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Biogeosciences Discussions

Interactive comment on "Decadal water balance of a temperate Scots pine forest (*Pinus sylvestris* L.) based on measurements and modelling" *by* B. Gielen et al.

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We would like to thank the referee for the thorough review of our manuscript and for her/his constructive and helpful criticism. Below are given our answers (A) to the referee's comments (C). We copied the comments and answered each comment separately.

C: From the abstract it is not entirely clear the hydrological and ecological motivations of the analysis, a point also noted by the other reviewers. To motivate the modeling, especially using different techniques it would be pertinent to cite Hanson et al. (2004), which I believe to be the most complete treatment of stand hydrologic modeling to date.

A: We thank the referee for pointing this out. We made the necessary improvements to the abstract, stating more clearly the motivation to use models and the necessity of comparing models to long term data and cited the suggested reference [Hanson et al. (2004)].

C: The measurements are comprehensive, but the future directions are lacking. What did we learn from this analysis? That the different methods differ? How could CI and WATBAL (and the other methods) be improved given the findings of this study?

A: To improve the MS we added a paragraph to the manuscript which describes the shortcomings for each of the methods used in this analysis. In addition, an uncertainty analysis on the EC fluxes was added to the MS and the results were added as a table (5). From this we could conclude that, given the uncertainties, there is no significant difference between the EC fluxes and the process based models. Furthermore, analyses on the relationships between climatic drivers and ET on monthly basis were added. We conclude that significant relations can be found on monthly basis, but that these are absent on annual scale. In contrast the opposite was true for drainage. Finally, future improvements for each approach were added to discussion.

C: An error or uncertainty analysis would add greatly to the paper. I do not think that the future climate scenario contributes to the analysis, which would be stronger if the methodology, rather than projections, was focused on in more detail, especially given the differences among methods.

A: As suggested by the referee we added a paragraph which discusses the uncertainties and possible improvements of each approach (see further). We also evaluated the referee's comment that the future scenarios do not contribute to this analysis, especially because referee # 1 was very supportive of this analysis. We feel that the future climate scenario does make the manuscript stronger, because it gives an indication of how ET and leaching will respond to future climate. We would like to highlight that the ORCHIDEE model fits the EC fluxes and sap flow measurements quite well and

that given the right model parameters its results could give an indication of how future direction of ET and drainage will look like. In future, precipitation patterns will change both in terms of amounts as well as seasonal distribution. Simultaneously, VPD and air temperature will change. These climatic perturbations did not occur within the 10 year study period. In fact, if we were to extrapolate the monthly – or annual regression of ET to its drivers to the expected future conditions, we would obtain different estimates of ET than those predicted by the model that, unlike the regressions, takes into account the simultaneous changes in all drivers.

C: I would argue that the water cycle is a part of the climate system. The discussion on the so-called 'acceleration' of the hydrologic cycle is lacking in the introduction.

A: We thank the referee for pointing this out. We added the discussion on the acceleration of the hydrological cycle to the introduction. We understand the importance of the long term ET measurements in the ongoing research on the acceleration of the hydrological cycle and pointed this out in the introduction.

C: 'autochthonous' (10523, 10) is strictly speaking not incorrect to use here, but it is commonly associated with surface & subsurface hydrology. 'Native', or like word, would be better.

A: Done

C: Past tense on line 14: 'has occurred'. Also the next sentence; there may be times or places when/where these species do not emerge.

A: Done

C: Please specify soil saturation more explicitly; if the rooting zone extends to the perched water table, this layer of the soil is saturated.

A: We made the formulation more precise and changed "soil" to "upper soil layers". We also argue that the root zone is probably limited to the upper 1m of the soil so they probably do not extend into the saturated soil layer.

C: P. 10524 L. 7: This paragraph should be in the past tense.

A: Done

C: Has there been any work on this approach since the Eriksson and Khunakasem 1969 CI reference? I understand the concepts given the description, but I can also see how biweekly measurements may not be enough under some circumstances of precipitation statistics.

A: Chloride proved to be a nearly inert tracer for long-term computations of downward water fluxes. Unlike sodium (weathering) and sulphate (sorption) it is considered as a conservative ion, although some exchange may occur with the organic molecules. The chloride budgets are therefore often used as possible check of results from hydrological models, where no water flux measurements are available to partition the precipitation inputs into ET and drainage (De Vries et al., 2001, 2003). Long-term chloride budgets may be expected to be close to zero (De Vries et al., 2001, 2003) and allow therefore for assessing a possible bias in the output from hydrological models. In this case the Cl- budgets have been calculated for one decade and give therefore some indication whether the order of the magnitude is realistic. For short term calculations of water fluxes, the chloride budget is probably less appropriate because there is a time lag between the Cl- coming in and going out of the rooting zone. One solution might be to correct for storage of Cl- in the rooting zone in addition to more frequent sampling intensity.

