Anonymous Referee #1

The authors investigate in this very interesting paper the interannual and seasonal differences of carbon and water exchange between vegetation and atmosphere in a temperate forest ecosystem dominated by Scots Pine. By combining Eddy-Covariance-Data and a local process-based vegetation model (LPJ-GUESS) the authors show that environmental factor do not explain annual exchange rates, but can successfully for monthly and daily ones – except for winter seasons and droughts. By analyzing other temperature responses and different mechanisms of water uptake the authors emphasize the need for testing and developing modelling approaches in vegetation modelling. To my opinion this paper is well written and comprehensible, except for a couple of minor comments. I recommend publication of this paper.

——————————— Minor comments: ———————————

Page 270, line 17-21: These sentences are written a bit complicated (line 17) or too general (line 18-21). Please try to rewrite in order to better understand what you mean with “more direct” (line 17) and whether you used “factorial experiments” instead of one could do that.

AUTHORS:
We thank the reviewer for pointing out that this was not entirely clear. With more direct responses, we meant the response of vegetation, and in particular photosynthesis, to direct environmental variables (temperature, precipitation (drought), and radiation) which act on the carbon and water balance on an hourly or daily time scale. The first part of the paragraph describes the indirect responses (phenology, C allocation etc.). To illustrate this, we added the word ‘indirect’ before ‘short-term’ in line 11, and before the word ‘long-term’ in line 15. We further try to clarify the above issues by now writing (Page 270, line 16−21):

“Before addressing these complex process representations within models, however, it can be useful to test whether IAVcw can be explained by rather simple relationships with direct environmental drivers, such as drought, temperature and radiation, which can affect, e.g., photosynthesis and soil respiration rather directly and instantaneously. Factorial experiments with a dynamic vegetation model can then be used to generate hypotheses concerning simple and/or complex interactions of processes driving IAVcw.
These vegetation models can be expected to capture at least some of the complexity of real ecosystems, and the factorial experiments can be used, for example, to keep certain environmental drivers constant (i.e. switching of their effect, e.g. Hickler et al., 2005), or to implement different hypotheses concerning the most important processes within an ecosystem. The latter can also be achieved by data-model intercomparisons with several models, that differ in their process representation (e.g. Medlyn et al., 2015). In this study, the factorial model experiments refer to model setups with different process representations. With this purpose in mind, we used ...”

Reference:
Medlyn, B. E., Zaehle, S., De Kauwe, M. G., Walker, A. P., Dietze, M. C., Hanson, P. J., Hickler, T., Jain, A. K., Luo, Y., Parton, W., Prentice, I. C., Thornton, P. E., Wang, S., Wang, Y.-P., Weng, E., Iversen, C. M., McCarthy, H. R., Warren, J. M., Oren, R., and Norby, R. J.: Using ecosystem experiments to improve vegetation models, Nature Climate Change, 5, 528−534, 2015.

REFEREE:
Page 271, line 12: Please use another word for “model error”.

AUTHORS:
We changed this into:

“can explain differences between modelled values and observations for...”

REFEREE:
Page 273, line 4: Please be clear what you mean with “for all years”. Do you mean 1997, 1998 and 2009 or more years?

AUTHORS:
We meant the years considered in this study, and changed the text into:

“Soil moisture data are available for all years considered within this study (1997–2009), ...”

REFEREE:
Page 273, line 6: please explain “litter” in terms of “a depth” as it is written in this sentence! Is it the “aboveground litter layer” or the “soil surface”?

AUTHORS:
Indeed, this is unclear. The depth is at 0.00 m, so the aboveground litter layer. We clarified by now writing:

“In 2005 all sensors were replaced and positioned at different depths: 0.00 (aboveground litter), 0.03, 0.20, 0.50 and 1.0 m.”

REFEREE:
Page 273, line 9-10: Are exact dates known for these additional measurements? Please provide the years in brackets.

AUTHORS:
Yes, we can provide the years and changed the text into:

“Additional site measurements at less frequent intervals include the leaf area index (LAI) of trees (1997 – 2009, every few months at irregular intervals) and, to a lesser extent, the understory (1999, 2002, 2012).”

REFEREE:
Page 273, line 16: Please provide some references for forest gap models.

AUTHORS:
The gap model we refer to here is called FORSKA. It was published as a PhD thesis in 1989, by Leemans and Prentice. Model name and reference are now added:

“.. gap models (FORSKA, Leemans and Prentice, 1989).”

