

Interactive comment on “A vertically discretised canopy description for ORCHIDEE (SVN r2290) and the modifications to the energy, water and carbon fluxes” by K. Naudts et al.

Anonymous Referee #2

Received and published: 3 January 2015

General comments

Naudts et al. present a description and validation on the ORCHIDEE-CAN model, which is a new version of the standard ORCHIDEE model, a widely used global vegetation model. The amount of improvements that the authors introduced to the model is impressive. I have been using the standard ORCHIDEE model myself since several years, and the changes to the model code presented in this manuscript reflect an enormous amount of coding work. Nevertheless, the authors succeed to present this complex effort in a very clear and transparent way. The text is written very clearly, and the model equations are presented rigorously in a very consistent way. This makes this manuscript very useful, not only for the extensive community of ORCHIDEE users, but

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also for the wider community of vegetation modelers. After reading the manuscript I felt very motivated to start using the model myself, because several limitations of the standard model are tackled (e.g. the parameter that caps the max LAI). This paper has a lot of value as “model description paper”. However, I have several important comments that need to be addressed – in my opinion- before this manuscript can be accepted for GMD.

1. This paper is part of a series of papers that are currently under discussion or submitted (e.g. MvGrath et al.; Ryder et al.; ...), all these papers are related to the different aspects of the developed ORCHIDEE-CAN model. This fact is not a problem at all for me, because I understand that the variety of aspects covered in the series of papers, can never be covered in one manuscript. However I think that the motivation of the Naudts et al. manuscript (and the relation of this paper to the other submitted manuscripts) should be presented better in the introduction of the paper.

2. At the end of the introduction I missed a paragraph that clarifies the specific goals of the paper. I did not read any research questions. The paper appeared to me as a real model description/validation paper. It would be good to state this very clearly at the end of the introduction. In order to tell the reader what she/he can expect. For me this is not a hypothesis-drive research paper, but rather a model description paper. The last paragraph of the current introduction nicely motivates why the model development is needed. Adding 1 paragraph that tells the reader what will exactly be presented in the paper should solve my comment. This paragraph should also contain a sentence that the manuscript is focusing on temperate and boreal forests in Europe, and that tropical forest are not treated (yet) in this manuscript.

3. The validation –which is a very important part for a manuscript of this type– is visually presented in a very condensed way (only 2 figures). I think the validity of the model would be much more convincing for the readers of GMD if some additional graphs would be shown, to illustrate what is discussed in the validation section of the manuscript. Some maps illustrating the spatial variability of some key model outputs

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would be very informative, both for cases where ORCHIDEE-CAN performs very good or for cases where the model performs not good (e.g. the spatial variability of height and basal area).

4. The assumption on the circumference class distribution, using the “target distribution” (page 8594) worries me. I have the feeling that this kind of assumption can have a large effect on the simulated results. This is an essential point for a model that attempts to simulate the impacts of forest management on the climate. It remains unclear how sensitive the model is for this assumption. The authors need to elaborate more on this point.

5. The authors explain in detail how the model parameter values were determined for Europe (section 4.2), but the manuscript is lacking a discussion on how the model can be applied globally. It would be interesting to see how ORCHIDEE-CAN performs over Europe if only the original MTC classification was used. This would allow estimating how important the specific parameter value settings are to get good results over Europe.

6. Probably my most important general remark deals with the “tuning” of some of the parameters of the model. The authors are not explicit about this tuning process. For example (1) the kls parameter was “tuned to match observations of GPP, ET and LAI”; how was this tuning performed? With an optimization procedure? Were the different observation weighed in this process? (2) The basal rate of autotrophic respiration was optimized using “a rigorous statistical framework (Tarantola 2005)”: how was this framework implemented and used for the ORCHIDEE-CAN model? Was this done in a similar way as this has been done in the past for the standard ORCHIDEE model (Santaren et al. 2007 GBC)? (3) the allocation scheme required parameter values from fitting regression models to inventory data. (4) on page 8599 the authors mention posterior and prior standard deviations. Is this “posterior” after optimization? What kind of optimization? (5) page 8602, “kalfa1 and kbeta1 were estimated by fitting”... It is really needed that the authors are explicit on how this tuning process was performed,

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in order to judge the quality of the presented simulation results. An additional question is what the order was of the different tuning steps? If you tune parameter x against datasets B and C and after that you tune parameter y against dataset C, you might get another result compared to a situation where you fit y first and x after that . . . I am aware that a “perfect” tuning procedure does not exist for a very complex model like ORCHIDEE-CAN. However, for me the parameter optimization phase is a very important step in model development. Therefore the authors need to bring the description of their parameter tuning at the same high level as their model description in this paper. The tuning of the parameters should be described in a transparent way. Maybe a scheme describing the process and the used datasets will help to do this. In addition, in the validation section I miss a discussion on the impact of the tuning process on the performance of the model for Europe. An independent validation for North America, without tuning, would shed an interesting light on this issue.

Specific comments 1. Page 8574, line 23: I was a bit confused by the statement that the radiation scheme (which is used in this manuscript I assume) is not “available” yet. . .