C: How well does the HFD sapflux method match more common approaches (e.g. Granier, Kucera)? Given the low LAI and the potential for substantial evaporation (and also interception), please describe the scaling strategy of Verbeek et al. (2007) briefly for the readers here.

A: Kostner et al (1998) made a comparison between the Granier method and the HFD method (Čermák and Kucera) concluding that both methods were appropriate to measure daily sap flow. The methodology for the sap flux measurements on tree level

was explained in Verbeeck et al (2007). Sap flow was scaled-up to ecosystem transpiration using biometric parameters. This approach was described by Čermák et al (2004) as suited for even aged stands, and uses the ratio of basal area at plot scale to that of the measurement trees to scale-up the sap flow measurements to stand scale transpiration. This last part was added to the MS.

C: 10526, 7: LAI is prescribed to have a fixed seasonal pattern. Is this accurate given the variability in climate, and would this influence results? (see also 10528, 16).

A: Working with a fixed LAI seasonal pattern had an impact on the results from simulations. It can especially cause a discrepancy between the models and the measurements at the beginning and the ending of the growing season since budburst and senescence is not taking place at the same moment every year. This problem was mentioned in the discussion section of the MS. Also, note that the yearly maxima were not constant in time, but derived from measurements of peak LAI as mentioned in the MS. In our opinion, the approach used in the manuscript was the best guess as the 2007 seasonal LAI pattern is the only one available with a 14-day resolution over the 10 year time series.

C: Why is WATBAL used rather than some of the other models explored for example in Hanson et al. (2004)? On this point I agree with Referee #2. Rather than both process & data driven, in Figure 1, I would suggest that WATBAL is 'simpler'. On 10529, 10 this distinction is made more clearly in the text than the figure.

A: The reason why we wanted to incorporate WATBAL into this analysis is because this empirical model will be used in the future to estimate the water balance of the ICP-II forest network. The goal was thus to evaluate the model based on the dataset available at our site and compare it the process based models that need a lot more parameters that are not available at the other ICP-II sites. We conclude that the Kc factor introduces a large uncertainty for this type of models which is discussed in an extra paragraph that we added to the discussion. We removed Figure 1.

C: The wording on 10530, 5-7 suggests that the Tair – precipitation relationship is causal, but this is not proven. Say maybe instead '. . .1998 and 2002 had above average precipitation and more narrowly defined growing season Tair profiles' or remove this passage entirely.

A: Done.

C: Given the errors in models and measurements, there may not be a significant difference between ORCHIDEE (or SECRETS) and sapflow or ET.

A: We gratefully thank the referee for this suggestion. After performing an uncertainty analysis on the EC fluxes (see table 5 in revised manuscript) we could conclude that the simulations of both process based models lie within the uncertainty of the measurements. We added this to the discussion and conclusion of the MS.

C: Section 3.5: Is ORCHIDEE explicitly accounting for elevated CO2 in the stomatal function terms here? Might one expect LAI to also increase? I do not think that the future climate scenario adds much to the analysis, especially given the differences in measuring and modeling the stand hydrologic balance in the present. The paper would be stronger without the future studies section.

A: Indeed the simulations of ORCHIDEE account for the elevated CO2 on stomatal conductance. The model accounts for the effect atmospheric CO2-concentrations on stomatal conductance through the coupled Farquhar-Ball-Berry model (Farquhar et al., 1980;Ball et al., 1987) This was inserted in the MS. We thank the referee for this comment but in our opinion the future climate scenario is useful for our study site (also indicated by referee 1) as it gives an indication of how ET and leaching will respond to future climate and increasing atmospheric CO2 concentrations. We would like to argue that ORCHIDEE fits the EC fluxes quite well and that given the right model parameters the results could give an indication on what future direction of ET and drainage will look like.

C: 10534, first paragraph of Discussion: The referencing is incomplete and I'm not sure how the particular references were chosen. A complete list of ET estimates for like forests in a table would be interesting. A comprehensive list of T/ET for global forests would be an interesting hydrological comparison. I'm thinking of a few more available references than those cited.

A: We thank the referee for pointing this out. We removed the overview of the first paragraph because in our opinion the climatic conditions and site conditions differ too much to make a decent comparison. We agree with the referee that the T/ET ratio comparison would be of great interest but we feel that this is out of the scope of this analysis. We would like to point out that such analysis is not straightforward as there is a large discrepancy on the time scale of the published data. Therefore we expanded the discussion on the factors that have a potential influence on the T/ET ratio.

