We would like to thank both reviewers for their useful comments on the manuscript. Both reviewers touched on several long standing discussions in land surface modelling, i.e., validation, optimization and the use of PFTs. We make use of the open discussion to better justify the choices we made but we also decided to keep those justification as concise as possible in the manuscript itself. The reason for this is that we prefer to focus on the ORCHIDEE-CAN developments rather than providing a more general discussion on the current opinions and issues in land surface modelling. As an outcome of this approach, the review comments are largely addressed through new and revised figures and tables (which are the result of several additional simulations), whereas substantial changes and edits to the text are limited to a few sections. We believe, however, that with this approach we were able to address most of the reviewers' comments.

The page and line numbers where changes have been made to the manuscript are included at the end of each reply and the lines are indicated in green in the revised manuscript.

Response to referee 1

General comments

<u>Comment 1</u>
This manuscript presented a new version of ORCHIDEE including a new canopy scheme (-CAN) applicable to forest management simulations. As claimed by the authors, forest management is one of the most important and uncertain factors in the global terrestrial carbon cycle, affecting the future climate change. For example, recent carbon-related debates on climate change mitigation, such as REDD+ and BECCS, are deeply related to forest management. The development of the ORCHIDEE-CAN seems to be a great progress toward taking account of forest management in global terrestrial models. The manuscript seems a bit lengthy, because model structure, parameterization, and validation were described separately for each process such as allocation and energy budget. I recommend revising the structure of manuscript in a more simplified manner.

We agree that the manuscript is rather lengthy and felt this is partially due to the extensive tables. We therefore moved the tables with the parameter values (Table 3, 4, 5 and 6) to the supplementary material. The approach and data differ substantially between the sections on model structure, parametrization and validation which seems to justify treating these items in separate sections. A structure that repeats the same subsections for model structure, parametrization and validation where possible, enables a detailed description in an easily navigable structure for the reader. In addition, integrating all information in one coherent paper rather than splitting it into several papers, avoids duplication of the introduction, cross references and leaving it up to the reader to search and combine information from different sources.

<u>Comment 2</u>
Another concern on the manuscript is that more validation (e.g., site-level comparison with filed data) would be necessary for this kind of study. Also, an intimate comparison with other models (e.g., ORCHIDEE-trunk and ORCHIDEE-FM by Ballassen et al.) is strongly recommended to clarify characteristics of the new model. Although the authors showed region-level, snap-shot comparisons with data, I could not be convinced that the new model simulates long-term transition of carbon budget including the impacts of human and natural disturbances. In conclusion, this manuscript needs major revision before being accepted for publication.

We would like to thank the reviewer for this comment as it gives us the opportunity to add some *details to the validation but also to discuss some more philosophical issues of model validation.*

As stated in the conclusions of the manuscript, we recognize that additional validation (and parametrization) is recommended for other regional applications, higher resolution studies and/or studies with a different objective (for example studies that relate to croplands or grasslands). Additional validation will be the subject of follow-up studies (see for example Ryder et al.2014), and are not the focus of the current manuscript. Nevertheless we added more detail to the large-scale validation by performing additional testing:

- *We performed an additional validation at higher resolution by comparing simulated basal area and tree diameter at the species level with observed site-level data (depending on the species 639-8227 plots) over France (Figure 6, section 5.7).*
- *To give the reader more insight in the behaviour of the model regarding the impact of human disturbances we added an evaluation of the impact of different forest management strategies at the pixel level (Figure 5, section 5.7).*
- *A test was added in which the effect of species-level parametrization versus MTC parametrization was assessed (Figure S2, section 5.1)*
- *Maps showing the differences between observed and simulated GPP and basal area are shown (Figure 4)*

Although a comparison between ORCHIDEE-CAN and previous versions of ORCHIDEE seems straightforward, it cannot be performed for many of the new features which were the objective of these developments. This problem is already apparent from the European-wide validation where we could validate seven variables for ORCHIDEE-CAN of which only four exist in ORCHIDEE-trunk (Figure 3). In addition many of the more fundamental changes (i.e. the layering of photosynthesis, the implementation of water stress and the overhaul of the allocation scheme) make it impossible to assign differences in model version to specific changes. Recoding the model in a more modular way where changes can be switched on and off would not overcome this issue because in the case of the new albedo scheme the changes to allocation are essential to the albedo calculations. Similar code dependencies exist for water stress and allocation, albedo-photosynthesis, forestry-allocation, and water stress and photosynthesis (Figure 1).

