Referee #1:
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.
We thank the reviewer for recognizing the relevance of our manuscript and the constructive comments that they have provided.

1. Structure
a. Throughout the manuscript verb tense is highly inconsistent and needs checking
We have checked and corrected verb tense to make sure the grammar is now consistent

b. More than half of the abstract is introduction and much less emphasis is given to results and implications. A better balance is needed.
We have expanded the abstract to include more detail on the results in terms of how the new land cover dataset compares to existing/outdated land cover datasets used by the modeling teams involved with the analysis.

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?)
The LC_CCI product is an improvement over MODIS, GLOBCOVER and GLC2000 because it provides a multi-year classification (GLOBCOVER and GLC2000 are for one year), at high spatial resolution (300 meters versus 500 meters for MODIS) and more detailed thematic resolution (UN LCCS legend compared with the IGBP legend of MODIS). While the accuracy of LC_CCI is similar to GLOBCOVER, GLC2000 and MODIS, we describe how the combined advantages of LC_CCI are the basis for an improved product in the Introduction.

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.
We have modified the Introduction to provide a more balanced description of the LC_CCI methodology.

e. Section 2.6 reads partly as discussion and is indeed partly repeated in the discussion section. At the same time though, several re-marks (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.
We have modified the text in the Methods section to read less like a discussion and be more consistent with a technical/methodological point.

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.
We agree that a Table would be clearer, but the description of the areal distributions is meant to be a rough order of comparison. Because the thematic classes and definitions are different between the MODIS, FAO and LC_CCI, making a 1:1 comparison and interpretation of land cover area is highly subjective, and thus we prefer to leave the estimates in the text as a descriptive analysis.

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.
We have clarified the text to make this split more explicit.

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.
The concern regarding the uncertainty of the classification and cross-walking approach is justified here and Phase 2 of the European Space Agency LC_CCI program is designed to evaluate this uncertainty. Phase 2 began in year 2014 and will end in 2017, and the full uncertainty analysis will be considered during this Phase. The flexibility of the tools lies in the fact that the LC_CCI team provides the cross-walking approach, but that individual users can modify this if they would like to evaluate different assumptions or the underlying uncertainties in the approach provided. These subsequent analyses that take advantage of the flexibility of the conversion tool would be required to make their own documentation, independent of our publication describing the tool. We have extended Section 2.2 to help the reader understand how the methodology was developed and where the uncertainties emerge.

b. The way how some of the uncertainties are solved, while maintaining (or creating?) consistency in phase 2 needs to be better explained.
Please see our previous comment. The uncertainties of the cross walking methodology are being comprehensively evaluated in Phase 2 of the LC_CCI program, which continues into year 2017. However, the data from the Phase 1 of the LC_CCI program are available currently for modeling teams to use and to update their initial conditions in model set up.

3. Presentation/figures
a. Figure 2 does not add any information to the text available and I suggest removal.
Providing the processing chain is useful for readers to gain a clear understanding of how the analysis was made. We prefer to keep this figure in the manuscript.

b. Figure 5: why presenting this for ORCHIDEE only and not for the other models? That would be at least as interesting.
We choose ORCHIDEE as being illustrative of the changes in PFT fractions between the original and the LC_CCI product. Figure 4 provides a comparison of the areal changes in box plot format – the spatial difference maps are quite similar and do not add sufficient new information to justify the additional figures.

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.
The aim for this figure is to highlight the bias between biome and model in a succinct manner using the 1:1 lines as a benchmark. We feel that this figure easily conveys this information to the reader and have clarified in the text to emphasize this point.

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 per 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.
This point is appreciated and addresses one of several issues that are problematic to the PFT concept. We include this point now in the Discussion, using the example of C3 and C4 grasses.

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.
Yes, we agree with this, but at the global scale, trait and species databases are problematic for earth system models. The usage of PFTs is still the most commonly used approach in Earth System modelling.

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?
Temperature thresholds to distinguish C3 and C4 photosynthetic pathways and species are based on species inventory data. We reference the work of Still et al. (Still et al., 2003) to justify these thresholds and their relationship with observations. We clarify this in the text that ground-based observations are used.

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.
The difference in the forest cover threshold used by UNLCCS and MODIS to define forest leads to more forest area in savanna/shrub systems for UNLCCS as compared to MODIS. This difference extends from the original resolution of data through to the 0.5 degree resolution data.

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.
The importance of tropical phenology and its seasonality is very much unresolved and has highly significant implications for drought monitoring, forest vulnerability assessment, carbon cycling and climate science. The simplicity of the PFT concept does not preclude the earth system science community from addressing this issue as has been shown in several studies (De Weirdt et al., 2012; Ichii et al., 2007; Poulter et al., 2009). We have extended the discussion on this topic to reflect the potential for Phase 2 in improving tropical PFTs.

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
Most DGVM models represent pasture as a land use – grazing in pasture lands is simulated as grass harvest when leaf area index reaches some threshold value (Bondeau et al., 2007).