Editor Comment:

Both referees provided detailed, constructive and feasible comments on the manuscript which the authors seem to be willing and capable of incorporating in a revised version. I therefore invite the authors to prepare and upload a revised manuscript.

Some additional points of attention:

- As a reply to R1, L112 it is stated that the variation between PFTs is larger than the variation within PFTs. There are several papers on plant traits contradicting this statement. The PFT-paradigm is still widely used but it is also frequently and rightly questions and alternatives are under development. Either add some solid references (based on synthesis or meta-study) that support your statement or provide some more nuanced text on this issue.

We thank the editor for this pertinent comment. We have decided to rephrase the statement as follows:

"This approach is associated with a certain degree of uncertainty, since the parameter spaces of deciduous and coniferous trees have a certain overlap (e.g. Wright et al., 2005, van Bodegom et al., 2012). This means that the characteristics of several deciduous tree species and several coniferous tree species do show a high level of agreement. However, we assume in our approach that in general the parameter similarity is higher within the respective deciduous and coniferous tree classes than between the two classes (e.g. Gitay & Noble, 1997). The averaged parameter values of deciduous and coniferous trees can consequently be used to differentiate between the general characteristics of these tree species. Therefore, the usage of two representative forest classes in this study is assumed to be suitable to investigate the general effects of deciduous and coniferous trees on heat period characteristics." (Lines 134-143)

- Just changing from BROAD to DECID does not solve the issue. Did you add Larch in DECID and did you exclude the Mediterranean oaks from DECID? Changing the name should be reflected in changing the classification of the two PFTs on the vegetation map used to prescribe the model.

- larch as a deciduous conifer and the Mediterranean oak as an evergreen broadleaved tree are not considered for the deciduous and coniferous tree classes. In the deciduous tree class, only deciduous broadleaved trees are included, while in the coniferous tree class, only evergreen needle leaved trees are considered. Therefore, the editor is correct and the composition of the different tree classes needs to be described more precisely in the manuscript. With this aim, we have included a clear definition of the two tree species classes used in this study:

"In this context, for the deciduous forest class, only deciduous broadleaved trees are considered, while in the coniferous forest class, only evergreen needleleaved trees are included. Evergreen broadleaved trees (e.g., Mediterranean oaks) or deciduous needleleaved trees (e.g. larch) are consequently not considered." (Lines 131-134)

- R1, L272-274. There has been several synthesis and meta-studies on this issue. One of the first synthesis studies might be Law et al "Environmental controls over carbon dioxide and water vapor exchange of terrestrial vegetation". Use this lead to find more recent and more comprehensive studies on this issue. Citing two random site-studies is not sufficient to justify model parameters or approaches.

We thank the editor for this comment. We have extended the literature review as follows:

"This simulated property of coniferous forests in northern Europe depends of course on the considered vegetation parameters. As mentioned in section 2, the used vegetation parameters in table 1 are averaged values over the parameter space of deciduous and coniferous forests and vary consequently for specific deciduous and coniferous tree species. This variability in the vegetation characteristics is

also reflected in observations. For instance, higher transpiration rates of deciduous forests are reported in Baldocchi et al. (2000), who reviewed several field studies in Canada, Siberia and Scandinavia, in Eugster et al., (2002) based on the analysis of several eddy-covariance stations in the boreal regions of Europe and northern America, and in Grossiord et al., (2013) for measurements in a boreal plantation in south-western Finland. On the other hand, contradictory results have been reported in Augusto et al., (2015), who reviewed studies on the water-use efficiencies of deciduous and coniferous forests in boreal regions, implying higher transpiration rates of coniferous forests, in Ewers et al., (2005) for a comparison between pines and poplar in the BOREAS Northern Study Area in Canada, and in Baumgarten et al. (2019), where higher transpiration rates of coniferous forests are measured in hemiboreal regions even during the warm summer months.
The increased transpiration rates of coniferous forest identified in our model study are consequently within the range of the observed transpiration variability in boreal regions and lead in our simulations to slightly increased heat period intensities for northern Europe (Fig. 2d)." (Lines 332-349)

- R1, L293. Your management/policy recommendation should be conditional on the objectives of the forest management. Specify for which objectives it is not recommended to convert to deciduous broadleaved trees. For biodiversity, control of bark beetles pests, reducing wind throw, and limiting fires, … it could still be recommended to increase the share of deciduous broadleaved trees (except Eucalypt). Note that for mitigating heat extremes changing to deciduous trees would not have considerable negative effects. In the end it still might be a reasonable strategy. More carefully phrase your science-based management/policy advice.
- we agree with the editor that the recommendations should depend on the forest management strategy. Thus, the policy advice needs to be specified, and the statement is rephrased as follows:
"These results indicate that an increase in the deciduous forest fraction has no potential to reduce the intensity of heat periods in Scandinavia." (Lines 480-481)