Given these limitations we deemed it more important to document the performance of the model against observation-based spatially explicit product than against other model versions (Figures 2, 4 and 6). After all, the presented validation over Europe is performed on the last ten years of 160 years of simulation for biogeochemical fluxes, biophysical fluxes and forest management related vegetation characteristics (section 5). The results show the ability of the model to sufficiently capture large-scale spatial patterns and interannual variability over Europe. Where the comparison can be made, ORCHIDEE-CAN outperforms the trunk over Europe for four variables (Figure 2 and 3, Table 3), where at the same time empirical approaches were replaced by more process-based approaches. The fact that a more process-based code could outperform ORCHIDEE-trunk made us conclude that the model performs well enough for a first forest-related application over Europe.

Specific comments

Page 8568 Line 3–4 In addition to Dixon et al. (1994), Pan et al. (2011, Science 333, 988–993) should be cited.
The citation was added.(Line 26)

Page 8568 Line 18 By definition of albedo, “darkness” should be replaced by “reflectance” or “whiteness”.
This was changed.(Line 39)

Page 8569 Line 5–6 The statement “Vegetated surfaces are sub-divided in patches of different plant functional types” would be incorrect. Several DGVMs (e.g., LPJ-Guess and SEIB-DGVM) have individual-based vegetation dynamics.
The statement was adjusted.(Line 51)

Page 8571 Line 11–12 “to confirm convergence” Do you mean stabilization of carbon budget or agreement between the different spin-up methods? This statement is ambiguous and I recommend clarifying.
The statement was clarified.(Line 110)

Page 8578 Line 5 For “sapwood area”, do you mean “sapwood cross-section area”? Please clarify.
The statement was clarified.(Line 290)

Page 8580 Line 10 Eq. (15) Please add a reference for Eq. (15).
A reference was added. (Line 335)

Page 8581 Line 24 A recent study (Sun et al. 2014, PNAS 111, 15774–15779) implied that leaf mesophyll conductance in DGVMs. Your new model, ORCHIDEE-CAN, does not include mesophyll control in the hydraulic architecture. Is it correct? If so, please discuss potential uncertainties due to neglect of the mesophyll conductance.
Although the photosynthesis model and its implementation could easily account for mesophyll conductance (Yin & Struik, 2009) leaf mesophyll conductance is indeed not accounted for in our photosynthesis model in order to maintain compatibility between the model formulation and its parametrization This is explained in the manuscript, and the potential implication of neglecting mesophyll conductance is discussed (Lines 594-603).

Page 8584 Line 14–16 What kind of numerical method was used to get a solution by iteration (e.g., the Newton-Raphson method)?
A stationary iterative method was used. This information was added to the manuscript.(Line 419)

Page 8585 Line 21–22 No symbol in Eq. (29) has overbar. Please check.
The overbar was added to the equation (Equation 29).

Page 8590 Line 16–17 “Rs,i ... may be calculated using...”. What do you mean for “may be calculated”? Do you mean "was calculated"?
The suggested change was made (Line 558)

Page 8591 Line 20 Remove “)” after “model”.
The additional bracket was removed.

Page 8593 Line 4 No natural disturbance such as wildfire, insect outbreak, and windthrow, was explicitly included in the present model. It is correct?
Yes, this is correct. These natural disturbances are all assumed to be included in natural mortality. It is explained in the manuscript on lines 656-664.

Page 8597 Line 25 Can you give a list of the 126 site observations as a supplementary material?
The list of sites and their meta-data is given in Campioli et al. (in prep).

