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Geoscientific Model Development Discussions

Interactive comment on "Evaluation of a Dynamic Global Vegetation Model using time series of satellite vegetation indices" *by* F. Maignan et al.

Anonymous Referee #1

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*General Comments:

The first of objective of the research, which is to develop a quantitative method for evaluating DGVMs based on satellite data, is both very relevant and timely given the plethora of models and versions of models, and the need to benchmark and compare performances. That being said, however, I think that looking at FPAR by itself would not be a sufficient test (not that the authors are claiming it to be). The bigger significance perhaps would be possibly in adapting the methodology for other parameters of interest, based on specific research objectives, and for other models. In this sense, the paper can potentially have broad impact. The second objective, which is to evaluate versions of ORCHIDEE, is specifically relevant to ORCHIDEE users, but it still informative and useful.

In general, the authors do a good job of explaining and accounting for their results, particularly with regards to the model deficiencies and unexpected outcomes. However, editing and minor additions may help improve clarity, flow and reproducibility.

*Specific Questions:

1. Why have the authors chosen to use a static vegetation distribution rather than a dynamic vegetation distribution (section 2.1, line 17)? As the authors point out in the model description, vegetation responds to climate not just in terms of changes in LAI but also in terms of fractional coverage of their respective grid cells. Using a fixed PFT distribution restricts the model response, and the LAI may act to compensate for the inability to change fractional coverage. Intuitively, one would think that having vegetation respond to climate and competition would be a more "realistic" representation of land surface processes and we would therefore want to test this to correspond to satellite observations. Admittedly, since the objective is to evaluate this particular version of the model using the new technique described and since many modelers do use fixed vegetation anyway, the choice of static vs. dynamic vegetation may not be a crucial issue. However, the text may benefit from a sentence or two just clarifying the rationale behind this choice.

2. The authors cite Hansen et al. (2000) and Heymann et al. (1993) as the basis for deriving the fixed PFT distribution map used in the simulations. It would be more helpful to actually present a figure showing the PFT distribution in terms of fractional coverage, especially since these references are dated earlier than the MODIS data from which NDVI is derived for the evaluation. Over what time period was the input PFT distribution determined? If the NDVI time series covers years 2000-2008, and the vegetation maps used as input to the model are based on earlier references, how sure are we that the PFT distribution used in ORCHIDEE reasonably matches actual PFT distributions over the period of interest for the research? Couldn't this potentially affect the correlations, depending on land cover changes between the period over which the input vegetation maps were determined and during 2000-2008? The authors already state in Section 5

that there were no significant trends in satellite data over 2000-2008 so maybe just a comment on the input PFT distribution and how well it represents 2000-2008 coverage is required.

3. In the Discussions section, line 17, the authors state that they “observe no significant trends over the nine year-period of the study, neither on the satellite data nor on the modeled FPAR.” What about the meteorological forcing data? If there was no significant trend in the meteorological forcing data that would cause changes in PFT fractional cover in a version with dynamic vegetation, then this would be additional justification for just using static vegetation.

4. Not being an ORCHIDEE user myself, I am just curious as to why NEP is used as an indicator of steady-state equilibrium (Section 2.1.2, line 25 onwards) rather than Net Ecosystem Exchange (=-(NEP-fire flux)) as is done for LPJ, Community Land Model DGVM, CNDV. Does ORCHIDEE not calculate fire flux? In addition, what forcing is used during the spin-up to reduce the decadal mean value of NEP?

5. Could the authors speak a little more about the significance and purpose of comparing the results of two different meteorological forcings? I am not sure that the full added value of this section is clear to me – it just seems that modelers can use a variety of forcings depending on their purpose. For example, I might use meteorological forcings describing a paleoclimate or future SRES or RCP. So why make a comparison between CRU-NCEP and ERA-Interim forcings, in particular? Aside from the impact of spatial resolution, how can the findings from this comparison be useful for researchers utilizing different meteorological input? Is the purpose to test the sensitivity or stability of the model between different forcings with the new phenology scheme?

6. In Section 4.2, Evaluation of meteorological forcings, the paper states in line 13 “as the simulations have two different vegetation maps…” This part slightly is confusing to me. Why do ERA-I and CRU-NCEP have different vegetation maps – is it because of interpolation to their respective spatial resolution despite being based on the same PFT

distribution? The authors cite Jung (2007) as justification for having different vegetation maps but perhaps in supplemental material, these maps can be presented with some discussion of their differences.

7. In the section “Scoring of PFT” line 7, the paper says “we have used a high-resolution vegetation map to identify the dominant PFT…” Which map is being used here – is it the same one as was used to develop the PFT distribution? Perhaps a reference is needed here, and maybe just something to state or show that this map reasonably corresponds with actual vegetation distribution from 2000-2008.

8. One rationale for doing this study as stated in the Introduction is because DGVMs are used to address the question of the impact of increased CO2. However, the rest of the manuscript makes no mention of CO2. Is it kept at a fixed value? At what ppmv? Can the correlation scores be partially explained by any effect of increasing CO2 over the time period 2000-2008 not captured in the model but reflected in satellite data?

*Technical Comments:

1. I appreciate that the authors try to make the methodology clearer by illustrating it as they do in Section 3, but in some parts I find that it detracts from the flow of the paper. For example, section 3.3 already presents important results from the ERA-Interim simulation regarding areas where the correlations are high and where they are low, and discusses the deficiencies in the model to account for the latter. However, these may better be presented and discussed more systematically in Sections 4 and 5, including also the correlation map for the other meteorological forcing. The authors might consider restructuring the paper slightly to improve organization and flow.

2. Figure 1 label reads “a mean annual cycle for both the NDVI and FPAR signals is derived (middle right plot)” but you are referring to the plot on the left of the middle panel right?