Furthermore, we replaced the following statement on management recommendations in the manuscript,
"In combination with the low ecological potential of deciduous forest to replace coniferous forests in boreal regions (e.g. Högberg et al., 2017), we conclude that an increase of the deciduous forest fraction is currently not realistic nor a beneficial mitigation strategy for heat extremes in northern Europe",
by
"However, considering the missing significance of the warming effect of an increased deciduous forest fraction in Scandinavia in the model and the observed variability in the sign of the transpiration response, a final assessment of the mitigation potential for heat extremes in northern Europe is not possible" (lines 353-356)

In addition, we adapted the last sentence of the manuscript:
"Thus, the method can only be considered as a supporting mitigation measure to complement other, more effective mitigation strategies to reduce heat extreme intensities" (499-501)

- R1, Fig 3 and 5. Based on the text it seems that there is still some duplication between 2 pairs of figures. Revise these figures to avoid duplication.
We agree with the editor that the figures should not be duplicated. The mentioned figures were deleted and the significances were added to the previous figures (old Fig 2, Fig. 4)

- R2, comment on feasibility of growing deciduous. Birch, willow and poplar are growing all over Fennoscandinavia. Also in Fennoscandinavia humans have favored the conifers. Establishing forest of early succession deciduous forests is feasible in this region. In the hemiboreal region of Southern Sweden and Norway, late successional deciduous forest are possible. Note that in scientific literature the term "Fennoscandinavia" refers to a geological shield including parts of Russia but not all of Sweden and Norway. Probably better to use country names unless you really refer to this plate.

- The term “Fennoscandinavia” will not be included in the manuscript, it was not used by us but rather by one of the reviewers. The text about the ecological limits of planting deciduous forests in boreal regions in adapted in the following way:

“As a consequence, deciduous forests in this simulation are located in regions, in which their growth is ecologically limited to some extent (e.g. Högberg et al., 2017). For instance, broadleaved deciduous forests have generally high nitrogen needs. However, soils are generally nitrogen-poor in boreal regions, explaining why the ecological conditions are not optimal for the growth of deciduous forests, and only deciduous tree species with low nitrogen demands are naturally growing, like birch or poplar. Therefore, coniferous trees have a naturally high proportion in boreal forests. However, these ecological limitations of an increased deciduous forest cover fraction are not considered in this idealized sensitivity study, the focus is only on the potential climatological effects.” (Lines 145-152)

- R2, comment on “not realistic”. I disagree, it would largely disrupt the forest sector in that region but that is not the same as not realistic. The simulation is idealized so I agree the simulation by itself is not realistic but what can be learned from this simulation? Favoring more deciduous species would not have a negative effect on the climate. Deciduous and Evergreen species could also be mixed. It Doesn’t need to be 0 or 100%. Carefully phrase science-based management/policy advice.
- The statement is rephrased. Please see the reply to R1, L293.

Reviewer 1:

This paper plays with the idea that all coniferous forests in Europe are replaced by deciduous forests. The climatic response to this vegetation change is simulated by one RCM. As such this is an interesting exercise since different kind of afforestation or vegetation alterations are discussed as mitigation methods. The effect of changing the type of forest is, however, mostly small and statistically insignificant.
The experiment is well organised and executed, but there I have some concerns about the methods and the conclusions that I would like to raise. My main concern is that the results, or the implications of the results, is a bit exaggerated. The effects are small and mostly insignificant, i.e. changing conifers to deciduous is not a viable mitigation strategy. This also an interesting result. I don't see the need of exaggerating the model response, or trying to see connections between e.g. evaporation in western Europe and precipitation in eastern Europe. I don't think that the authors in a satisfactory way should that the model response is due to these proposed mechanisms and not just random. The difference between the first 15 years and the last 15 years of the REF simulations could be bigger than the difference between REF and BROAD. Natural variability is large and I don't see why the difference between REF and BROAD could just be random variability. I still think the paper is worth publishing, and therefore I don't see the need of making a large effort to try to explain all differences between REF and BROAD. Especially since these differences are mostly small and insignificant.
Reply: Thank you very much for reviewing this manuscript, for your assessment and your very helpful comments. Detailed answers to your comments can be found below. Changes in the revised manuscript will be implemented with tracked changes.

