EGUsphere, referee comment RC1
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Comment on egusphere-2022-923
Anonymous Referee #1

Referee comment on "Mapping of ESA-CCI land cover data to plant functional types for use in the CLASSIC land model" by Libo Wang et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-923-RC1, 2022

We thank Referee #1 for their helpful comments. Our replies to his/her comments are shown in bold below.

General comments:

This study evaluates the impact of uncertainties and biases in plant functional type (PFT) maps that are used as inputs to land surface models. The specific aim is to quantify the impact of a revised PFT map on winter albedo simulations by the Canadian Land Surface Scheme Including Biogeochemical Cycles (CLASSIC) land surface model. The improved PFT map is generated through a multi-step process that combines multiple land cover maps at different spatial and categorical resolutions with ancillary data on tree cover and vegetation height. First, the authors combine two existing land cover maps (North America Land Change Monitoring System, NALCMS; and Virtual Land Cover Engine, VLCE) to produce a harmonized 30 m land cover map for North America with improved categorical precision (i.e., more precise and accurate category labels). Next, the authors perform a direct mapping of classes from this hybrid land cover map onto the CLASSIC PFT scheme, such that each land cover class corresponds to a particular mix of PFTs as represented in CLASSIC. This step is supported by insights from vegetation height data from an airborne LiDAR campaign over parts of Canada. Next, the authors jointly combine the 30 m hybrid land cover dataset above with a 30 m tree cover fraction dataset (based on the Hansen Landsat analysis) to map sub-pixel fractional composition for classes in the European Space Agency (ESA) Climate Change Initiative (CCI) land cover map (300 m spatial resolution). Next, the authors use this analysis to map the ESA-CCI land cover classes onto PFT mixtures as represented in CLASSIC. Since the ESA-CCI dataset is global, this then allows the authors to perform CLASSIC simulations globally (with some corrections based on exploratory analysis of the resulting PFT map). Finally, the authors perform simulations for Canada and Alaska with the CLASSIC model using its original PFT map (GLC2000) and the revised ESA-CCI scheme described above, specifically looking at differences in simulated winter albedo (which is also compared to the MODIS MCD43C3 white-sky albedo product). Results show that albedo predictions are generally more accurate using the new PFT

scheme, though both PFT schemes retain some albedo biases related to model structural errors.

Uncertainty from PFT maps is an important and relevant topic to land surface modeling specifically and Earth Science more generally. The specific impact of PFT maps on albedo simulations is highly relevant to studies of global climate, as albedo feedbacks are one of the most important mechanisms for vegetation impacts on regional and global climate, especially at high latitudes. The land surface model (CLASSIC) and the simulation setup appear appropriate for the research questions about the sensitivity of albedo simulations to PFT maps. The description of the land cover and ancillary datasets is thorough, and the data are well-suited to the study objectives. The implementation of mapping these land cover and ancillary datasets onto an improved PFT map is well-described, well-thought-out, and appears robust. The results are clear and compelling, and the conclusions are appropriate to the scope of the results.

Thank you for your overall positive review of our manuscript.

I have a few suggestions, primarily related to the paper's organization and presentation.

(1) Most importantly, the exact way that PFT fractions are used in CLASSIC, *especially for the physical calculation of albedo*, needs to be explained more clearly (see detailed comment below).

Thank you for noting this. We agree that these information will be helpful for better understand the linkage between biases in PFT fractions and simulated albedo by CLASSIC. We will add these when revising our manuscript.

(2) I found the description of the study's workflow around generating PFT maps (Section 3) confusing and hard to follow; even after multiple reads, I'm not 100% certain exactly what was done or how the pieces fit together. I would suggest adding a more detailed high-level description of what was done at the beginning of Section 3 (the authors should feel free to borrow text from my summary above, assuming it's an accurate reflection of what was done). I would consider a much more detailed version of the flowchart in Figure 2 that indicates exactly which information is flowing where, with reference to the subsections describing that flow of information.

Thank you for your efforts in summarizing the PFT mapping methods above. It is an accurate reflection of what was done. We will include a more detailed high-level description of the workflow (may borrow some of your text) at the beginning of Section 3

and provide a more detailed version of the flowchart in Figure 2 as suggested when revising our manuscript.

(3) I found the somewhat unorthodox structure of the paper --- where both the methods for PFT mapping and the results thereof (in terms of both land cover distributions and simulated albedo) --- to be confusing. I would suggest having a single methods section clearly focused on how the study was done, and a separate results section that in turn is broken down into (a) differences in land cover and PFT maps between the different approaches, and (b) resulting differences in simulated albedo. Somewhat related to this, I would also only keep details that are directly relevant to this analysis in the methods and move asides and mentions of related work to the discussion (or remove them from the paper altogether). This was especially true of the global maps described in Section 3.3 --I read this section expecting to see global simulations and was surprised to see these absent...which is fine --- they are not necessary to the success of the paper --- but adds confusion to what is already a pretty dense paper.

The Methods section in the first draft of the manuscript was relatively short, so we combined the Methods and Results into one Section. However, the Methods section has evolved into a considerable length during the revising/finalizing process. We agree that it is better to put the Methods and Results into two sections. Thank you for pointing this out and for your helpful suggestions on separating them.

The global maps are described in Section 3.3, which provide a reference to Arora et al. (2022). We agree that it would improve the logical flow of the text if moving this and some other discussion to a separate Discussion section. We will incorporate these excellent suggestions when revising our manuscript

(4) A minor suggestion: Somewhere in the introduction and/or discussion, it may be worth explicitly distinguishing several categories of approaches for modeling PFTs: (1) Static, where the PFT for a particular pixel is assigned once, exogenously, and persists over the course of the simulation; (2) Forced, where PFTs are still assigned exogenously but can vary through time (e.g., based on scenarios of land cover/land-use change); and (3) Dynamic, where PFTs compete with each other within a pixel through explicitly represented ecological processes (e.g., see the review of vegetation demography models in Fisher et al. 2018 DOI: 10.1111/gcb.13910). I suspect that the relative sensitivity of model results to input PFT maps will vary across these different model types (though I fully expect all of these model types to be sensitive to input PFT maps!).

Thank you for your suggestion and providing the relevant reference. We agree that it will be helpful to describe explicitly the different approaches for modeling PFTs considering the focus of the manuscript. We will add these when revising our manuscript.

Overall, I found this to be a well-thought-out and well-executed technical study on an important and relevant topic that is presented in an awkward way. My recommendation is for a significant but almost entirely cosmetic and organizational revision.

Detailed comments:

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[L190-195]
This is unclear. How does vegetation heterogeneity --- i.e., the four PFTs used for the physics --- represented in the physics scheme? Are the two sub-grid areas with vegetation (with an without snow) in turn a weighted average of parameters from these 4 PFTs? Or is just one PFT selected for the parameterization? Or are parameters for the physics identical? This is especially important to describe clearly and thoroughly because the interpretation of the results hinges primarily on this component.

In CLASS (the physics module), the albedo of each PFT/vegetation category is calculated separately, first over bare soil and then over a snow pack. The aggregated visible and near-infrared albedos for the bulk canopy are incremented using the current values weighted by the fractional coverage of the vegetation category (Verseghy 1993). We agree that these details will be helpful in understanding the results, which we will add when revising our manuscript.

[L205-210]
Please clearly indicate which configuration was used in this study --- i.e., was the biogeochemistry on or off? Information about whichever configuration was *not* used in the study is extraneous and can be removed.

The biogeochemistry was on in the simulations performed in this study. We will clarify this when revising our manuscript.