Final author response to anonymous referee #2

Bg-2014-478: "Long term effects on regional European boreal climate due to structural vegetation changes" by J. H. Rydsaa, F. Stordal, and L. M. Tallaksen

The referee comments are answered in the following way:

I. the referee comment in italics
II. author response comments
III. reference to corresponding changes in the manuscript.

General comments:

Comment 1

Due to the length of the comment, we have taken the liberty to divide it into smaller sections and for clarity answer each section separately.

I. "*The manuscript aims to use the state-of-the-art Weather Research and Forecasting numerical meteorological model investigate the impact on regional scale surface variables and mesoscale circulation due to land cover change alone. The experiment is conducted over a sufficiently long enough period to capture considerable inter-annual variation in meteorological drivers. The subject matter is interesting and relevant to a range of researchers, and correctly identified that mesoscale effects have not to date been full addressed, comparatively to global scale or local experiments. Despite the relevance of the work conducted here I have concerns regarding the model setup used. There are a number of details which need to be explained first".*

II. We greatly appreciate the referee taking the time to read and comment on our manuscript, and the positive response regarding the subject of this study. Regarding the model setup used, we have aimed to use the model in a way that is specific enough to meet the requirements of the study, yet general enough for results to be of interest for other users in the community. Also, considerations have had to be made with regard to the cost of the simulations required. However, we realize that a number of these considerations have not been properly addressed in the manuscript, and expand the manuscript accordingly.

I. "*The authors choose to use the default Noah land surface model (LSM) as opposed to the more advanced Noah-MP or Simple SIB models which are also available (and noted in the discussion). The Noah model has a number of, in my view, important disadvantages for this study. The most critical is that LAI in Noah is determine through an interpolation between min and max LAI parameters based on a greenness index provided to WRF as inputs from its geogrid files and it therefore insensitive to changes in meteorological drivers which would impact LAI through changes in terrestrial carbon cycle. Moreover I'm unsure how realistic it would be to use new PFT applied to the existing vegetation greenness?*"

II. We acknowledge that the Noah LSM is not the most advanced LSM available and does have weaknesses in its representation of the land surface. This could yield less realistic results for land-atmosphere interactions than more advanced models. The LAI in the NOAH land surface model is as the referee points out, insensitive to meteorological factors, which does lead to less detailed modeling of land surface behavior. However, as the aim of our study is to test effects of changes in certain surface properties related to simplified vegetation perturbations as isolated as possible, it is our opinion that this much used and tested LSM does represent the land surface well enough for the purpose of this study. We do however admit that we may not have justified the choice of model thoroughly enough in our manuscript and will give a more thorough explanation for our choice. The choice of model had to balance between satisfyingly representing important surface properties, while also yielding easily interpretable results and one way responses.

I. *"I also wonder how appropriate it is to use a model which is insensitive to the feedbacks it may drive in mesoscale circulation (i.e. no carbon cycle is included). An alternative would be to use Noah-MP which includes a carbon cycle and has the option of allowing the LSM to dynamically respond to meteorology. The authors could also then considered the magnitude of these feedbacks by running both with dynamic LAI switched on and off."*

II. The inclusion of a carbon cycle in these experiments would certainly be interesting, and will be considered for future work. However, in this study, the feedback loops would give results that would be more complex and add complexity that we wanted to avoid in this study. It would also, as the referee points out in the next comment, yield a setup more sensitive to representation of the atmosphere, and require forcing representative of some future climate scenario. As a first step, it was in our opinion more informative to run a model with a more simple response vegetation perturbations, and as a next step, we increase complexity and include more dynamical factors. As a follow-up study to this, we would like to increase spatial and temporal resolution, and aim at more specific response mechanisms and if possible also include a more complex surface parameterization, possibly with different climate forcing.

I. "*Regardless I have number of issues with the approach taken, if the objective is to know the response of the land surface in isolation then this is difficult. Under current climate we don't expect to find these ecosystem so far north therefore their impact will be difficult to interpret as the response of the simulated land surface here will not respond to climate as the real forest would, due to the lack of a C-cycle. Similarly the response of an LSM with a C-cyle may not be informative as to the response under dlimate change as the vegetation is being exposed to current and may respond differently. I see this as a difficult question to address in comparison to say land cover change experiments when both PFTs already exist within the same climate envelope (e.g. afforestation experiments).*"

II. We appreciate that the referee acknowledges our dilemma when it comes to balancing accuracy with respect to vegetation and climate response, versus simplicity in experimental setup. Modeling a realistic atmospheric response to theoretical future vegetation perturbations does represent several uncertainties, as the referee points out. There are many ways of handling this, e.g. by either increasing model complexity to take into account as many influencing factors as possible (i.e. dynamical vegetation, climate scenarios, chemistry and dust/fire modeling), or by reducing complexity and dividing response mechanisms into smaller, more isolated effects which can then be better understood, such as the aim is in this study. Although the climatic response as such is not complete, it is our opinion that such simplified experiments may still contribute to increase understanding of likely atmospheric response mechanisms to future vegetation changes, when interpreted with caution.

