

Interactive comment on “Comparative analysis of the actual evapotranspiration of Flemish forest and cropland, using the soil water balance model WAVE” by W. W. Verstraeten et al.

Anonymous Referee #1

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General comments:

This manuscript describes ambitious and interesting research to mechanistically compare water budgets of forests and croplands. The complex model employed has been most extensively tested on agricultural systems, and relies on the concept of crop factor for modeling evapotranspiration. This strategy is understandable because of the objectives of the work, but leads to some difficulties that are not adequately discussed. The work should be published, but only after substantial revision to clean up the presentation and to discuss the findings in context of other work. Lacking this discussion, the manuscript reads more like a technical report than a paper in an international journal.

The model parameter definitions and symbols are difficult to follow (for example, ETO

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is "potential reference crop evapotranspiration", while ET_c is "potential crop evapotranspiration" ... ?). The section explaining the WAVE model should be re-worked to (1) eliminate reliance on Figure 1, (2) include equations for definitions, and (3) be systematic about defining variables. A table of parameter definitions would be more helpful than Figure 1 if suggestion (2) is implemented. Also, the manuscript places odd emphases. For example, we get a detailed description of the Richards equation formulation with two equations but no equations to define the critical variables unique to the WAVE model.

The results of the modeling are discussed mainly in the context of the model itself. That is, most of the comments are directed to how the model did or did not represent a particular piece of field data well. This is good, but too detailed to be useful. Much more important is discussion of the results in the context of other work. How realistic are the results and the parameters? How far can they be extrapolated? What about the unique conditions of the stands measured (forest fragments)? Another important issue is the root uptake function, which is mentioned in the methods and conclusions as an important parameter, but oddly justified (see comment P770L19, P771L3) and never presented in the results at all. Finally, the crop factors derived as the ultimately tuned parameter are not discussed in context of other work. This is very important given that these values are likely to be used in other work.

The description of model calibration is too detailed in light of the other needs in the manuscript. Trimming this section to include only the essential information about what the authors actually did in the calibration, with very brief presentation of the context of the choices, would leave room for more process-based discussion of the results and implications of the model parameters chosen.

Specific comments:

P766L10 "variably saturated"

P766L10 "infinitesimally small"

P767L14 "interception and ponding"

P767L24 need better justification than "calculated from [grey literature]". This is a systemic problem throughout the manuscript. Please try to substitute international references where possible. E.g., instead of Dolman et al (2000), try an English-language textbook or review paper from the refereed literature.

P767L26 this description of the model is not sufficient; Fig 1 is not clear - perhaps it just needs a more professional format to improve clarity, but I don't see how the math would still be clear. Why not simply present the equations in the text and eliminate some textual description? Presenting the water balance model in a clear way is certainly more important than, e.g., presenting the Richards equation, the discussion of the meaning of calibration on P768, or the detailed discussion of goodness-of-fit measures on P769. It is imperative to present justification for the equations for E_p and T_p .

Fig 1. What are "c" and "f" in the equation for E_p ? How do you justify the values presented in the caption? Shouldn't there be an arrow from the LAI box to the expression above the E_p box? Is this the equation for E_p ?

P767L23 Inserting this justification for parameter values into the model description is confusing.

P767L22 What is the definition of ET_0 ? This is an important detail not to be overlooked. I see a reference on P770L19, but a short summary of the theory involved is required. Also, what is the definition of the crop coefficient?

P768L14 "address different aspects"

P769L11 "values is as good"

P769L13-14 Coefficient of determination is the proportion of variance that is explained by the model, not the proportion of observed data. However, I am not familiar with the definition of CD as presented in eq 5. I do not see how it is possible to discern bias from this measure, because it squares differences between observations and model.

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P770L7-8 should be "saturated volumetric water content is highly variable"

P770L19-23 I do not see how differences in roots between forest and crops are sufficient to justify similarity of crop factor, and it is also not clear what information this sentence is conveying. What is "much"?

P771L1 Choosing the crop coefficient as a purely tuned value has important implications for the work. For example, errors estimating drainage from the soil profile will manifest in K_c . More justification is therefore required for K_c values. Perhaps improving the discussion on the previous page will help.

P771L3 How does one conduct a root profile description? This sentence is not clear.

P771L11 "October 1999 to November 2001"

P771L23 What is a "mutual distance"? There were two TDR sensors 50 cm apart?

P771L24 What were the characteristics of the throughfall samplers? What, if any, steps were taken to reduce evaporation? Given the long intervals between visits, it seems very likely that evaporation from these containers was substantial at some times of the year, and would therefore strongly influence estimates of interception.

P772L1 "tube was installed"

P772L19 "LICOR type formulas" is an insufficient description. The follow-up in Sec 2.4 is not much more helpful because it is not quantitative and refers to gray literature. The description also seems to imply that the model was calibrated against pine and used for both pine and broadleaved trees, which is also not satisfying.

P773 The first sentence is incorrect grammatically. I suggest "... intercepted by the canopy was derived from the canopy water balance."

P773 I do not understand the meaning of " $i = [x]$ ". Why not simply list the time periods in the text?

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P773L8-11 This method "was proposed" - but apparently not validated? There are several problems with this approach that have potentially large effects on the results: (1) Setting intercept to zero is not appropriate because it is well known that the intercept of such regressions is a negative number approximately equal to the canopy storage; (2) Using all pseudovalues of TF in the regression inflates the importance of values obtained from the long-interval periods (e.g. up to 14 "observations" in a two-week period) because these are in fact not independent observations. (3) Pseudovalues obtained from long-interval periods are likely less reliable because of evaporation from the collectors.

P774L8 Nackaerts et al (2001) is not in reference list.

P774 I do not see why it is necessary to statistically assess whether model runs of forest and cropland are different because we know exactly how they are different by their construction. I may be missing something, however, as I am unfamiliar with the statistical technique.

Throughout paper: check for errors in verb tense: many statements in present tense should not be so.

Table 2 The units presented are inconsistent: choose whether PAI is m^2/m^2 or unitless.

P776L16-17 CD 1.51 indicates underestimation? From Eq 5 it appears to overestimate variance (see also P777L10-12). Also, what is the interaction between the calibration procedures and these measures of model fit? That is, how do you justify using crop coefficients that sometimes vary by > 100 percent in the same plot over time? How are the model fits when assigning a single K_c ? A two-season K_c ?

P776L24 I disagree that all plots but #4 are "simulated satisfactorily." Model efficiency of soil moisture content is also very low for plots 6, 7, and 8.

P777L1-3 (1) Where are these efficiencies presented? (2) How do you know root uptake is the cause? This is important given the conclusion that root uptake is a remaining

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modeling bottleneck.

P780L1 "which may indicate"

P780L3 "ratio of ETact to ETc"

P781L5 Juxtaposing a discussion of LAI determination methods with a water budget is a non sequitur

P782L22-24 Please discuss why crop water use is less responsive to variations in climate than is forest. This seems an important finding in the context of climate change.

P783L2 and P784L18 If we know crop interception is 3-10 percent, why not take it into account? Especially given that the average difference between forest and crop water use is 93 mm, an expected interception rate in crops of 25-82 mm could dramatically change the conclusions.

P783L10-11 "not normally distributed"

Has there been any work done in the region that can be used to directly validate/compare the conclusions? Examples might be watershed-scale assessments of water yield across land uses or citations P765.

The Introduction cites the fragmented nature of forests in the region as a reason to pursue the modeling approach, but this situation is never mentioned again. Discussion of the context of the results is lacking in general.

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