Regarding your major comment on the significance of our results, we fully agree that the natural variability within a 30-year simulation is large and that the simulated effects of an increase of the deciduous forest fraction on heat period intensities are small. We also agree that for the majority of the grid cells local effects (55 %) and non-local effects (77 %) are not significant. This is now stronger emphasized in the revised manuscript (see Lines 251-258 and 307-312).
However, for 45 % of the grid cells in which a replacement of coniferous forests with deciduous forests took place, a local reduction of the heat period intensities is found, and the effects are statistically significant. This is also the case for 23 % of the grid cells with a non-local cooling. In our opinion, these are small but significant effects that are not random and cannot be explained by the natural variability. Therefore, we think that it is important to investigate the reasons for these significant effects and provide a reasonable explanation. For the local effects, the process chain is quite straight forward. Deciduous trees absorb less solar radiation and transform at the same time a larger amount of this reduced energy input into latent heat, with the consequence that less energy is used to heat up the surface and daily maximum 2 m temperatures are reduced. Furthermore, the locally increased evapotranspiration rates increase the atmospheric water content and thus affect the mean downwind distribution and sum of precipitation. Non-local changes in the available soil water amount for evapotranspiration during heat periods are the consequence, leading again to non-local changes in the heat period intensities. Since these downwind effects display a high temporal and spatial variability, the non-local process chain is harder to identify, resulting in non-significant effects for the majority of the grid cells (77 %). The high spatial spread of the downwind effects just prevents clearer results for single grid cells.
Of course, this physical reasoning does not mean that chance can be refuted as a possible reason. However, we were able to identify the same local and non-local process chains for almost all grid cells in which an increase in the deciduous forest fraction leads to a local or non-local reduction of the heat period intensities, regardless whether the results are significant or not. The meaning of non-significant results is just that we cannot exclude random causes. However, because of the high spatial consistency of the simulated process chains, random causes are from our point of view very unlikely to be the main reason for these local and non-local effects. Of course, we cannot derive a general validity of the described process chains for the respective regions mentioned in the manuscript on the basis of our

results. This interpretation and discussion of the results is now included in the revised manuscript (see Lines 447-464).

Major comments

L112, and Table 1: What do these classes represent? The difference in characteristics are large between e.g. birch and oak. Also, not the same kinds of tress grow in northern Turkey and northern Norway. Please describe the forest types a little better.

Reply: the reviewer is correct, the kind of trees that grow in northern Turkey and northern Norway is different, and the characteristics (i.e. parameter values) of birch and oak are not the same. Thus, there is a certain spread in the parameter values of deciduous trees on the one side, and coniferous trees on the other side. In CCLM-VEG3D, different deciduous and coniferous tree species are combined to one representative deciduous tree class and one representative coniferous tree class, respectively. This means that for each vegetation parameter a mean value of the parameter spread over the different deciduous and coniferous tree species is applied. CCLM-VEG3D does consequently not distinguish between oak and birch trees. This may be a shortcoming of the approach and is now explicitly mentioned in the manuscript:

"In addition, the vegetation characteristics of different deciduous tree species (e.g. beech, oak, etc.) and different coniferous tree species (pine, spruce, etc.) are all combined in one representative forest class, respectively. This means that for the different vegetation parameter, describing the characteristics of these different tree species, the mean values over the parameter space of the respective deciduous and coniferous trees are used." (Lines 125-130)

Furthermore, we included in the manuscript the following discussion about the uncertainty and applicability of this approach:

"This approach is associated with a certain degree of uncertainty, since the parameter spaces of deciduous and coniferous trees have a certain overlap (e.g. Wright et al., 2005, van Bodegom et al., 2012). This means that the characteristics of several deciduous tree species and several coniferous tree species do show a high level of agreement. However, we assume in our approach that in general the parameter similarity is higher within the respective deciduous and coniferous tree classes than between the two classes (e.g. Gitay & Noble, 1997). The averaged parameter values of deciduous and coniferous trees can consequently be used to differentiate between the general characteristics of these tree species. Therefore, the usage of two representative forest classes in this study is assumed to be suitable to investigate the general effects of deciduous and coniferous trees on heat period characteristics." (Lines 134-143)

L114: You call the simulation BROAD, by you constantly use the term deciduous forest, not broadleaved. Consider renaming the simulation to something more corresponding, e.g. DECID.

Reply: we agree with the reviewer and changed the naming of the simulation from BROAD to DECID throughout the whole manuscript according to the suggestion.

L117: Is the 90th percentile calculated on all days of the year? This would mean that on average 36 days per year pass the threshold, more than a month. In that case you don't study intense heat but rather summer conditions. Please clarify.

Reply: the 90th percentile was calculated only for the summer days (JJA). Thus, we analyzed on average the 9 warmest days of the year. We apologize for this inaccurate formulation and rephrased the paragraph in the following way:

"In order to quantify changes in the heat period intensities, days above the 90th percentile of the daily maximum temperatures in 2 m height in summer (JJA) are analyzed. In this context, we define the heat period intensities as the mean daily maximum 2 m temperature for these warmest 10 % of summer days, and compare these mean values for DECID and REF with each other" (Lines 155-161)

L118: "analyzed" How do you analyse them? Do you compare the means of the warm days of the different simulations?

Reply: yes, that's exactly what we did. This is now explained in the manuscript, see the response to comment L117 above.

