Interactive comment on “Biogeophysical impacts of peatland forestation on regional climate changes in Finland” by Y. Gao et al.

We deeply appreciate all the reviewers for their constructive comments in improving the scientific quality of this manuscript. Our point-by-point response to all the reviewers' comments are listed below, and corresponding modifications are also made for the manuscript. We hope our reply will satisfy the expectations from reviewers.

Response to Anonymous Referee #1

General comments:

I commend the authors for a timely study about effects of human actions on the climate system. The paper describes how the representation of vegetation in the calculation cells of the REMO model have been improved with the aid of the data of the Finnish National Forest Inventory (FNFI). This allows for estimating the effects of peatland drainage (that allows tree growth, that is, forestation) by using results of two inventories, between which a substantial change has occurred. My expertise is forest modeling, I am not able to judge the details of application of the REMO model.

The results are derived from two 18 year long simulations with REMO that use vegetation cover data from two FNFI measurements. The main finding is that peatland forestation results in strong spring warming that is highly heterogeneous spatially and temporally. There are also effects on albedo, precipitation and net surface radiation throughout the year.

The results compare favorably to some observations. They are presented and discussed somewhat from the point of view of their sensitivity to input data and parameter values. However, the paper would be even better if a more comprehensive sensitivity analysis had been made by additional model runs. For example:

- The paper discusses uncertainties in background albedo values (l. 527- 558)

- Local effects of peatland forestation areas on maximum net surface solar difference (l. 503-525)

- Uncertainties in translating FNFI cover information to a compatible form with REMO (l. 220-224)

Authors response (AR): Systematically changing surface parameters, such as background albedo, may help to test the robustness of simulation results. However, it requires heavy computing to do this kind of sensitivity test with a regional climate model, which makes it not really realistic. Instead, we will add figures showing correlations between changes in climate variables and changes in land surface parameters, which is helpful in understanding the effects of land surface parameters on climate changes.

- The uncertainties in background albedo values (L. 527-558)
We will add the climate impacts of uncertainties in background albedo in the discussion part of the manuscript. The uncertainties of background albedo values do not influence much on the surface albedo during snow-cover period because snow cover leads to a much higher increase of surface albedo.

- The local effects of peatland forestation area on maximum difference of net surface solar radiation (L. 503-525).
Our reasoning is as follows: The maximum difference in net surface solar radiation is caused by the advanced snow clearance day due to peatland forestation, when the differences of surface albedo are biggest between snow covered peatland and non-snow covered forest. This means that the maximum difference of surface albedo is mostly dependent on snow albedo. As snow albedo has a negatively linear correlation to forest ratio (Fig. 4 in the original manuscript), the maximum difference in net surface solar radiation could be roughly estimated according to the difference of forest ratio. This part will be added in discussion.

- The uncertainties in translating FNFI land cover information to a compatible form with REMO (L.220-224).
We cannot use REMO with too low resolution, e.g. 100 km, for this study because it will make us lose too much information about the dynamics of the local effects of land cover changes on climate. We translated the ten FNFI land cover classes to the standard GLCCD land cover classes through comparing the definitions of land cover classes and allocating appropriate surface parameter values. We agree that it would be good to use a set of land surface parameter values produced for Finnish conditions, but it would require complete and consistent data on each parameter. Unfortunately, at this moment it is beyond our ability.

Runs with systematically changed input data/parameter values would give a better understanding of the relative importance of different factors to the results. The results of simulations are discussed in terms of peatland forestation. However, the two FNFI measurements that are 80 years apart record also many other changes of forest cover apart of peatland drainage. I would like too see a discussion what other factors (e.g. stocking) may have affected the simulations.

AR: Yes, climate effects are also shown in summertime in the southeast of Finland where mixed forest decreased and coniferous forest increased. This will be discussed with the spatial correlations required in the reply for general comment 3 from reviewer #3. Our simulations are performed with two static land cover states, and not coupled with dynamic vegetation model. So, we do not have stocking changes of the same type of forest.

The paper is well written. I have marked to the MS (Supplement) some passages that could be improved as well some other small comments.

Specific comments (in supplement: http://www.biogeosciences-discuss.net/11/C4600/2014/bgd-11-C4600-2014-supplement.pdf):

(1) Line 79-80: Is this for peatlands or in general?

