Response to Referee #1

We thank R1 for this detailed review, especially for going through the equations, which has enabled us to significantly improve the description of the new process implementation in our article. We apologize for the erroneous formulations of several equations and have corrected them in the revised manuscript. We double-checked in the code that the lines of code correspond exactly to the revised formulation of equations. Enclosed please find a detailed explanation of the revisions we made based on R1's comments. For your convenience, comments are in bold and our response is in italic. Revisions we made in the manuscript are presented in italic with grey background.

This paper represents a great amount of work in model development, and in general it is well justified, well written, and the availability of such a model will contribute towards science both through using the improved model and informing other model developers. Therefore I recommend that it should be published in this journal, but with some clarifications and a bit of consolidation.

Firstly, the paper is rather long. I am not convinced that separating the analysis in figures 8-10 into different continents (Europe/Asia/America) is really relevant to the model developments here. Differences between the PFT's should still be visible in the aggregate results. Consolidating these would reduce the figures and you could remove some of the discussion of inter-continental differences from the text. These are interesting but the paper would benefit from being a bit shorter.

We are aware that the article is rather long: this is due to our wish to introduce together the two or three vegetation types needed to improve the current representation of Artic vegetation in the ORCHIDEE model. As the reviewer suggests, we removed the division by continent for figures 8-10 and the analysis associated (in Sect. 3.2 & 4.2), which was replaced by the Fig. 11. The old figures and analysis by continent is moved to the supplementary material (Figs. S1 to S3).

Throughout the manuscript you have used the word "summergreen", which I have never heard before and we always use "deciduous". I'm not sure summergreen in really a word in English and maybe you should used deciduous instead? Sorry if I'm wrong here.

In the model ORCHIDEE, the use of the word summergreen is required to compare the deciduous summergreen and raingreen (present only in tropical climates, as presented in Table 1). Considering that we are working only on boreal landscapes, it seems simpler, as suggested, to use the word "deciduous" in this article (p3. l.15-17, p5. l.29, p.20 l.8, p.23 l.26-27, Table 1. and Table 2.)

Specific comments

* **P1 Line 17 what you mean by "a larger phenological plasticity" isn't entirely clear to me. Maybe because I am not a specialist in vegetation but I think this will be read by other 'general' land surface modellers so could maybe be a bit clearer. Do you mean the phenology varies more in the season? Or more quickly over time?**

"Phenological plasticity" means that the phenology of the plant can be shifted under hard climatic constraints, without causing its death. To be clearer, in the article we added a short description (in brackets p.1 l.17-18): "(i.e. adaptability and resilience to severe climatic constraints)".

* **P1 Lines 23-26. Please check all of these numbers for the percentage changes. I can't find them all in the main text or they don't seem to be consistent - for example, the change in roughness is quoted as 25% in the main text (page 20, line 33), but 41% in the abstract.**

We have checked all numbers (value and %) present in this article. We have corrected the mistake (p.21 l.14 "decrease of 41% from 55°N" and we added in the main text: p.20 l.13-14 "For example, the NPP is lower by 31% north of 55°N", p.20 l.38 "+3.6% North of 55°N", p.21 l. 20 "(-33% from 55°N), as expected mainly …", p.21 l. 36 "(+11% with 140 $km^3.y^{-1}$ north of 55°N)".

* **P5 line 6 "coefficients a1 and a2" - should be "b1" instead of "a2" as it seems to be called b1 in the table. Furthermore, you said you chose values so that stomatal conductance would not depend strongly on VPD but then the multiplier of VPD (b1) takes a larger value for NVP's than for the original grasses so this seems a bit counter-intuitive. Could you add a bit more explanation here?**

Indeed, as you have pointed out, the coefficient should be "b_1" instead of "a_2" (p.5 l.23). For the second comment, as you noted, the objective was to reduce the dependency of stomatal conductance to the humidity and CO_2 concentration, so to reduce the second term of eq. (1). The only adjustable constants are in eq. (2) (with the calculation of f_{VPD}): to reduce f_{VPD} we had to increase the term "$1/(a_1-b_1.VPD)-1$", and so to reduce "$a_1-b_1.VPD$". Our choice to modify b_1 is based on the fact that a_1 is the same constant for all PFTs, when b_1 was already dependent on the vegetation type (trees, C3 grasses or C4 grasses). We propose here not to add more detail to the article, which is