L118: "duration" and "number of periods" If you count the number of periods, you are not studying the duration. Duration is the length of the events not how many they are.
Reply: We meant "number of days" instead of "number of periods". Thus, we actually investigated the duration of heat periods. This is now changed in the manuscript and we apologize for this incorrect formulation:
"Changes in the duration of heat periods are in both simulations quantified by counting the number of days in which the daily maximum 2 m temperature exceeds the 90th percentile of daily maximum temperatures in REF over at least three consecutive days (Russo et al., 2015)." (Lines 159-161)

L133-135: This is a description that is a bit backwards. It's a bit strange to describe Scandinavia as the exception, when in fact this is the largest and most obvious change. This description also mainly applies to Fig 4b, it's not the most appropriate to but it after "(Fig 2c)".
Reply: we agree with your assessment and changed the description of the local effects of deciduous trees in northern Europe in the following way:
"At the same time, in central and southern Europe, the latent heat fluxes of deciduous forests are increased (Fig. 2b) and the sensible heat fluxes are reduced (Fig. 2c). This indicates that in a deciduous forest in central and southern Europe, the radiative energy input during heat periods is reduced and a larger part of this reduced available energy at the surface is additionally used for evapotranspiration instead of heating up the land surface. During heat periods, the replacement of coniferous forests with deciduous forests leads consequently to a local reduction of the daily maximum 2 m temperatures, and thus the heat period intensities (Fig. 2d). In northern Europe, or more precisely in the northern part of Scandinavia, this is not the case. In this region, a warming of the mean daily 2 m temperatures during heat periods is simulated with an increase in the deciduous forest fraction. This warming effect is directly caused by a reduction of the evapotranspiration rates in Scandinavia (Fig. 2b)." (Lines 174-184)

L138-139: Maximum 2 m temperature is not necessarily the same as heat period intensities (but again it's not entirely clear how you define heat intensity). Did you also look at Tmax? It would be interesting to see how the signal in Tmax compares to the signal in heat intensity.
Reply: we define the heat period intensity as the average daily maximum 2 m temperature during the warmest 10 percent of all summer days. This is now explicitly defined in the manuscript (see response to comment L117).

L140: "notably Norway" Here one might add also the northern half of Sweden and large parts of Finland.
Reply: the whole description of the effects of an increase in the deciduous forest fraction on local heat period characteristics in Scandinavia is rewritten. See answer to comment L133-135. In this context, we denominate the region now as "the northern part of Scandinavia".

L175: Is it really correct to call a correlation of 0.2-0.4 high?
Reply: we agree with the reviewer that the wording was imprecise. The correlation in Scandinavia is just higher than in central and southern Europe. In the revised manuscript we rephrased the sentence as follows:
"This is shown by the higher correlation between latent heat fluxes and daily maximum 2 m temperatures during heat periods in Scandinavia in comparison to central and southern Europe (Fig. 2f)." (Lines 218-220)

L179-184: I would suspect that this is very model sensitive, and sensitive to the exact vegetation description used. I think this should be mentioned, either here or in the Discussion.
Reply: we agree and mention this now in the discussion section:

“This simulated property of coniferous forests in northern Europe depends of course on the applied vegetation parameters (table 1) and is consequently model-dependent.” (Lines 332-333)

L203: I find it a bit strange that you support the small changes with the statement that the signal is statistically significant in 45 % of the grid cells. This means that the local effect is small and insignificant in more than half of the grid cells.
Reply: we agree with the comment. Significant results at 45 % of the grid cells in Europe, in which an increase in the deciduous forest fraction led to a local reduction of the heat period intensities, also means that for 55 % of these grid cells no significant effects were simulated. This is now explicitly mentioned in the revised manuscript:
“Although this local cooling effect is just slightly pronounced, it is statistically significant for 45 % of all grid cells in which an increase in the deciduous forest fraction resulted in reduced daily maximum 2m temperatures, uniformly distributed all over Europe (except Scandinavia, Fig. 2d). This means that for nearly half of these grid cells the process chain of reduced absorbed solar radiation and increased evapotranspiration results in a significant reduction of local heat period intensities for deciduous forests. However, this also means that for slightly more than half of these grid cells the simulated reduction of the daily maximum temperatures is not significant. Thus, for these grid cells random causes for the temperature reduction cannot be excluded.” (Lines 251-258)

However, in all grid cells in which an increase in the deciduous forest fraction led to a local reduction of the heat period intensities in Europe (significant as well as non-significant grid cells), the same chain of physical processes is simulated. The net short-wave radiation is reduced in all these grid cells. At the same time, a larger part of this reduced available energy at the surface is used for evapotranspiration, resulting in a local reduction of the daily maximum 2 m temperatures during heat periods. Thus, also in the non-significant grid cells, an increase in the deciduous forest fraction has the same physical effects on heat periods as in significant ones. Non-significant results do not necessarily mean that no physical connection exists. It just tells us that we cannot entirely exclude that these results are achieved by chance. However, since for all grid cells in which a deciduous forest has a local cooling effect the same process chain is simulated, a random cause for these effects is from our point very unlikely. This is now discussed in the revised manuscript:
“With this in mind, we could show that an increase in the deciduous forest fraction has significant as well as non-significant effects on local and non-local heat wave intensities. While for the grid cells with significant effects, consistent physical process chains are the reason for the local and non-local temperature reductions during heat periods, random causes for the temperature reductions in non-significant grid cells cannot be generally excluded. However, a missing significance does not necessarily mean that there is no connection (Wasserstein & Lazar, 2016) between an increase in the deciduous forest fraction and reduced heat period intensities in these grid cells. On the contrary, also for the non-significant grid cells with reduced daily maximum temperatures, the same process chains were identified as for the significant ones. From our point of view, this high physical consistency of the simulated processes is a strong indicator that the reduced heat period intensities are also not random in the non-significant grid cells. Particularly downwind processes are spatially and temporally highly variable. Thus, locally induced changes in the atmospheric moisture conditions do not always lead to precipitation at the same downwind locations (Perugini et al., 2017). This high spatial and temporal variability, therefore, has the consequence that the physical processes are difficult to detect and the temperature reductions are statistically not significant. However, comparing the potential reduction of heat period intensities with the substantial intensification of heat extremes of about 2,3 K in Europe since the 1950s (Lorenz et al., 2019), the effect of an increased deciduous forest fraction is small. “ (Lines 447-464)