AR: This refers to the averaged temperature changes over southern and northern Finland in general. This part will be modified to make it more clear to readers.

(2) Line 134 : This is unclear: do you consider the change of vegetation (e. g. forest growth) during the 18-year simulation? If this the case the growth factor should be explained in a detailed manner.

AR: The growth factor in REMO land surface scheme only describes the intra-annual cycles. Our simulations are static modeling based on two land cover maps. The definition of growth factor in the manuscript will be modified to be for clarity: the factor determines the seasonal growth characteristics of vegetation.

(3) Line 152: This is a bit unclear: later on Line 170-174 you explain that CORINE land cover map is used.

AR: GLCCD is the default land cover map to represent present land cover surface in REMO as mentioned in the manuscript. The subgrid-scale heterogeneity resolution of the improved hydrology scheme of REMO was set based on the standard land cover map (Hagemann and Gates, 2003). That work is independent to implementation of CORINE land cover map in REMO (Gao et al., 2014).

(4) Line 170: Earlier you say that there are 9.7 Mha peatlands.

AR: 9.7 million ha was the total peatland area of Finland in 1950s in Ilvessalo (1956). 22377 km^2 (7.4%) is the area of naturally treeless or sparsely treed peatland in the 10th FNFI (2000s). They are different.

(5) Line 178-179: So you mean in this paragraph that the spatial resolution (or the units are) is the same as in CLC but contents have been taken from FNFI? Maybe this paragraph is a bit difficult to follow.

AR: FNFI maps are in 3km resolution, where as CLC map is in 1km resolution. In the earlier study (Gao et al., 2014), CLC map is used instead of the standard GLCCD map to represent present land cover conditions for our model domain. In this study, we used both historic (1st) and present (10th) FNFI maps to describe the land cover changes in Finland, for consistency in land cover classification and spatial resolution. Therefore, CLC is substituted by FNFI10 to represent present land cover situation. All the land surface parameters allocated according to land cover maps are aggregated to 18 km resolution in REMO simulation.

(6) Line 200-223: This paragraph is difficult to follow. I suggest presenting the information (percentages) as table.

AR: This part of information has been presented as table 1. An introduction sentence will be added for Table 1 in the revised manuscript.

(7) Line 223-226: I do not understand what you are trying to say here.

AR: We are trying to explain the uncertainties of land cover changes in the selected subregions. To make it more clear, we will modify the original text as follows.

" One should notice that some uncertainties may arise from sampling in the FNFI1 and FNFI10 data. This applies especially for FNFI1, where the distance between inventory lines was as high as 26 km. Therefore, subregions that are smaller than 100 km × 100 km may not be sufficient to represent the actual land cover changes spatially. However, the dynamics of the local effects of land cover changes on climate cannot be detected when averaging climate signals over large areas with diverse land cover changes. Therefore, small subregions, which cover a range of land cover change intensities, are chosen to reflect local climate impacts due to different land cover changes."

(8) Line 282-283: This relationship requires a better explanation: either from physical principles or references to work, in which it was developed.

AR: Kotlarski (2007) is given as a reference for the linear relationship in snow albedo scheme in this paragraph. We found the last sentence of this paragraph is redundant with the sentence with Kotlarski (2007) as a reference in the above. We will delete this sentence.

(9) Line 288: Why this is the reason for 6 km resolution?

AR: The resolution of subgrid-scale heterogeneity adopted for the improved soil hydrology scheme (Hagemann and Gates, 2003) is set to be 10 times higher than the model resolution by using the default GLCCD. This is the context for setting the resolution of subgrid-scale heterogeneity in this study to be 3 times (18 km/3=6 km) higher than the model resolution (18 km), because the resolution of FNFI maps are 3 times lower than GLCCD.

(10) Line 298: add some words about calculation of dynamics of snow cover. It is an important model component in relation to the main result.

AR: We agree that the dynamics of snow cover is an important factor. The dynamics of snow dynamics in REMO is well described in Kotlarski (2007), therefore we will suggest that interested readers to refer to Kotlarski (2007) on the dynamics of snow cover.

(11) Line 447: Put this in caption of Fig. 10.

AR: Yes. It will be changed according to this suggestion.