In case the reviewer would prefer a broader reference, we would like to suggest the extensive review of forest gap models by Bugmann et al. (2001) in Climatic Change.

References:
Bugmann, H.: A Review of Forest Gap Models, Climatic Change, 51, 259-305, 2001.
Leemans, R. and Prentice, I. C.: FORSKA, a general forest succession model, 1989.PhD, Institute of Ecological Botany, Uppsala University, Uppsala, Sweden, 70 pp., 1989.

REFEREE:
Page 273, line 18: Please explain what you mean with “main tree species”. Is it the dominant species or a mean species? Moreover, authors write that the study is based on main tree species rather than PFTs. However, on the next pages authors refer to PFT-specific parameters (e.g. page 275, line 12). Please try to avoid this inconsistency.

AUTHORS:
We agree this is not entirely clear. We meant dominant species here, and replaced 'main' with 'dominant' on page 273, line 18. In addition, we agree with the reviewer that we should clearly distinguish between single species and PFTs, as the latter represents a broad class that can contain multiple species. We therefore changed the document accordingly:

Page 275, line 12: 'PFT/species-specific' instead of 'PFT-specific'
Page 277, line 10-12: 'Only *Pinus sylvestris* and the PFT for herbacious vegetation with C3 photosynthesis (to represent the understory) were allowed to establish.'
Page 277, line 13: 'species' instead of 'PFT'
Page 277, line 17: 'PFT/species-specific' instead of 'PFT-specific'
Page 277, line 18: 'PFT/species' instead of 'PFT'
Page 279, line 13: 'PFT/species-specific' instead of 'PFT-specific'
Page 279, line 18: 'PFT/species-specific' instead of 'PFT-specific'
Page 279, line 26: replaced 'PFTs' with '*P. sylvestris* and C_3 grasses,'
Page 285, line 8: replaced 'PFTs' with 'PFTs and species'
Page 290, line 10: 'PFT/species-specific' instead of 'PFT'
Table 1: Replaced 'Species PFT' with 'Species/PFT'
Figure 1: removed 'PFT' from caption

REFEREE:
Page 273, line 20: Please explain in more detail, what "neighboring individuals" means on a patch.

AUTHORS:
It means neighbouring individual trees, which we added to the text.

REFEREE:
Page 273, line 21: I would recommend switching "cohort" to the brackets and "age class" to the sentence for a better understanding.

AUTHORS:
Changed according to the reviewer's suggestion.

REFEREE:
Page 277, line 7: Did you check whether there is a significant trend in the climate data from 1997 to 2009?

AUTHORS:
We checked whether there is a significant trend in the climate data (linear regression in R) and did not find one for temperature or radiation. For precipitation and AET we do find a small but significant increase between 1997 and 2009, but this was to be expected because precipitation is known to have increased over the last century in the whole of The Netherlands (Buishand, 2011). So during the spin-up period, when we repeat the meteorological dataset this could have caused some IAV, but during the simulated result period this should not have an effect.

Reference:
Buishand, TA., De Martino, G., Spreeuw, JN., Brandsma, T., 2013. Homogeneity of precipitation series in The Netherlands and their trends in the past century. International Journal of Climatology 33, pp. 815-833. doi:10.1002/jov.3417.

REFEREE:
Page 278, line 10-11: Please also provide for this reference a location of that study.

AUTHORS:
The location is Creag Fhiaclach, situated in Cairngorms national park in Scotland. We have added this information to the text, in brackets.

————————————————- Technical comments: ————————————————-

REFEREE:
Page 275, line 3: Please write “with” using lower case letter.

AUTHORS:
Changed according to the reviewer’s suggestion.

REFEREE:
Please use consistent labels in the figure and table captions concerning the change of the temperature response function (Fig. 3, 5, 6, Tab. 3).

AUTHORS:
We thank the reviewer for pointing out this inconsistency. We changed all labels (and, if necessary, captions) according to the descriptions used in Table 3.

REFEREE:
Fig. 7: In the figure captions, it should be stated (consistent to the figure) that solid lines are modelled values and the dotted line is the observation.

AUTHORS:
Thank you, reviewer, for finding this mistake! We completely missed that one ourselves..
Changed according to the reviewer’s suggestion.