Page 8597 Line 28 What is the definition of “acceptable GPP”? Please explain. Otherwise, replace “acceptable GPP” by other words such as “GPP sufficiently close to observations”.

This section has been revised to address comment 6 of reviewer 2 and the statement was removed from the text.

Page 8604 Line 11–13 I agree the statement that there is no data-derived net carbon flux data. However, there should be soil carbon stock maps (e.g., Harmonized World Soil Database) that can be used for model validation. In this regard, there is a need for carbon spin-up.

Thank you for this suggestion, Soil carbon is indeed an essential variable when studying the carbon pools, fluxes and their budget. However, the soil carbon module within ORCHIDEE-CAN was not changed compared to ORCHIDEE-trunk (see Table 1). Therefore, an evaluation of this module is not a priority given the many modules that were changed between ORCHIDEE-trunk and ORCHIDEE-CAN. A validation against the proposed soil data will be part of the first application of ORCHIDEE-CAN in which in-situ carbon sequestration or net ecosystem production is simulated,

Recently, an evaluation of the soil carbon module in ORCHIDEE against empirical data from the Harmonized World Soil Database (HWSD) and the Northern Circumpolar Soil Carbon Database (NCSCD) was published by Todd-Brown et al. (2013). This study shows that the global soil carbon stocks as simulated by ORCHIDEE falls within the 95% confidence interval for both the soil carbon stock of HWSD and NCSCD. Regarding the spatial distribution of soil carbon stocks, ORCHIDEE showed good agreement with HWSD at the biome level, but mediocre agreement at the 1° scale. ORCHIDEE generally overestimated tundra but underestimated desert and shrublands (Todd-Brown et al., 2013). Given the same approach and parameters are used in ORCHIDEE-CAN as in ORCHIDEE-trunk, a rather similar behaviour to the one reported by Todd-Brown et al. (2013) is expected for ORCHIDEE-CAN.

Page 8606 Line 20 Additionally, the ORCHIDEE-CAN might have a few tuning parameters (e.g., plant water storage and soil-root resistance, p 8598 line 14–15), leading to higher likelihoods. If correct, this fact should be mentioned.

Thank you for bringing this up. Model tuning is an interesting issues that inherently comes with model development. While developing ORCHIDEE-CAN we aimed for a more consistent approach and tried to reduce the number of tuning parameters compared to the trunk (i.e. ORCHIDEE-CAN no longer has a cap on LAI). We made use of one tuning parameter to represent plant water storage and soil-root resistance pending a more mechanistic model representation. Besides this parameter we tried to avoid hiding model flaws in the parametrization by extracting the parameters from a wide range of sources including original observations, large databases, primary research reports and remote sensing products. By using this transparent parametrization approach we should have reduced the amount of tuning parameters compared to the trunk version. In the absence of new developments the level of tuning typically increases with the number of years that a model is used as more and more applications allow for more in-depth testing which might result in more tuning. From this perspective ORCHIDEE-CAN is at present most likely less tuned than the trunk. It should be noted that as long as the tuning is within the physically observed range of the parameters, it is justifiable and does not necessarily hints at a model deficiency.

Page 8612 Line 7 Can you show “this finding” by some materials (e.g., figure or map)?
We have added a map that shows the spatial variability of simulated and observed basal area over Europe (Fig 4).

Page 8613 Line 14–16 I don’t agree with the statement that “PFTs have no meaning outside the modeling community”, because plant biologists use a similar concept called “guild”. Also, the concept of “functional group” has been widely used in plant ecophysiology (e.g., W. Larcher 1995, Physiological Plant Ecology, ecophysiology and stress physiology of functional groups, 3rd ed., 506 pp., Springer-Verlag). As there are more than 200,000 plant species on the Earth, global models inevitably use some simplifications such as the concept of PFTs, for the time being.