*** Section 2.2.3: For the NVP's, when you have negative NPP you induce a biomass loss function. But presumably the negative NPP itself should also lead to a biomass loss. I am interested to know how this works - are these are somehow linked or are they two separate loss terms?**

This is a very good point. In ORCHIDEE there is no explicit biomass loss when the NPP is negative. If NPP is negative, this means GPP < Ra (respiration) and this leads to a loss of biomass by the respiring tissues (to support Ra). But here, for NVPs we added a new explicit (and unlinked) loss term, to compensate for a reduced leaf biomass mortality (compared to the C3 grass PFT used as the starting point) due to the suppression of seasonal leaf fall and the increase of leaf longevity. Moreover this loss of biomass appears also if the NPP is null (not necessary negative).

*** Section 2.2.3 and Figure 1. Why did you reduce the turnover again after a certain amount of time? (ie why does the line on figure 1 decrease again after it reaches its maximum?) It would be helpful to provide some evidence from the literature or some more scientific justification here.**

The aim of this turnover, presented in section 2.2.3 Eq. (3) and Fig. (1), is to represent the behaviour of NVPs in extreme conditions, such as snow cover or dryness, during a long period (more than 1 month). If the turnover was maintained at the maximum (k_{lmax}) when there is no NPP, rapidly most of the biomass would be removed and the plant would die. But that doesn't correspond to general observations of the presence of NVP biomass after snow removal, or after long very dry periods (with the desiccation process). To account for this resilience, we propose to reduce the biomass loss after a certain period of severe conditions. Note that there is still some biomass lost due to senescence. As suggested, we added a small description p.6 l.17-18: "After a maximum, the turnover decreases in order to represent the induced resistance and thus survival to extreme conditions, i.e. under snow cover in winter or under dryness".

*** P6 A few issues around equation 4 (which is labelled as 3 by the way!). Underneath the equation you wrote "b is the daily leaf biomass" but this is in units of gCmˆ(-2), which doesn't have any units of time, so it isn't 'daily'? Do you mean the value gets updated daily? I suggest removing the word 'daily' here. However, there should be some units of time in the turnover rate and I think these might actually be in lcoef, which you have given as no units, I think this should maybe have units of dayˆ(-1) or similar? Difficult for me to tell from the information here but please check it. Another point about this equation, why does it only apply when LAI>LAI_max instead of LAI>LAI_lim? Using LAI_max means it will jump from zero when you reach LAI_max, whereas if you**

from zero. Maybe this was a typo, but if not, can you explain why you do it? Thanks!

As you suggest, the confusion came more from a lack of information/description than from real mistakes. We thank the reviewer a lot for these comments. Here are all the changes we made:

- *The label of equation 4 was changed (p6. l.34).*
- *The "daily" was removed (p6. l.35), because it stood for "updated daily" and that could be confusing.*
- *We added the unit of l_{coef}: d^{-1} (p6. l.35 and table 2).*
- *There was some confusion between LAI_{max} used for the photosynthesis and LAI_{lim}. So we checked the LAI_{xxx} and changed the syntax when that was necessary (p6. l.32-35).*

*** P8 Equation (10). This is quite a complicated equation and it would be really useful to see what the moisture function actually looks like. I suggest you add a plot of this. I looked in the paper that you referred to but it was not easy to immediately see it, and the moisture function for respiration is important so would be great to include the plot here.**

Indeed, this equation is very complex. We followed your recommendation adding a new figure (Fig. 4) in order to have a better understanding of the new function and some text in p.9 l.12: "Equation (10) and Fig. 4 describes…"

*** P9 line 4/5 says that albedo and roughness were set the same as C3 grasses. I guess for NVP's the roughness could be quite different from grass? Could you add a comment on possible differences? Either here or in the discussion.**

The roughness of NVPs can probably be considered to be of the same order of magnitude (compared to shrubs and trees), because they are both less than few tens of centimetres. However the albedo is very different, because their colour can vary widely especially depending the hydric status. We add in the discussion p.25 l.15-18 some precision about this issue: "However the albedo of the new boreal vegetation is still considered the same as that of the PFT they are derived from, although the colours of these PFT may vary substantially, with important impact on the albedo. In particular for NVPs (Porada et al., 2016) the colour may vary according to the relative humidity (Hamerlynck et al., 2000), an effect that is not easy to model globally."