REFEREE:
Page 284, line 11: A section number is missing.

AUTHORS:
This change was made by the ESDD editorial team. Our original manuscript referred to this paragraph as Section 3.3.1. We have added a remark in the revised manuscript regarding this matter.

REFEREE:
Please use “g” instead of “gram” consistently in all figures and tables.

AUTHORS:
We agree with the reviewer that consistency is important, and changed Figs 2, 3, 4, 5, 6, 7 and 10 accordingly.

Anonymous Referee #2

General comments:
The paper studies vegetation – atmosphere carbon and water ex- change using multiple years of Eddy Covariance data from Loobos, a Dutch Scots pine forest and LPJ-GUESS, a process based ecosystem model. Specifically, the paper looks at multiple timescales (daily, monthly and yearly), and shows that interannual variability in carbon and water exchange (IAVcw) is not well captured by models. Next, different model versions are used in order to explain and minimize the IAVcw. Overall, I think the paper is well written, and interesting. The extension of the model with carbon uptake up to -10 degrees Celcius is interesting, even though it is not improving IAVcw much. Also, the different water uptake routines show interesting results. I would recommend this paper for publication, with minor revisions. Please see comments below.

——————————— Specific comments: ———————————

Page 275, eq 1. What is pstemphigh and pstemplow? This is not clear from the next, nor from figure 1. Also, I would recommend not just explaining the abbreviation in the figure caption, but also in the text.

AUTHORS:
We would like to point out that both parameters $pstemp_{high}$ and $pstemp_{low}$, are described extensively on page 275, lines 14-18, as 'the optimum temperature range for photosynthesis', where we also refer to Figure 1. This optimum range for each species/PFT is taken from Table 3.7 in Larcher, 1980. In the caption of Figure 1, these optimum ranges per species/PFT are provided: for *P. sylvestris* it is 15−25°C, and for C_3 grass 10−35°C (Larcher, 1980).

We think that some confusion may have been caused in this paragraph by some edits from the ESDD editiorial team. Our original submitted text was "Within the optimum range, tscalar equals unity (1),..." and the editorial team changed this into "Within the optimum range, tscalar equals unity (Eq 1),..".

We try to be very exact in our meaning, by changing the sentences (page 275, lines 14-18) regarding the optimum temperature range into:

"This optimum range (i.e. the upper plateau in Fig. 1) represents an effective temperature response of many enzyme and transport related processes. Within this optimum range, tscalar equals unity (i.e. tscalar is equal to 1), and creates a slight rise in maximum carboxylation capacity (Vm), but reduces photosynthesis with increasing temperature. Outside this optimum range, both ..."

REFEREE:
Page 278, section 2.3.2. In this section, the authors show some results for the alternative temperature response function, also referring to the supplementary information, and in section 3.2.1 (page 281) more about the temperature response is shown. I would suggest to move large parts of section 2.3.2 to section 3.2.1 so the paper is better structured.

In the originally submitted manuscript this structure was as the reviewer suggests. On personal request of the handling editor (20-06-2014), we changed the structure to the current version. We would only make this change again with the specific consent of the handling editor.

REFEREE:

Page 283, line 20-22. Figure S3, the authors state that the soil moisture patterns are captured reasonably well compared to observations, but I see quite a distinct difference in the years 2000 and 2005-2008, with the model showing much more dynamics that the data. 2003, on the other hand, is captured much better where the data show much more rewetting in the beginning and the end of the year, which the model also simulates for the other years. However, the data don't follow this rewetting patterns. Could the authors please comment?

AUTHORS:
We thank the reviewer for pointing this out to us, and would like to clarify a bit more. The model's soil hydrology is a very simple, two-layer bucket model. Therefore, the description of soil moisture to a depth up to 1.5 m, is captured with only two layers. Our observation data (Section 2.1.3) are measured at 5 different depths. So, for the comparison of the modelled soil moisture to our measurements we averaged the data corresponding with the depths the first model soil layer (l_1, 0−50 cm) and the second soil layer(l_2, 50−150 cm). We agree that this is important information that should be included in the manuscript, and added it to the text, at line 21.

With this information in mind, we still think the model performed reasonable well in capturing the seasonal dynamics. The model, especially upper layer (l_1),shows more dynamics than the observation data because 1) it responds quickly and strongly to plant water uptake as most modelled roots are located in the upper soil layer (Table 1), and 2) it quickly and strongly responds to precipitation and soil evaporation (description in Supplement, Section 1, and Sitch et al. (2003)). The second soil layer (l_2), receives moisture through percolation and loses water through plant water uptake, but the changes in water contact are less strong than for the upper layer.

