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Interactive comment on "Controls on root zone storage capacity in boreal regions" *by* Tanja de Boer-Euser et al.

Anonymous Referee #3

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The study is on an important and interesting topic, as little is known about potential changes in catchment storage properties under climatic and land use change. Potential to improve our hydrological predictions under climatic and land use change is limited by lack of information and understanding, so studies in this area can be expected to be in demand by HESS audience. Another strength of this study is the dataset, which is very well described and referenced. The article is nicely structured, and the results are presented well (though I would prefer to see more numerical information to back up some claims).

My main concern about this study is about how much of the results originate from self-prediction given the high correlation between source and comparison data. Sr is derived based on climatic records, and then Sr is compared with climatic and vegeta-

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tion properties which are known to be related with the source data Sr was derived from. The authors do acknowledge the relationship (e.g. p 5 l 24-25, p 6, l 2-3, p 9 l 16-17), but they still interpret results in a way where (higher) correlation implies control over Sr, which I think is questionable. The results might reflect just the closer correlation with the source climatic data for Sr, and not causal relationship with the soil storage properties. For example, it remains unclear to what extent the relationship between Sr and vegetation properties/land use are just a consequence of both being related to the climate. In this case the change in vegetation would not influence Sr as it can be expected from the results, if the vegetation is the only thing changing. This would particularly apply to cases where the results are somewhat counterintuitive (e.g. p6 l26-27 and Fig 4a, or p7 l23-25 and Fig 7b where decrease in forested area is associated with increase in Sr).

In this light, I think it would be more informative and would give more confidence in the results to apply some method which can account for a number of potential "controls" and assess their importance against each other, for example PCA or multimodel inference (e.g. Saft et al, 2016).

I am also a bit puzzled about the gap between snowmelt and onset of PET, as both are governed by exactly the same increasing energy flux (temperature/sunshine). I would assume that this gap should be very closely related to the maximum SWE ($\sim$more snow takes longer to melt). Anyway, it would be interesting to calculate this gap (using some threshold for snowmelt) and include it directly as yet another factor along with the other characteristics used. I wonder why it was treated separately.

On a different note, it would be good to see more numerical information (i.e. Spearman's rho, and associate p value) associated with positive / negative correlations described in the text. It is difficult to extract relevant information from figure 8, especially since it is not numeric. Fig 8 also does not include correlation results for sub-regions which are mentioned in the text, and I could not find these results anywhere else.

Importance and implications:

What is the use of the derived Sr and discovered relationships with other characteristics? And in the context of climate change, would not it be easier to derive new Sr following the original method accounting for climate change in the source data instead of looking at the correlations?

Specific comments:

p2 l 8-10 – and vegetation WUE / transpiring properties

p2 l 17 If you talk about climate, do you mean balance between evaporation and precipitation? Transpiration is not purely climatic.

Section 2.1 – Just checking, is there any permafrost in northern catchments, and if so, can there be any impact (e.g. thawing permafrost -> higher storage)?

p4 l 1 – how it was calculated?

Formula 2 – why in the middle line Pi = 1? What does 1 mean?

Section 4 – Can the changes in Sr be related to changes in WUE (e.g. Troch et al 2009)?

p9 l 8-9 – Is it just direct numerical effect of having higher runoff from drained peatlands?

p9 l 11 – suggest changing 'many affects to' to 'many effects on'

p9 l 15-16 – I still struggle with the idea of how Sr calculated with pan evaporation would change if only vegetation properties change (as 'or' implies independency) – see my general comment in the beginning. In any case, the argument is based on the assumption of trading space for time (Wagener et al, 2007, Singh et al, 2011), and this and associated assumptions can be acknowledged better (possibly also in introduction).

References:

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