(12) Line 495: Do you mean that REMO predicts winter time temperatures with bias?

AR: Yes. The cold bias over this model domain in wintertime simulated by REMO has been shown in Gao et al. (2014). The content of this paragraph will be changed according to general comment 1 from Reviewer #3.

(13) Line 503-525: You could test this by a simulation, in which you make this kind of change for the whole subregion1 (or all regions). I do not understand why this "Only around 20% ..." constitutes an explanation for differences in max. differences – the 20% change is also in the observations. Could it be that there are factors involved in max. observed differences that your simulations do consider?

AR: Indeed, the original text could be misunderstood. About 20% of subregion1 is changed due to peatland forestation (Table 1), whereas the observational data is measured at sites with open peatland and with forests. We have clarified this paragraph as follows.

"Furthermore, regional averaged difference in the simulated 11 day running mean net surface solar radiation of subregion1 (Fig. 8, d) agrees well with the observed differences in daily mean (1971-2000) net surface solar radiation (Fig.4 in Lohila et al., (2010)) between open peatland and forest sites located in southern and northern Finland. The maximum differences in the observed net surface solar radiation at nutrient-rich sites are 40-45 W/m^2 (on DOY 70) in the south, and 80-90 W/m^2 (on DOY 110) in the north of Finland. At nutrient-poor sites, the maximum differences are 30-40 W/m^2(on DOY 80) in the south, and 60-70 W/m^2 (on DOY 115-120) in the north of Finland. The maximum difference in the simulated 11 day running mean net surface solar radiation averaged over subregion1 is 6.5 W/m^2 (on DOY 107). The timing of the maximum difference in our simulated results, for subregion1, falls within the range of that in the observed data. The much smaller magnitude of the maximum difference in the simulated results could be explained by the fact that only around 20% of the land was transformed from peatland to forests in subregion1. The maximum difference in net surface solar radiation is

caused by the advanced snow clearance day due to peatland forestation. The differences in surface albedo is biggest between snow covered peatland surface and non-snow covered forest surface, i.e. the maximum difference of surface albedo is mostly dependent on snow albedo. Snow albedo has a negative linear correlation with forest ratio (Fig. 4 in the original manuscript). Assuming that the entire land of subregion1 would have been changed from peatland to forests, the maximum difference in net surface solar radiation could be estimated to be five times larger, i.e. 32.5 W/m^2*, which is within the range of observations."*

Response to Anonymous Referee #2

General comments:

The authors provide an analysis on the biogeophysical effects of the dominant land cover change on regional climate in Finland. They found a spring warming due to the conversation of peatlands to coniferous forests that can be mostly related to the modification of the corresponding albedo values. The slight cooling in the growing season is explained with the increased evapotranspiration. The spatial distribution of the climate impacts are introduced for the whole country, furthermore the local scale effects are investigated more in detail for 5 selected subregions.

It is a very recent and important topic, with several practical aspects, especially regarding to the projected climate change and land cover change. The concepts of the manuscript are understandable, the results are interpreted correctly. The novelty of the presented work as well as the need of the regional scale and the use of a regional climate model is clearly explained.

The abstract of the discussion paper provide a concise summary of the paper but I would suggest referring to the practical application also in this place.

AR: We will add descriptions of practical application at the end of abstract as follows.

"The results from this study can be further integrally analysed together with biogeochemical effects of peatland forestation to provide background information for adapting future forest management to climate change mitigation. Moreover, they provide insights about the impacts of projected forestation of tundra in high latitudes due to climate change."

The Methodology chapter contains a very detailed and complete introduction and evaluation of the applied land cover maps and the land surface scheme and parameterization of the regional climate model. It underlines the importance of the appropriate representation of the land cover in climate models that has been improved by the corresponding author. I suggest keeping sect. 2 shorter and including the technical details in the Appendix.

AR: We will move Section 2.3 (Modifications in REMO LSS in this study) to Appendix.

The uncertainties and the limitations of the applied methods are well discussed at the end of the work.

