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Geoscientific Model Development Discussions

Interactive comment on "PALADYN v1.0, a comprehensive land surface–vegetation–carbon cycle model of intermediate complexity" *by* Matteo Willeit and Andrey Ganopolski

Anonymous Referee #1

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The authors developed a comprehensive land model of intermediate complexity for long-term simulations and paleoclimate studies. The model descriptions provided enough details for readers to understand the model, and the overall global offline model evaluations using a variety of datasets demonstrated the adequate performance of the model. This manuscript can be accepted after addressing the following concerns.

Specific comments/suggestions:

L186: Does eq. (5) converges to eq. (4) (bare soil) when canopies (e.g., LAI+SAI approach zero) disappear? It is also unclear how the stomatal resistance rs is dependent on LAI in the model formulations Eqs. (33), (69), (70).

L432: The effects of snow metamorphism and snow melting on snow density are neglected in the model. Is this the main reason for the model's deficiency in the snow simulation (e.g., Figs. 13-15)?

L753: While the equilibrium spin-up mode is fine, the authors should at least test it against the actual spin-up for "at least 10,000 years" (say, 50,000 years). If global testing is too time-consuming, the authors could pick up a few model boxes over different climate regimes for the test.

L834: For biogeochemistry, the authors should compare the model LAI with MODIS LAI (e.g., the seasonal cycle over different latitudes with limited crop coverages). After all, MODIS LAI is one of the most reliable vegetation datasets.

L845: The value of comparing potential vegetation from one model to another in Fig. 19 seems to be limited. It may be more useful to do a comparison similar to that in Zeng et al. (2008, e.g., their Fig. 10).

L762 (Section 8: Evaluation): The authors did an excellent job in using comprehensive datasets in model evaluations. However, most of the discussions were quite qualitative. The authors should compute some simple quantities (e.g., root mean square difference, correlation coefficient, mean bias, . . .) for some of the comparisons for two purposes: to back up the qualitative statements, and for other groups to compare their models' performance against the PALADYN v1.0 model in the future. For instance, there seems to be years with opposite anomaly signs in Fig. 18 (L840), and the authors should at least compute the simple correlation coefficient to quantify the agreement.

Minor comments:

L52: It is appropriate to cite Dai et al. (2013) here

L418: Where and when does "frozen water runoff" occur?

L815: Another reason is the assumption of global uniform soil porosity in the model.

L820: The agreement of wetland areas between the model and multi-satellite data is not very good in spatial distribution (Fig. 11). Please comment.

L852: Explain “flux weighted discrimation”

Table 5: Canopy diffuse snow-free albedos (0.005 for vis and 0.154 for nir) for needle-leaf trees seem to be too small. In addition, does the model consider evergreen versus deciduous trees for LAImin = 1 and LAImax = 9 (broadleaf) or 7 (needleleaf)?

References:

Dai, Y., and 11 coauthors, 2003: The Common Land Model. Bull. Amer. Meteor. Soc., 84, 1013-1023.

Zeng, X. D., X. Zeng, and M. Barlage, 2008: Growing temperate shrubs over arid and semiarid regions in the Community Land Model–Dynamic Global Vegetation Model, Global Biogeochem. Cycles, 22, GB3003, doi:10.1029/2007GB003014

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