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Earth System Dynamics Discussions

Interactive comment on "Biogeophysical impacts of forestation in Europe: First results from the LUCAS Regional Climate Model intercomparison" *by* Edouard L. Davin et al.

Anonymous Referee #1

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General comments

The study by Davin et al. seeks to understand the impact of vegetation cover across Europe, using multiple terrestrial biosphere models coupled with regional atmospheric models. To test the sensitivity of the coupled models, they carried out theoretical simulations using minimum (i.e. grassland) and maximum forest cover in the domain. The authors found that, except for a few consistent changes, such as increase in albedo in the grassland simulations, the response of the models diverged considerably, and seemed to be more related to differences amongst the terrestrial biosphere models than the atmospheric model. This study provides an interesting insight of the drivers

of divergent responses of coupled models to the same scenarios, and has a potential of becoming an important contribution to our understanding of biosphere-atmosphere interactions at regional scales. However, I think the manuscript needs some improvement and further development in the analysis and the interpretation of the results before it can be published.

First, the current analysis is a bit thin in assessing how atmospheric changes caused by the land cover scenarios are driving the near-surface variability across models. The authors briefly discuss these effects as potentially important, but this could be much more developed and quantified, considering that the authors are using coupled models. Just to cite one example, in the fraction of unexplained variance analysis (Fig. 10), the authors found that albedo and evaporative fraction show little explanatory power during the winter in Scandinavia. I think it should be straightforward to include some atmospheric variables such as cloud cover or incoming radiation, rainfall, or precipitable water as additional predictors. This analysis could help to quantify how surface and indirect atmospheric processes modulate surface temperature, and how this varies across regions and season.

In addition, as the authors noted in the text (Line 96), some combinations of models differ by only a few specific parametrisations. I think the authors could provide more insights on how the model settings and parameters are driving the differences in simulations. For example, between WRFa-NoahMP and WRFb-NoahMP simulations, the only relevant differences are the spin-up period and the sub-grid convection scheme, yet the results for precipitation in the summer are quite different (e.g Fig. 12). In this case, a brief discussion on how Grell and Freitas (2014) and Kain and Fritsch (1990) differ and how the changes in surface could impact the precipitation response given the assumptions of these schemes would be very informative. Likewise, the authors included CCLM simulations with three different land surface models and a similar discussion could be included.

Finally, the authors need to be more careful about the role of scale when discussing the impacts of changes in land cover on the coupled biosphere-atmosphere system. For example, the authors stated that simulated changes in diurnal cycle due to forest/grass cover are the opposite of what previous observations suggest (near line 230). While I agree that this deserves further analysis, we cannot ignore that the impacts of replacing the vegetation of an entire continent on the diurnal cycle of temperature ought to be completely different from the impact of patchy deforestation/afforestation. At least in the tropics, the extent of deforestation in tropical forests may completely change the impact of land cover change in precipitation (e.g. Spracklen et al. 2018, doi:10.1146/annurev-environ-102017-030136, and references therein), and I would imagine that scale would also matter in higher latitudes and in other variables.

Specific and minor comments

Abstract. It would be helpful if the authors quantified their statements. For example, when they say that the albedo decreased with forestation, they could provide the range so readers would be able to judge whether the changes are important or not. This also applies to the range of temperature responses.

Discussion. I found somewhat hard to judge most of the results because little information is provided about how these different models usually perform across Europe. For instance, it would be very helpful to know whether some of them always underestimate rainfall or overestimate evapotranspiration, for example. Ideally the authors could show some model assessment with known benchmarks, but I understand that the model cannot be validated for idealised simulations. One suggestion would be to include one or two paragraphs describing previous studies using these models.

Line 36. Give concrete examples of atmospheric processes that dictate more directly the simulated response.

Line 51. A better justification is needed for the opening sentence. One could still see

some regional effects of land cover and land use change in global models, especially the extreme land cover scenarios used in this study.

Line 110-112. This is fine for the experiment, but the current approach does not completely prevent placing trees in areas naturally dominated by grasses or shrubs like some of the Mediterranean maquis.

Section 3.1. Figures 3-6 are interesting, but I think you could move some of them to the SI, and bring some of the atmospheric figures that could help explaining these differences to the main text (e.g. net radiation or precipitation).

Line 162. One interesting feature is that all CLM runs (CCLM, WRF, RegCM) seem to show increases in ET during the spring, but not during the summer. Could this be an extreme response to drought stress, like the beta factor being too low that stomata are closed for most of the summer?

Lines 165-170. Add references to figures/tables.

Lines 169-170. The last sentence belong to discussion instead of results, and I think this deserves to be expanded a bit more: this is likely to be related to my previous comment. One possibility could be that trees may be transpiring too much during the spring then running out of water during the summer. However, this is not so clear over land in Fig. S11. The strongest negative tendencies seem to be over the Mediterranean Sea and Black Sea. Just to confirm, do the averages in Figures 7–10 exclude grid cells over water?

Lines 185-189; lines 195-196; 212-213. If most of the variance in winter is not explained by albedo and evaporative fraction, then what is causing the variability? Could the variations be attributed to changes in weather patterns? The authors could and should include predictors that were not so surface-centric. The authors suggest that precipitation has no consensus amongst models (unsurprising, this is often more uncertain than other meteorological variables).

Lines 228-232: I do not necessarily see an inconsistency between model and independent observations. Europe is not a grassland continent nor a forest continent, and when an entire continent changes land cover, then the response will be very different from patches of forest next to patches of grassland that are side by side.

Line 242-243. I agree with this sentence, but the authors did not provide any insight on this. The authors could at least use the simulations in which differences are contained to indicate some of the origin of uncertainties. This would make the discussion much more interesting and informative, and go beyond the "models showed little or no agreement" storyline that we often see in model inter-comparison papers.

Table 1.

- I found this table a bit hard to read, I wonder if it would make it easier to separate the atmospheric and land surface settings.
- Was there any reason why the spin-up period was different for the three WRF simulations?
- In the greenhouse gas row, What is the difference between historical and constant? I assume historical means time varying, but this should be clarified, and a citation should be provided. For the simulations with constant values, provide these values, at least for CO_2.

Figures 1-6. What is MMM? I assume that it is the average response. Explain this in the figure captions.

Figure 9. Consider adding a similar panel for the Mediterranean.

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