2. Page 8574, line 26: the energy budget is calculated “implicitly”: what does this mean?

3. Page 8574, line 27: you mention the time step for the first time here. I suggest mentioning the time step of the model early in section 2. This is important information for readers not familiar with ORCHIDEE.

4. Page 8576, line 6: “biomass and soil carbon pools are mixed”: what does this mean?

5. Page 8576, line 28: what are the two additional pools that you introduced? (are these the branches and coarse roots?, this was not completely clear for me).

6. Page 8577, line 9: maintenance respiration is a function of nitrogen concentration. Does this mean that the ORCHIDEE-CAN model contains the N-cycle of the O-CN

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model? This is not clear from the current text.

7. Page 8579, lines 16-19: "... the same parameter values ..." the same as what? This sentence was confusing for me.

8. Page 8581: line 15-17: This is a very important statement; it should be included at the start of section 3.1.

9. Page 8586, title 3.4: here and elsewhere in the text the authors use "albedo" when talking about the entire radiation scheme. I suggest to use the wording "radiation scheme for tall canopies" here.

10. Page 8645, fig 1: I suggest to rename the box "albedo" and make it "radiation scheme"

11. Page 8587, Line 11: "similar equations" are used for diffuse sources. Are these equations explained in another publication? These equations should be available for the reader, as accounting for diffuse radiation is an important issue which is lacking in the standard ORCHIDEE version.

12. Page 8587, line 21: what is the impact of the assumption of an extinction coefficient of 0.5? this is an important assumption with probably a strong impact on the results and the model performance. I suggest adding some discussion on this assumption.

13. Page 8592. I found the wording to describe the mortality a bit strange and sometimes confusing. "A whole PFT is now killed": what does this mean? All trees belonging to 1 PFT within 1 grid cell? "when it comes to time to actually kill the trees": a strange formulation. "We mainly kill the smallest trees" (page 8593): I would reformulate such statements, for example "we assume that mainly the smallest trees die". In addition, the meaning of the word "mainly" is too vague in this sentence. . .

14. Page 8592, line 19: I suppose it should be "leaf, carbohydrate reserve, and labile pools". I assume that mortality takes place when all these pools are depleted.

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15. Page 8593, line 4, line 6: at this point the actual time step of the mortality processes in the model was not clear for me. This could be resolved by adding a paragraph that clearly explains the adopted time steps for the different processes in the model.

16. Page 8601, line 24-28: this piece of text belongs to the model description section.

17. Page 8604, line 10: it was a bit disappointing to read at this point that the multilayer energy budget was not used for validation in this manuscript. I understand why you did not do this. I suggest mentioning earlier in the text (maybe end of intro) that the validation is restricted to the single layer energy budget.

Technical comments

1. Page 8567, line 14: ‘occurred’ is a bit strange wording according to me.

2. Page 8568, line 6-8: add a reference for this statement

3. Page 8570, line 25: 3000 simulation years

4. Page 8571 line 16: “the largest gains were realized . . .”: please add a reference for this statement.

5. Page 8571, line 26: Pretzsch (2009)

6. Page 8572, line 17: it is not clear what “the latter” is pointing at. (“resistance” in general or “sapwood resistance”?).

7. Page 8572, line 28: you mention the kvcmax parameter for the first time here, without explaining what it means, and you haven’t referred to the Table with parameters at this point in the text yet.

8. Page 8572, line 28: you mention for the first time the word ‘trunk’. I think you should explain here that the trunk is the “standard” version of ORCHIDEE. Otherwise this might be confusing for the reader.

9. Page 8580, eq 15: kalfa1, kbeta1: these parameters where not explained in the text.

10. Page 8582, eq 20: $\rho\Delta$ is represented differently in the equation compared to the subsequent sentence (where the greek symbol for delta is used).
11. Page 8585, line 21: I do not see an “overbar” in eq 29.
12. Page 8590, line 16: “may be calculated” is it calculated according to Ball et al. or not?
13. Page 8590, line 18: “transport of sensible heat flux”. In my view this formulation is a bit awkward. I suggest to say “sensible heat flux” or “transport of sensible heat”. A “flux” always implies transport.
14. Page 8591, line 1: “transport of latent heat flux” same remark as above.
15. Page 8596, line 26: “12 forest species (groups)”
16. Page 8598, line 12: “hydrology”
17. Page 8605, line 12: “Deleuze and Dhote” add publication year.
18. Page 8634: there is no reference to Table 2 in the text.
19. Page 8635, dcirc: I suppose this is “stem circumference”? I suggest to add “stem”
20. Page 8637: I suppose reference to other tables are wrong: 3 should be 4, 4 should be 5.
21. Page 8640: something is wrong with the columns in table 3
22. Page 8641, table 4: “kresid” should be “ktresid”.
23. Page 8646, fig 2: mention in the caption that the greyscale is corresponding to the symbols. (same for fig 3)

Interactive comment on Geosci. Model Dev. Discuss., 7, 8565, 2014.

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