

Interactive comment on “Effects of shrub cover increase on the near surface atmosphere in northern Fennoscandia” by Johanne H. Rydsaa et al.

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Firstly, we would like to express our sincere gratitude towards the two anonymous referees for taking the time to review our study, and for the constructive suggestions and comments to improve the manuscript. See point-by-point replies to the comment below (some technical comments are grouped together), answers are given below each cited comment.

“This paper extends prior research into the potential climatic effects of a hypothetical increase in shrub and tree cover (in this case, shrub and tree height) in a region in northern Fennoscandia. My overall impression is that this is a solid paper that provides some interesting new results on the topic of vegetation feedbacks onto climate in the

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Arctic region. This work does not represent a giant leap forward, but the study is sound and, in my opinion, the paper is worthy of publication.

The paper can be divided into two main parts. The first part focuses on the potential changes in vegetation distribution under a 1K temperature change. The second part focuses on an assessment of the impact of such a shift in vegetation on temperature. From my perspective the first part is fine. One could quibble with aspects of the method and argue whether or not the projected changes in vegetation distribution are completely realistic or not, but I don't feel that the realism is really the point. The main goal is to generate vegetation distribution changes that are at least quasi-realistic and that can then be applied in the subsequent vegetation change experiment. Perhaps the only recommendation that I would have here is for the authors to be a bit more explicit about this with a statement to the effect that predicting vegetation change based primarily on a climatic envelope should just be treated as a first-order assessment of potential vegetation distribution change. “

Firstly, we appreciate the reviewer's positive comments. Regarding the use of the bioclimatic envelopes in this study, the reviewer's assessment is correct in that the aim of the re-distribution of vegetation applied here is mainly aimed at providing a pseudo-realistic experiment for investigating the atmospheric feedback mechanisms, rather than providing an accurate and realistic map of present/future vegetation. We agree with the reviewer's suggestion of highlighting this and communicating this aim more explicitly in the manuscript (also see our response to comments from Reviewer #1).

“For the second part, my main recommendation to the authors is that they work to put this study into better context. Prior studies, cited in the paper have looked at the impacts of shrub and tree area expansion in models and concluded, mainly, that these vegetation changes can lead to warming. So, the authors need to clearly establish what is new from this study. I see two main areas where this is new. The first is that this is being done within a regional climate model, which allows a more detailed assessment

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of the response. The second new result relates to the variability in the impact across high and low snowfall years and warm and cool summers. The authors should strive to emphasize these points.”

We will follow the reviewer’s constructive suggestion and better emphasize these aspects of the study in the revised manuscript.

With regard to the variability of the impacts across seasons, part of our goal has been to investigate the sensitivity of the atmospheric response to varying conditions represented here by choosing two contrasting years with respect to temperature and snow cover (as further explained in the answer to point 1 from Reviewer #1). In line with this comment, and with similar comments from Reviewer #1, this aspect of the study will be emphasized and presented more clearly in the revised manuscript.

Minor points: “1. For the summer feedback, the authors note that the impact of shrubs on summer temperatures is less sensitive to the mean summer temperature (warm or cold summers) than spring temps are to high versus low snow years. That is not surprising. Nonetheless, it would be good to explain why one would think that the summer temperature sensitivity could be related to mean summer temps. “

This is a good point, and we see that this side of the expected feedbacks has not been properly introduced. This will be amended in the revised manuscript, and theory and previous research on this more clearly presented.

“2. p. 12, line 26. 0.16K versus 0.15K is essentially the same. Shouldn’t say that one season has a slightly larger response when they are effectively identical. “

We acknowledge that the sentence is placing too much emphasis on the second decimal number here, and will adjust this statement in accordance with the reviewer’s comment.

“3. Figure 11 and other figures. It would be clearer to be specific that you are talking about warm summer seasons and cool summer seasons. Just writing cold seasons

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and warm seasons can lead to ambiguity about whether referring to different seasons (spring versus summer, for example).”

This will be amended in the revised manuscript, and figures will be adjusted where needed for clarity.

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