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 #2

Received and published: 20 July 2016

Comments on "PALADYN 1.0, a comprehensive land surface-vegetation-carbon cycle model of intermediate complexity" submitted by Matteo Willeit and Andrey Ganopolski to Geoscientific Model Development

General comments

In this manuscript, the authors presented a new integrated terrestrial model, PALADYN, which includes major physical and biogeochemical processes at an intermediate complexity. The model was developed on the basis of previous models such as LPJ and TRIFFID but includes several recent findings such as new stomatal conductance model. Although many terrestrial models for similar purposes have been developed, this model has several unique and intriguing features. In particular, inclusion of peat

land and permafrost carbon dynamics will allow the model to simulate long-term (e.g., glacial time scale) simulations as proposed by the authors. This manuscript includes more than hundred equations, many tables, and schematic diagrams to fully describe the model.

To demonstrate the model performance, the authors compared major terrestrial variables with contemporary observational datasets. Overall, these results show good performance of the model, but the authors provided only very brief explanations. Although I agree that scientific insights are not necessarily included into the manuscript, I recommend clarifying the characteristics of the PALADYN model, especially in comparison with other models. Therefore, I conclude that the manuscript needs minor revision before acceptance for publication. Please look my specific comments for details.

Specific comments

Line 103: It seems that the model don't have a separate type for crops. Do you have an idea to include croplands to account for agriculture?

Line 171: In Eq.(2), the symbol sigma seems to represent Stefan-Boltzman constant, but no definition was provided.

Line 286: Vegetation height hv is later estimated by Eq. (81). It is helpful for readers to explain this at this point.

Line 397: In Eq. (42), what kind of phenomenon does the last turnover term represent? Stem flow?

Line 456: It seems that Figure 2 does not include the surface runoff Rw. Can you include Rw into Figure 2?

Line 566: Is this the single-sided (or projected) specific leaf area?

Line 606: The statement is at least partially incorrect. In East Siberia, a broad area of forest is dominated by larch, a deciduous needleleaf species.

Line 745: How did you determine the stable and radiocarbon isotope ratios of the atmospheric CO_2? As you know, it has been changed by the Suess effect.

Line 772: "Manua" should be replaced by "Mauna".

Line 821: In terms of wetland extent, the model estimate seems to underestimate in Southeast Asia. I guess that the GIEMS data includes a substantial fraction of paddy fields. Is it correct?

Line 852: Can you say something about the simulated discrimination in relation to C3 and C4 plant distribution? Do you confirm that distribution and functional contribution of C3 and C4 plants were reasonably simulated?