Specific comments:

Following are few comments and questions that the authors should consider clarifying:

(1) The simulated changes of temperature, evapotranspiration, . . . and their magnitude are closely related to the modification of the corresponding main land surface parameters in the climate model. Therefore for the better representation and interpretation of the process chain, I would suggest to include some maps about the changes (2000s vs. 1920s) of albedo, leaf area index and fractional vegetation cover for the whole domain (e.g. on monthly timescale, next to figure 3).

AR: We agree that to show the monthly changes in land surface parameters together with the changes in climate variables is helpful for representation and interpretation of the process chain. For this purpose, we adopted the approach suggested by reviewer #3 in general comment 3 to show correlation relationships. Moreover, we want to keep the length of the manuscript not too long as suggested by Reviewer #3 in general comment 1 to cut down the number of figures. Therefore, please refer to the response to general comment 3 of Reviewer #3 about this comment.

(2) In order to support the better understanding of the main outcome and to make possible to compare the results of the 5 subregions, please add a summary-table that includes the modification of the land cover types (in %), the corresponding change of the albedo, leaf area index and fractional vegetation cover as well as the impacts on the analysed climatic variables for each subregions (complete table 1 with the above mentioned information).

AR: The impacts on analyzed climatic variables for each subregion with daily time resolution have been shown in Fig. 8 in the original manuscript. The change of surface parameters of five subregions for the most interesting periods will be shown in the correlation figures as mentioned in the above specific comment 1. Thus, we believe that there is no longer necessary to add this table anymore.

(3) I would suggest preparing a sensitivity study with unchanged vegetation cover for the same time periods. In this way the contribution of the GHG emission and land cover change to the observed climate tendency could be separately assessed.

AR: The two simulations in this study were conducted over the same time period (1979.1.1 - 1996.12.31) with two different land cover maps. ERA-interim is used as our boundary forcing data. The GHG concentrations for the two simulations are the same. Therefore, the impacts on climate conditions are only from the changes in land cover.

To estimate the contributions of increased GHG concentrations to the observed climate tendency, we cannot simply use our boundary forcing data over the same time period to do the simulations with two levels of GHG concentrations. It is because that ERA-interim reanalysis data is based on observational data. For complete consideration, a global model is needed. Additionally, in response to the general comment 1 from Reviewer #3, the trend maps for monthly mean daily maximum temperature and daily minimum temperature are investigated for March and April. We consider that the trend of daily maximum temperature is influenced by albedo-mediated temperature changes locally, while the trend of daily minimum temperature is more closely related to general climate change caused by global GHGs increases. The local effects in the trends of daily maximum temperature suggest that our modeled results show qualitatively a good correspondence to observational data.

(4) Outlook: How does projected climate change affect the existing land cover (primarily forests and peatlands) in Finland? How could these changes alter the regional climate?

AR: The land cover in Finland is strongly managed. Therefore, we will generally discuss the potential land cover change under the projected climate for high latitudes, and its influence on climate. The content below will be added in discussion part.

"The biogeophysical impacts of vegetation-climate feedbacks on climate are modest in comparion to the effects of increased GHGs for Europe, but local, regional and seasonal effects can be significant (Wramneby et al., 2010). However, studies with dynamic vegetation models under climate projections with increased GHGs indicate that more carbon will be gained to terrestrial ecosystems in high-latitudes by the end of this century (Fallon et al., 2012; Zhang et al., 2014). This is due to increase in woody plants that induce biogeophysical feedbacks with an earlier onset of growing season."

(5) Please refer short in the discussion part also to the possible biogeochemical feedbacks: how are the carbon sequestration and methane concentrations altered by the forest cover increase/peatland decrease? What are the climatic impacts of these changes?

AR: The discussion about biogeochemical aspects will be added as follows.

"Peatland is a significant source of CH4 emissions, and the amount of CH4 emission is sensitive to temperature, water table level, plant root depth and soil nutriention level, etc. (Melton et al. 2013; Turetsky et al., 2014; Lohila et al., 2010). After peatland forestation, the soil water table level goes down leading to increased CO2 release at the expense of CH4 release (Minkkinen and Laine, 2006). As time goes by, carbon sequestration by the tree growth and the formation of a new litter layer could compensate the carbon loss from peatland. Lohila et al. (2010) combined the radiative forcing effects from the differences of albedo and GHG fluxes due to peatland forestation at site-level, and showed net cooling at two soil nutrient-rich sites in the south and north and one soil nutrient-poor site in the south of Finland. Accounting for such local impacts in a regional climate model requires very sophisticated process descriptions and detailed parameterisation of soil properties."