Thank you for this comment which refers to the long standing debate on PFTs. It is beyond doubt that PFTs have a role to play in modelling and other scientific communities that have to deal with the huge plant diversity but at the same time the approach has strong limitations for outreach and communication outside the scientific community. Furthermore, some of the initial PFTs pool such diverse plants, for example Eucalyptus and Mediterranean Oaks are in the same PFT because both are temperate evergreen broadleaved species that the approach hampers parametrization of ecophysiological processes such as plant water conductivity and photosynthesis. Furthermore if wood production is among the output variables (which is the case for ORCHIDEE-CAN) the differences in wood use alone could justify abandoning the PFT concept in favour of a species/genus based parametrization (i.e. ORCHIDEE-CAN) or a hybrid parametrization of PFTs and plant trait relationships (van Bodegom et al., 2014; Reichstein et al., 2014). Taking into account the above and the comment of the reviewer, we changed the wording to "PFTs are of limited use outside the scientific community". (Line 1214)

Page 8637 Table 3 Several values in Table 3 is "Table 3". Do you mean Table 4, 5, or 6? Otherwise, please explain.
The references to the tables were corrected.

Page 8646 Figure 2 and Page 8647 Figure 3 Please add units to each y-axis.
Units are now added (Fig 2 and 3).

Response to referee 2

1. This paper is part of a series of papers that are currently under discussion or submitted (e.g. MvcGrath 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.

The motivation and the relation to the other submitted manuscripts are explained in the introduction (Lines 67-75): "The aim of this study is to describe the model developments and parametrization within ORCHIDEE-CAN and to evaluate its performance. ORCHIDEE-CAN is validated against structural, biophysical and biogeochemical data at the European scale. A more detailed description and evaluation of the new multi-layer energy budget and multilevel radiative transfer scheme is given by Ryder et al. (2014), Chen et al; (In prep.), McGrath et al. (In prep.). A new forest management reconstruction which is needed to drive forest management in ORCHIDEE-CAN is presented in McGrath et al. (In prep.) and the interactions between forest management and the new albedo scheme have been discussed by Otto et al. (2014)".

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.

This is indeed a model description/validation paper. We added a paragraph to the introduction to frame the content of the manuscript. See our reply to the previous

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 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).

We included three additional validation figures:

1. *a European map which compares simulated and observed GPP (good performance) and basal area (weak performance) and which illustrates the spatial variability of the model output (Figure 4),a figure showing regional validation results of tree diameter over France (Fig 6),*
2. *a figure showing the impact of the different forest management strategies (Fig 5).*

In combination with the figures that were already available, the new figures give a better insight in the performance of the model and better details the performance of the modules simulating forest management.

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.

A more intuitive numerical approach would set a fixed number of diameter classes with fixed boundaries and would let the distribution to vary. Such and approach would would computer resources as at any point in time several diameter classes would be empty. In addition, for different points in time, a different number of diameter classes would contain trees. This would not be a problem until only one diameter class would be populated. At that point the rule of Deleuze and Dhote which is required to differentiate tree growth for different diameter classes would collapse and the model would not be able to recover from that situation. To avoid such a collapse and make the best use of the computer resources (both computational and storage) we choose for the counter intuitive solution of fixing the number of diameter classes and the number of trees per diameter class but letting the boundaries of the classes to vary. Given that both alternatives have the same degree of freedom, they should have the same flexibility and capacities. In our approach a normal distribution could be approximated by using three diameter classes with a prescribed number of trees per class but with very narrow and very similar diameter ranges for each class. Initial tests showed a limited effect of the number of diameter classes and the initial distribution on the model behaviour for stand level variables (i.e.,stand density, basal area, diameter, height). However, the size and growth of individual trees is very sensitive to these settings. Finding robust intial distributions to correctly simulate tree ring width is under-way but is falls outside the scope of this study and the initial objectives of developing a model code that can simulate the large scale biogeochemical and biophysical effects of forest management over Europe. We added a concise comment on the issue in the manuscript on Lines 696-698. The comment reads "Note that the *boundaries of each diameter class are recalculated at each time step, this approach is a numerically efficient alternative to fixing the boundaries of each diameter class but letting the distribution to vary."*

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.