L203: “all grid cells” Do you mean *all* grid cells, all grid cells with vegetation changes or all grid cells with with reduced daily maximum 2 m temperature? Please rephrase and clarify.
Reply: The sentence is rephrased as follows:

“Although this local cooling effect is just slightly pronounced, it is statistically significant for 45 % of all grid cells in which an increase in the deciduous forest fraction resulted in reduced daily maximum 2m temperatures, uniformly distributed all over Europe (except Scandinavia, Fig. 2d).” (Lines 251-253)

L207: “95 % of the areas” Do you mean grid cells? If not, please explain what these areas are.
Reply: we replaced “areas” with “grid cells”.

L222-223: Do you actually mean that the precipitation sum is driving the temperature increase? Please explain.
Reply: We mean that differences in the summer precipitation sums (JJA) between DECID and REF change the available amount of water for evapotranspiration during heat periods and thus, the evapotranspiration rates. Changes in the evapotranspiration rates during heat periods affect the amount of energy that is available to heat up the surface, and in this way the 2 m temperatures. This is now extensively explained in the revised manuscript:
“These warmer non-local temperatures arising with an increase in the deciduous forest fraction are also caused by non-local differences in the summer precipitation sums between DECID and REF. In all these regions, mean precipitation sums in summer are reduced (Fig. 3b), and thus, the available amount of water for evapotranspiration during heat periods is also reduced. Lower evapotranspiration rates are the consequence (Fig. 3c), which means that more energy is available at the surface to heat up the surface, finally leading to non-locally intensified heat periods.” (Lines 273-279)

L227: “Spatial precipitation” Do you mean the mean precipitation over a particular area, or do you mean spatial changes in precipitation? Please explain or rephrase.
Reply: we mean spatial changes in precipitation. We rephrased the sentence as follows:
“These changes in the spatial distribution of precipitation sums in summer are most likely caused by local changes in the vegetation characteristics, associated with an increase in the deciduous forest fraction in Europe”. (Lines 280-282)

L240: The band from Greece to the Baltics is very narrow, and the changes in evaporation in western Europe is very small. How can you know that this is the connection? Especially since precipitation sums are reduced also to the west of this band. Without a proper explanation I would say that this is a random effect.
Reply: we agree with the reviewer. For 77 % of the grid cells in which an increase in the deciduous forest fraction leads to a non-local reduction of heat period intensities, no significant temperature effects are actually simulated. Therefore, random effects cannot be excluded for these grid cells. This is now explicitly discussed in the revised manuscript:
“As already identified for the local effects, non-local cooling effects are statistically significant for 23 % of all grid cells with reduced daily maximum 2 m temperatures (Fig. 3a). However, this also means that the simulated non-local reduction of the daily maximum temperatures is statistically not significant for the other 77 % of these grid cells. Although for these non-significant grid cells the same process chain is simulated as for the significant grid cells, random causes for the temperature reduction during heat periods cannot be excluded.” (Lines 307-312)

However, for 23 % of these grid cells, a significant non-local cooling is simulated. The local replacement of coniferous trees with deciduous trees leads consequently to a systematic non-local reduction of the heat period intensities in these grid cells. Although we agree with the reviewer that the simulated effects are rather small, we think that it is important to think about the physical reasons for these significant results and present a reasonable explanation.
This said, the downwind effects of local perturbations are quite difficult to detect, since downwind processes are highly variable, with the consequence that the locally induced changes in the atmospheric moisture content do not always lead to changes in the precipitation sums at the same downwind locations. Nevertheless, we are able to qualitatively show a clear physical connection between locally increased evapotranspiration rates, downwind precipitation sums, and non-locally

changed evapotranspiration rates and heat period intensities. Given the high natural variability, this downwind process chain has surely a certain spatial spread, with the consequence that the effects are in mean rather small and for 77 % of the grid cells not significant. This is now discussed in detail in the revised manuscript:
(see again (Lines 447-464))

L249-251: This could as well be used as an argument for that all differences are random.
Reply: as mentioned in the response to comment L240, the discussion about the statistical significance of the results is rewritten.