Please also note the supplement to this comment: http://www.biogeosciences-discuss.net/11/C4689/2014/bgd-11-C4689-2014- supplement.pdf

Specific comments (in supplement):

(1) Page 11253, Line 5: Suggestion: keep shorter sect. 2.1, 2.2 and 2.3, and include the technical details in an Appendix.

AR: Answered in the response to general comments.

(2) Page 11256, Line 2: Where exactly? Figure 2 should be mentioned here.

AR: Fig.2 is mentioned in the following sentence for the regional differences, where the total fractional changes over Finland is shown.

(3) Page 11263, Line 14: This kind of information is hard to follow in this form (i.e. long paragraphs), please add a table that summarizes the main outcome for the 5 regions.

AR: Answered in specific comment 2.

(4) Page 11265, Line 23: Please show the corresponding LAI and fractional vegetation cover changes on figures for the whole domain.

AR: Answered in specific comment 1.

(5) Page 11266, Line 19: It would be interesting to have some information on the effect of the GHG concentration increase on the observed temperature tendency (i. e. without any land cover change)

AR: Answered in specific comment 3.

(6) Page 11288, Figure 8: the ET values with negative signs are confusing.

AR: Agreed. ET values in Fig.8 will be changed to be with normal signs.

Response to Anonymous Referee #3

General comments:

Gao and co-workers mainly analysed the climate effects of peatland afforestation as simulated by REMO. As an experimental set-up they used the land cover in 1920s and compared it against the land cover in the 2000s and compare 5 subregions with contrasting land cover changes. Although the manuscript is already in good shape, its potential impact is likely to further increase by implementing the following general suggestions:

(1) A more careful selection of the figures could reduce the length of the manuscript and better distinguish the details from the main messages. Fig 5 and Fig 6 could be display with fewer months. That would allow plotting larger subplots without loosing information. Figure 6 is barely mentioned in the manuscript, the patterns are correctly described by random. The figures add little information.

AR: Agreed. Fig. 5 in the original manuscript will be only shown with spring and summer months, and autumn and winter months will be excluded. Fig. 6 in the original manuscript will not be shown in the revised manuscript. The excluded figures will be submitted as supplements.

The information contained in fig 1 could easily be added to any of the subsequent figures (or better repeated on all subsequent maps). Fig 1 shows the altitude of the sites but nothing is done with that information.

AR: We consider Fig. 1 should be kept because it is the only figure in the manuscript that shows the entire model domain, and orography is an important factor for regional climate. However, we will revise Fig. 1 with a more proper color bar to show the orography, and we will also add the relaxation zone used in REMO simulations for this domain (Please see the *revised Fig. 1 below).*

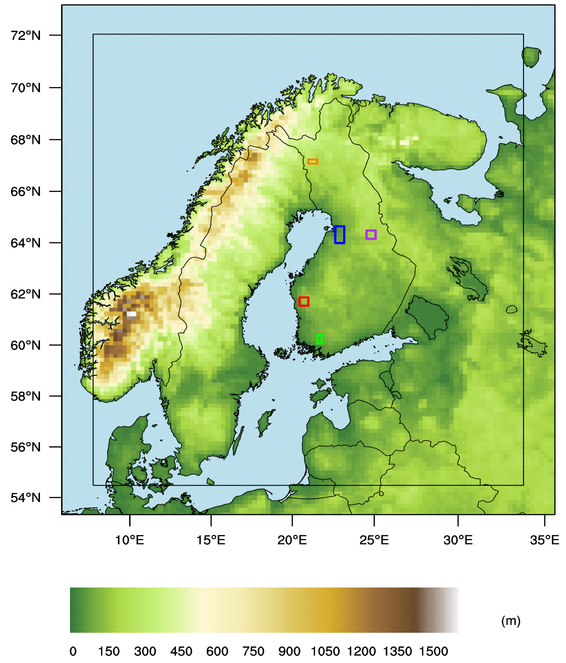

Fig. 1 Orography of the model domain, and the five selected subregions (subregion1 – blue; subregion2 – red; subregion3 – purple; subregion4 – green; subregion5 - orange). The inner black frame shows the extent of the relaxation zone, i.e. the eight outer most gridboxes in each direction.