I. "*If the intention is to consider structural impacts explicitly then a more logical approach (to me at least) would be to frame the research question as an impact /*

sensitivity analysis of changing parameters / characteristics. If this is the case then I think that the simulations conducted are sufficient but need to be discussed in the context of the structural changes rather than forest expansion specifically you have not simulated a forest that can respond dynamically to meteorology."

II. This is our intention precisely, and we understand that we have not been able to communicate this intention well enough in the manuscript. We realize that our linking of the changing parameters to certain vegetation migration patterns may have given the impression that we felt these changes were sufficient to represent a full dynamical forest migration. However this is not the case. Our intention is to provoke an atmospheric response to certain physical surface changes related to specific vegetation changes in the boreal domain. This is done through theoretical perturbations of vegetation patterns, and is as such, as the referee points out, a sensitivity study and will be framed more clearly as this.

III. P. 15507, line 1: The title is changed in accordance with the manuscript changes: Altered:" Sensitivity of regional European boreal climate to changes in surface properties resulting from structural vegetation perturbations."

P. 15512, line 11-19: Rewritten; "This study is a sensitivity study in which we investigate the effects of altered land cover properties on fine resolution surface fluxes and regional scale atmospheric response mechanisms by conducting two experiments with manually altered vegetation distribution. These experiments represent two potential future boreal and arctic ecosystem changes, and are compared to a control present day state of vegetation distribution. An invariant vegetation distribution in each simulation aims to isolate effects of changes in land surface properties related to structural vegetation changes on the regional climate. Meteorological forcing data in both simulations matches that of the control run to further isolate the long term effects of vegetation changes on the overlying atmosphere."

The abstract and the rest of the manuscript is further adjusted throughout to more precisely communicate the focus of the manuscript and reframe the research question more clearly towards a sensitivity study.

I. "*If the impact of the PFTs themselves is the targeted objective then offline runs of the LSM may be useful to show the coupled and uncoupled impacts or running WRF with boundary conditions provided by one or more climate change scenarios. I'm quite willing to accept that I may have misunderstood the authors objectives and methods.*

Much of my above comment may be irrelevant depending on what further details can be provided as the model arrangement. I think there is a lot of detail on how the land surface is parameterised and driven which is not specified (i.e. how LAI is derived in this experiment).”

II. We realize that our objective should be clarified and framed somewhat differently as mentioned in the previous point. Also, more details of the model parameterization and forcing will be provided as part of the methods section.

III. See previous general comment and specific comment number 11 for adjustments in the manuscript.

Specific comments:

Comment 1

I. “*The introduction contains insufficient review of previous works which looked at land cover change and their impact of surface meteorology. I realize that many of these studies have a focus on the terrestrial carbon cycle and land use change but many also consider impacts on surface meteorology e.g. Betts et al., 2007, Arora and Montenegro 2011. The introduction also suffers from some awkward sentences”.*

II. The introduction will be revised and the suggested references are added. In addition, other citations to studies related to atmospheric response to vegetation changes are included in the revised introduction.

III. Page 15512, line 10: “Snyder and Liess (2014) applied a one grid cell northward shift in boreal vegetation in a global climate model, to explore the response of the overlying atmosphere to a vegetation shift expected to occur within a century. Results of this shift show an annual warming of 0.3 ◦C mainly due to decreased surface albedo.”

Page 15512, line 15: “For instance, by investigating the climate benefits of afforestation mitigation strategies, Arora and Montenegro (2011) found that in high latitudes, the warming effect of decreased surface albedo related to increased forest cover, dominated the cooling effect of increased carbon sequestration, supporting similar findings of Betts et al. (2007).”

P. 15509, L. 26: Jeong et al., (2011,2014) citations are added.

P. 15510, L. 5: Revised; “Bhatt et al. (2010), link increased high latitude ecosystem productivity to a decrease in near-coastal sea ice and summer tundra surface temperatures, supporting the findings of Jeong et al. (2014), who concludes that vegetation-atmosphere-sea ice interaction gives rise to additional positive feedback of the Arctic amplification based on a series of coupled vegetation-climate model simulations under 2xCO2 environment."

See below comments number 3 and 4 for revision of awkward sentences.

