Response to reviewer 1 of the paper entitled
"Different response of surface temperature and air temperature to deforestation in climate models"
Ref.: esd-2018-66

We would thank the reviewer for the time he/she devoted on reviewing the manuscript, and for his/her helpful comments.

Below are the reviewers comments (***bold italic font***) and our responses to each point (normal font). All line numbers that we provide in our responses refer to the revised version of the manuscript in which track changes are not shown.
The original manuscript contained one paper (Winckler et al., 2018) that had not been accepted yet. This manuscript has now been accepted (doi: 10.1029/2018gl080211) and can be made available to the reviewers.

This study endeavors to tease out the differences in the local response to deforestation on surface temperature and near surface air temperature on global scales as derived from an Earth system model and several climate models from the CMIP5 archive. The study uses a clever approach to first estimate non-local effects by considering only non-deforested grid points and producing a map of non-local effects by interpolation on deforested grid points. The local effect is then the difference between the total signal (total change in temperature due to deforestation) and the non-local effect. The main findings are that 1) deforestation mainly results in a non-local cooling and drying of the lowest atmospheric level, T2m and Tsurf with warming in the tropical land regions, 2) local effects are more strong and heterogeneous at the surface, 3) in the mid-latitudes the local response to deforestation of Tsurf and T2m can be of different magnitudes and sometimes even opposite. Authors then also try to explain this opposite local response of Tsurf and T2m in the mid-latitudes but the reasoning does not come across very clearly and in my opinion should be revised with details. Overall, the study proposes a potential new statistical method (based on the author's previous work) to address some previously observed differences between the response to defor- estation of Tsurf and T2m. This is a very important research question pertaining to our understanding of the impacts of deforestation on regional climate. This study points out a very important distinction that should be made while interpreting results from datasets of surface temperature versus near surface air temperature. In this regard the study contributes to current knowledge significantly and so is worthy of consideration. However, several important questions regarding the methodology and physical inter- pretation of the results remain which need to be addressed. I would like the authors to comment on my questions with some further analysis if possible/needed as seen fit by the authors. My comments are rather minor but I recommend publication of the study after another round of revisions which I'll be happy to review.

We are happy that the reviewer considers our study to contribute to current knowledge significantly and is worthy of consideration.

1. ***This is probably outside the scope of the present study but one still questions - what is the mechanism that results in opposite responses of Tsurf and T2m in the mid- latitudes? Can any mechanism be generalized to all such land regions which show opposite responses of dTsurf and dT2m? Probably not because otherwise all land regions between 35 and 55 north as well as south would show the opposite response. The authors do provide an explanation using the model physics and parametrizations (Page 8, line 29) but it is hard to interpret the underlying physics from this argument. Also it is not clear from this argument why such an opposite response will be observed only in the mid-latitudes. I think it will be worthwhile for the authors to include any hypotheses about candidate mechanisms in the manuscript? A bit more explanation in the present manuscript***

is needed if the authors intend to explain this opposite response using the Richardson number, because the argument in its present form is not very clear.

This paragraph had obviously not been clear yet, see comments of the other reviewers. We now hypothesize in the middle of section 3.1 that 'Part of the difference in the response at the surface and near-surface air could be explained by averaging daytime and nighttime response and averaging the response in different seasons', and in the last sentence of section 3.1 that 'for both variables, the annual mean response then depends on the balance between the daytime and nighttime response, and the balance between the responses in different seasons'. Furthermore, we hypothesize that the way T2m is calculated in the MPI-ESM could cause the opposite response of Tsurf and T2m. In the last two paragraphs of section 3.1, we now discuss separately what one could expect in reality (e.g. stronger near-surface vertical mixing during daytime), and how this is taken into account in the model's calculation, separately for Tmax and Tmin.

2. ***a) The cross product between dTlocal and dTnonlocal have been neglected based on some analysis by previous studies. But there are other non-local factors that can impact and couple with dTlocal, for example precipitation changes due to circulation changes corresponding to a particular pattern of deforestation can bring about changes in Tsurf via the surface energy budget. These changes will be counted as non-local because they are not a direct consequence of local deforestation. So this component of dTsurf should be accounted for in the non-local dTsurf which is estimated using neighboring grid points. But the neighboring grid points could have an entirely different land cover which could result in a nonrepresentative non-local dTsurf at deforested grid points - because the surface energy balance in these grid points will be different due to different vegetation types.***
b) So the effect of such a dTsurf can not be obtained from interpolation from neighboring points. How are such non-local effects from changes in variables other than Tsurf, T2m and Tair considered in the methodology?
c) Do the authors think such cross terms will also be negligible as is the case with dTlocal and dTnonlocal? If so can that be explicitly shown?

