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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.

Anonymous Referee #3

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Gielen et al. investigate decade-long forest hydrology measurements in a Scots pine forest in Belgium using different measurement and modeling techniques.

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.

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?

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.

Specific comments:

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.

I like the clear statement of the objectives.

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

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

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

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.

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.

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).

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.

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.

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

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.

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.

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

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, the 63% energy balance closure is extremely low. ET is almost certainly underes-

timated to some degree, but please discuss the arguments made by Foken (2008) to clarify potential other factors.

10536: How much would the WATBAL predictions improve with different realistic values of Kc? Why does CI not work so well?

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

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

Can it be concluded that CI 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.

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?

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

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?

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

References Foken, T. 2008. The energy balance closure problem: an overview. Ecological Applications 18:1351-1367. Hanson, P. J., J. S. Amthor, S. D. Wullschleger, K. F. Wilson, R. F. Grant, A. Hartley, D. F. Hui, E. R. J. Hunt, D. W. Johnson, J. S. Kimball, A. W. King, L. Y., S. G. McNulty, G. Sun, P. E. Thornton, W. S., M. Williams, D. D. Baldocchi, and R. M. Cushman. 2004. Oak forest carbon and water simulations:

model intercomparisons and evaluations against independent data. Ecological Monographs 74:443-489. Roberts, J. 1983. Forest transpiration: A conservative hydrological process? Journal of Hydrology 66:133-141.

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