of the tensiometer"

L430 "Eliminating this bias did not allow model fitting during dry periods." Yes, however, a model is usually not calibrated on the whole data set but only on a shorter calibration period and then applied to the whole data set.

L456: "These differences resulted in higher summer drainage to deeper layers below the spruce canopy." ==> These differences resulted in higher summer drainage to deeper layers below that of the spruce canopy.