

Future changes in water availability: Insights from a long-term monitoring of soil moisture under two tree species

Nikol Zelikova et al.

Author's response to Reviewer#1

Comment #1

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.

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? Which approach did you use Oudin et al. (2005), or the Penman-Montheith equation?

Response#1

- Thank you for pointing out the difference in aerodynamic resistance between the two species, which we address below. Importantly, the model we used does differentiate between spruce and beech on two important processes: (1) the interception is estimated differently for both sites and the estimation is based on the measured characteristics, (2) soil water balance model parameters (namely θ_S , θ_R and K_{sat}) used for the estimation of beech and spruce transpiration and drainage are different as they were obtained by the model calibration on the different soil water regimes. This results in higher transpiration of beech in dry summer periods than for spruce – which is proven by the observed soil water regime.
- As the hydrological models are usually based on the two-stage modelling scheme – potential (PET) and actual evapotranspiration (AET) we have chosen Oudins approach for the estimation of PET.
- The reasons for the Oudin's approach were:
 - o Oudin's method represents a **robust approach** relying only on the air temperature, and therefore it can be used for the estimation of PET also in the periods with limited data availability without the loss of consistency in the input data series when they are replaced by the data from the neighbouring meteorological station
 - o **Side-experiments** utilizing the values of the P-M reference evapotranspiration aside from Oudins approach (in the period of available data) documented the influence of selected approach only on the soil water balance model parameters and not on the resulting water fluxes.
 - acceptable differences among P-M and Oudin's values of PET, especially when e.g. monthly means are considered. The day-to-day fluctuations are more averaged by Oudin (Fig.R1).

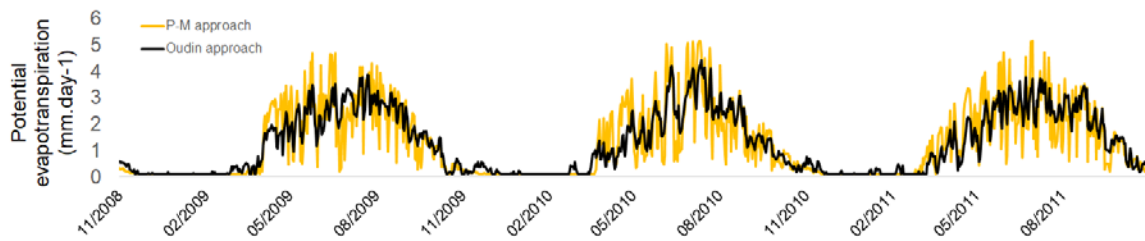


Fig. R1 Comparison of Penman-Monteith and Oudin PET

- the above-mentioned differences led to similar performance of the soil water balance model using both P-M and Oudin approach for the estimation of potential evapotranspiration (Fig. R2).

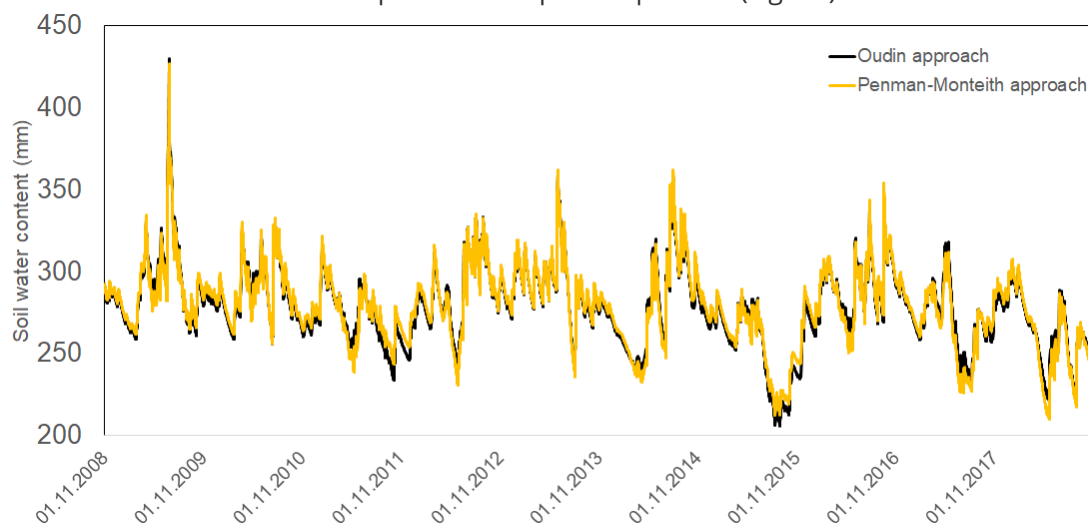


Fig. R2 Modelled soil moisture using Penman-Monteith and Oudin PET

- canopy specific parametrization of P-M approach by means of adjusted aerodynamic resistance (difference in canopy height was set to 5 m) resulted in the seasonal difference in PET of only 3.7 mm in 2010 and 1.5 mm in 2009. If the amount of soil water was changed by only 1%, it resulted in a change of AET by 9.4 mm in 2009 and 8.9 mm in 2010, respectively. This documents the limited influence of aerodynamic resistance compared to the influence of soil water availability (reflected in stomatal resistance) which is an inherent part of the model.
- Literature review:** 1) Oudin et al (2005) paper showing reasonable results of hydrological models when using his approach compared to state-of-the art models in the conditions of limited data availability and 2) Touskova et al. (2025) paper showing a reasonable correspondence of Oudin's PET values and pan evaporation data in the Czech Republic and also with P-M reference evapotranspiration in terms of the seasonal sums
- The necessary **data for the Penman-Monteith (P-M) method are available only from 2008** and they originate from the nearby grass covered meteorological station (300

m distance) at the forest opening. Hence, we do not have the opportunity to obtain site-specific information about wind profiles separately for beech and spruce forest.

- The reasoning and justification of the utilized approach was added to '2.3 Soil water balance model section' l. 192-195 and to '4.5 Modelling limitations' section of Discussion l. 551-557

Comment#2

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).

Response#2

Yes, long-wave radiation was not necessary – it is a mistake in the manuscript text which was deleted.

Comment#3

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 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?

Response#3

We ensured the representativeness of our measurements for the sites in several ways.

