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Earth System Dynamics
Discussions

Interactive comment on "Different response of surface temperature and air temperature to deforestation in climate models" *by* Johannes Winckler et al.

Anonymous Referee #1

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GENERAL COMMENTS

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 dry-

ing 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 deforestation 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 interpretation 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.

SCIENCE/SPECIFIC COMMENTS

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.

2. 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 grind 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. 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? 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?

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.

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.

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?

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.

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?

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

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?

TECHNICAL COMMENTS 1. Page 11 line 7 – remove an extra 'the'

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

3. Latitude markers on all maps will be helpful.

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