Initial tests showed that the forest grows reasonably well across the globe but at present model performance was not tested outside of Europe. Global runs would make use of the MTC parameters which are derived by averaging the parameters of the species within that MTC. If less details are required, the same MTC parameters can be used over Europe to perform simulations for the original 13 MTC classes which are distinguished in ORCHIDEE-trunk. To give an idea of the impact of the species level parametrization we ran a new 160 year simulation with the MTC parameters and validated the simulation in the same manner as the species level simulation (Fig. S2). For the seven parameters under study, the effects of introducing species are almost negligible. This test result is added to the manuscript (Lines 974-978): "In ORCHIDEE-CAN the PFT concept was refined by parametrizing the main European tree species groups (section 4.1). To evaluate the effect of the species parametrization, we performed a companion simulation for the configuration described above, but at the MTC level. Model performance was barely affected by the use of the MTC parameters, compared to the simulation with the species parameters (Fig. S2 for RMSE scores).

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, 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.

We would like to thank the reviewer for raising this issue. We re-wrote substantial parts of the parametrization section. First we added a paragraph describing the order and steps that were *performed during the parametrization process (Lines 704-722). This paragraph is followed by sections that provide more details. Attention was paid to address the concerns of the reviewer (See lines 756-762,824-826-,865,888-901 and 921-927) by adding more information on the procedures, by better describing the parameters that were parametrized, by better referring to the supplementary information that contains detailed and complete information for each parameter and by better describing the data and simulations that were used in the process.*

An independent validation for North America as suggested by the reviewer would require a species or species-group map for North America which we currently do not have access to. However, outside Europe the current model set-up only uses MTC parameters derived from European species because the focus of the manuscript and the first applications of ORCHIDEE-CAN is on Europe. Therefore, we chose to perform the validation only over Europe. To give the reader an idea of the impact of the optimization, we ran the model at the pixel-level with the prior values of k_{cmaint} and k_{lsmin} for Scots pine and compared the results to the simulation with the optimized values (Fig. S1). The text now reads: "Limited tests over a period of 100 years in a Scots pine forest at 51 − 52 ◦ N, 13 − 14 ◦ E (Fig. S1) suggested that optimizing kcmaint and kls had the largest effect on the maximum LAI, which decreased with almost 17% after optimization compared to a simulation with prior parameter values. Mean annual GPP, mean annual transpiration and basal area decreased with, respectively 6, 6 and 7% compared to a simulation with prior parameter values (Fig. S1). "

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. . .
This statement was rephrased.

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

As outlined in (Polcher et al., 1998), the use of an implicit solution for coupling between the atmospheric model and the surface layer model is the only way to keep profiles of temperature and humidity synchronised across the two models when the coupled-model is run over large time steps. The difference between explicit and implicit schemes is that an explicit scheme will calculate each value of the variable (e.g., temperature and humidity) at the current time step entirely in terms of values from the previous time step. An implicit scheme requires the solution of equations written only in terms of those at the current time step. The term implicit is now explained in the manuscript (Lines 195-209).

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.
The 15 minute time step is the time step of the coupling to the atmospheric model, we only mention it here because it is the first time we write about the coupling in section 2. We rewrote this part of the text to clarify that the 15 minute time step concerns the time step of the coupling (Line 205). The time steps of the other processes were added to figure 1 to avoid confusion.

4. Page 8576, line 6: "biomass and soil carbon pools are mixed": what does this mean?
Following a land cover change, the biomass, soil and litter pools of the vegetation that undergoes a land cover change is added to the biomass, soil and litter carbon that already existed for that PFT. If a land cover change results in the loss of an entire PFT, biomass, litter and soil carbon need to be properly re-assigned to other carbon pools. This was clarified in the manuscript by changing the wording as "Following a land cover change, biomass and soil carbon pools (but not soil water columns) are either merged or split to represent the various outcomes of a land cover change (Line

239).