- Our sites are even-aged, single species stands with closed canopies and no gaps; this relative homogeneity of vegetation makes representing bulk soil moisture/potential mostly a question of sufficient replication and avoiding placement of sensors at non-representative microsites.
- Locations of the plots in which the tensiometers were installed were carefully picked so that they will represent the **average slope** of the catchment and they will be located in **between trees**. Hence, they do not represent the places close to trees, which will be influenced by preferential water flow by stem flow, as well as they are not located in the forest openings. The measuring profiles are approximately 3.6 m from spruce trees and 2.7 m from beech trees. The average distance between two neighbouring trees is 5.4 m in spruce forest and 4.5 m in beech forest, indicating locating the measuring profiles approximately in between the trees.
- Both beech and spruce forests are of uniform age and the **spatial variability of canopy cover** represented by coefficient of variation of LAI is 12.8 % in spruce and 8.9 % in beech forest, respectively. The coefficient of **variation of soil moisture** ranged from 2.2-2.4 % for particular depths in beech and from 3.5 to 10.8 % in the spruce forest. The spatial variability of forest canopy and soil moisture measurements is of similar order as the error in precipitation measurement.

- In our previous work (Sipek et al., 2020), we have compared the average values of measured pressure heads for several depths in spruce site with another three profiles equipped with UMS T8 tensiometers located nearby (20 m from original profiles). The comparison proved similar pressure head values demonstrating a **good correspondence with other measurements** (see Fig. R4).

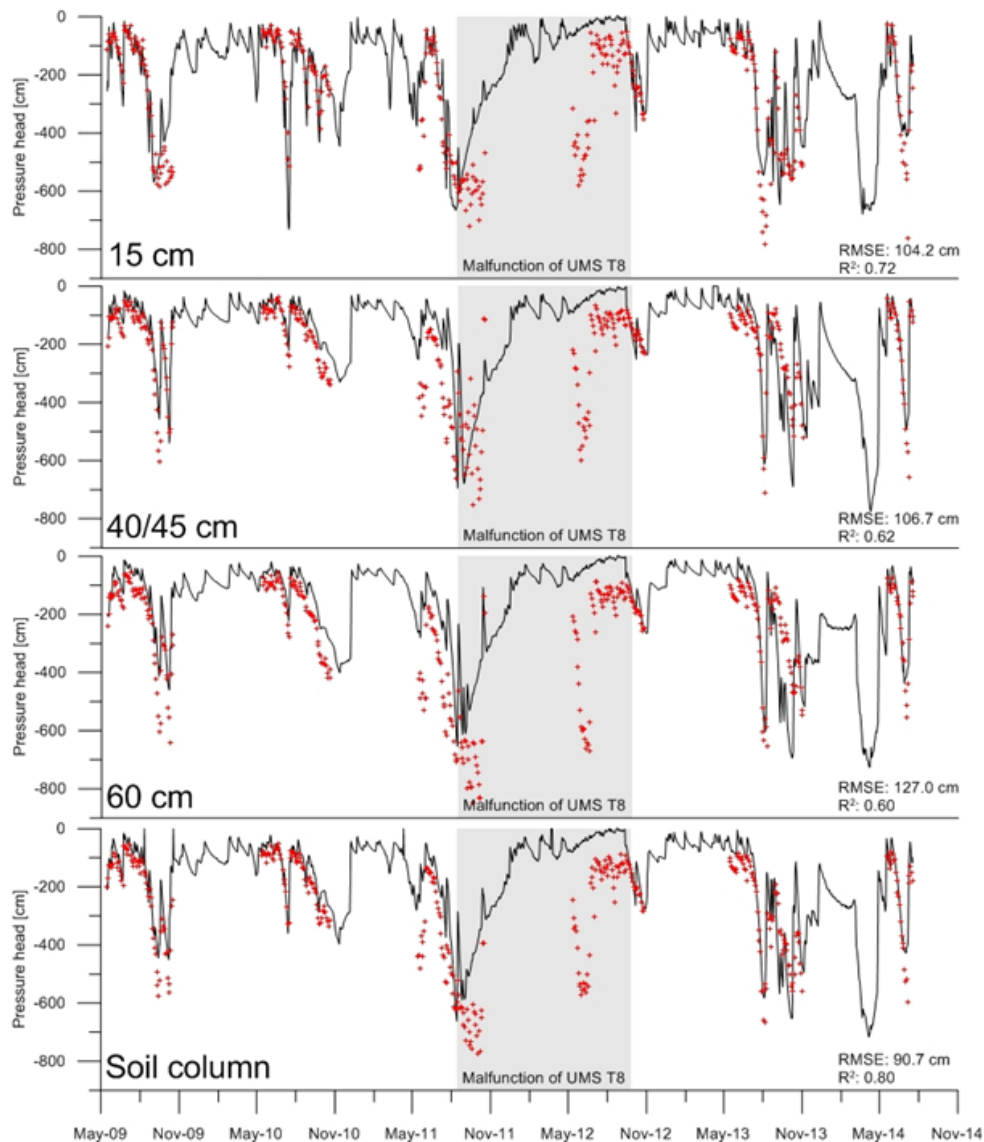


Fig. R4 Comparison of pressure heads measured by UMS T8 (black lines) and Thies (red crosses) tensiometers at the SPR site in the period of 2009–2014. The grey area represents the malfunction of UMS T8 tensiometer and was not included in the statistical assessment. Each plot represents a particular depth of measurements (bottom left corner). RMSE stands for a root mean square error and R2 for a coefficient of determination (Sipek et al., 2020).

- Above-mentioned information was added to lines 144-150 of '2.2 Field measurements' section

Comment#4

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).

Response#4

Thank you for the valuable comment. We have newly tested the existence of trends in soil moisture time series using a trend-free pre-whitening Mann-Kendall approach (Yue et al., 2002) and statistically significant negative trends were observed in both soil moisture time-series documenting gradual changes in soil water regime, which were also observed by the reported increasing occurrence of water limited seasons. This was added to the manuscript (l. 290-292).

A climograph is a thoughtful comment and the figure below (Fig. R5) documenting the differences between beech and spruce plots was added to the manuscript (new Fig. 8).