Comment 2

I. *"Page 15508, line 8: The WRF model number is given but this should be included in the methods as well."*

II. This is a good point, and the model number is added.

III. P. 15512, line 21: Added;" WRF V 3.5.1"

Comment 3

I. "*Page 15511, lines 14 – 17. This is poorly written, in the context of the statement the author, I assume, means that complex canopy structures led to a reduction in albedo and an increase in net radiation (?), rather than "...greater affected radiation erms...". As for being closely linked to sensible heat, they are coupled but so is latent heat being that both are driven by net radiation."*

II. As the referee points out, the sentence is unclear and is rewritten.

III. Page 15511, lines 14 – 17: Rewritten: "Thompson et al. (2004) found that increasing canopy complexity greatly affect the surface radiation fluxes, and is closely linked to both the latent- and the sensible heat flux. They conclude that the heating associated with more complex canopies may influence regional feedback processes by increasing boundary layer height through increased sensible heat flux."

Comment 4

I. "*Page 15511, lines 21-24. Nothing wrong with the statement but it is incomplete as both sensible and latent heat fluxes are turbulent fluxes and as the author correctly states the partitioning between sensible and latent heat will have an impact on boundary layer processes (although I realize how significantly the impact is poorly defined). But why is the impact on 'the overlaying atmosphere' instead of higher LAI impacting soil moisture?"*

II. We acknowledge that the sentence was confusing, and have rewritten it for clarity.

III. Page 15511, lines 21-24.: "They also argue that shifting the vegetation type towards one with higher leaf area index, may cause a shift in the dependence of the latent heat flux on the state of the overlying atmosphere and mesoscale weather systems, through the link between transpiration and the vapor pressure deficit in the surrounding air. As the correlation between latent heat flux and soil moisture was found to be low at all sites, shifting vegetation type was found to have less impact on the dependence on soil water content. "

Comment 5

I. *"Page 15512-15513, Description of WRF and Noah models, as alluded to in the general comments sections should be extended"*
II. As suggested, the model description is extended, with special emphasis on the land surface model.
III. Page 15512: changed/added; "WRF has been run with the Noah land surface model (Tewari et al., 2004), which is a well tested model widely used in the modelling community. In Noah, the ground surface consists of four soil layers, that are 10, 30, 60 and 100 cm thick respectively, adding up to a total soil depth of 2 m. The top layer is a combined vegetation, snow and soil layer, and surface properties are dependent on soil- and vegetation category. Each vegetation category is assigned range values for parameters related to the vegetation influence on land atmosphere interaction, such as the albedo, roughness length, stomatal resistance and leaf area index (LAI). The vegetation properties are further dependent on the greenness vegetation fraction, describing the vegetation density in each grid cell. The LSM controls the surface and soil water budget and computes surface water- and energy fluxes from the surface to the atmosphere. The turbulent fluxes are dependent on vegetation properties such as the stomatal resistance and LAI, and the surface roughness length in addition to the wind speed."

Comment 6

I. *Page 15513, lines 5-13. This information is probably better placed in a table.*
II. The suggested information is moved to Table 1.
III. Table 1 added to the revised manuscript.

Comment 7

I. *Page 15514, lines 5-16. Would be good if you provided area or proportional cover estimates to place the cover changes in context.*
II. This is a good point, the manuscript is changed as suggested.

III. Page 15514, line 16: Added; "This adjustment affects in total 12 grid cells representing 8748 km^2."

Comment 8

I. *Page 15515, lines 9-10. How short of time span?*

II. It is our understanding that many such models update the vegetation cover as an immediate response to prescribed meteorological thresholds, or at some set frequency during the simulation (i.e. once a year), forcing the vegetation to follow their climate envelope with no delay time to migrate into new areas all through the length of the simulation period. This approach serves the purpose of assigning potential vegetation as response to new climate, however is has been pointed out that it might yield too fast response compared to observed migration rates, which we would like to point out in the manuscript. In order to clarify, we added the following to the manuscript.

III. Page 15515 , line 11: Added; "Many of these models simulate vegetation response to climate change based on accumulated values such as growing degrees days and precipitation values, forcing the vegetation to keep within their respective climate envelopes. In cases of rapid climatic changes, the vegetation may therefore move unrealistically fast into new areas, often within the timespan of a century."
The section is also moved to introduction in the revised manuscript.

Comment 9

I. *Page 15517, lines 11-26. Some of this could be placed in the methods or is a repetition of the introduction and is not results.*

II. The section is revised, and parts are moved and deleted in accordance with the referee's suggestions.

III. Page 15517, lines 11-13: Moved to Methods section. "The only difference between the simulations is the described changes in vegetation distribution and resulting feedback effects of these, as simulations are in all other aspects identical."
Page 15517, lines 14-17: Deleted, as it is repetition of introduction.