L251: "several parts of Europe" Which parts are these, are they defined?
Reply: the statement is specified in the revised manuscript in the following way:
"The non-local warming effect in eastern Europe, at the North Sea coast of central Europe and the Balkan Mountains is in mean 0.1 K, and is statistically not significant" (Lines 303-305)

L251-251: "95 % of these regions" Do you mean regions or grid cells?
Reply: we mean "grid cells". The sentence is rewritten accordingly.
"The warming at 95 % of the respective grid cells is again below 0.4 K." (Line 305)

L252: "the rest of Europe" How large proportion of Europe is this? Is this actually the dominating effect?
Reply: the statement is specified in the revised manuscript in the following way:
"The non-local cooling effect over central, western and southern Europe is in mean also 0.1 K with a 95th percentile of 0.3 K" (Lines 306-307)

L254: The sentence reads like you only looked at grid cells with reduced Tmax. I guess this is not what you did. Please rephrase.
Reply: For the analysis described in this sentence, we indeed only considered grid cells with a non-local reduction of the daily maximum temperatures. In 23 % of these grid cells, the temperature reduction is significant. In the grid cells in which an increase in the deciduous forest fraction led to a warming, no significant results were simulated.

L257-260. Is the threshold calculated from the 90th percentile in REF of per simulation? See also the comment on L118.
Reply: the threshold is calculated from the 90^{th} percentile in the REF simulation. This is now changed in the manuscript and we apologize for this inaccurate formulation:
"Changes in the duration of heat periods in both simulations are quantified by counting the number of days, in which the daily maximum 2 m temperature exceeds the 90th percentile of daily maximum temperatures in REF over at least three consecutive days (Russo et al., 2015)." (Lines 159-161)
And:
"Therefore, a heat event is in both simulations defined as a period in which the daily maximum 2 m temperature exceeds the 90th percentile of daily maximum temperatures in the reference run over at least three consecutive days" (Lines 316-318)

L272-274: How does this relate to observations and actual conditions? I guess that there are data. It would be good to check whether the models give expected results.
Reply: In our literature review we have realized that there is a certain variability in the evapotranspiration rates of coniferous and deciduous forests in boreal regions. This is now mentioned and discussed in the manuscript:
"This simulated property of coniferous forests in northern Europe depends of course on the considered vegetation parameters. As mentioned in section 2, the used vegetation parameters in table 1 are averaged values over the parameter space of deciduous and coniferous forests and vary consequently for specific deciduous and coniferous tree species. This variability in the vegetation characteristics is

also reflected in observations. For instance, higher transpiration rates of deciduous forests are reported in Baldocchi et al. (2000), who reviewed several field studies in Canada, Siberia and Scandinavia, in Eugster et al., (2002) based on the analysis of several eddy-covariance stations in the boreal regions of Europe and northern America, and in Grossiord et al., (2013) for measurements in a boreal plantation in south-western Finland. On the other hand, contradictory results have been reported in Augusto et al., (2015), who reviewed studies on the water-use efficiencies of deciduous and coniferous forests in boreal regions, implying higher transpiration rates of coniferous forests, in Ewers et al., (2005) for a comparison between pines and poplar in the BOREAS Northern Study Area in Canada, and in Baumgarten et al. (2019), where higher transpiration rates of coniferous forests are measured in hemiboreal regions even during the warm summer months.
The increased transpiration rates of coniferous forest identified in our model study are consequently within the range of the observed transpiration variability in boreal regions and lead in our simulations to slightly increased heat period intensities for northern Europe (Fig. 2d)." (Lines 332-349)

L293: This depends on how you define drought conditions. In summer in northern Europe precipitation will not decrease, but not increase that much either. Agricultural drought might increase anyway because of increasing evaporation.
Reply: In consideration of your comment, the discussion about the future development of drought conditions in northern Europe is rewritten in the following way:
"In northern Europe, precipitation will slightly increase, while in southern Europe the opposite is the case, particularly during summer. Simultaneously, the atmospheric water demand will increase in both regions, due to the generally increased atmospheric temperatures. In northern Europe, this will likely lead to a slight reduction of the available soil water amount, although precipitation sums are slightly increased (Cook et al., 2020). This might have the consequence that the evapotranspiration of shallow rooted coniferous forests in northern Europe will become water limited, and the cooling effect of coniferous forests on heat period intensities might get smaller in comparison to deciduous forests." (Lines 369-376)
See also the changes in the conclusions section:
"These results indicate that an increase in the deciduous forest fraction has no potential to reduce the intensity of heat periods in Scandinavia. This might change in future to a certain extent, since a slight decrease in water availability is projected in this region by regional as well as global climate models. This might limit evapotranspiration rates of shallow rooted coniferous forest during heat periods, and the cooling effect of coniferous forests on heat period intensities might get smaller in comparison to deciduous forests." (lines 480-485)

L297: It's not true that the water availability will not decrease in Scandinavia in summer. The projected change in summer precipitation is small. This combined with increased evaporation because of higher temperature will lead to decreased soil moisture.
https://interactive-atlas.ipcc.ch/permalink/iUVVp1nj
Reply: In consideration of your comment, the discussion about the future development of drought conditions in northern Europe is rewritten (see comment above).