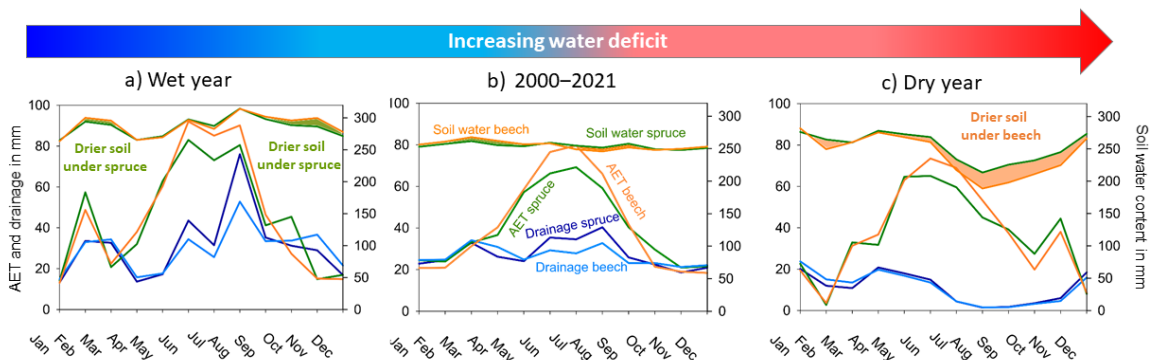


Fig. R5 Average monthly sums of soil water balance components in beech and spruce forest

Comment#5

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).

Response#5

Thank you for suggesting a better way of documenting depth differences in soil water regime between beech and spruce site. We removed figures 3 and 4 and we added the pressure head exceedance probability plot instead (Fig. R6 below). The manuscript text was modified accordingly – new Figure 3 and text in lines 262-282.

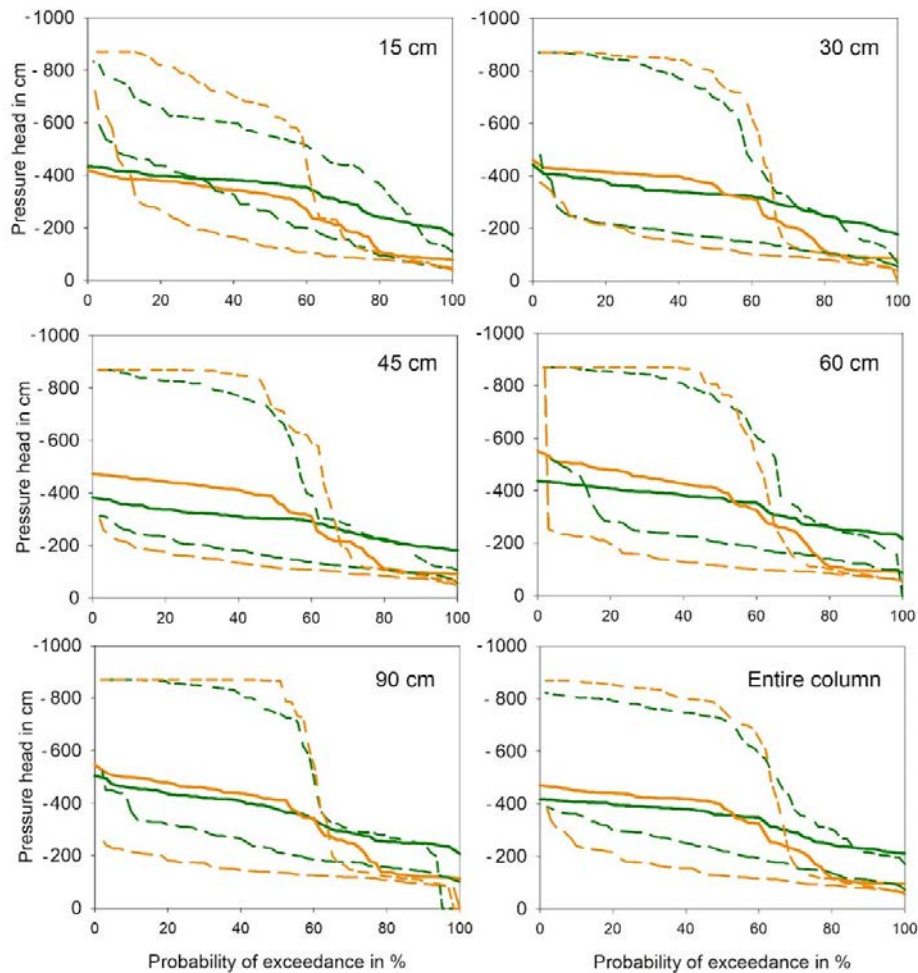


Figure R6. Exceedance probabilities of pressure head for particular depths for averaged the entire period (thick solid lines), dry year 2015 (short dashed lines) and wet year 2020 (long dashed lines). Green colour represents spruce and orange beech forest.

Comment#6

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.

Response#6

The definition of the soil moisture was in the lines 245-254 of the original manuscript but we newly summed it up in lines 314-320. It originates from the observed soil moisture regime:

- category A - spruce retained lower pressure heads throughout most of the season
- category B - only one single event when the beech site attained lower pressure heads than spruce

- category C - the pressure head decreased more pronouncedly at the beech site for a significant part of the summer season
- category D - refers to the seasons when the tensiometer measurement limit of – 865 cm was reached (mostly at the beech site)

Comment#7

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.

Response#7

Thank you for the valuable comment. We added following important information about model validation, which was done prior to the overall model calibration presented in the manuscript (new section '2.4 Model parameterisation, validation, and forward simulation'. Our previous omission of this information unnecessarily undermines confidence in the results.

At the very beginning, we started with the standard procedure as we calibrated the model using several 5y calibration periods. For this purpose, we **split the period of interest into 4 sub-periods** – each covering 5y (2000-2004,2005-2009,2010-2014,2015-2019) and calibrated the model separately for each of these periods, always carefully maintaining the fit of drainage to the measured runoff. The model parameters were fit using the **genetic algorithm using the RMSE of volumetric water contents as an objective function**.

As the model parameters and also the model performance did not change substantially (see Fig. R7 below, which was added to supplementary material Fig. S3) we have chosen to calibrate the model for the entire period so that the water balance (i.e., discharge) can be maintained as close as possible to the measured long-term mean. The amount of drainage estimated from the water balance is more precise, and we could utilize this approach with only minor deterioration of an objective function compared to the situation when parameters from each of the 5y periods were used.

