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Geoscientific Model Development Discussions

Interactive comment on "Plant functional type classification for Earth System Models: results from the European Space Agency's Land Cover Climate Change Initiative" *by* B. Poulter et al.

Anonymous Referee #1

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Poulter et al present the results of a study on a new EO-product to provide PFT-maps as inputs for ESMs. Such new products are strongly needed and this paper describing the efforts undertaken to derive such products is generally well-written. Altogether, this paper (and the data platform) will fulfill an important need. Having said so, I have a number of comments on both structure as well as on the analysis/presentation of the results. Inclusion of those results would to my opinion result in a better paper.

1. Structure a. Throughout the manuscript verb tense is highly inconsistent and needs checking b. More than half of the abstract is introduction and much less emphasis is given to results and implications. A better balance is needed. c. More explanation is needed to explain how LC_CCI is an improvement of earlier analyses for MODIS, glob-

cover and GLC2000. Moreover, it is not clear to which extent LC_CCI uses insights and algorithms from those earlier efforts and merges some of those in order to make them consistent (as seemed to have been the aim) or entails the development of an entirely new set. In the latter case, why isn't LC_CCI ''just another land cover product" (and how did it ensure consistency?) d. Section 2.1 is partly redundant with parts in the introduction. A better split seems needed. I would suggest moving those text blocks from introduction to section 2.1. e. Section 2.6 reads partly as discussion and is indeed partly repeated in the discussion section. At the same time though, several remarks (e.g. on the distinction between C3 and C4 grasses) in section 2.6 miss nuance (because climate maps tend to map C3 vs C4 grasses very poorly and maps based on species inventories seem to do a better job there), which is partly repaired in the discussion section. f. A table showing estimates of global distributions in comparison to other classifications would have been easier to read than the current section 3.1. g. Section 3.2 and 4.2 are partly redundant. Section 3.2 tends to incorporate discussion on the results, while section 4.2 is mixture of a discussion on differences (as in section 3.2) and challenges (as partly done in 2.6). A better split is needed.

2. Analyses done a. To me, the science presented in this paper is mainly related to the classification decisions presented in Table 2. Based on those decisions, all else follows. Therefore, the decisions taken to derive Table 2 should be the core of the results section, but those decisions are now barely discussed. To which extent are the decisions on partitioning consistent with decisions made when converting IGBP DISCover to JULES and ORCHIDEE PFTs? If different, why? How uncertain are the various estimates (I imagine that if multiple experts are involved, multiple estimates are available) and what are the implications of those uncertainties to the outcome? The authors mention that confidence intervals are available. Also, if the tool is flexible and allows modification, how is consistency ensured? However, it is not explained how those were derived and none of those results are presented. Along the same lines, I would strongly be in favour of a more systematic sensitivity analysis on impacts of choices made for global distributions and consistency. This is why the science occurs and therefore, that

should be analysed. Without any of such information, it is very difficult to interpret the results and the differences (not 'changes' or 'increases/decreases as phrased by the authors) and then it reads as just another land cover product. b. The way how some of the uncertainties are solved, while maintaining (or creating?) consistency in phase 2 needs to be better explained.

3. Presentation/figures a. Figure 2 does not add any information to the text available and I suggest removal. b. Figure 5: why presenting this for ORCHIDEE only and not for the other models? That would be at least as interesting. c. Figure 6: I would prefer the maps (suggested above) over the correlation maps. You do not expect a structural bias (with a given slope <>1) or a different deviation given area. Therefore, presenting it in such a way is distracting. If the maps become available, this figure is redundant.

4. Other comments a. A weakness of the current approach (and the same weakness underlies many current PFT classifications), is that it assumes that structure follows function. This is certainly not always the case. For instance, the biochemical characterisation of PFTs is in many cases not directly related to structure pe se). This is mostly not something to be solved here (as most PFT classifications are prone to the same limitations), but it would merit some discussion. It does, for instance, affect some interpretation and particularly the C3 vs C4 grasses distinction is an example on how structure (as observed by EO) does not follow function. b. There are alternatives to PFTs and optical types is only one of those examples. Other approaches use mapping of traits and species based on database analyses. c. How would the authors suggest ingestion of species inventory data to make C3 vs C4 classification while still being consistent with the rest of the framework? d. I don't understand how the differences in forest threshold between UNLCCS and MODIS can explain the differences in global distribution estimates. I would guess that part of those differences should disappear when using PFT equivalents and its fractional cover. So, why is it still different? To me, that suggests that the conversion from IGBP to (MODIS and) PFTs is not consistent with the conversion from UNLCCS to PFT, whereas mostly the same structural

EO-information is used. That is also why I consider table 2 an important result, meriting discussion. e. I do not see (see remarks 4a and 4b) how semi-deciduousness can be solved by the approach outlined. A better phenology scheme allowing LLS to vary between location and between years for a given PFT would be a much more obvious solution. Moreover, semi-deciduousness is not mentioned anymore in the specific actions of phase 2. Rephrase or remove. f. Likewise, I don't see how herbivory information can help ESMs (the topic at stake here), given that herbivory is hardly ever included in ESMs.

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