

## ***Interactive comment on “Improving the dynamics of northern vegetation in the ORCHIDEE ecosystem model” by D. Zhu et al.***

**Anonymous Referee #1**

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### **1 General Comments**

This paper provides a description of revisions to the ORCHIDEE-HL (high latitude) land surface model intended to improve the simulation of Northern Hemisphere vegetation cover. The results are evaluated against several fractional land cover datasets and gridded observations of GPP, biomass and soil carbon. The authors claim “significant improvements” in simulated tree distributions and this appears to be justified. A particularly strength of the paper is that simulated PFT fractions are compared with multiple observational estimates, which takes into account the combined uncertainty in the source data and in the mapping from land cover classes to model PFTs. This allows the authors to place an informed emphasis on model errors and improvements

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in different regions.

The manuscript is well-written throughout and the figures are clear and understandable. With only a couple of exceptions, details of the model description that were not provided explicitly in the manuscript were found easily in the references provided (e.g., Krinner et al, Gouttevin et al).

### **2 Specific Comments**

1. Despite being well used, it's not clear to me whether the 6-hourly CRU-NCEP forcing resolves the diurnal cycle adequately. In particular, the simulation of photosynthesis will depend strongly on the sub-daily representation of surface insolation. How are the forcing data downscaled from 6 hours to the 30 minute model time step? If these forcing fields are valid at the same UTC time rather than the same local solar time, is there any significant longitudinal variation in how well the diurnal cycles of insolation and GPP are represented?
2. The  $\beta$  diversity metric shows well the improvement in the high latitude tundra (Fig 5), but it doesn't highlight the greatly improved tree PFT fractions in northern Europe and eastern Canada. I would have expected this improvement between simulations to be more apparent in the metric, especially in the mean  $\beta$  given the agreement between the observational datasets in these regions (Fig 3). It is more visible in the skill score (Fig 6) so, are there model errors and improvements that we should not expect to be able to evaluate through the use of this metric?
3. The authors highlight that these metrics ( $\beta$ ,  $D$  and  $S$ ) provide a framework that could be used by other models, and this type of multi-dataset analysis should undoubtedly be done in other studies. But how resolution dependent are these metrics likely to be? This would be a tradeoff between the smoothing of coarser

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grids making it easier for a model to match observations, but also easier for observations to match each other. So would it be reasonable to compare models using significantly different grids? Could I calculate values for another model and compare them fairly with those in Table 3?

4. In the sensitivity experiments one piece of information that I couldn't glean was how do variations in the "1850" forest cover and GPP owing to spin up methodology (e.g., 1901 vs 1914, Fig 14) compare with the magnitude of 20th century change in the NEW and OLD simulations? Section 6.2 quotes 11.5% and 4.8% 20N-90N forest fraction increases with and without CO2 fertilisation, but it is difficult to compare these aggregate figures with the maps in Fig 14. This would provide some context for the warnings about spin up methodology. Also in section 6.3, the apparent motivation for the individual year simulations ("...recycled one-year climatic data are sometime used...") appears near the end after the results. It would be clearer if this was mentioned earlier in the section.

### 3 Technical Comments

Title: "...northern..." is a bit too vague. "...Northern Hemisphere high latitude..." would be more informative (and would reflect the model version).

P2219,L20: The repository that "rev1322" corresponds to isn't mentioned until Section 2.3.

P2220,L13-15: It's not clear if  $V$  can be negative, e.g., though net biomass loss, which makes the range of possible  $M_{BG}$  values unclear.

P2222,L21: " $M_{SF}(t)$ " should be " $M_{SF}(t, T_{min})$ ", if I've interpreted the model correctly.

P2224,L8: Kuppel et al (2012) references a PhD thesis; can the same information be gleaned from Kuppel et al (2012), Biogeosciences, doi:10.5194/bg-9-3757-2012 ? If

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so, the latter reference is preferable.

P2224,L19-21: It's not clear whether the leaf age dependency was switched off entirely or whether just very long time constant ( $a_{crit}$ ) was used. The values in Table 1 for evergreen needleleaf are unchanged from Krinner et al, so is  $a_{crit}$  used elsewhere in the model? If not, why quote unused  $a_{crit}$  values at all?

P2230,L3 L9: (Equation pedantry) The sum should be from " $k = 1$ " rather than just " $k$ ".

P2230,L19: Similarly, the sums are missing upper limits.

P2232,L26: Should be  $\sigma_O$  rather than  $\sigma_O$ .

P2233,L1: Are there missing modulus symbols, i.e.,  $|X_{c,M} - X_{c,O}| < \sigma_O$ ?

P2237,L7: "SG" should be " $S_G$ ".

Fig 2: "Brighter colors..." is ambiguous wording, "Deeper colors..." would be better. Should "...relative fraction..." be just "...fraction..." , else it's not clear what it's relative to?

Fig 2 4: I find it difficult to determine how deep or pale these maps are relative to each other (e.g., OSIB vs IIASA). A limited scale (e.g., 25%,50%,75%,100%) for the pure RGB hues would be useful.

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