C: 10535, 5: Be more precise this is low vapour pressure deficit, not atmospheric pressure.

A: Done

C: Please expand a bit on the error/uncertainty paragraph(s) that begin on 10535. Errors and bias in each measurement & modeling strategy deserve at least a paragraph, preferably with clear methodological improvements listed for future studies. In particular, energy balance closure is rather low. ET is almost certainly underestimated to some degree, but please discuss the arguments made by Foken (2008) to clarify potential other factors.

A: We agree with the reviewer that the uncertainty could be more structured and focussed on in the manuscript. Therefore we added, as suggested by the referee, a paragraph to the discussion specifically focussed on the uncertainty of each method. For the eddy covariance we mention the arguments made by Foken et al (2008).

C: 10536: How much would the WATBAL predictions improve with different realistic

values of Kc? Why does Cl not work so well?

A: We did additional simulations with WATBAL to investigate the effect of the Kc factor on the ET estimates. Results show that an uncertainty of 10% on the Kc factor results in a 6% change in ET. From which we can conclude that not only the Kc factor but also the Jensen-Haise model introduces a bias because it calculates potential evapotranspiration from radiation and thus is not as sophisticated as the process based models. As suggested by the referee we added an extra paragraph where we discuss the uncertainties and improvements for each approach. We argue that estimations of the Cl approach can be improved if the concentrations are corrected for storage of Cl- in the rooting zone in addition to a higher sampling intensity. This was added to the uncertainty paragraph in the discussion section of the MS.

C: 10537: clarify 'leached out' in a system with a perched water table.

A: The water table is only perched during certain periods were there is low ET and high precipitation. We improved the description of the water table in the MS.

C: The conclusion that ET has low interannual variability suggests that it is conservative, discussed first – to my knowledge – by Roberts (1983).

A: To our knowledge Roberts (1983) suggested that transpiration is conservative and not ET. Since transpiration is only part of the ET as mentioned in the results it may not be correct to assume that also ET is conservative.

C: Can it be concluded that Cl and WATBAL overestimated ET given the uncertainty in its measurement? Probably, even if the missing term of the water balance is entirely comprised of missing ET, but please demonstrate such things clearly in the conclusion of a study.

A: We gratefully thank the referee for this comment. After correcting the EC estimates for energy balance closure according to the Bowen ratio (as suggested by referee nr 1) and computing its random uncertainties, the estimates from WATBAL and Cl are still

higher than the EC estimates. We added this to the discussion in the MS.

C: 10537, 24: This is a very scale-dependent statement and only holds in my understanding at the annual time scale. Does annual ET have a relationship with mean annual soil moisture or various drought indices (e.g. Palmer's)? Are the trees tapping the saturated layer?

A: We thank the referee for the remark and added "annual" to specify the timescale. In addition we looked at the drivers of intra-annual variability and inserted these results into the MS. As suggested by the referee we investigated the relationship between annual ET and annual mean SWC and Palmer's drought index, but no significant relations were found. As mentioned in our MS, our study site has ample supply of water and rainfall is distributed evenly over the year. Consequently, drought periods do not occur often and if so they do not last for a long time. Therefore, mean annual SWC does not show much variation from year to year. Since we have no exact data on maximum rooting depth we are inconclusive about the possibility that these trees are tapping water from the saturated layer.

C: Figure 1: Please make the title consistent with the diagram

A: We removed the figure and only left the descriptive part in the text.

C: Figure 3: LE seems to be higher later in the day in 2005 compared to 2004 or 2006 (the fingerprint is shifted up). Is this real?

A: We thank the referee for pointing this out. Indeed the fluxes were shifted due to a mistake while making the footprint figure. We corrected the figure. However, this mistake has no impact for the further data analysis.

C: Useful in addition to Figures 6 & 7 would be a time series of each approach with uncertainty.

A: We agree that an uncertainty analysis on the measured fluxes would add greatly to the quality of the paper, therefore we inserted the estimates of the random errors

on the latent heat fluxes. From these uncertainties we can conclude that there is no significant difference between the process based models and the EC fluxes. Additionally, we would like to argue that detailed uncertainty estimate for the process based models is beyond the scope of this paper. To do this one should estimate probability density functions for each parameter, or try to reduce the parameter uncertainty by data assimilation. In a second step the uncertainty on the parameters can then be propagated through the model in order to estimate the model output uncertainty (Verbeeck et al. 2006). A detailed uncertainty analysis combined with data-assimilation focussed on the simulation of ET and the water balance would be of great interest for the improvement of the process based models. However, as suggested by the referee, we added a paragraph to describe the possible uncertainty of each approach and made suggestions for improvements.

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Interactive comment on Biogeosciences Discuss., 6, 10519, 2009.