*** P10 Equation 11a) The text says it's a logarithmic function, but this does not seem to be the case? Equation 11b) Bottom line of fraction should have Dˆ(gamma) not Dˆ2 Given these equations, I am not sure it makes sense to fix the crown area but still vary the biomass and height. This means that the allometric relations don't hold (for the case without dynamic vegetation), because the allometric relations**

are basically the relationship between height and area (or diameter- but these are related), yet you are varying the height and not the area. Could you comment on this? Are you assuming that the number of individuals changes in order to keep the crown area fixed? If so, please make that clearer in the text.

*Equation 11.a) it not expressed as a logarithmic function, but in order to describe the appearance of this function, we can consider that the usual function closest to eq. 11.a is the logarithmic function: starting from 0, increasing similarly to the logarithmic function and assuming a maximum (H_{max}). To be clearer, we propose to replace the "is" by "*resembles to*" (p.10 l.14).*
We corrected equation 11.b. ("D^gamma" in place of "D2"), p.10 l.22.
*The last part of this comment is about a fundamental choice of the developments performed in this article with the aim to obtain a realistic height of the vegetation to compute roughness, albedo or the height of shrub above the snow. The two strongest constraints were that i) without activating the dynamical vegetation (DGVM) module the total area of each PFT was fixed and ii) to be consistent, the equations with and without DGVM have to be the same. In order to have the vegetation height as a function of the biomass, we chose to implement a dynamical height depending on the biomass, following these equations (Eq. 11). Thus, as noticed by the reviewer, to account for vegetation height and diameter variations within a fixed area, the number of individuals has to vary. As a consequence, we can have only few tall shrubs or many short shrubs for a given area and biomass. To be clearer in the text, we added p10. l.18-19: "*and the number of individuals is adapted in order to keep the crown area fixed (Eq. (11.c. & d.))*".*

*** P10 Section 2.3.2 In the introduction you said that shrubs accumulate more snow in winter than trees (p3 line 13), but in this section you seem to treat them both together. What is the reason for this?**

The initial aim was to represent differences of snow accumulation on vegetation, not usually represented in ESMs. In this paper we started with the most significant difference between woody and non-woody species. In order to represent the differences between shrubs and trees, we would need to take into account precisely the spatial heterogeneity (vegetation coverage…), the phenology (evergreen and summergreen) and the wind effects. Given the complexity of the involved processes, it was beyond the scope of this already long paper and we thus focused only on the woody vs non-woody species difference. The sentence in the introduction is general and defines the ultimate objective.

*** P10 equation (13) I can guess what you are doing here - assuming**

accumulate much snow, and with a lot of shrubs of course the snow will be the same as the grid box mean because they are covering the whole grid box. But what is the justification for peaking in the middle? Maybe with just a few shrubs they would still accumulate snow? Did you get this function from somewhere or did you come up with it yourself? Could you either (in the first case) add a reference or (in the second case) give a bit more explanation of the physical reasoning?

This comment is very constructive. As mentioned above, we did not find a simple and robust approach in the literature to take into account the differences of snow accumulation on vegetation. The best solution would have been to separate the snow accumulation (and the energy balance) by vegetation type, but this was not possible within the scope of the study. We thus chose a simplified approach, as explained by the reviewer. However, in light of your comment, we realize that we probably over-simplified the equation: indeed few shrubs should still accumulate snow. With this suggestion we could revise the threshold used in Eq. 13 and define a new equation:

"$1+4.\,f_v$" if $f_v < 0.2$ and "$2.-f_v$" if $f_v \geq 0.2$.

That may produce more realistic snow depth variations, with a peak of snow depth for high vegetation if its fractional cover is 0.2 instead of 0.5. However, given the small overall impact that is expected with such change and the difficulties to launch again the optimization and validation we choose to keep the initial formulation but to add a comment in the text p.11 l.16-17 "Note that this equation is a heuristic formulation discussed in section 4". In the discussion we added: "Equation (13), with a maximum snow depth obtained for a fraction of high vegetation of 0.5, probably underestimates the impact of shrubs on snow in the case of low shrubs cover. Having only few shrubs still leads to significant snow accumulation. We suggest further investigation of this sub-grid scale parameterization, possibly with the use of a similar equation but where the maximum snow depth on shrubs would be obtained for high vegetation cover around 0.1 to 0.3, instead of 0.5."