The information in Fig 3 could be added to Table 3.

AR: Agreed. This information will be added to Table 3 in two additional columns.

In its current presentation, figure 11 does not help to convince that the model does a good job. I suggest a correlation graph between the modelled and observed temperature changes in February, March and April would better present the message.

AR: Agreed. However, we found that the spatial correlations between modeled and observed temperature changes could not help us in this problem. To address this, we investigated the temperature trends over 40 years (1959-1998) for March and April based on monthly mean daily maximum and daily minimum temperatures from E-OBS gridded observational dataset in 0.25 degree resolution. We consider that the trend of daily maximum temperature is influenced by albedo-mediated temperature changes locally for March and April, while the trend of daily minimum temperature is more related to the general climate change caused by global GHGs increases. The local effects in the trends of daily maximum temperature suggest that our modeled results show qualitatively a good correspondence to observational data. The major areas of peatland forestation, subregion1 and subregion2, are highlighted and statistically significant in the trends of maximum temperatures in both March and April but not shown in the trend of minimum temperature. The new temperatrue trend maps are shown below.

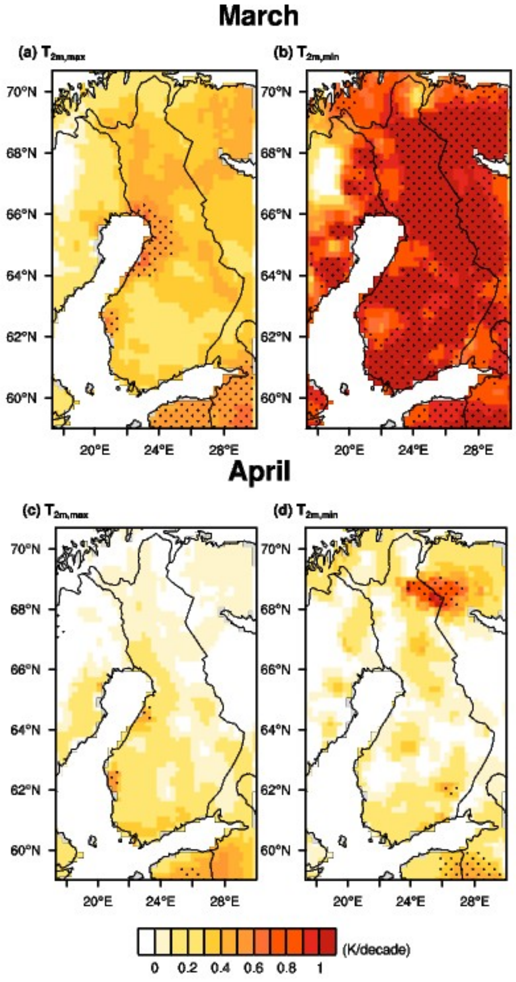

Fig.2 Temperature trends over 40 years (1959-1998) for monthly mean daily maximum (a, c) and daily minimum temperatures (b, d) of March and April. The areas covered with black dots are statistical significant ($p<0.1$).

(2) The manuscript deals with the effect of land cover change and one of its strengths, i.e., that it has also an area of peatland restoration, is hardly used. Subregions 1 and 2 are discussed in detail much fewer attention is given to subregion5

but this could add a very interesting perspective to the discussion.

AR: We cannot really say subregion5 as a peatland restoration area because the land cover change of subregion5 is an artificial effect due to the uncertainties in FNFI maps (discussed in Section 2.2). However, we included subregion5 in the analysis because it is interesting to see the modeled climate effects of this area that with decreased forests and increased peatland. Thus, we chose subregion5 as a comparison to subregion1 and subregion2 where the land cover change actually took place, with less attention given to subregion5 .

(3) There is no figure showing the relationship between land cover change and climate change. Simple correlations between all land covers in table 1 and the observed temperature and precipitation differences may result in some interesting perspective(s). The same analysis could be repeated for the drivers, i.e., change in albedo, change in ET, …

AR: Agreed. We investigated the spatial correlations between the changes in the two surface energy balance relevant variables, surface albedo and ET, and T_{2m}. Consequently, the changes in surface albedo and ET are correlated to the changes in the surface parameter values which describe land cover changes. Monthly means of 15-year averaged changes of March and June are selected to represent springtime and summertime effects, respectively. The following plot and descriptions about those relationships will be added in the manuscript.