The data, on the other hand, are averaged to meet the modelled depth, and show less dynamics because there is not such a strong and direct link to water uptake in the top layer, especially because the soil is very sandy and water percolates to lower layers easily.

The last point the reviewer makes here is that the data do not show the rewetting patterns of the model. This is partly caused by the modelled winter temperature response of photosynthesis, where the model assumes a shutdown of photosynthesis and underestimates GPP and AET. When we look at Fig. 3, these are also the years where AET is underestimated by the model the most. The data do not show rewetting because vegetation is still actively assimilating and retracting water from the soil (in particular the understory remains active during mild winters), whereas the model keeps all the water in the soil as plants are not retracting much water from the soil.

REFEREE:
Page 284, line 1-5. In figure 8, modeled transpiration versus observed sap flow is shown. In the text, the authors stress the good correlation in these figures. However, I would like to ask them to comment on the difference between S1 and S2-S3. The latter two have much less modeled transpiration (ranging from 0-1.5) than the observed sapflow (ranging from 0-3). However, S1 has a similar range in modeled and observed transpiration (except for 2009, where sapflow had a smaller range, up to 1.5 rather than 3 mm/day as in the other years). In the manuscript, the authors indicate the conservative water uptake by S3 (as is also shown in figure S1). Could they explain why we also see a smaller range in simulated values for transpiration for S2? Also, S2 indeed increases the r (table 3), but increases the RMSE at the same time.. How do these changes affect figure 2?

AUTHORS:
We would like to thank the reviewer for this keen observation. Unfortunately, we confused the reader by making a typing error in line 4-5. setups 'S1 and S2', should be replaced with 'S2 and S3'. This is now corrected in the text.

Furthermore, we find the reviewer's observation on the differences in range of modelled transpiration between setups S1 and S2-S3 very sharp and helpful, so we have added this information to the text on page 284, line 3:

"(Fig. 8, r = 0.68–0.74). For setup S2 and S3, the range of modelled plant transpiration is lower than the observed plant transpiration (0-1.5 mm day^{-1} and 0-3 mm day^{-1} respectively). For setup S1, the range of modelled plant transpiration matches that of the observations for 1997 and 1998 (0-3 mm day^{-1}). This relates directly to the shape of the response curve for each setup (Supplement, Fig. S1), where S2 and S3 reduce the water supply S more strongly than S1 in response to declining soil water. Correlations for individual years are lowest for 1997, especially for setups S2 and S3, where modelled transpiration..."

We would also like to address the reviewer's comment that for 2009, the observed data range is lower (0-2 mm/day). Our site PI (J. Elbers, jan.elbers@wur.nl) acknowledges that since the installation of the new sap low measurement system (data for 2009, Granier thermal dissipation probes on 6 trees, see section 2.1.3) all sap flow measurements have been slightly lower than those measured with the previous system (data for 1997 and 1998, tissue heat balance systems on 3 trees). There are several reasons why the new data read slightly lower than the old ones: the systems have different measurement uncertainties, and the spread in the older data may be larger because it was measured on fewer trees. Furthermore, the conversion method to calculate sap flow speed (meters s^{-1}) to tree transpiration (mm/day) is based, a.o., on the density and height of the trees which may cause additional errors. For sake of readability of the manuscript, we prefer not to include all this information in the results section of the paper.

Finally, we would like address the reviewer's last comment regarding the correlations and RMSE. For model setup S2, r only increases for GPP (Table 3), and not for AET. RMSE for AET actually decreases on the monthly and daily time scale. The reviewer asks how this would affect Fig. 2, and we would like to refer them to the bottom panel of Fig. S2 in the Supplement. This shows the effect of setup S1-S3 on the residuals of AET. At low soil water content, S2 and S3 underestimate AET much stronger than S1. A representation of these data, similar to Fig. 2, would therefore show more skewedness and a larger deviation from the 1:1 line.

REFEREE:
Page 284, line 19-21: and to a lesser extend S2?

AUTHORS:
Yes, but only for the lower soil layer. We added this to line 21.