Comment 10

I. *“Page 15518, lines 10-15. How is the increase in LAI achieved? This comes back to exactly how was the model setup. Is this purely to do with the max/min LAI parameters or has the ’greenness’ index been altered etc?”*

II. In our experiments only the vegetation type (the pft) has been altered, and as our aim is to investigate sensitivity to vegetation type-related parameters rather than vegetation density, the greenness fraction is left unaltered. This is to avoid speculations related to the fractional cover in the applied vegetation shifts. The response in LAI is therefore as the referee points out only a result of the scaling between the vegetation categories specific maximum and minimum values. This is specified in the revised manuscript, and reference to model parameter table added for clarity. Furthermore, possible impact of the chosen method is included in the discussion.

III. Page 15518, lines 3-5: Rewritten; “The applied vegetation changes lead to alterations in key surface parameters such as surface albedo and leaf area index (LAI), corresponding to their new minimum and maximum values assigned to each vegetation category in the model parameterization (see Table 1)”
See also technical comment 11 for manuscript alterations to the discussion.

Comment 11

I. *“Page 15525, lines 11-19. Again this relates to how was the model setup. Assuming that greenness index is used to provide the LAI estimates how have you dealt with differences in seasonality for the index? You will have applied evergreen PFTs to locations for which the index may have been deciduous.”*

II. As the referee correctly assumes, the greenness fraction is used to scale the LAI between each vegetation category’s respective minimum and maximum values in this study and as the referee points out, this is not well enough explained in the manuscript. We have revised the manuscript to include a more thorough explanation for the chosen setup (see previous comment). Finally, possible impacts of these choices are added in the discussion.

III. Page 15527, line 17: Added: “Also, the specifics of vegetation perturbations made might influence these results. Here, we have chosen to only perturb the vegetation type in each area. The greenness fraction is not altered, which might influence the evapotranspiration and thereby available energy for sensible heat, as demonstrated by Hong et al. (2009). However, we considered this approach sufficient, as the greenness fraction only acts to scale the LAI (and other vegetation parameters) values within the vegetation specific range (as indicated in the plotted results for the variable). Applying the new vegetation category to new areas would not imply a scaling of the LAI to values outside the respective categories assigned range. Further investigation of the sensitivity of these parameters is beyond the scope of this study, but certainly important subjects for further work."

Comment 12

I. *"Page 15526, line 16. So why did you not use Noah-MP?"*

II. As mentioned in the general comments, although we agree with the referee that there are several good options with respect to the model chosen, and there are of course advantages with using a more sophisticated LSM. Although it is beyond the scope of this study, we agree that using Noah-MP would be an interesting choice for a more specific and in-depth further investigation of several of the aspects of vegetation –atmosphere interactions touched upon in this study. However, we considered the Noah model sufficient in order to provide answers for the research questions in this study, and advantageous with respect to simulation cost and simplicity of model setup. A more clear explanation of the choice of model is added to the methods section.

References

Arora, V. K., and Montenegro, A.: Small temperature benefits provided by realistic afforestation efforts, Nat Geosci, 4, 514-518, Doi 10.1038/Ngeo1182, 2011.

Betts, R. A., Falloon, P. D., Goldewijk, K. K., and Ramankutty, N.: Biogeophysical effects of land use on climate: Model simulations of radiative forcing and large-scale temperature change, Agr Forest Meteorol, 142, 216-233, DOI 10.1016/j.agrformet.2006.08.021, 2007.

Bhatt, U. S., Walker, D. A., Raynolds, M. K., Comiso, J. C., Epstein, H. E., Jia, G., Gens, R., Pinzon, J. E., Tucker, C. J., Tweedie, C. E., and Webber, P. J.: Circumpolar Arctic Tundra Vegetation Change Is Linked to Sea Ice Decline, Earth Interact, 14, 8, 2010.

Hong, S. B., Lakshmi, V., Small, E. E., Chen, F., Tewari, M., and Manning, K. W.: Effects of vegetation and soil moisture on the simulated land surface processes from the coupled WRF/Noah model, Journal of Geophysical Research-Atmospheres, 114, Artn D18118
Doi 10.1029/2008jd011249, 2009.

Jeong, J. H., Kug, J. S., Linderholm, H. W., Chen, D. L., Kim, B. M., and Jun, S. Y.: Intensified Arctic warming under greenhouse warming by vegetation-atmosphere-sea ice interaction, Environ Res Lett, 9, Artn 094007
Doi 10.1088/1748-9326/9/9/094007, 2014.

Tewari, M., Chen, F., Wang, W., Dudhia, J., LeMone, M. A., Mitchell, K., Ek, M., Gayno, G., Wegiel, J., and Cuenca, R. H.: Implementation and verification of the unified NOAH land surface model in the WRF model, 20th conference on weather analysis and forecasting/16th conference on numerical weather prediction, 2004.

Thompson, C., Beringer, J., Chapin, F. S., and McGuire, A. D.: Structural complexity and land-surface energy exchange along a gradient from arctic tundra to boreal forest, J Veg Sci, 15, 397-406, DOI 10.1111/j.1654-1103.2004.tb02277.x, 2004.