*** P11 Equation (15) I am not sure I agree about the form of this. Because you are integrating, the mortality rate (as a fraction of biomass) depends on the height of the shrub. Imagine your temperature is just constant with z, then the mortality rate will be proportional to (H-Hmin) and thus higher for a taller shrub - despite both being at the same temperature. Is this something you wanted to include in the model? If so, you should discuss it. If not, I would suggest you instead divide the RHS of the equation (15) for Mce by (H-Hmin).**

We thank the reviewer for spotting the inconsistency. We indeed forgot to divide the RHS in equation 15 by the height. However, we do not divide RHS by *mortality is not applied below "H_{min}". If the temperature is constant with z that means the mortality is applied only on the fraction of the vegetation above h_{min}: ((H-Hmin)/H).*

*** P12 first paragraph: I don't quite understand what f_v_max is. Do you prescribe a certain fraction of the grid cell to be occupied by a PFT but then it doesn't necessarily occupy that whole fraction? Please explain this term a bit more.**

F_{v_max} is the maximum fraction of the grid cell occupied by each vegetation type (PFT), prescribed in the case of no dynamical vegetation. However, for grasses (and NVPs), which don't have woody parts, we consider that the real fraction of vegetation cover can differ from F_{v_max}. The idea is to take into account, for roughness and albedo, the lack of leaves in winter. We use the Leaf Area Index (LAI) as a proxy for the vegetation cover, as usually done in global models, with an exponential decay. In order to improve the text, we added two sentences: p.12 l.22 "The fraction of vegetation (fv) is used" and p.12 l.25-26 "to take into account the variation of leaf cover (for example absent for grasses in winter)".

*** P12 equation (17) - you do the weighted average in terms of 'log's, I assume this is standard procedure from somewhere but I haven't see it before. Please add a reference.**

Indeed, it is a standard simplified way of doing it, as detailed in "Vihma and Savijärvi, 1991" (p.12 l.31). The main principle follows from turbulence theory and the computation of the so-called drag coefficient that is a log function of the roughness length.

*** P13 L21-24 not sure what you mean by these things: - "survival or estabilishment limits" - limits in terms of what? Temperature? - "a cumulated degree-day threshold for the development" - maybe here you mean "..for the development of leaves"?**

We agree that the terms that we used were inaccurate. We added p14. l.69 "temperature" and changed the word "development" p14. l.7 to "plant growth".

*** P14 line 1, talks about methods for wetlands, but surely not all of your sites are wetlands?**

The published and unpublished data provided by Peregon et al. are more about lowlands. We take these data because we did not find any other data with total living biomass and productivity, on different sites, with multi-annual observations, and for the three new PFTs. Aware of this limit, we added an evaluation with biomass measurements from two other transects, *2011b; and previous reports since 2007). The evaluation of the model with these observations is supported by a new figure, Fig. 9, and associated comment:*

P.17 l.14-19: "We further compare the simulated biomasses with two other Arctic transects. The first one is the North America Arctic Transect (NAAT). It is situated in a continental area, and includes eight field locations (70°N 149°W to 79°N 100°W) sampled from 2002 to 2006 (Walker et al., 2011b) chosen as representative of zonal conditions. The second, located in a marine-influenced area, is the Eurasian Arctic Transect (EAT). It includes six field locations (58 to 73°N, between 67 to 81°E) sampled from 2007 to 2010 (Walker et al., 2008, 2009a, 2009b, 2011a)."

P.19 l.1-15: "Carbon stock with two Arctic transect

To evaluate the modelled biomass in other Arctic sites (not used in the calibration step), including uplands and lowlands, Fig. 9 shows scatter plots of observed and simulated biomass along two transects: the NAAT (North America) and the EAT (Eurasia) Artic Transect. The NVPs and shrub biomasses are relatively well reproduced by the model (i.e. within the error bars). For both PFTs, the standard deviation of the observations includes the 1:1 line, but the observed biomasses are on average higher than the simulated biomasses. Simulated shrub biomasses are biased low for the NAAT transect but not for the EAT transect.