   a) For our method it does not matter whether other large-scale quantities (besides temperature) undergo changes. For the case of precipitation, we have demonstrated this explicitly in (Winckler et al., 2017a) where in Fig. 3c it can be seen that large-scale deforestation (7 of 8 grid boxes) substantially influences nonlocal changes in precipitation, but this has little influence on the resulting local effects on surface temperature (Fig. 2*a* and *b* in Winckler et al. (2017a)).
   b) Concerning the problem related to surface heterogeneity: To extract the local signal using our method, also the local conditions (albedo, roughness,...) must vary to a good approximation linearly across the neighboring grid cells. The error that is induced by the horizontal interpolation was assessed in a previous study by two simulations in which the deforestation pattern is shifted, and it was found that in most regions the interpolation error is much smaller than the local effects on surface temperature. (e.g., Fig. D2*e* in Winckler et al. (2017a)).
   c) Indeed there are interactions between the local and nonlocal signals. These interactions would be, in our case, lumped with the local effects. We now state this in section 2.1 and explain that the interactions were found to be small across a wide range of deforestation scenarios (Winckler et al., 2017a).

3. ***Page 6, line 7- I hope I understand this correctly – so land cover change is not the only difference between the historical and picontrol simulations? They differ also in terms of changing greenhouse gases? How is this difference going to feedback onto the impacts of deforestation in historical-picontrol? The authors say in the same paragraph that the method assumes that the greenhouse***

gases affect Tsurf and T2m in neighboring grid points in the same way but that will still cause a constant anomaly in the temperature values owing to the greenhouse gas increase. How is that taken care of in the algorithm so that it is similar to the simulations with MPI-ESM? No further analysis is needed. Only a more clear explanation of the experimental design with the CMIP5 models will suffice.

We are sorry that this was not clear in the original version. In section 2.3, we added the following: 'Linear regressions are fitted between temporal changes in temperature (the so-called predictand) and forest cover change (the so-called predictor) within a spatially moving window encompassing 5 x 5 model grid boxes. In the center of this moving window, the local effects are then defined as the temperature change for a hypothetical conversion of 100% forest into 100% open land (given by the slope of the regression) and is by construction largely independent of the changes due to the nonlocal greenhouse gas forcing and nonlocal deforestation effects (given by the y-intercept of the regression).'
The greenhouse gas forcing is a nonlocal forcing and therefore removed by this method. We think it is appropriate to implicitly put the CO2 forcing into the nonlocal effects because in Fig. 3 (for which this method is applied) we focus on the local effects only.

4. ***What type of spatial interpolation technique is used? is it linear or non-linear? Given that the variable field under study could be so heterogeneous (especially Tsurf), it seems that the interpolation technique can have significant impacts on the derived non-local and local fields which can impact the final interpretation of results.***

   We now state in section 2.1 that we use bi-linear interpolation. We are aware that the interpolation of both local and nonlocal effects may cause an interpolation error, but this interpolation error is comparably small in most areas (see Winckler et al. (2017a), Figs. D2 and D3 e) and f) for the interpolation errors in comparable deforestation scenarios but only 30-year simulations). Furthermore, we think that an interpolation error would affect both dTsurf and dT2m in a similar way and thus would not affect our conclusions regarding the *difference* between dTsurf and dT2m.

5. ***What would be the impact of topography and background climate on the interpolated local and non-local signals? Do the authors assume that because an extensive deforestation scenario is considered, the impact of elevation, terrain and background climate on the local and non-local effects is already represented in the deforested simulation?***

   Due to the heterogeneous topography of the land surface, the horizontal interpolation may introduce some error which is generally small (see answer to comment 4). A changing background climate can influence the local effects of deforestation to some extent (see Winckler et al. (2017b) for the change from present-day to RCP8.5 background climate). The only change in background climate in our simulations is caused by nonlocal effects; this change is substantially weaker than the greenhouse-gas-induced change in RCP8.5, so we think that local deforestation effects are not substantially influenced by changing background climate in our simulations.

6. ***As I understand the deforestation in the MPI-ESM simulations has a regular pattern (3 of 4 grid boxes). Although there is nothing intrinsically wrong in choosing such a deforestation pattern, but there is evidence from previous studies that regular deforestation patterns can trigger climatologically important mesoscale effects. Could the chosen deforestation pattern and any subsequent mesoscale effects have an impact on the simulated local dTsurf? Only an insight from the authors***

is requested without any additional analysis.