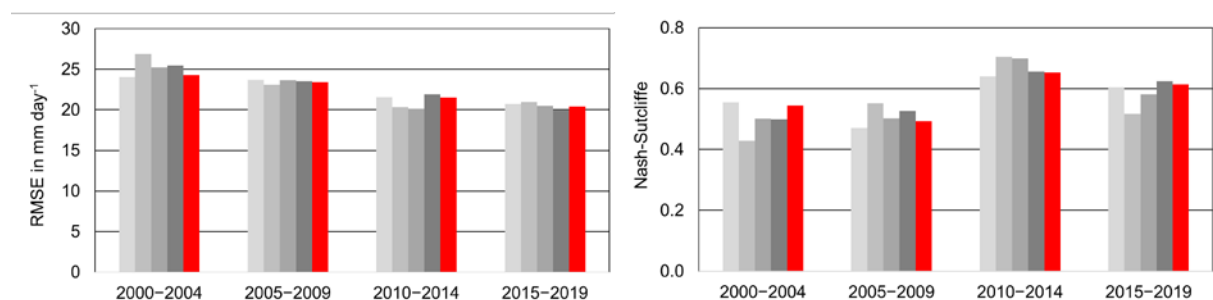


Fig. R7 Model performance when calibrated in particular periods. Values from first columns represent calibration from 2000 to 2004, the second and following columns represent the following calibration periods (2005-2009, 2010-2014, 2015-2019, and the last column is an overall calibration)

Comment#8

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.

Response#8

Thank you for pointing out this lack of clarity in the manuscript, which gives the impression that we assumed that the beech plot experienced the same drainage as spruce. In fact, we assumed similar or lower values of drainage based on the fact that in summer periods when no changes in groundwater level were observed, we observed more pronounced declines in measured soil water content under beech. This assumption is supported by the literature (most relevant papers cited), as studies on the topic predominantly report higher transpiration rates of beech (lines 228-231 in the 2.4 section).

Moreover, we have started the measurements of sap flow in August 2024 at seven trees at each site using Trunk Heat Balance method with EMS-81 sensors (EMS, Czech Republic). The monthly sums (August) of transpiration of 60.2 mm in the case of spruce forest and 76.2 mm in the beech forest were observed. Further, the soil water model run was extended to August 2024 and modelled monthly transpiration sums of 61.2 and 81.5 mm for spruce and beech forest were modelled. This indicates that the modelled differences in transpiration are in an acceptable agreement with measurements - although we are aware that one-month period is very short for a proper analysis and hence, we will not add this analysis to the manuscript.

To sum up, it arose from the facts that (1) the soil moisture declined more pronouncedly, (2) reported transpiration of beech is higher and (3) no changes in groundwater level were observed during these declines.

Comment#9

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

Response#9

The influence of tree type is reflected through different parametrization of the effective wetness (θ_E) restricting the rate of PET. The parameters include θ_S and θ_R , which govern the linear relationship of $S(t)$ representing the rate of actual evapotranspiration to potential one based on the available soil moisture similarly to the approach of (Feddes and Rijtema, 1972). The different parameter values are documented in Table 1. In most cases, this results in the effective wetness ranging from 0.29 to 0.60 in the case of spruce and from 0.40 to 0.80 in the case of beech. Hence, in the case of beech plot, the rate of actual ET is following PET more closely.

We are aware that the utilized modelling approach is not describing the physiological behaviour of plants entirely, especially in the drought stress periods as more complex reaction to the water deficiency stress was reported both in the case of beech (Walthert et al., 2021) and spruce (Zweifel et al., 2002), but it is a current state-of-the-art approach in hydrological modelling.

Comment#10

Looking for a 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".

Response#10

The information about snow cover duration was used to demonstrate the limited role of winter characteristics on the summer soil moisture (correlation coeff = 0.08). The same is valid for the maximum snow water equivalent (correlation coeff = 0.15), winter precipitation (correlation coeff = 0.09; see Fig. R8 using winter precipitation in comparison with original Figure R9) and the length of continuous snow cover (correlation coeff = 0.01). Both figures show similar limited influence of winter meteorological characteristics. Hence, we stuck to the original one.

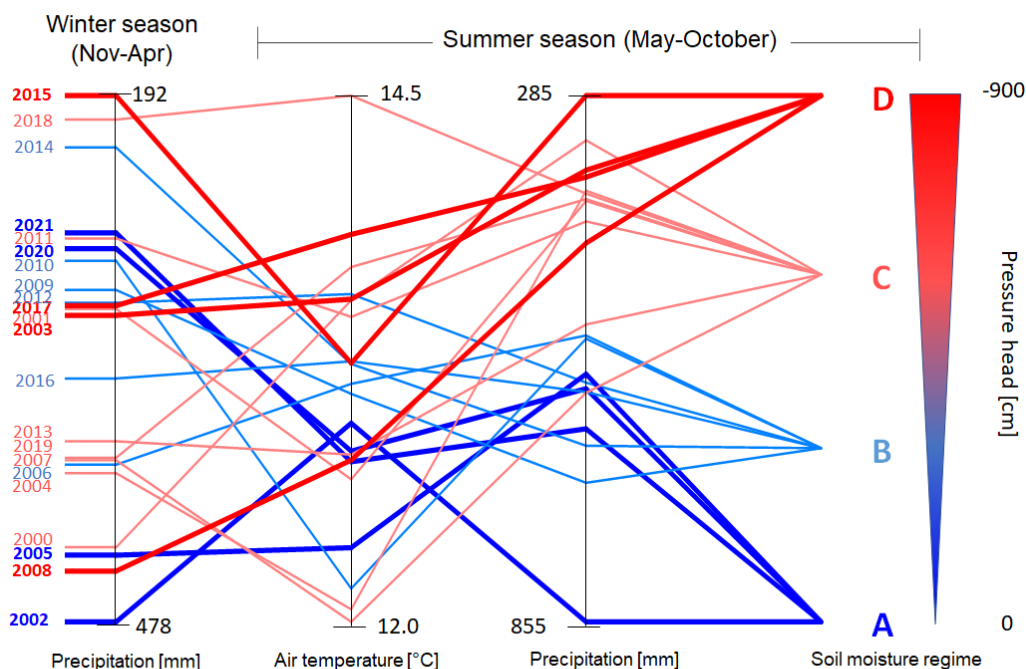


Fig. R8 Demonstration of winter precipitation (very left column) influence of summer soil moisture regime