In contrast, the mean value of observed biomass for boreal C3 grasses (Fig. 9.c) is low compared to the simulated biomasses for both cases. For half of the sites the simulated low biomass is in accordance with the observations, but for the other half the values are much larger (> 300 gC.m² whereas the observation do not exceed 54 gC.m²). Despite the optimization with observations from western Siberia (Fig. 7; leading to a decrease of biomass compared to temperate C3 grasses) there is likely an overestimation of the biomass for boreal C3 grasses, probably associated with an overestimated productivity."

Walker et al, 2011a: Vegetation of zonal patterned-ground ecosystemsalong the North America Arctic bioclimate gradient. Applied Vegetation Science 14, 440–463. Doi: 10.1111/j.1654-109X.2011.01149.x

Walker et al, 2011. 2010 Expedition to Krenkel Station, Hayes Island, Franz Josef Land, Russia, Data Report, Alaska Geobotany Center, Institute of Arctic Biology, University of Alaska Fairbanks, Fairbanks, AK. 63 pp.

*** P14 line 32/33, it seems odd that the Arctic grasses are assigned to cold climates but then they all end up in the South! Have you checked this?**

We have checked again the distribution of the vegetation, and we obtain the same result. That corresponds also to the "boreal trees" limit around 50 °N *and to mountainous regions. Note that our definition of the boreal region, based on Koppen Geiger climatic zones, has a relatively large extent given that we grouped several "continental cold climate" zones of the Koppen Geiger classification.*

*** P15 line 7/8 What was the justification for these new distributions, especially with the grass fraction. Why did you include grass but not include any shrubs? Also a bit concerning that your percentages don't add up to 100%. What is the rest?**

*In the standard ORCHIDEE version, the sparse vegetation class (from the ESA map) was distributed into 25% of trees + shrubs, 35% of bare soil and the rest as grasses; the NVPs were not considered. As explained in section 2.5, the too small cover of NVPs in the satellite – derived product, led us to propose a new interpretation of the sparse vegetation class for boreal regions (based on other artic land cover maps), i.e. 45% of sparse vegetation class is considered as NVP cover. Thus we removed 15% of bare soil and 30% of grasses to represent NVPs (see Table S1). To be clearer in the text, we added p.15 l.34-35: "*The remaining fraction of sparse vegetation (25%) has not been modified and is considered as a mix of trees and shrubs."

*** P16 : last sentence in section 2.6.1 talks about simulations and spin-up with no context (eg forcing data, soil characteristics?). I assume that the same simulation protocol as described in 2.6.2 is used for these simulations, and you extract the closest grid cells? But then the start of the simulation that it refers to at the end of Section 2.6.1 is not the same as described in Section 2.6.2. You need to more clearly explain what simulations are done/used for the parameter optimization.**

Indeed, the lack of details could lead to confusion. We did not include enough explanation about the set up of the simulations for the optimisation, which is different than for the evaluation step. The biggest differences come from the spinup (as already explained) and the spatial scale (at 0.5° for the optimisation). We clarified that in the text by adding p.16 l.34-36 and p.17 l.1: "The simulation for the optimisation was done with CRU-NCEP meteorological forcing (Wei et al., 2014; Viovy, 2015), at 0.5° resolution".

*** Section 3.1 - the first 3 lines here are more like methods than results. Can you make this an extra (final) section in the methods perhaps?**

,This is a good point. We have changed accordingly (p.17 l.23-30).

*** P17 line 23 How do you know the water stress in the model is too large? Could you show some evidence for this, or that it was seen in previous studies with ORCHIDEE?**

The text was probably confusing, as we did not pretend that the water stress in the model is too large, in Arctic or elsewhere. We only have few grid points corresponding to the "forest-steppe", where the observations indicate a substantial vegetation development, when the model simulates a low development (low biomass). This forest-steppe ecosystem is situated at the foot of a mountain region (in the south), with less rainfall. So probably the local observation site has more soil water available for the plants than the large-scale (2°) mean soil water. To avoid misunderstanding (without much text increase), we add p.18 l.16-17: "due to too large a local water stress… at 2° resolution in a mountainous region".