L319-322: The best model study of this is probably Belusic et al. (2019). They show that changes in roughness length alone can explain a large part of the precipitation changes. It may be worth having a look at that. It also worth noting that Strandberg & Kjellström (2019, already cited in the manuscript) hardly find any significant down-wind non-local effects on precipitation at all.
Reply: thank you very much for this comment. We included both references in the discussion in the following way:
"The interrelation between evapotranspiration rates of forests and downwind precipitation sums was already investigated by Belušić et al., (2019), Strandberg & Kjellström (2019) and Meier et al., (2021). While Strandberg & Kjellström (2019) could find almost no connection, Belušić et al., (2019) and Meier

et al., (2021) provided evidence that increased evapotranspiration rates of forests can lead to increased downwind precipitation sums." (Lines 398-403)

L348-355: One could add that the response to vegetation changes differs between models. Please add a note on that.
Reply: we added the following statement in the discussion section:
"Of course, the simulated responses of other modeling systems to changes in the forest cover composition might be different." (Lines 439-440)

L371: "significant effects" I guess you mean insignificant effects given what you write on lines 203, 210, 251 and 254.
Reply: the paragraph is rephrased in order to mention both, the significant as well as the insignificant results:
"Results show that an increase in the deciduous forest fraction has significant as well as non-significant effects on the local and non-local scale. Locally, mean heat period intensities are slightly reduced about 0.2 K, except for Scandinavia, where a mean warming of 0.1 K is simulated. The simulated temperature reductions in grid cells with replaced coniferous forests are statistically significant at 45 % of the grid cells and not significant at 55 % of the grid cells. The simulated local warming in Scandinavia is not statistically significant.
Non-locally, mean heat period intensities are slightly reduced in central, western and southern Europe about 0.1 K, but slightly increased in Eastern Europe, the North Sea coast of central Europe and the Balkan Mountains also about 0.1 K. Significant results are only simulated for 23 % of the grid cells in which an increase of the deciduous forest fraction leads to a cooling." (Lines 468-478)

L375: You don't study the duration of heat periods, but the number of heat periods.
Reply: As mentioned in the response to comment L118, we analyzed the duration of heat periods and not the number of periods.

L393-394: "Positive impact" Do you mean 'improvement' or increased temperature? Please explain.
Reply: We thank the reviewer for this comment. In order to avoid further misunderstandings, the description of "positive" and "negative" effects/impacts is replaced by "cooling" and "warming" effects/impacts throughout the whole manuscript.

Figs 3 & 5: Just to clarify. Is Fig 3 the same as Fig 2d, and Fig 5 the same as Fig 4a, only with grey points? If that is the case I would suggest that you mark the significant results in a more visible way. The grey dots are really difficult to see. Why don't use hatching, or borders around significant grid cells, or something like that.
Reply: we agree with the reviewer and changed the figures according to the suggestions. Significant grid cells are now indicated by black circles.

Fig 4b: It very non-intuitive to show increasing precipitation with red and decreasing with blue. Please reverse or change colourscale.
Reply: the color scale is changed according to the suggestion.

Minor comments
L24: (see also L382 and more). It is not always to understand what positive/negative effects are. It is often better to use increase/decrease or similar.
Reply: As mentioned in a previous reply, the description of "positive" and "negative" effects/impacts is replaced by "cooling" and "warming" effects/impacts throughout the whole manuscript in order to avoid misunderstandings.

L100: “yearly updated maps”. Does this mean that the land cover is changed in every simulated year in the model? If not, rephrase. If this is what you do, how does the changing vegetation in the REF simulations affect the results?
Reply: We thank the reviewer for pointing this out. In fact, not the complete land cover is changed every year, but only the grid cells are updated in which a land cover change took place. In this way, all land cover changes during the simulation period are considered within the model domain. This ensures that the regional climate model simulations are consistent with the driving ERA5 reanalysis in which all historic land use changes are considered. Since this yearly updated land use information is used in both simulations (DECID and REF), the changes in the land cover do not have an effect on the simulated differences between DECID and REF. The only difference between DECID and REF is that in DECID all coniferous forests are replaced by deciduous forest and this land use change is fixed throughout the whole simulation.

L166: “due the generally” -> “due to the generally”
Reply: is corrected

L215: “non-locally simulated” I think you mean that the simulations give no non-local effects. Please rephrase.
Reply: We mean that in general, a non-local cooling of the daily maximum 2 m temperatures is simulated. The sentence is rephrased as follows to avoid missunderstandings:
“Over central, western and southern Europe, a non-local cooling of the daily maximum 2 m temperatures is simulated in general” (Lines 264-265)

L290: “in future” -> “in the future”
Reply: is corrected

L305: “in future” -> “in the future”
Reply: is corrected

References

Belušić, D., Fuentes-Franco, R., Strandberg, G.and Jukimenko, A., 2019: Afforestation reduces cyclone intensity and precipitation extremes over Europe. Environ. Res. Lett. 14,https://doi.org/10.1088/1748-9326/ab23b2

Reviewer 2:

Overview:

Breil et al. present research on modeling the local and non-local climate effects of afforesting existing coniferous forest in Europe with deciduous forest. They hypothesize that deciduous forest may help mitigate warming during periods of extreme heat through increased evapotranspiration and/or increased surface albedo. They run two model scenarios – one with present day forest annual changing forest cover and a second with all coniferous forest replaced with deciduous forest types. The results indicate that there is a slight cooling effect, but it is marginal and unlikely to serve as a single solution to reducing the duration and magnitude of extreme heat. Instead, they suggest that afforestation of coniferous forest with deciduous forest would serve as one of several efforts that could complement rather than completely mitigate future warming.

