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

The authors evaluated the impact of climate forcings in the past 22 years (2000-2021) on the water balance components (evaporation, transpiration, drainage) in two experimental soil plots, one covered by Norway spruce, and the other one covered by European beech. The authors exploited a good data set of water potential data monitored by tensiometers installed at different soil depths. This wealth of data support a bucket model to simulate water balance.

The evaluation of this manuscript is based on the following questions:

1) Is it a novel work based on a reliable scientific technique?
2) Is it clearly structured and well-written?
3) Are the experimental design and analysis of data adequate and appropriate to the investigation?

The paper is well-written and well-structured. However, I would like to state that HESS is a high-ranked Journal and should receive novel, robust, scientifically-sound studies. My main concerns are:

1) The title mentions about 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.
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?

In the discussion session (line 395) the authors state that:

"This is the first study we know of to cover more than a few seasons and thus to provide a robust interannual comparison of soil moisture regime under beech and spruce canopies integrated over the entire soil column. Viewed at this scale, the comparison of soil moisture regimes proves to be precipitation dependent. Our results have some clear implications for how combined changes to climate and forest species composition in Central Europe should be expected to impact forest water regimes. Climate change is expected to shift the intra-annual distribution of precipitation totals from summer to winter. We found that higher winter drainage under the beech canopy is even more pronounced with increasing winter precipitation. In combination with the increasing representation of beech trees in Central Europe, this can generally lead to higher winter groundwater recharge and runoff. By contrast, summer season differences indicate lower future recharge from beech forests given their higher transpiration demand, which generally consumes soil water even during drought periods. Overall, our results suggest that ongoing trends in climate and forest composition are pushing these forest systems from energy-limited towards water-limited states, at least on a seasonal basis."

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 km^2) cannot be representative for the impact of climate and land use change in Central Europe. The waster 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.

3) 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.
4) 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? 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. 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?
5) The M&Ms would benefit from the use of a schematic figure that presents the overall study (measurements, modeling calibration/validation, data analysis, etc.)