*** P20 line 3/4 "too low LAI seems to be simulated in western Siberia" This looks more like the middle of Siberia to me?**

We now write "in the central-west of Siberia*" (p. 19 l.21) ?*

*** P22 line 14 "plant resistance to water stress" - I thought you added something that made the NVP's recover more slowly from drought, and lose biomass, rather than resist the drought. Sorry if I missed the point here - do the other types of plants instead die in those circumstances? If so, could you clarify this?**

This is complex, and given your comment, we realize that it was not clear enough in the manuscript.

For NVPs, first we removed the leaf fall and decreased the senescence. To partly compensate for that, we added a biomass loss when NPP≤0, but only during the first weeks in order to represent the cost linked to a resistance to extreme conditions. With this cost, the plant becomes more resistant and is able to survive during severe conditions. We made this more explicit (p.6 l.6-7 "extreme conditions introduced by lower leaf senescence and no leaf fall" and p.6 l.17-18 "After a maximum, the turnover decreases in order to represent the resistance induced and the survival, i.e. under snow cover in winter or under dryness").

Moreover, we reduced the maintenance respiration of NVPs in case of dryness (section 2.2.4) to represent the desiccation and the ability to resist efficiently to dryness. We added a reminder to these two processes p.22 l.34-35: "(through resistance to negative NPP (Sect. 2.2.3) and desiccation (Sect. 2.2.4))"

*** P22 line 32 "especially for NVP's" - Not sure about this. Aren't NVP's less nitrogen limited than other plants?**

Indeed, the interest to introduce the NVPs is to be able to represent the vegetation in stressful condition. We wanted to insist that simulating in a realistic way any stress condition is important to estimate and model the

NVP's" by p. 23 l.23-24: "This is especially important for NVPs, which have an ecological advantage in these stressful conditions (such as poor nitrogen availability)".

*** P22 at the bottom of the page, you are talking about splitting shrubs into different types. It would be helpful to add in a comment about why it would be useful to do this? (What impact it might have?)**

Such split would be useful to represent evergreen shrubs, which represent nearly half of shrubs cover in Arctic. Separating evergreen from deciduous shrubs can have important consequences, especially for the albedo in winter. We added in the text p.23 l.30-31: "such as evergreen phenology type, which represents more than 48% of shrubs North of 55°N according to the CCI product and Table S1"

*** P23 line 14/15, you are talking about how the seasonal cycle of NVP productivity differs from the vascular plants in the model, but there is no comment about whether these differences are realistic. You also mentioned earlier in the paper about 'representing the observed temporal dynamics of lichen and bryophyte biomass', but no reference to actual observations. It would be helpful to refer to some studies to discuss whether the behaviour of the model is realistic.**

We agree that important ecological functionalities have to be justified by observations, using the literature. We thus have changed the text by adding p.24 l.8-10: "This behaviour corresponds to the observation that NVPs are, compared to vascular plants, most active during the shoulder seasons, due to less severe water stress and reduced competition for light (Williams and Flanagan, 1996; Campioli et al., 2009)"

*** P23 line 37/38, "the new PFTs are more sensitive to climate change than the original ones" - the plots do not seem to fully support this. The fractional changes are maybe larger with the new PFT's, but the 'old' PFT that you show on the plot (boreal broad- leaved trees) seems to have the largest absolute change and so potentially the biggest impact on the carbon cycle. I recommend modifying this discussion to account for this.**

We agree that we have overstated the response of the new PFTs to climate change. We have to put into perspective the larger fractional changes, and the relative "absolute" contribution. To clarify that, we added p.24 l.33: "even if their overall contribution remains lower"

*** P25, Acknowledgements - I suggest you add more details of the projects, not just the acronyms i.e. full names and project numbers.**

We add p.26 l.20-22: "The authors acknowledge financial support by the European Union Seventh Framework Programme (FP7/2007-2013) project PAGE21, under GA282700 as well as a French – Swedish program that has funded the first author's PhD, through the GAP project".

