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Earth System Dynamics Discussions

Interactive comment on "Leaf Area Index in Earth System Models: evaluation and projections" *by* N. Mahowald et al.

Anonymous Referee #1

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General comments The authors must be congratulated on analysing a large number of datasets and making a useful evaluation for the ESM community. However there are several issues with the paper.

First of all the paper is plagued with grammatical and syntactical errors, making it awkward to read and repetitive. Many paragraphs need to be completely re-written. I understand how difficult it is for a non-native English speaker to write a paper, but it is no justification for this many errors.

Secondly, I find the evaluation on the historical runs to lack originality. Anav et al. 2013 has already addressed the ability of ESMs at reproducing LAI in the high Northern Hemisphere and Mao et al. 2013 attributed the relevant driving mechanisms to the change in LAI globally. Both papers used the satellite product that the authors in-

cluded in this evaluation. It is hardly surprising to read that models overestimate LAI in the NH, as this has been shown before. I also can't believe the results over the tropics, as satellite has been shown to saturate leading to lower LAI values that reality. Many other comparisons between models and satellite derived LAI are contained here: http://www.mdpi.com/journal/remotesensing/special_issues/monitoring_global

Thirdly, there is no reason why a model that performs well in the historical run also does it for future scenarios. Important factors such as the representation of vegetation dynamics and the effects of nutrient limitation may play a more important role in the future may lead to biases in models that currently perform highly. For example all IPSL modules are ranked highly but non include a full N-cycle module. Another example, models that include prescribed vegetation tend to perform better, but there is no reason to believe ecosystems will remain in the same place over the future. A shift in vegetation may lead to rapid changes in LAI.

Next, only one RCP (8.5) was analysed. With the data been available for all four RCPs I don't see why this was not done. The paper would benefit from comparing the response of LAI to the drivers in the different scenarios (i.e. does LAI response in the same way to climate in all RCPs?)

There are several methodological mistakes. The way the growing season (GS) is calculated is poor. There are plenty of papers that use simple methodologies (e.g. Murray-Tortarolo et al. 2013) that can account for changes in GS trough time. The assumption that precipitation only plays an important role in the three months before the growing season in simply wrong, particularly over the tropics, but also for the boreal forest (where autumn browning has been linked to drought later on the year). The authors claim the results for the correlation of climate and LAI are the same annually than over the GS, but show no evidence for this. The authors' definition of drought based on LAI is simply wrong, drought can only be defined based on climate; additionally low LAI can be driven by fire and disturbance and having drought 1/6 of the time is ecologically implausible.

The inclusion of Kenya in the analysis seems completely out of the blue.

All figures need to be improved as they are poorly and inconsistently formatted.

Generally the paper feels like a collection of preliminary results that have not been properly analysed. A more in depth analyses are needed and simpler graphics and tables would greatly benefit the paper.

Particular comments

Tables

Tables 3, 4a, 4b, 4c are difficult to read as they contain too many metrics. A simpler approach is needed to facilitate the results to the reader. Table 6 is highly irrelevant.

Figures Figures are badly formatted, difficult to read (some I would say impossible) and generally seem to be missing a more in depth-analisis.

Figure 1 has been shown before in the literature many times. Figure 1b seems to be missing parts of the planet. Figure 2 is impossible to read, as are figures 5 and 6. Figure 3 has been shown before in the literature (or similar). Figure 4 does not include all ESMs, not even a ESM that is comparable to CLM. Figures 8-11 are poorly formatted and clearly contain many mistakes (e.g. saturation of the legend). Figure 12 is unreadable. Figure 13 contains is poorly formatted.

Abstract Generally I feel the abstract is poorly written. While it does explain in detail the motivations of the authors, nothing is said on the methodology and the formulation of the main results is very ambiguous. I am also missing the key point of the paper as the last line of the abstract.

Particular comments: Plant Canopy: Canopy is understood as part of the plan community or the ecosystem, not of a single plant. Needs rewording. Objective (3): interannual variability of LAI Lines 21-23: awkwardly written Lines 29-31: last sentence is out of place.

Introduction âĂć Generally the introduction is poorly written and needs to be corrected for grammatical and syntactical errors. âĂć There are also many fundamental theoretical errors (e.g. Line 7: “Carbon Cycle Modules” should state Land Surface Schemes, as it CCM can also refer to ocean; LAI is not a land C variable, but a vegetation parameter.). âĂć I am also missing key literature such as: Anav et al. 2013 J. Climate, Sitch et al. 2015 Biogeosciences and Kala et al. 2014 J. of Hydrometeorology. âĂć Missing the discussion on how LAI is represented on the models (i.e. prescribed vs. dynamic) âĂć Missing all arguments regarding satellite uncertainty (e.g. satellite saturates over high-dense forest, leading to lower LAI estimates)

Methods âĂć There is really no need to explain what CMPI5 is. âĂć The definition of growing season is poor. Other simple approaches lead to better results (e.g. Murray-Tortarolo et al. 2013 remote sensing). âĂć Several paragraphs correspond to the introduction. âĂć Informal English used in many sentences. âĂć The inclusion of CLM (a DGVM is not justified in the introduction), also why not using JULES and ORCHIDEE? The LSM is the same in the coupled and uncoupled runs. âĂć Murray-Tortarolo and Anav et al. 2013 proved that the selection of the LSM is more important for the correct representation of LAI than the climate relationship. Using only one DGVM for comparison is misleading. âĂć The definition of drought is poor. Low LAI can also be driven by fire and disturbance (real-world). Drought cannot be defined based on vegetation but only on climate.

Results âĂć Poorly written âĂć Some results are hardly surprising (e.g. LAI is higher in the tropics) âĂć Can’t believe model overestimation over the tropics. There is no discussion of satellite errors over highly-dense vegetation. âĂć Seasonal cycle is usually defined as max-min LAI. âĂć No discussion of why some models over or under predicts SA, IAV and LAI. Was this related to the inclusion of vegetation dynamics? N-dep? Own climate? âĂć Climate-LAI relationships have been explored in detail before (e.g. Mao et al. 2013) âĂć Murray-Tortarolo does not compare LAI-precipitation metrics. âĂć The analysis of East Africa is out of the blue and not justified or introduced anywhere before. They feel unnecessary for the evaluation of global ESMs. âĂć Thirdly, there is no reason why a model that performs well in the historical run also does it for future scenarios

Summary and conclusions âĂć Repetitive âĂć Not summarizing the main results clearly

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Interactive comment on Earth Syst. Dynam. Discuss., 6, 761, 2015.