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

Tanja de Boer-Euser et al.

tanjaeuser@gmail.com

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Dear referee,
Thank you for reviewing our manuscript, your positive evaluation of the relevance and presentation of the results and the relevant questions you posed. The more detailed comments you have given made us realise that the aim we have in mind with the paper needs to be discussed better throughout the manuscript and that especially some elements of the used method need more attention in a revised version of the manuscript. Below we have replied in more detail to all your comments.

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

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Discussion paper

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. S_r is derived based on climatic records, and then S_r is compared with climatic and vegetation properties which are known to be related with the source data S_r 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 S_r, which I think is questionable. The results might reflect just the closer correlation with the source climatic data for S_r, and not causal relationship with the soil storage properties. For example, it remains unclear to what extent the relationship between S_r 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 S_r 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 S_r).

We agree with you that the variables used in the analysis are subject to internal correlations. As discussed in the reply to the review of Maik Renner, the exact variables used for the calculation of S_r are not the same as the ones used in the remainder of the analysis.

With respect to the influence of vegetation on S_r, this can be reflected by using a different drought return period (Wang-Erlandsson et al., 2016): different vegetation types probably have different survival strategies and therefore are likely to adjust to a different drought return period. However, for this study we only used a 20-year return period as the majority of the land cover consists of forest. So, if only the vegetation would change, a different return period can be used, which would influence the derived S_r. However, more testing would be required to see if changes can be assessed by using different return periods and how (quickly) new equilibriums would be established.

In addition to this, the catchments with a lower forest cover are generally the ones with a higher agricultural cover and a milder climate. These catchments are likely to have higher transpiration demands, leading to higher S_r values, than colder forested catchments.

Having said this, the term ‘control’ might be misleading, especially as our main aim with the study is to compare the calculated S_r values with a set of catchment characteristics to explore possible relations and better understand the derived S_r values and what influences their calculation. Your comments and those of the other reviewers made us realise that we did not discuss this aim consistently throughout the paper and in the revised version of the manuscript we will make this clearer.

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).

Thank you for this suggestion; we think as well that the paper would benefit from such further elaboration. Both multimodel inference (as used by Saft et al., 2016) and PCA are useful methods for this. In line with our methodology, we will work this out further using the PCA. However, we do believe that the multimodel inference is very interesting when evaluating the possibilities to use climate derived S_r values to assess the effects of change.

The results of a PCA are presented in Figure 1, containing the different variables we compared with S_r in the manuscript. As already expected, this analysis shows that the majority of the climate variables (shown in blue) are positively correlated and negatively correlated to the mean annual temperature and transpiration demand (which follows from the water balance). What can also be seen is the limited correlation between the majority of the climate variables and (summer) precipitation.

With respect to vegetation properties (shown in green), these are strongly correlated with forest and agricultural land covers, but limitedly correlated to the majority of the climate variables. Only peatland covers are positively correlated with the majority of the climate variables.

The plotted catchments (top plot) indicate that the eco-regions mainly differ in climate characteristics and that especially in the mid- and south boreal regions a large range of vegetation properties and land covers occur.

We will incorporate these results in a revised version of the manuscript.

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 (âĹijmore 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.

E_p and snowmelt are indeed governed by the same increasing energy flux. However, the gap exists because of the measurement methods: E_p is based on pan-evaporation and can thus only be measured if temperatures are above zero. Therefore, E_p can already be slightly above zero before the pan measurements start.

With respect to treating both variables separately, this was done because in the S_r calculations one determines the water demand, the other the water supply. The balance between these variables mainly determines the calculated S_r. So, although they are governed by the same energy flux, they have a different effect on the calculation. For the aim of the paper, we think this influence is interesting to investigate and explore.

Having more insight into the combined effect of the onset of E_p and snowmelt can help to assess what can happen in case of a changing climate. For the gap between these two to change, the most important variable will indeed probably be the maximum S_{SWE}.

The gap between snow-off and the start of E_p (measurements) is incorporated in the PCA presented in Figure 1. It can be seen this property is strongly positively correlated with the day and amount of maximum S_{SWE}.

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.

As discussed in the replies to the two other referees as well, we will include more numerical information about the correlations in the text, both for the entire data set as for the sub regions. We preferred not to include them in Figure 8, to prevent it from overflowing with information. However, for the revised manuscript we will see if there is a way to include some more numerical information in the figure as well.

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

As discussed before, our main of the paper is to explore the relations between the climate derived S_r and a set of catchment variables. We should keep in mind that S_r is a conceptual parameter, originally used as input for hydrological models. However, it is very interesting to know if and how the calculated S_r is related to other variables and if it can be wider applicable. Knowing more about the relations between S_r and catchment variables can help us to better understand the influences on the S_r calculations and therefore how we can, possibly, use it to assess the behaviour of catchment under changing conditions.

Thus, by looking at the relation between S_r and catchment variables, more confidence can be obtained in the meaning of S_r. The found relations are not directly meant to assist to assess change. To assess change indeed recalculating S_r based on new climate predictions would be the most logical approach.

We will make the division between better understanding S_r and using S_r for assessing effects of change clearer in the revised version of the manuscript.

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

The partitioning is indeed influenced by both water availability (in the root zone storage) and water use efficiency of the vegetation. However, with the vegetation surviving in a certain catchment, it must have had sufficient water supplies and at the same time it is not logical that it would have invested more carbon in creating storage capacity for water than it needed to survive. So, by deriving the transpiration demand from the water balance, the long term water use is estimated, making the water efficiency of the vegetation less relevant for the calculation. The balance between transpiration and runoff is of course influenced by the water use efficiency of plants.