REFEREE:
Page 288, line 25-26: Could the authors explain further? Because of drainage below the rootzone? Water that is not intercepted will enter the soil, correct? And then be available for AET? Unless it is drained?

AUTHORS:
Yes, we can explain this. Modelled AET is calculated as the sum of plant transpiration, soil evaporation and canopy evaporation. So yes, water that is not intercepted will enter the soil and be available for soil evaporation and plant transpiration. During winter, when the model simulates little or no photosynthetic activity because temperatures are low, simulated plant transpiration will also be low. Therefore, by not handling shower intensity explicitly, the model further underestimates interception evaporation which contributes to the underestimation of modelled AET. To clarify this issue, we changed the introduction of the paragraph (page 288, lines 17-18):

"Simulated AET is calculated as the sum of plant transpiration, soil evaporation and canopy evaporation. Underestimation of canopy evaporation, or interception loss, in relation to.."

REFEREE:
Page 289, line 26: the onset of drought, rather than drought? In figure S3, the 2003 drought seems to be captured well in general..

AUTHORS:
The reviewer is correct here that in Figure S3 the onset of drought is captured well in terms of soil moisture. In this paragraph, however, we meant the response in terms of simulated GPP and AET. To clarify, we changed the text into:

"However, none of the uptake parameterizations capture the observed response in terms of GPP and AET to a drought such as occurred in the summer of 2003 (Fig. 10)."

——————————— Technical corrections: ———————————

Finally, some minor grammatical and style suggestions:

REFEREE:
Page 272, line 21-22: Reichstein instead of Re-ichstein

AUTHORS:
Changed according to reviewer's suggestion.

REFEREE:
Page 275, line 6: change into 'The scalar c1 is ...' and remove c1 from the end of the sentence?

AUTHORS:
We apologise for the confusion, as we meant t_{scalar} here, and not c1. We clarified by rewriting the sentence to:

"The inhibition function t_{scalar} is used for..."

REFEREE:
Page 275, line 17: change) to be after Vm instead of after temperature
Page 275, line 21: add comma after kinetics
Page 278, line 13: change into: 'than, but in a similar range as, observed..'

AUTHORS:
All these points were changed according to the reviewer's suggestion

REFEREE:
Page 280, line 24: add (2.4 and 1.6 respectively) after observed
We agree with the reviewer that this information should be added, but the value 1.6 the (s)he suggests here is incorrect, and should be 1.0 (please refer to Table 2). We therefore wrote (2.4 and 1.0 respectively) after observed.

REFEREE:
Page 284, line 6: add (S2) after 'species specific uptake'
Page 284, line 7: add (table 3) after 'coefficient'
Figure 6: add 'winter' in the beginning of caption, rather than at the end

AUTHORS:
All these points were changed according to the reviewer's suggestion

REFEREE:

Figure 9: add 2003 and 2005 in the figure itself (e.g. above the different figures, 2003 on the left and 2005 on the right)

AUTHORS:
Thank you, we agree this makes the figure more clear. We added the corresponding years to the top left of each subpanel.

——————————— AUTHOR corrections ———————————

We found some additional mistakes and inconsistencies while working on the revisions of this manuscript. The proposed changes are:

Page 275, line 9: replaced the period with a colon after (Haxeltine and Prentice, 1996)
Page 276, line 1: changed 'the supply of water' to 'water supply'
Page 277, line 2: added) after Haxeltine and Prentice, 1996b
Page 279, line 9: replaced 'Sylverstris' with 'sylvestris'
Page 279, line 22: replaced 'Sylverstris' with 'sylvestris'
Page 279, line 24: replaced 'Sylverstris' with 'sylvestris'
Page 283, line 22: changed 'observation' to 'observations'
Page 288, line 21: 'Elbers et al. 2010' instead of 'Elbers et al. 201'
Page 297, line 11: replaced 'Sylvestris' with 'sylvestris'
Page 298, line 25: replaced 'The" with 'the'
Page 298, line 26: replaced first author 'E.' with "E.J.' (EndNote sorting algorithm)
Page 302, line 1-3: updated reference for Wolkovich *et al.* (2012)

Caption Fig. 8: added 'for' before 1997.

Figs. 2, 3, 4, 5, 6, 7, 8, 10: For consistency with the text and figure captions, we changed all units in these figures, using superscript instead of ' / '. For example, AET in 'mm day^{-1}' instead of mm/day.