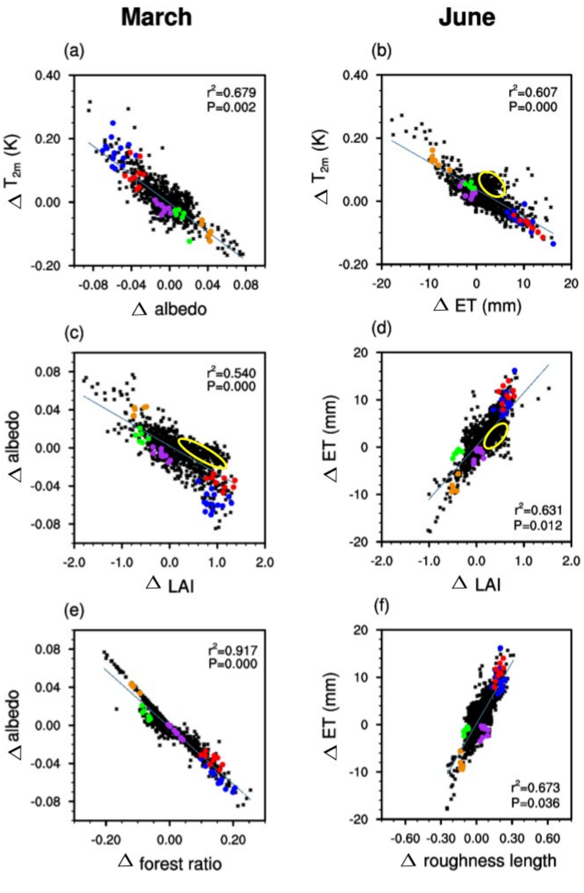

Fig. 3 Spatial correlations between changes in T_{2m} and changes in albedo for March (a), and between changes in T2m and changes in ET for June (b), and also relationships between changes in albedo (c, e) (changes in ET (d, f)) and changes in land surface parameters in REMO LSS following land cover changes in the corresponding month. The changes in the gridboxes in selected subregions are shown with coloured dots. (subregion1 - blue; subregion2 - red; subregion3 - purple; subregion4 - green; subregion5 – orange). The gridboxes in yellow circles shows the changes in the southeast area of Finland.

"To assess the generality of the causal relationships between land cover changes and climate variables, the spatial correlations between changes in the two surface energy balance relevant variables, surface albedo and ET, and T_{2m} are investigated. Consequently, the spactial correlations between changes in surface albedo and ET and changes in the surface

parameter values are also explored. The correlations with green vegetation ratio is not shown in Fig.3, because LAI and green vegetation ratio are both modulated with the monthly varying growth factor by the same scheme, and they are highly correlated (pearson correlation coefficient, r^2*= 0.984 for March,* r^2*=0.674 for June). Monthly means of 15-year averaged changes in March and June are selected to represent springtime and summertime, respectively. The changes in* T_{2m} *are in accordance with the changes in surface albedo in March (Fig.3, a), which is almost linearly correlated with the changes in LAI (Fig.3, c) and forest ratio (Fig.3, e). The* T_{2m} *changes in June are linearly correlated with ET changes over most of the area (Fig.3, b). In general, the changes in ET are also correlated with the changes in LAI (Fig.3, d), roughness length (Fig.3, f) and forest ratio (yearly-constant, not shown), despite the influences from drought that may happen in late summer. Overall, the changes in surface albedo and ET are closely dependent on the changes in land surface parameters, which are induced by the changes in fractional coverages of land cover types in the five subregions (Table 1). The changes in* T_{2m} *are mainly modulated by the changes in albedo and ET in spring and summer, respectively. Some gridboxes located in the southeast of Finland, where mixed forest was substituted by coniferous forest mainly, show deviations in the correlations with LAI (marked by yellow circles in Fig.3, b, c, d). In this area, LAI increased with almost no change in forest ratio, which lead to relatively smaller decrease in surface albedo compared to other areas with the same magnitude of changes in LAI in March; the ET-induced cooling is outweighted by the albedo-induced warming, which causes a slight warming effect in June. In the following summer months, July and August, the ET-induced cooling effect typically gets smaller because of surface water limitation and consequent warming."*

(4) At several places in the results and discussion, cloud cover and atmospheric inversions are mentioned as drivers of some of the observed changes but no evidence is provided to the reader. Is this a result from the analysis or a (logical) induction by the authors.