We will acknowledge this aspect in the revised version of the manuscript, together with a more extensive description of the calculation method for S_r and the assumptions involved.

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

Actually we are talking about the difference between the long term average supply and demand of water to the active storage of the soil. These are mainly climatically driven (ie. via precipitation and potential evaporation). As processes like interception and snow melt are important as well, we decided to use the terms infiltration and transpiration (demand). Where infiltration is the total precipitation (rainfall and snow) minus interception evaporation and the transpiration (demand) is derived from the water balance ($\overline{T} = \overline{P} - \overline{E_i} - \overline{Q}$). We will clarify these terms in a revised version of the manuscript, mainly by extending the description of the method used to calculate S_r.

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

Thanks for asking this clarification, but in these sites there is no permafrost. If there would be, changes in permafrost should indeed be included in the calculation of S_r, just like snow storage is.

p4 l 1 – how it was calculated?

Data of all three biomass variables (root biomass, tree height and leaf cover) are based on field data from national forest inventories, satellite images, digital map data and other georeferenced data sets. More information can be found in Mäkisara et al. 2016 (http://jukuri.luke.fi/handle/10024/532147).

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

Thank you very much pointing this out: it should be Pi = 0. We will correct this in the revised version of the manuscript.

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

In this study we did not yet incorporate any changes in the catchments, so we suppose you mean the variations in S_r between the different catchments. The calculation of S_r is based on a daily simulation of soil moisture deficit and an extreme value distribution. The input into the simulation is effective precipitation and a transpiration demand which is estimated from the water balance. As the transpiration demand is the water that should have transpired to close the water balance, different water use efficiency probably will not really influence the derived S_r values. However, water use efficiency could influence the amount of biomass production (root biomass, leaf cover, tree height). Thank you for this suggestion; we will elaborate this in more detail in a revised version of the manuscript.

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

One of the assumptions underlying the climate derived root zone storage capacity is that a certain type of vegetation needs a specific amount of water to survive; independent of the climate they are located. If they are located in a drier or more seasonal climate, they will need a larger storage capacity to supply the required amount of water. This does neglect the fact that vegetation may have higher water use efficiency (Troch et al., 2009). As discussed earlier, this does not influence the calculation, as the used transpiration demand is derived from the water balance. By deriving the transpiration demand from the water balance, the runoff is already accounted for. However, differences in water use efficiency could help to explain the pattern found for the pine root biomass. We will elaborate this in more detail in a revised version of the manuscript.

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

We will change this in the revised version of the manuscript.

p9 l 15-16 – I still struggle with the idea of how S_r 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).

S_r is based on a daily simulation of soil moisture deficit and a drought return period the vegetation adapts to. Different vegetation types are likely to adjust to different return periods, as they have different survival strategies. In addition to that, E_p data is only used to add seasonality to the long term averaged transpiration demand, which is derived from the water balance (precipitation and runoff). So, a change in vegetation probably works in two ways: a different drought return period can be applicable and of course the balance between precipitation and runoff can change. In either way, a new equilibrium needs to be established.

In addition, the principle of trading space for time can be used as well, especially as we see shifting conditions in the study areas. However, to go into that direction in this paper would make it lose focus, but we will make the change in vegetation properties we had in mind clearer. Although we have discussed the change part in the paper, and we definitely think a thorough understanding of a climate derived S_r can help to assess hydrological change, our results do not discuss any of these elements.

Having said this, in a revised manuscript we will make clear that the aim of the paper is to better understand the climate derived S_r and its relation with certain catchment properties and mention the change as an outlook and an, in our opinion, important possible applicability.

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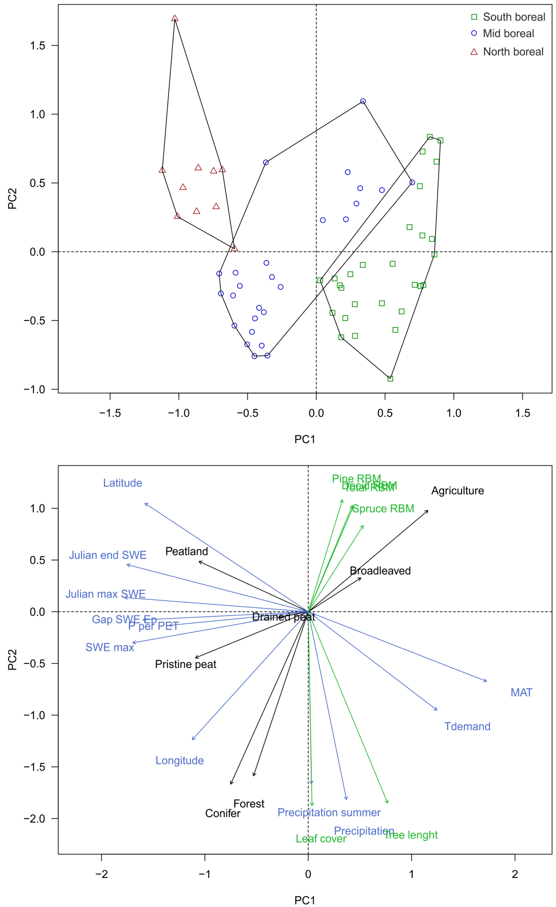

Fig. 1. Results of principal component analysis with variables compared to Sr. Top plot shows catchments in the three boreal regions