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Earth System Dynamics
Discussions

Interactive comment on "Biases in the albedo sensitivity to deforestation in CMIP5 models and their impacts on the associated historical Radiative Forcing" *by* Quentin Lejeune et al.

Anonymous Referee #2

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Summary: The authors attempt to train statistical models to extract the albedo of specific land cover classes in CMIP5 models, with the intent to then calculate the albedo change, and associated radiative forcing (RF), due to deforestation over the historical period. The paper is concise, and reasonably well written, although I found the description of the reconstruction methods to be somewhat unclear. The goals of the research are novel and highly relevant for the land surface and climate modelling communities, and I believe that this work will be suitable for publication once several important concerns are addressed.

Major comments:

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Discussion paper

-L158: I found the description of the reconstruction method hard to follow, perhaps because several different types of regression models were being applied simultaneously, and perhaps also because non-technical terms like "big box" were introduced. I think a simple diagram, showing the big box and the target (central) cell, and some of the most important quantities involved in the regression models, would be helpful to better explain the methods.

-L235: The authors use an empirical parametrization to relate changes in surface albedo from deforestation to RF at Top of Atmosphere. While this approach is simple and straightforward, I wonder why the authors could not apply a surface albedo radiative kernel instead, as it removes the assumption of temporal and spatial homogeneity in atmospheric transmittance. Given that both surface and TOA clear sky kernels are publicly available, this minor methodological revision would be efficient and most useful. At the very least, the authors could validate their empirical parametrization against a radiative kernel with sample data. The authors are clear to cite the use of such a parametrization in other work. However, given that the the paper attempts to provide a precise, constrained RF estimate from historical deforestation, removing any limitation associated with such a result would provide a significant improvement to the manuscript.

-L284: In Figure 2, the authors show the reconstructed albedo of crops/grasses over northern Eurasia is essentially the albedo of snow (>0.8). Therefore, I take issue with the authors describing their reconstruction as the "extraction of the correct albedo values of specific land cover". This statement is true for the July reconstruction, but the underlying albedo of the vegetation in January is not 0.8, it is most likely very similar to the July value ($\sim$0.2). Therefore, to avoid any potential for confusing the reader, I would like the authors to describe clearly what is being extracted, which is the surface albedo of grid cells with different *underlying* land cover classes".

I note that the same issue also appears to be present in the reconstructed estimate of January albedos in Figures 6 and 8. Therefore, is it possible using this method to

be certain that the albedo change due to LCC (e.g. Figure 10), and associated RF (Figure 11), is properly separated from the albedo change due to changes in snow accumulation and melt over the historical period?

-L311: In Section 3 the authors perform a validation of the reconstruction, and find errors in the reconstructed albedo in the range 10-40%. They conclude at the end of the Section that this method is appropriate to apply to CMIP5 models. But I would have appreciated a little more rigour in this part of the analysis; for example, the authors should define a priori what an acceptable tolerance of error would be. In other words, define what constitutes a "useful" estimate of albedo, which would provide the reader with a stronger basis for interpreting whether 10-40% error is acceptable. Since the focus later is on RF, perhaps one way to define "useful" is in terms of the perturbation that the uncertainty in albedo estimate passes on to the RF calculation, in energy units?

-L321: The authors find considerable intermodel differences in albedo biases, but I couldn't see any discussion linking the different biases to the underlying satellite-derived vegetation datasets used by each modelling group to calibrate their land models. In the case of CanESM2, it's the GLC2000, whereas for models using CLM it is MODIS. Could the authors investigate whether this difference is a contributing factor to the biases? And if so, perhaps the authors could make recommendations to the community as to which datasets produce the lowest biases?

-L335: The discussion of biases in forested albedo when snow is present reminded me of the work by Thackeray et al. (2015, doi=10.1002/2015JD023325), and Wang et al. (2016, doi=10.1002/2015JD023824). I think that citations and connections to these previous studies would be helpful here to explain your results. In addition, Thackeray (2014, doi=10.1002/2014JD021858) shows that the parameterization of canopy albedo in CLM4 was overly sensitive to temperature, resulting in a seasonal cycle that differed significantly from observations.

Minor comments:

-L82 and throughout: I suggest replacing all occurrences of "associated to" –> "associated with".

-L300: "albedo difference between albedo and crops/grasses" –> should this say "between *trees* and crops/grasses"?

-L305: "remain similar" to what?

-L309: I suggest here citing some previous work on computing the "snow-masking effect of forests", for example by Essery (2013).

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