Overall comments:

The authors have come back with an improved manuscript that addresses many of the comments and concerns raised in the initial assessments. I am recommending publication but still believe that some changes are necessary. I am not sure that the authors have established “control” (i.e. page 1 line 5) versus “correlation”, especially given the intrinsic relationships and correlations between the variables used in calculating $S_{r,20}$ and the assessment. I do appreciate the analysis conducted and do not dismiss the results. I believe there may be merit in the hypothesis, which warrants future studies in different locations. I don’t believe it is necessary to come to a strong conclusion in this paper. What I would like to see is a more frank discussion of the caveats and limits to what has been presented, but with a view towards the possibilities associated with extensions of this work, which may indeed prove useful.

Specific Comments:

Figure 7: There appear to still be inconsistencies in the data presented. In the mid-boreal region there is a catchment with almost 60% drained peatland and $S_{r,20}$ of around 40 mm. However, while there are some points representing pristine peatlands with a similar $S_{r,20}$, there are no corresponding lower percentage points for forest cover and agricultural areas. I am speculating that that there are some zero points or points very close to zero that are not shown. Where are the corresponding fractions of forest and pristine peatlands for these points? There apper to be other such examples. The removal of zero or near-zero points (some or all?) might be biasing the interpretation of results. Am I reading the plots incorrectly?

Page 6 line 20-21: I suspect that the correlation between $S_{r,20}$ and leaf cover or tree length for the entire dataset is better than for any of the three regions. It appears that the forest structure follows a rough latitudinal gradient (the authors have noted climate effects in both directions in their previous response). Separating the data into regions defined by latitude makes the relationships harder to see because some of the main drivers associated with latitude are excluded. The figure does not need to be changed but this point could be added.

Page 6 lines 25-27: Figure 4 may be showing the 'preference' of spruce, pine and deciduous trees. Pines often locate on sandy or rapidly draining soil, which has a small S_r because the difference between field capacity and the wilting point is small. Pines do not grow well in wet soils so the largest root biomass is likely found where they grow well. Spruce trees do not grow as well in sandy dry areas so the largest root biomass is found in the areas with more moderate drainage. In the north where there are thick peat soils, these soils developed because of the persistent poor drainage and high water table (and slow decomposition). If the peat soils did not dry out during the 20-year drought periods, they will have small estimated $S_{r,20}$ values and since trees do not grow deep roots in waterlogged soils this creates an association with trees in low $S_{r,20}$ regions having a low root mass, except for pines which are in areas with a small $S_{r,20}$ for different reasons described above. I see this sort of discussion as helpful in understanding the multi-facetted relationship between the climate, soil, hydrology and vegetation, rather than implying undue control by vegetation.

Page 7 line 15-17: The words “leads to” implies causation which I am not sure exists. I suggest changing “leads to” to “is associated with” which is softer and less open to criticism. Some discussion about causal relationships and correlation is warranted in the paper overall. Tree growth is affected by seasonal and annual temperatures as is evaporation, rain/snow partitioning and the snow off date.

Page 7 line 23-24: I am not asking for these to be answered directly but the authors might consider them and add to the discussion only if relevant to this region and the results. What role do natural drainage (or lack hereof) caused by soil depth, soil texture or topography and the effect of temperature on evapotranspiration play in the $S_{r,20}$ values? Is $S_{r,20}$ greater in the south because evaporative demand is higher and forest cover smaller because the forests were cleared and/or planted on drained peatlands? Are forested peatlands classified as forests or peatlands? I have read that many of the drained peatlands were planted as forests. In which category are these included?

Page 8 line 11-19: I present this as an alternative to the threshold interpretation. For catchments with more forest, the snowpack is sheltered by the forest and the snow melts later but the air temperature is warmed by the dark canopy albedo. So the snow in the northern forest probably melts at warmer (weather station) air temperatures than in the south. I suspect that the evaporation pans are generally placed at weather stations located in the open, not under the shelter of forests, so they will experience the warmer air (relative to the cold sub-canopy air) and Ep will start while the snow is still on the ground under the canopy. Where there is less forest the more exposed snow melts faster and more in-line with increases in air temperature and solar radiation and is gone before Ep becomes significant. I am not certain whether the differences in forest cover are enough to be the main driver of this apparent relationship with Ep and $S_{r,20}$ but if it contributes it supports softening the conclusions.