AR: It is a logical induction according to the results shown in Fig.8. In autumn and winter, there are varied differences of temperature but no differences in net surface solar radiation. Also there are no differences in ET, as well as in latent heat flux. Thus, the differences of long wave radiation is the only factor affecting surface energy partition.

(5) In fig 8 subplots have different units. In the text these subplots are compared as if they have the same units (p11262, 20-22). Converting the units would result in a more convincing presentation.

AR: We found by showing percentage changes for those variables are not helpful to illustrate the results. To make this part more clear, we will revise of the text as follows.

"T_{2m} *of subregion1 shows a warming of 0.1 K to 0.2 K from February till the end of March, and an evident peak of increase from early April to early May (from DOY 95 to DOY 125), which reaches a maximum of 0.5 K in late April.* T_{2m} *of subregion2 has the same developement as subregion1 throughout the whole year, but the warming is much smaller and the biggest difference occurs in the beginning of April being only 0.12 K. This is consistent with the differences in snow depth. The snow-cover period in subregion2 is shorter along with an earlier maximum difference in snow depth. Moreover, those characteristics of the differences in snow depths are in agreement with the differences in surface albedo qualitatively because snow is the key factor that controls the surface albedo in the snow-cover period. From the beginning of May to the beginning of October,* T_{2m} *turns to show a cooling of less than 0.1 K in subregion1 and subregion2, because the cooling caused by ET exceeds the warming caused by the slightly lower albedo. The variability of the differences in net surface solar radiation in the growing season is induced by the variability of cloud cover rather than surface albedo. In November, December and January, the differences in* T_{2m} *vary in both directions. In high-latitudes, incoming solar radiation is quite small and cloud cover fraction is high in late autumn and winter. Therefore, the differences in surface albedo are not able to induce differences in net surface solar radiation in this period. Instead, the surface air temperature is sensitive to changes in the long-wave radiation balance that may lead to atmospheric air temperature inversion under a clear sky, manifesting itself as extreme cold surface air temperature. Thus, the variability of the differences in cloud cover caused by short-term variations in the climate contributes to varied differences in* T_{2m} *in this period."*

Specific comments:

(1) The term 'unproductive peatland' contains some contradiction as these sites are so fertile that they are drained and used for forestry and agriculture. What is the reference for the word 'unproductive'? Euro's, water, carbon, . . .?

AR: Unproductive land in Finnish National Forest Inventory is defined as naturally treeless land or land has the potential capacity to produce a mean annual increment of less than 0.10 m^3*/ha of stem wood over bark, which can be referred to Tomppo et al. (2011). Thus, unproductive peatland means naturally treeless or sparsely treed peatland. On unproductive peatland, the growth limiting factor is not site infertility, but excess of water. Therefore, peatlands were drained to stimulate forests growth in Finland in the past. To make it more clear, the term 'unproductive peatland' will be changed to 'naturally* *treeless or sparsely treed peatland' in the manuscript.*

(2) The objectives (top page 11253) are rather vague.

AR: Agreed. We will modify it as: The intention of this study is to understand how peatland forestation that took place in Finland influences regional climate conditions from biogeophysical aspects.

(3) Reword and add some details. Mention the effects on keeping land cover unchanged outside of Finland. This basically means that your experiment can quantify the impact of land cover change for Finnish climate but is not suitable to attribute observed changes in climate to land cover change.

AR: Agreed. We will add the discussion below about this point in the part that compares simulated results with observational data.

"However, it is difficult to compare the exact magnitudes and patterns of temperature changes because observational data contains contributions from other factors, for instance, the effects of climatic teleconnections from land cover changes in surrounding areas of Finland and short lived climate forces, such as aerosols and reactive trace gases (Pitman et al. 2009)."

References:

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