While the results do not show a large cooling effect of replacing coniferous forest with deciduous forest, I still believe it is an important addition to the peer-reviewed literature. Please address the major and minor comments below in a revised submission.

Reply: Thank you very much for reviewing this manuscript, for your assessment and your very helpful comments. Detailed answers to your comments can be found below. Changes in the revised manuscript will be implemented with tracked changes.

Major Comments:

The introduction could use more context on the history and current state of forest types in Europe. How did we arrive at present-day forest cover distribution? Why does Europe's forest cover today differ so much from potential vegetation that would grow there if it otherwise hadn't been cleared for agriculture, timber, and fuelwood? Wouldn't beech and oak, two broadleaved species, dominate European forests were it not for forest management and harvest practices? Isn't secondary regrowth and forest succession part of the story? I would like to see this contextualized beyond the single sentence in lines 72-73 that states "the current composition of European forests is dominated by coniferous forests (Bartholome & Belward, 2005), due to forestry reasons."
Reply: thanks for this comment. We extended the description of the European forest composition according to your suggestions in the following way:
"The composition of primary European forests is different and depends on the regional conditions. In boreal and mountainous regions, cold and wet climate conditions cause a leaching and acidification of soils, favoring the dominant establishment of cold-tolerant coniferous trees (Bohn et al., 2000). Otherwise, primary European forests are mainly characterized by large deciduous tree fractions, like beech (Bohn et al., 2000). After humans started to cultivate landscapes in the course of the Holocene, European forests were extensively cleared for croplands, timber and firewood (Kaplan et al., 2009), particularly during the medieval period (Pongratz et al., 2008). As a consequence, the forest cover on usable land for agriculture declined to under 6 % in central and western Europe in the mid-19th century (Kaplan et al., 2009). The resulting scarcity of timber and firewood made the management of forests necessary, which led to an intensive plantation of coniferous trees (McGrath et al., 2015). The persistent cold climate conditions of the so-called "little ice age" during this period, and the high yields of coniferous trees, favored their cultivation in Europe. This yield-orientation is still driving forest management today (Ceccherini et al., 2020), and is the reason why today primary forests cover only 0.7 % of Europe's forest area (Sabatini et al., 2021)." (Lines 74-87).

I would also like to see a discussion paragraph on the feasibility of growing deciduous forests in Scandinavia and other regions in the modeling domain that have limited potential to be afforested into a deciduous forest. It seems soil nutrient availability and chemistry, specifically N-limited and acidic soils, would limit deciduous forest afforestation in the boreal forest biomes dominant in Fennoscandia.

Reply: thanks for pointing this out. In the revised manuscript, we discuss the potential to increase the deciduous forest fraction in Europe already in the method section:

"As a consequence, deciduous forests in this simulation are located in regions, in which their growth is ecologically limited to some extent (e.g. Högberg et al., 2017). For instance, broadleaved deciduous forests have generally high nitrogen needs. However, soils are generally nitrogen-poor in boreal regions, explaining why the ecological conditions are not optimal for the growth of deciduous forests, and only deciduous tree species with low nitrogen demands are naturally growing, like birch or poplar. Therefore, coniferous trees have a naturally high proportion in boreal forests. However, these ecological limitations of an increased deciduous forest cover fraction are not considered in this idealized sensitivity study, the focus is only on the potential climatological effects." (Lines 145-152)

Minor Comments:

Lines 72-73: Please include more recent references, such as Sabatini et al. (2021) and Ceccherini et al. (2020).

Reply: we included both references and more in the revised manuscript (see Lines 74-87).

Sabatini et al. (2021): https://onlinelibrary.wiley.com/doi/full/10.1111/ddi.12778

Ceccherini et al. (2020): https://www.nature.com/articles/s41586-020-2438-y

Lines 93-99: Please confirm and specify that two-way coupling is used between the land and atmosphere models.

Reply: The statement is rephrased in the following way:

"In the course of this study, the regional climate model COSMO-CLM (CCLM, Rockel et al., 2008) is two-way coupled to the Land Surface Model VEG3D (Breil & Schädler, 2021) and used to simulate the effects of an increased deciduous forest fraction on heat extremes in Europe." (Lines 109-111)

Line 109: Remove unnecessary comma after CCLM-VEG3D

Reply: is removed

Figures:

Figure 2f. Please use a monochromatic (light green to dark green for example) rather than rainbow scale bar.

Reply: we changed the color scale according to the suggestion of the reviewer.