We think that such mesoscale effects are substantially smaller than local/nonlocal dTsurf itself. First, if changes in mesoscale circulations were important (e.g., increased convection over forest, increased subsidence over grass), we would for example expect a reduction in precipitation for the local effects and an increase in precipitation in the nonlocal effects, or the other way round. But this does not seem to be the case; Precipitation is reduced both locally and non-locally (Winckler et al., 2017a). Second, if changes in mesoscale circulations were important for local dTsurf, we would also expect to see the regular spatial pattern both in the local and nonlocal effects. This does not seem to be the case: In a deforestation scenario with only 1 of 8 grid boxes are deforested (Fig. D1b in Winckler et al. (2017a)), the nonlocal effects are equally strong in grid boxes that are directly adjacent to a deforestation grid box and grid boxes that are only surrounded by other no-deforestation grid boxes.

7. ***Were there any apparent differences in conclusions due to the use of a coupled dynamic ocean model versus the previous studies which used prescribed SSTs? In other words, does a dynamic ocean have a substantial role in deciding the local dTsurf and dT2m responses studied here? I guess a dynamic ocean would be more important for deciding the non-local response. Does this study in conjunction with previous studies throw some light on the role of the ocean in deciding the local and non-local response?***

   The reviewer is right that a dynamic ocean influences mainly the nonlocal effects: in accordance with (Davin and de Noblet-Ducoudre, 2010), most ocean regions show a slight deforestation-induced cooling following large-scale deforestation (their Fig. 3*a*, our Fig. 1) which probably feeds back on the background climate over land regions and causes the nonlocal effects over land to be slightly more cooling/less warming compared to the nonlocal effects in simulations with prescribed sea surface temperatures (see (Winckler et al., 2017a) Fig. D2 and D3 b) for comparable deforestation scenarios). This change in background climate could potentially also affect the local effects. However, also with an interactive ocean the local effects are largely insensitive to the areal extent of deforestation (Winckler et al., 2018), suggesting that the change in background climate, caused even by large-scale deforestation, is too small to substantially affect the local effects on surface temperature.

8. ***Page 6, line 25 – why is the non-local effect cooler and drier?***

   The first paragraph of section 3.1 now contains a more detailed explanation. The atmosphere becomes cooler and drier because locally deforestation reduces the input of sensible and latent heat from the surface into the atmosphere, see also (Winckler et al., 2018).

9. ***When comparing MPI-ESM results with CMIP5 models the authors point out that the similarities in the results could be due to the similarities in the way models estimate T2m (Page 11, line 14). Could there be other ways to test whether the results obtained are independent of the model parametrizations? Could this methodology be repeated with some observed/reanalyzed climate time scale global datasets of Tsurf and T2m? Such an analysis need not necessarily be included in the present manuscript but it will be helpful to know author's insights about using observed data with the same methodology. What would be the challenges in such an analysis?***

   It would be desirable to repeat the analysis with observation-based gridded datasets. However, we are not aware of any such dataset for T2m, which in reanalysis datasets is based on a model or semi-empirical formulas. Thus, the results would again depend on how T2m is calculated for day/night conditions and over different vegetation types in these formulas.

10. ***TECHNICAL COMMENTS***
Page 11 line 7 – remove an extra 'the'

    Done.

11. ***Stippling showing significant differences on the difference maps (Figs 1 and 2) would help.***

    We added stippling for grid boxes where results are not significant at a 5% level according to a student t-test accounting for lag-1 autocorrelation.

12. ***Latitude markers on all maps will be helpful.***

    We added latitude markers on all maps.

References

Davin, E. L. and de Noblet-Ducoudre, N. (2010). Climatic impact of global-scale deforestation: radiative versus nonradiative processes. *Journal of Climate*, 23(1):97–112.

Winckler, J., Reick, C. H., Lejeune, Q., and Pongratz, J. (2018). Nonlocal effects dominate the global mean surface temperature response to the biogeophysical effects of deforestation. *Geophysical Research Letters*.

Winckler, J., Reick, C. H., and Pongratz, J. (2017a). Robust identification of local biogeophysical effects of land-cover change in a global climate model. *Journal of Climate*, 30(3):1159–1176.

Winckler, J., Reick, C. H., and Pongratz, J. (2017b). Why does the locally induced temperature response to land cover change differ across scenarios? *Geophysical Research Letters*, 44:3833–3840.