*** P38 Table 5. I think it is interesting that one of the calibrated parameters (b) was calibrated to zero. This appears to remove the acclimation behaviour from the photo-synthesis model. Could you add a comment about this in the text? Do you think it's because the air temperature never gets very warm so acclimation isn't necessary?**

We thank the reviewer for this interesting suggestion. We therefore added p.23 l.4-8: "Note that for boreal C3 grasses the constant **b** *of the entropy factor for the photosynthesis process (eq. (19) and eq. (20)) was optimized to zero (Tables 5 and S2), involving de facto the removal of seasonal temperature dependence of the photosynthesis process. That shows a limit of the Yin and Struik (2009) expression and could be due to the fact that the air temperature never gets warm enough to justify the need for acclimation."*

Technical comments (In general the writing is good but I picked up some gram- mar/typos on the way through so will list these here.)

All of these technical comments were taken into account in the new version of the article.

*** P1 Line 24, "transpiration (+33%)" -> "transpiration (-33%)"**

*** P2 Line 23/24, "is relatively simple and discretized on few" -> "has been relatively simple, with few"**

*** P2 Line 26 "either trees or grasses PFTs." -> "either trees or grasses."**
*** P2 line 27 "in the reality" -> "in reality"**

*** P2 line 36 "interactions part" -> "interactions as part"**

*** P3 line 4, I'm not sure about how you have referenced the CAVM, you have written "Mapping Team et al.", I wonder if it should just be "Mapping team" (and then the names listed are the members of the mapping team, not additional people?)**

*** P3 line 7 "does not allow to" -> "does not allow it to"**

*** P3 line 9 "mosses and lichens and shrubs" -> "mosses, lichens and shrubs"**

*** P3 line 12 "more resistant for hydric" -> "more resistant to hydric" And "or for nitrogen limitation" -> "or to nitrogen limitation"**

*** P3 line 15 "to warming whereas trees" -> "to warming, whereas trees"**

*** P4 line 16 "C3 grasses plants" -> "C3 grasses"**

*** P6 line 1 "cold temperatures" -> "cold temperature"**

*** P6 line 33 "(use in ORCHIDEE)" -> "(used in ORCHIDEE)"**

*** P7 line 13 "when NVP get desiccated." -> "when NVPs get desiccated."**
*** P7 line 30 "NVPs layer" -> "NVP layer"**

*** P8 line 24 "to define the control litter" -> "to control litter"**

*** P9 line 12 "processes as trees." -> "processes to trees."**

*** P9 line 22 "additional shrubs types" -> "additional shrub types"**

*** P11 line 2 "dynamically the vegetation distribution" -> "the vegetation distribution dynamically"**

*** P11 equation 16 Change 'else' to 'otherwise'**

*** P12 line 6 "there is no woody" -> "there are no woody"**

*** P12 line 27 "equation described previously" -> "equations described previously"**

*** P12 line 27 "as well as few" -> "as well as a few"**

*** P12 line 29 "Cold climates" -> "Cold climate"**

*** P12 line 34 "themselves function of" -> "themselves functions of"**

*** P13 line 12 "list of variable" -> "list of variables"**

*** P13 line 31 "observations located in" -> "observations are located in"**

*** P16 line 9 "number of iteration" -> "number of iterations"**

*** P18 line 12 "referred as" -> "referred to as"**

*** P20 line 25 "occur in early spring" -> "occurs in early spring"**

*** P20 line 27 "impact the albedo" -> "impacts the albedo"**

*** P20 line 34 "Contrariwise" -> "Conversely"**

*** P21 line 14 "5mmd-1" should be "0.5mmd-1" ?**

*** P21 line 20 "permanent frozen soil" -> "permanently frozen soil"**

*** P22 line 27 "implies to introduce" -> "implies introducing"**

*** P22 line 30 "availably" -> "availability"**

*** P23 line 19 "on the same time" -> "at the same time"**

*** P24 line 7 "in liason with" -> "in conjunction with"**

*** P24 line 13 "ecosystem occur" -> "ecosystems occur"**

*** P24 line 33 "permafrost extension" -> "permafrost extent"**

*** P24 line 33 "soil water dynamic" -> "soil water dynamics"**

*** P25 line 5 "and reach around" -> "and reaches around"**

*** P25 line 11 "reduce locally" -> "locally reduce"**

*** P25 line 12 "snow dynamic" -> "snow dynamics"**

*** Table 2 (df) "Maximum number of day for this extra turnover" -> "Maximum number of days..."**

*** Table 3 caption "values are choose" -> "values are chosen"**