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).
Yes, the two additional pools are the branches and the coarse roots. The manuscript now reads (Lines 258-261): "The inclusion of forest management resulted in two additional carbon pools, branches and coarse roots (i.e., abovegound and belowground woody biomass) and therefore required an extension to the semi-analytical spin-up method (see section 2.1). The semi-analytical spin-up is now run for nine C pools."

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 model? This is not clear from the current text.
No, the ORCHIDEE-CAN model does not contain the N-cycle of O-CN. However, the maintenance respiration is adjusted for an estimated nitrogen effect by assuming that the nitrogen distribution decreases exponentially with LAI, following the Beer-Lambert law. To avoid confusion on the use of the N-cycle, the reference to Zaehle and Friend (2010) is removed, while the Beer-Lambert assumption is added to the text (Line 268).

7. Page 8579, lines 16-19: ". . . the same parameter values . . ." the same as what? This sentence was confusing for me.
This sentence was rephrased as: "The allometric relationships between the plant components and the hydraulic architecture of the plant are both based on the pipe model theory, hence, both the allocation and the hydraulic architecture module use the same parameter values for root and sapwood conductivity". (Lines 320-322)

8. Page 8581: line 15-17: This is a very important statement; it should be included at the start of section 3.1.
The statement was moved to the start of section 3.1.(Lines 283-288)

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.
We replaced "albedo" by "radiation scheme for tall canopies" where appropriate.

10. Page 8645, fig 1: I suggest to rename the box "albedo" and make it "radiation scheme"
This was changed in the figure (Fig 1).

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.
The equations have been published in Pinty et al. (2006). We added a reference for the equations in the manuscript (Line 483).

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.
In fact we could have used any value for the extinction coefficient to calculate the effective LAI as long as we also would have used this effective LAI with the same extinction coefficient in direct mode (only the effective LAI would be changed, not the transmitted flux). However, we chose to use 0.5 as an extinction coefficient to be able to use the two-stream inversion derived by Pinty et al. (2011), which was derived using 0.5 (mathematically the simplest possible solution). To avoid confusion we shortly mention this in the manuscript. (Lines 491-492)

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. . .
This section was largely rewritten (Lines 612-636) to better explain how forest management was implemented (a request of reviewer 1). The specific suggestions and concerns of the reviewer where taken into account at lines 639, 646 and 669.

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.
The PFT will be killed if, at the end of the day, the labile pools are empty and there is no carbon available in the leaf or carbohydrate reserve pools to refill it. This is clarified in the manuscript (Lines 646-648).

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.
The time steps of the different processes are added to figure 1.

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

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.
The complexity and implication of developing and parametrizing a multi-layer energy budget justifies several manuscripts in its own. We therefore refer the readers to Ryder et al .(2014), Chen et al. (in prep) and McGrath et al..(in prep). The use of the single layer energy budget for validation will is mentioned in the introduction (Line 69-70).

Technical comments
All technical comments were taken into account in the revised manuscript.

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: pdelta 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)

References that are not in the manuscript:

Reichstein, M., Bahn, M., Mahecha, M. D., Kattge, J., & Baldocchi, D. D. (2014). Linking plant and ecosystem functional biogeography. *Proceedings of the National Academy of Sciences*, *111*(38). doi:10.1073/pnas.1216065111

Todd-Brown, K. E. O., Randerson, J. T., Post, W. M., Hoffman, F. M., Tarnocai, C., Schuur, E. a. G., & Allison, S. D. (2013). Causes of variation in soil carbon simulations from CMIP5 Earth system models and comparison with observations. *Biogeosciences*, *10*(3), 1717–1736. doi:10.5194/bg-10-1717-2013

Van Bodegom, P. M., Douma, J. C., & Verheijen, L. M. (2014). A fully traits-based approach to modeling global vegetation distribution. *Proceedings of the National Academy of Sciences of the United States of America*, *111*(38), 13733–8. doi:10.1073/pnas.1304551110