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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.

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 critique. Below are given our answers (A) to the referee's comments (C). We copied the comments and answered each comment separately.

C: First, there is a point that I do not understand well: why using the model WATBAL in this study? If the aim was to quantify its performance, this point is not really discussed here.

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.

C: Does the low performance of this model comes from the use of Thornthwaite potential evapotranspiration which is less realistic than Penman or Penman-Monteith formulas, moreover when calculated daily instead of monthly?

A: WATBAL uses a Jensen-Haise model to simulate potential ET or more specifically alfalfa reference crop ET values. It uses global radiation to derive the amount of energy available for ET and is therefore better than more simple temperature-based models such as Thornthwaite & Mather. But it is not as sophisticated as the Penman-Monteith model, which takes into account a few other factors such as wind speed, and which is considered the definitive ET model. As discussed in the newly added paragraph in the MS, the low performance come from both the Kc factor and the algorithm that is used to to calculate the potential evapotranspiration.

C: Very few is said about rainfall interception, which is often high in coniferous stands, except that the model SECRETS calculates it. What about ORCHIDEE: is there a rainfall interception routine in this model? Also, is the interception flux included in ET (equation 1)?

A: We agree that there wasn't much attention for rainfall interception in the manuscript. ORCHIDEE gives indeed rainfall interception as a modelled output so we included this in the model description. ET was descripted as total evapotranspiration, which is the sum of transpiration, soil evaporation and canopy evaporation, so canopy evaporation is included in ET. Furthermore we added the model outputs for canopy evaporation for SECRETS and ORCHIDEE in the model evaluation paragraph.

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C: I am not very confident in the accuracy of the eddy covariance technique for measuring rainfall interception: this could be a source of deviation between eddy covariance measurements and models.

A: To our knowledge it is not possible to measure different components of ET by eddy covariance as it always measures total ecosystem ET.

C: Even there is a nice agreement between two models (SECRETS and ORCHIDEE) and the stand-scaled sap flow (Fig. 4), the authors found a discrepancy under low transpiration condition. Before explaining this behaviour by a capacitance effect, the first step would be to compare sapflow and ET as measured by eddy covariance. According to what will be found, a capacitance effect can be suspected.

A: We agree with the referee that capacitance is probably not the main cause of the non-linear shape of the curve. As suggested by the referee, we compared ET measured by EC with the sapflow measurements. On this figure (added as attachment) the non-linear curve is less pronounced. Therefore, possible effect of the VPD response on gs was added to the discussion as secondary more plausible explanation to the discussion. Finally, as a third explanation, the transpiration could also be a limited by the hydraulic conductivity in the stems or the roots. This was also added to the discussion in the MS.

C: Another point is the proportion of transpiration to ET, which is supposed to come from soil plus understorey vegetation; quite abundant literature on this question is cited in the discussion (p 10534). The effect of tree LAI on understorey evapotranspiration has to be more clearly stated: the large range of T/ET is probably due to that of LAI. In the investigated stand, LAI is particularly low (ca. 1.8) and induces a low T/ET ratio.

A: A: We thank the referee for pointing this out. We clarified that the rather low T/ET ratio was probably caused by the low LAI of our stand.

C: I agree that nice relationships cannot be found on annual values between ET and its

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climatic drivers. Besides the fact that the “dormant” season could weaken the relationships, the range of variation of radiation, temperature and VPD shows a low interannual variability. But why not try to study those relationships on daily or monthly values?

A: We gratefully accept the suggestion of the referee and inserted new figures (6) and tables (1 and 2) with the relation between the climatic drivers and ET on monthly time scale. Additionally, a new paragraph in the MS was devoted to the intra-annual variability in the results chapter.

C: The first sentence of the abstract is not very informative and quite nebulous: I suggest the authors to remove it.

A: Done

C: Is the word “empirical” (§2.2.4) most appropriate for field measurements?

A: We changed empirical to field.

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

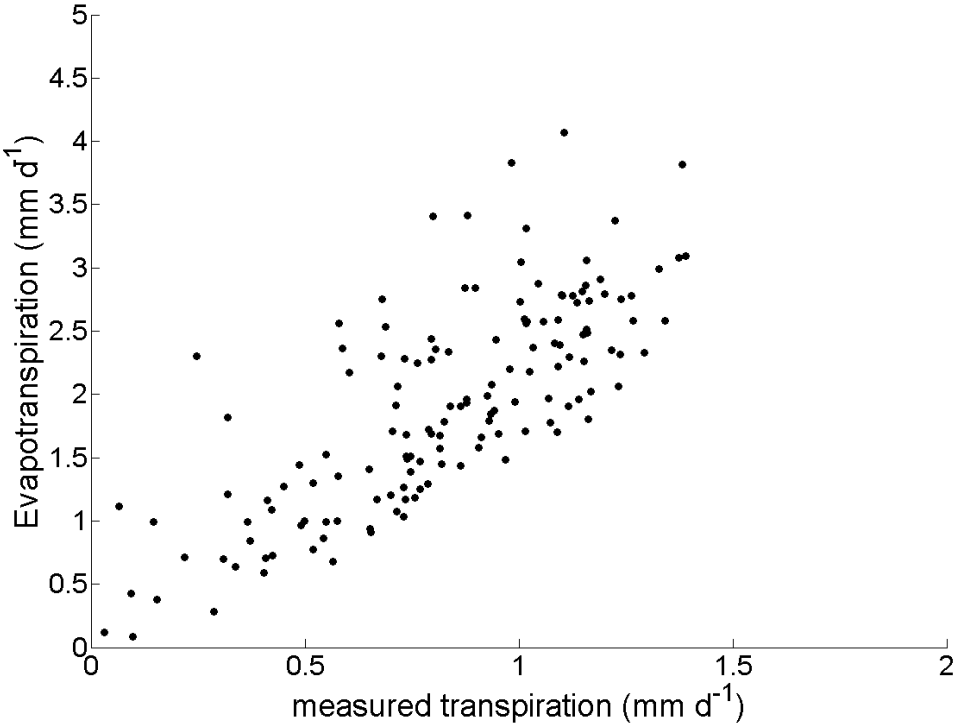

Fig. 1.

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