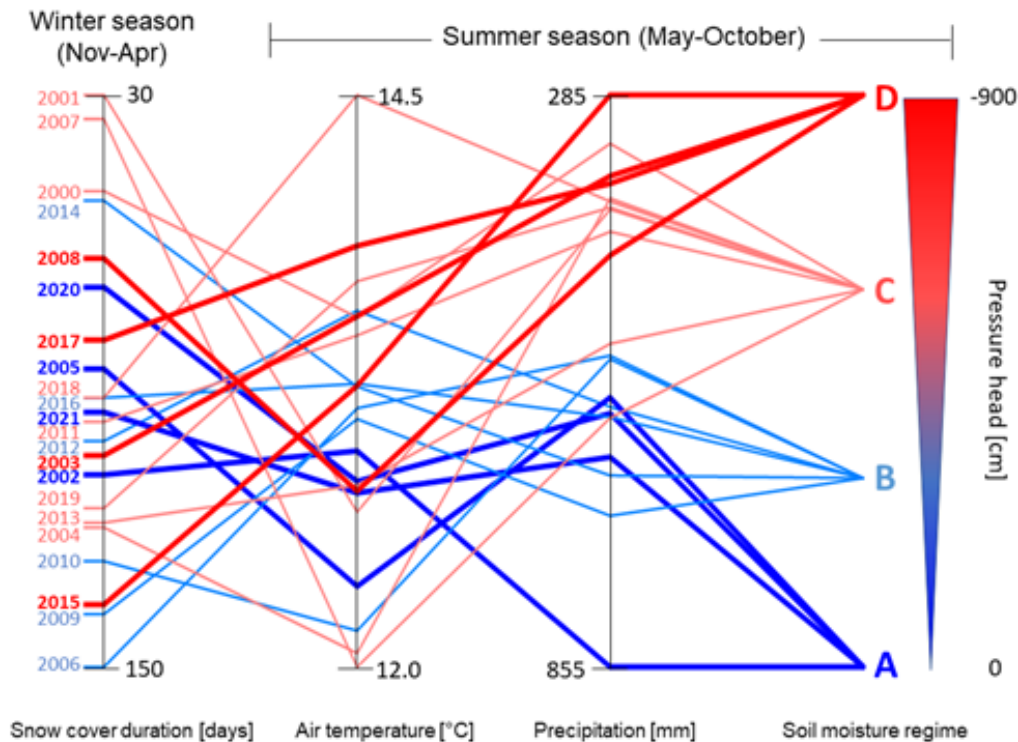


Fig. R9 Demonstration of snow cover duration (very left column) influence of summer soil moisture regime

Comment #11

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.

Response#11

Yes, thank you for the notice. We did not want to mix up our results with the results from the literature. We polished the mentioned parts of the text so it can be clearly distinguished what is the result and what are the comparisons with other authors (lines 464-465 and 563-565).

Comment#12

Technical corrections

Response#12

Thank you for mentioning several inaccuracies in the text. We carefully corrected all points that you have mentioned.

References:

- Oudin, L., Hervieu, F., Michel, C., Perrin, C., Andréassian, V., Anctil, F., and Loumagne, C. : Which potential evapotranspiration input for a lumped rainfall-runoff model? Part 2 - Towards a simple and efficient potential evapotranspiration model for rainfall-runoff modelling, *J. Hydrol.*, 303, 290–306, [doi:10.1016/j.jhydrol.2004.08.026](https://doi.org/10.1016/j.jhydrol.2004.08.026), 2005.
- Toušková, J., Falátková, K., and Šípek, V.: Estimating potential evapotranspiration in a temperate zone: The challenge of model selection, *Water Res. Manag.*, 2024 (under review).
- Šípek, V., Hnilica, J., Vlček, L., Hnilicová, S., and Tesař, M.: Influence of vegetation type and soil properties on soil water dynamics in the Šumava Mountains (Southern Bohemia), *J. Hydrol.*, 582, 124285, [doi:10.1016/j.jhydrol.2019.124285](https://doi.org/10.1016/j.jhydrol.2019.124285), 2020.
- Walthert, L., Ganthaler, A., Mayr, S., Saurer, M., Waldner, P., Walser, M., Zweifel, R., von Arx, G.: From the comfort zone to crown dieback: Sequence of physiological stress thresholds in mature European beech trees across progressive drought, *Sci. Tot. Environ.*, 753, 141792, [doi:10.1016/j.scitotenv.2020.141792](https://doi.org/10.1016/j.scitotenv.2020.141792), 2020.
- Yue, S., Pilon, P., Phinney, B., Cavadias, G.,: The influence of autocorrelation on the ability to detect trend in hydrological series. *Hydrol. Process.*, 16, 1807–1829.
- Zweifel, R., Böhm, J. P., and Häsler, R.: Midday stomatal closure in Norway spruce - Reactions in the upper and lower crown, *Tree Physiol*, 22, 1125–1136, [doi:10.1093/treephys/22.15-16.1125](https://doi.org/10.1093/treephys/22.15-16.1125), 2002.

Future changes in water availability: Insights from a long-term monitoring of soil moisture under two tree species

Nikol Zelikova et al.

Author's response to Reviewer#2

Comment#1

The title mentions future changes in water availability but the results refer to the past 22 years (2000-2021). I have no doubts that global warming is changing climate patterns in Central Europe. However, climate change can be detected only on very long time series by capturing decadal trends. In other words, climate change should be supported by data. The authors should state what is the baseline-historical climate regime in terms of rainfall and temperature observed in the past century. Climate change can be predicted by climate projections from 2020 up to 2100 which are based on scenarios depending on the carbon dioxide emissions (RCPs). There are many GCMs available in internet.