Page 8 line 21: I believe correlation has been shown and the results are intriguing but I don’t believe causation has been successfully argued.

Page 9 lines 1 and 2: The sample size of catchments with small agricultural leaf cover appears too small to make inferences about the effect of varying agricultural leaf cover. However, I am wondering where the zero values are? Are there no catchments with zero or near-zero agricultural cover, zero or near zero pristine peatlands?

Page 9 lines 6-9: While the authors do acknowledge that the density of pine trees may be too low in these catchments to have much influence on transpiration and storage, I still feel that the preceding statement goes too far. The authors do not present any information about the percentage or proportion of leaf cover or tree cover that is represented by pine, spruce and deciduous species. Are the RBM values calculated on a catchment basis, such that the sum of root biomass for each species in each catchment is divided by the entire area of each catchment, or is the root biomass merely the average for each species within the areas that contained that species (i.e. within sub-areas of each catchment, the fractions of which we do not know)? If these RBM values are averaged over the area of the catchment, then I would interpret Figure 4a as showing that pine do not grow well in catchments that have a very high or very low $S_{r,20}$. A very low $S_{r,20}$ might indicate a lack of sufficient soil, or perpetually water-logged conditions, such that all trees grow poorly. At slightly greater but still small $S_{r,20}$ values, we may see the

sandy areas favoured by pines represented; they grow well but need a high root biomass to access enough water in the rapidly draining sandy soil. As $S_{r,20}$ continues to increase, we likely encounter conditions in which the soil is wet enough of the time that spruce and deciduous species outcompete pine.

Page 10 line 4-7: Yes, the boreal ecosystem has been referred to as a "green desert". (Hall, 1999, https://doi.org/10.1029/1999JD901026; Betts et al. 2001, 2001 JD900047). There is ample water on the surface but either because of nutrient limitations or adaptation to cool environments, the vegetation is less productive and evaporation rates are generally low.

Page 10 line 16-18: The authors should expand on this idea of a climate-derived $S_{r,20}$ being useful to assess the hydrological effect of future changes in climate and land cover. This point could provide justification for this work. Can the authors discuss what would be required for this to happen? Additional studies need to be conducted to assess the usefulness of a climate-derived $S_{r,20}$ and its applicability in different locations. Then, this could be evaluated in models of sufficient complexity, and if the patterns are similar, in climate change scenarios. At this stage, stating that this method is useful is conjecture, but pointing to work that would serve to test this would set this paper in a better context.

Corrections:

Page 1 line 3: Change "enables to account" to "enables one to account".

Page 1 line 11: Change "besides from" to "apart from".

Page 1 line 20: change "alter magnitude" to "alter the magnitude".

Page 1 line 23: Change "in near future" to "in the near future".

Page 1 line 24: Awkward sentence. Change "The occurring land use changes consist of" to "These land use changes consist of".

Page 2 line 2: Change "as source for biomass" to "as a source of biomass".

Page 2 line 5: I am not sure what the authors mean by "measures".

Page 2 line 21: Change "boreal catch" to "boreal catchments".

Page 3 line 2: Change "Characteristics study catchments" to "Characteristics of study catchments".

Page 3: There are a number of very short paragraphs, some of which could be combined.

Page 3 line 5: Change "belong to National network" to "belong to a national network".

Page 3 line 13 and 14: Change “sites” to “catchments”. Unless a specific field measurement location is referred to, the use of 'catchments' is preferred over 'sites' because 'sites' suggests specific locations in space, and I believe all of the data are presented at the catchment scale.

Page 3 line 22: Change “additional data were used about leaf cover” to “additional data were used, including leaf cover”.

Page 4 line 1: Change “data was available” to “data were available”.

Page 4 line 2: Change “from Finnish” to “from the Finnish”.

Page 4 line 6: Change “data was adjusted” to “data were adjusted”.

Page 5 line 6: Change “sevaral” to “several”.

Page 10 line 11: Change “affects to” to “effects on”.

Figure 6a: The use of the term Julian day is not correct. It should be day of year. The authors indicated that this would be corrected but have not done so.