Response#1

Thank you for pointing out this discrepancy in the current manuscript's framing of the study. We agree that it will be helpful for the reader to see a baseline at the appropriate scale to assess longer-term climate-driven shifts. We also agree the text needs to be clarified on this point: our intention was not to make specific projections of future water availability; rather, we examine how ecosystem functioning modulates the hydrological impacts of climate change. As relevant vegetation processes play out on timescales shorter than the overall climatic drivers (incl. differences in seasonal and sub-seasonal vegetation functioning), their effects on local water flux partitioning are readily apparent in our data. We made this clearer both by changing the title (new title is 'Divergent water balance trajectories under two dominant tree species in montane forest catchment shifting from energy- to water-limitation') and in the text itself.

To provide a baseline for the reader to evaluate climate-driven shifts in flux partitioning, we added potential and actual evapotranspiration (PET and AET) and precipitation values since the start of local measurements in the experimental watershed in 1975, using the Budyko framework suggested by the reviewer (see Comment#3 below). The expected broader climate-induced shift is well supported by these local data (Fig. R10 of these revision notes, which is newly added to the manuscript as Fig. 7), which evidence an accelerating shift in the balance between atmospheric water supply and demand at a decadal time-scale. For this reason, we added new section '4.1 Transition from energy and water limitation' and also section '4.3 Scope of the study' into the discussion section.

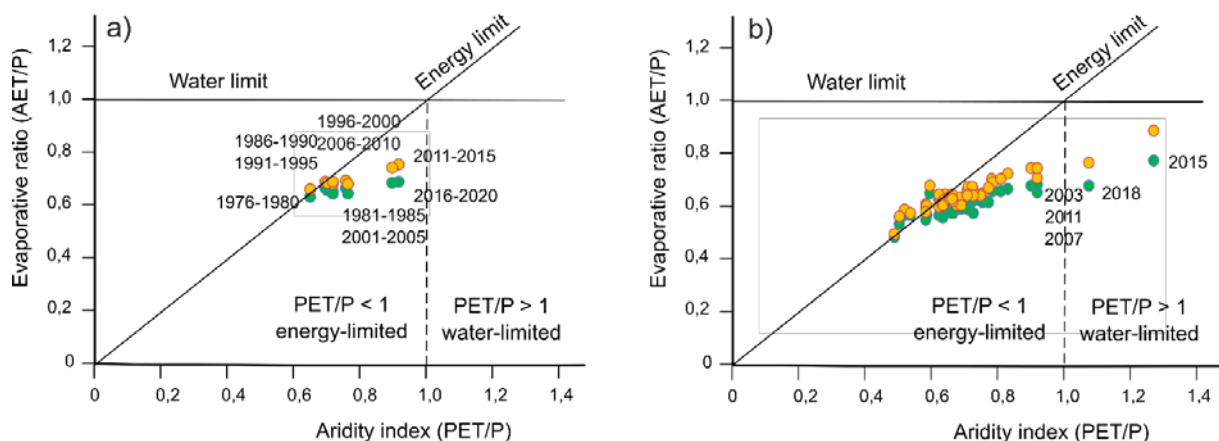


Fig. R10 Ratios of actual and potential evapotranspiration to precipitation from the experimental watershed covering the period 1975 to 2020 shown within the Budyko curve reference frame – (a) 5-year averages and (b) annual values. Green points represent the spruce site and orange points beech.

We believe that adding this local time-series (1976-2021) will be sufficient to situate our study within the broader climatically driven pattern. It shows our data cover a period (2000-2021) in which the climatic drivers are forcing a gradual shift from a fully energy-limited state (1976-1999) through more co-limited regimes, in which the most recent outlier years already show strict water-limitation. We do not feel our work needs to belabour the fact of climate change or its expected broader-scale, exogenous impacts on the hydrologic balance any further – there are plenty of studies demonstrating these, sufficiently referenced in the manuscript (lines 42-47).

Our work specifically poses questions about how climatic drivers interact with vegetation processes to produce hydrologic flux partitioning and ecosystem function. The processes we studied occur over sub-annual time-scales and their effects can be adequately evaluated in inter-annual differences in site/watershed hydrologic balance. The core 22-year dataset is thus entirely sufficient to evaluate their effects. It is also by far the longest available of its kind that we are aware of.

We see little added value from the use of GCMs to produce climate projections for our study. Mainly, this is because we are not aiming to make any projections. Instead, we analyse observable ongoing changes to advance process understanding of climate-vegetation interactions beyond what is currently represented in GCMs. Using GCMs that cannot adequately represent the feedbacks between climate, vegetation, and soil hydraulic properties to make projections of future water availability strikes us as a highly uncertain means of addressing our questions. Rather, we believe process understanding needs to be improved before GCM projections can meaningfully encompass such feedbacks. In response to this comment, we made our aims clearer in the manuscript introduction (lines 74-86).

Comment#2

The authors present a detailed and interesting analysis on the impact of climate forcings on the water balance components and profile-average pressure head under two different land uses. However, what is the novelty of this article? I appreciate the unique long-term data set, but what is new if compared to the state of the art? How can readers exploit the findings of this study?

Response#2

We appreciate these questions from the reviewer as they indicate that the manuscript still needs to state the novelty, significance, and usefulness of our work more clearly. The state of the art is currently a mosaic of short-term studies with mutually contradictory results (Manuscript lines 64-67, 466-474). Their unresolved contradictions stem mostly from the limited duration of each study, which makes each partial result context-dependent on the specific climatic conditions of the study period.

The main novel advantage of our long-term dataset is the ability to make **robust interannual comparisons** over a range of climate conditions (Line 439). This enabled us to disentangle interactions of specific climatic (summer/winter precip, temperature) and vegetation (phenology, rooting, hydraulics) drivers of forest hydrologic response to dry vs wet years and seasons. A second advantage of the dataset is **depth coverage over the rooting zone** and high replication, allowing us to overcome site-scale heterogeneity and study limitations due to limited vertical extent of

measurements (e.g., lines 471-474). Finally, the integration of this dataset within long-term observations from the experimental watershed enables us to impose **closure on the hydraulic balance** and estimate individual fluxes for both forest types.

Our main novel contribution, enabled by this unique combination of dataset advantages, lies in showing which climatic variables have driven water limitation so far (atmospheric water supply more so than demand), and which vegetation processes most exacerbate or dampen it in the studied forest types. We showed which vegetation traits or processes become important to the hydrological balance under which conditions, including during previously unobserved water-limited years. The revealed interactions produce feedbacks that will ultimately lead to differences in function and fate between these important forest types. Specific novel findings with broader significance include:

- whether beech or spruce forest soils end up drier in the growing season depends on intra-annual precipitation distribution due to seasonal differences in flux partitioning by the two forest types (lines 439-447); this helps to resolve previous contradictions in the literature as a majority of studies are limited to a single number of years.
- differences in winter drainage between the forest types increase with winter precipitation (lines 480-485); under expected summer to winter shifts in precipitation, this novel interaction should enhance beech forests' water limitation and affect their role in baseflow generation and intra-annual storage/discharge timing, with implications for forest and water management.
- beech hydraulic function capable of sustaining transpiration during drought interacts with summer atmospheric water balance (PET/P) to exacerbate recharge reductions in warmer or drier years (lines 361-363).
- by contrast, higher spruce contributions to summer drainage due to reduced transpiration initially persist as water limitation begins to affect the system, but eventually decline to zero under water *supply* limitation (lines 363-364).
- collectively, these findings demonstrate divergent patterns of forest hydraulic functioning under water limitation in summer/winter, due to atmospheric supply/demand, which advance process understanding in support of model development or direct application to ecosystem and water management.
- the difference in soil moisture between the forest types is dominated by depths > 30cm (lines 471-474); this is not entirely unexpected due to known rooting depth differences, but quantifying this dominance remains a finding valuable for scientific practice when the vast majority of soil moisture measurements are done at depths < 20cm.

A final point of novelty is the recent occurrence of annual-scale water limitation of AET, which is unprecedented over the 40+ year instrumented period and entirely unexpected in this montane system. The entire range, including our sites, is classically thought of as energy-limited, not just under "baseline" (1961-1990) climate but for past millennia. Our results include some of the first observations of differences in hydrologic functioning of these cold, humid montane forest types under water limitation. Anticipated climatic trends make the publication of these findings all the more timely. This finding is newly stressed in '4.1 Transition from energy and water limitation section' (lines 415-437) and documented by the long-term shift within the Budyko framework as we also prolonged the model simulation by the period of 1975-1999 when no information about soil moisture regime was available.

We believe diverse readers will make use of our findings because cross-scale interactions in land-atmosphere feedbacks such as these are one of the key sources of uncertainty in predicting shifts

in ecosystem function and composition driven by climate change. Our study contributes towards filling key gaps in the required process understanding by empirically resolving specific processes that contribute to differences in forest hydrologic functioning under shifting climate. For researchers, this understanding contributes towards the next generation of models and projections. Practitioners can use it to evaluate interactions between the ecosystem and water management. We reorganised the discussion to improve the explanation of the novelty and significance of the results.

Comment#3

It comes to no surprise that the comparison of soil moisture regimes proves to be precipitation dependent. The results related to this site-specific study (area of 1 km²) cannot be representative for the impact of climate and land use change in Central Europe. The water balance depends on soil depth, layering, and soil hydraulic properties, on the terrain features, on vegetation patterns and characteristics, on climate regimes and many other factors. The last sentence is usually supported by visualizing the Budyko curve, which is used to understand the long-term balance between water availability and energy in a catchment (a region drained by a river or stream). It helps us analyze how much precipitation is evaporated versus how much becomes streamflow.

Response#3

Thank you for suggesting the Budyko conceptual framework. We agree this is a very productive framing for our work and we added a figure to illustrate this statement. Fig. R10 (above) shows clearly that both study sites have slowly transitioned from energy-limitation towards co-limitation over recent decades and in the driest recent years, they indeed switch to a clearly water-limited regime.

Importantly, the plot also shows increasing divergence between the hydrologic functioning of the sites with increased water limitation. This further underlines the importance of not only the climate but also its interactions with vegetation to the hydrologic balance, placing the processes our study examines in context. We **updated the discussion** to make use of this framing in explaining the significance of our findings (lines 415-519).

We agree with the reviewer that our study does not achieve representativeness at the landscape, let alone regional scale. We appreciate this comment as it gives us an opportunity to better clarify the significance of our work despite this lack of representativeness.

The landscape-scale implications of our work do not depend on the watershed's broad representativeness so much as its particular landscape position. Due to the region's geography, montane forests in headwater catchments represent the areas of high precipitation and low evapotranspiration. Through both locally higher inputs and intra-annual storage, forested montane headwater catchments play an outsize role in baseflow generation, supporting regional hydrological stability. The broader landscape's (i.e., downstream) water regimes will thus be particularly sensitive to their seasonal functioning under climate change.

Due to land use patterns, these montane forests also represent the majority of strictly protected areas (IUCN categories Ia, Ib, and II) in Central Europe, with a dominant proportion of our two tree species. This includes the twinned Bohemian/Bavarian Forest national parks directly adjacent to our sites. Understanding the ecohydrology of montane forests dominated by these species will be one of the keys to regional biodiversity and ecological conservation during the ongoing hydroclimatic shift.

In sum, understanding vegetation-mediated land-atmosphere feedbacks in montane, mid-slope beech and spruce forest such as these is particularly important to projections of future ecological and hydrological dynamics across the region.

In terms of direct generalisation, our study is most like a paired watershed study in hydrology or a common garden experiment in vegetation ecology. Our study design allows the key processes to be examined at the appropriate scale, without needing to represent the entire landscape. The resulting process understanding is always only transferable to an extent circumscribed by an adequate consideration of conditions in the study system. Scaling the effects of the processes we described to an entire landscape would require a separate exercise that would take into account the factors mentioned by the reviewer, but is beyond the scope of our study.

That said, some of our novel findings generalise directly. Our catchment lies close to the cold, humid end of the spectrum of Central European climate zones (e.g., unit C7 on the Quitta Climatic Classification, Vondrakova et al., 2013 <https://doi.org/10.1080/17445647.2013.800827>). Observation of an annual-scale flip to a water-limited regime here is not only entirely out of line with historical experience. It is also strongly indicative for large parts of the generally warmer, drier Central European landscape: if it has started to happen here, it will be happening at least episodically in most places.

We improved the discussion section to clarify both the limits on generalisation of our conclusions and their broader landscape significance despite these.

Comment#4

Another concern is on the use of a bucket model. Bucket models are usually used at coarse spatial scales where data are poor or inaccurate (regional to continental to global scales). The rich data set at plot scale in this study could support a Richards-based model which is more complex than the bucket model and provides a better performance in terms of model simulations.

Response#4

Solving the Richards equation for soil water flows was our choice in our previous study (Sipek et al., 2020) dealing with a 5y data set at the same site. We found that modelling the soil water regime this way at multiple depths continuously for 5 years introduces significant uncertainty. RMSE ranged from 99 to 176 cm for the column average pressure head (it was even larger when we assessed specific depths), parameters of the root water uptake function needed to be far from physically reasonable values, and parameters of the soil water retention curves also had to be adjusted from their measured values.

The reasons for the doubtful performance were namely:

- large amounts of rock fragments in the soil. If a certain percentage of the profile is formed by the rock fragments, then the vegetation will extract more water from the areas between those rock fragments to fulfil the water demand. This could result in a higher actual drop in observed pressure heads, which would not be represented in the model.
- the hydrophobicity of the soils may result in non-uniform drainage of water into deeper soil layers and formation of a shallow biomat flow. The percolation of water can then be limited only to certain locations (eventually bypassing the measurement probes).
- occurrence of preferential flow in the forested catchment can cause non-sequential reaction of soil moisture sensors at different depths
- soil hydraulic properties are described by the soil water retention curves, which strictly define properties of the porous media. However, if 21 years are modelled continuously, soil properties undergo gradual changes, which were not measured.

Hence, for this long-term study we have chosen simpler bucket type of the model as (1) it is sufficient to answer questions posed (soil column water balance) without adding more complexity, (2) it uses “Feddes” type of equation for the estimation of plant water use as Richards-based models, (3) it is more convenient for the simulation of longer periods, (4) the soil column is represented by one unified domain with column average soil hydraulic properties, which is beneficial especially when the soil encompasses a lot of rock fragments. We added the reason for the utilizations of the bucket model to lines 154-158.

Comment#5

Model calibration is poorly described. The authors used a local or global optimization tool? What's the objective function? The RMSE of what? Of pressure heads? Or else?

Then in the results, close to line 270 (please add continuous line numbers!), the authors mention about the model calibration against observed snow cover equivalents. In Line 274 the authors state that the calibration was done against observed soil water content that pop out of the blue. In M&Ms I do not see the description of soil water content sensors. I rather see only the installation of tensiometers. Did I miss anything?

Response#5

Thank you for the valuable comment. We clarified important information about model calibration in the section ‘2.4 Model parameterisation, validation, and forward simulation’ of the manuscript. The type of line numbering is pre-described by the journal template for manuscript submission.

The model parameters were fit using the **genetic algorithm using the RMSE as an objective function**. The model was calibrated in two steps (lines 210-213). First, parameters of the degree-day snow accumulation/melt model were calibrated using measured snow water equivalents. Second, the remaining model parameters were calibrated using a measured soil water regime represented by pressure heads. For soil water balance modelling, the measured pressure heads were used to calculate the volumetric soil water content by means of the van Genuchten (1980) function. The function parameters were retrieved from the measured retention curves specific for each site and depth (lines 220-223).

Comment#6

The authors force the simulated annual cumulative drainage to be close to 360 mm year⁻¹ because this value corresponds to the mean annual observed runoff. In this case, the study area should be described more in detail by adding hydrogeological information to support this hypothesis which is strong.

Response#6

Yes, this is a fundamental part of the modelling framework. It is based on the previous hydrogeological survey which documented crystalline bedrock in the catchment which only allows water circulation in the weathered zone and does not communicate with adjacent catchments. Therefore, the hydrological catchment corresponds well to the hydrogeological catchment (Hrkal et al., 2009) (lines 106-108) and the assumption, while strong, is also well supported. We made this clearer in the manuscript.

Comment#7

The M&Ms would benefit from the use of a schematic figure that presents the overall study (measurements, modeling calibration/validation, data analysis, etc.)

Response#7

Thank you for the comment, we added the required figure into the supplementary material (Fig S1).

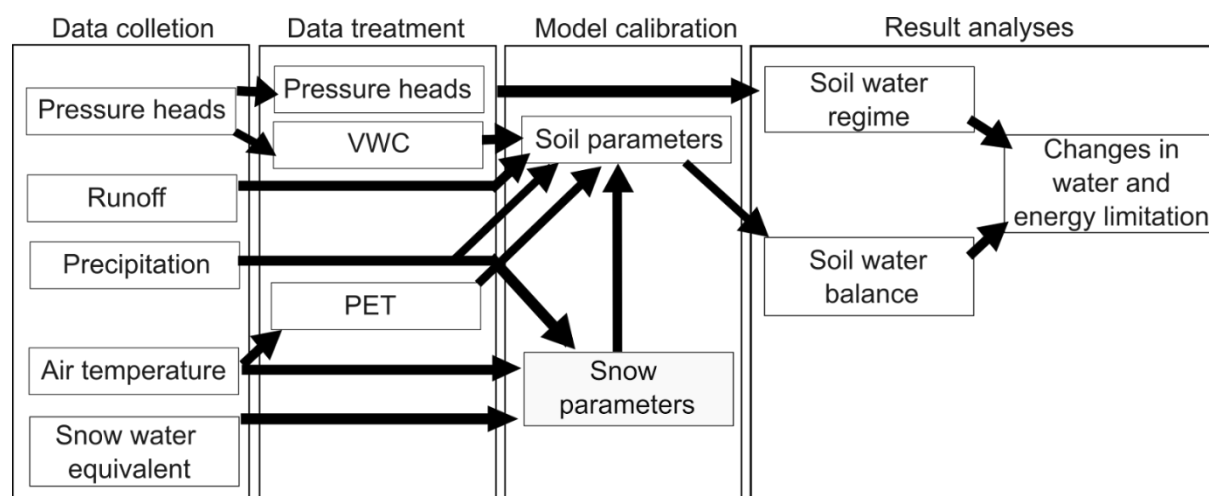


Fig. R11 Scheme representing the workflow of the study

References:

Šípek, V., Hnilica, J., Vlček, L., Hnilicová, S., and Tesař, M.: Influence of vegetation type and soil properties on soil water dynamics in the Šumava Mountains (Southern Bohemia), J. Hydrol., 582, 124285, doi:10.1016/j.jhydrol.2019.124285, 2020.