Hydrol. Earth Syst. Sci. Discuss., 2, S540–S553, 2005
www.copernicus.org/EGU/hess/hessd/2/S540/
European Geosciences Union

Hydrology and Earth System Sciences Discussions

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.

W. W. Verstraeten et al.

Received and published: 17 August 2005

Summary of the responses made

Given the nature of the comments of both Reviewers the authors opted to rewrite parts of the manuscript before going to Phase 2 of the review process.

As suggested by Reviewer 1 and 2 the Richards equation was removed and Fig. 1 was replaced by a set of equations outlining the way ET0, ETc, Ep, Tp, Eact, Tact and ETact were calculated. This allowed also explaining the physical meaning of the parameters included in the analysis. Since Reviewer 1 requires adding more discussion on the results this section was extended. To improve the structure of the manuscript subheadings were added in Section 3.3 ("Water balance in relation to forest stand characteristics"). Where possible, the "grey literature" was replaced with references to

international papers.

First the comment of the reviewer is given, next our answer (ANSWER:)

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.

ANSWER: The discussion was reformulated although we believe that the results (PAI, Kc, ETact, Tact, INT) were properly discussed in an international context. To improve the structure of the manuscript subheadings were added in Section 3.3 (“Water balance in relation to forest stand characteristics”). First the PAI is discussed, subsequently the crop factor, the root uptake function and finally the water use components.

The model parameter definitions and symbols are difficult to follow (for example, ET0 is "potential reference crop evapotranspiration", while ETc 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.

ANSWER: The Eqs. 1 2 and Fig. 1 were deleted and the equations replacing the

HESSD

2, S540–S553, 2005

content of Fig. 1 integrated in Section (2.1) “Model Description”. In doing so a more detailed description of the applied parameters is given. Parts of the model description may also be found in Meiresonne et al. (2003).

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. (1) 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 (2), 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.

ANSWER: (1) LAI, ETact, Tact, Eact and INT were explained referring to the results of similar studies. From the discussion it is clear that the results are in agreement with reported values. A discussion especially on the root water uptake function was added. The model validation should be regarded as an evaluation of the validity of the model parameters and whether or not the parameters can be extrapolated. The retrieved parameters are of course linked to climatologic conditions of the considered region. The question whether or not the obtained parameters can be used in for instance American temperate forests cannot be addressed here. However, they may be useful as a first approximation and then be tuned for the specific conditions. WAVE was chosen because of the fragmentation and not as a tool to study it. (2) As requested by the reviewers the crop factor is more thoroughly discussed in Section 3.3.

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

HESSD
2, S540–S553, 2005

the choices, would leave room for more process-based discussion of the results and implications of the model parameters chosen.

ANSWER: The description of the model calibration was partially trimmed in favour of the extension of the discussion of the results.

Specific comments

P766L10 "variably saturated"

ANSWER: OK.

P766L10 "infinitesimally small"

ANSWER: OK.

P767L14 "interception and ponding"

ANSWER: OK.

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.

ANSWER: OK.

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 Ep and Tp.

ANSWER: Equations 1 2 and Fig. 1 were deleted and the formulas replacing the

HESSD

2, S540–S553, 2005

concept of Fig. 1 integrated in the section on “Model Description”. In doing so permitted to address in more detail the description of the parameters.

Fig 1. What are "c" and "f" in the equation for Ep? 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 Ep box? Is this the equation for Ep?

ANSWER: Figure 1 provoked some confusion. Therefore this figure was deleted. c and f are default values as explained and used in Meiresonne et al. (2003). c is a parameter accounting for the interception of incoming solar radiation by the vegetation, which according to Huygen et al. (1997) has a value of 0.5. They used for the parameter f a value of 1. These values were essentially applied on agricultural crops. Perhaps that for forest vegetation other values should be used. However, this requires additional field observation data which we do not have. Moreover, these parameters may be stand structure depending; thus can be varying with each specific measuring plot. Maybe, the fractal dimension derived from hemispherical imagery (see also discussion on LAI) can provide us with this kind of information in the future. Hitherto, we have used these parameters “as they are”: default values.

P767L23 Inserting this justification for parameter values into the model description is confusing. ANSWER: Indeed. Therefore the justification was moved to Section 2.5, where the agricultural plots used in the analysis are described.

P767L22 What is the definition of ET0? This is an important detail not to be overlooked.

ANSWER: The formula to calculate ET0 was included in the section on “Model description”. ET0 [mm d-1] is the reference crop evapotranspiration corresponding to the water consumption of a grass lawn cover in an active growth phase without restriction of water and nutritional elements uptake (Choisnel et al., 1992). ET0 expresses the amount of water transferred from the vegetation-soil system to the atmosphere governed by meteorological and plant factors.

HESSD
2, S540–S553, 2005

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?

ANSWER: Some theory on the crop factor was included in the “Model description” section. Kc is the crop factor converting the reference evapotranspiration (ET0) to the potential evapotranspiration (ETc) of the crop (vegetation) under consideration and depends on and varies with the crop development stage. The crop factor lumps together the resistance to transpiration, crop height, crop roughness, reflection and crop rooting.

P768L14 "address different aspects"

ANSWER: OK.

P769L11 "values is as good"

ANSWER: OK.

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.

ANSWER: CD deals with the proportion of the total variance of observed data that is explained by the simulated data. The distance between each observations and the average of the observations is calculated in the numerator. In the denominator the distance between each prediction and the average of the observations is calculated. If denominator and numerator are different, it means that peaks are not well simulated.

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

ANSWER: OK.

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"?

HESSD

2, S540–S553, 2005

ANSWER: It is recognized that at field/stand scales the roots of agricultural crops are spatially (X-Y plane) dense but rather shallow in depth (Z direction into the soil) compared with forests trees (less dense in the X-Y plane, but more developed in the depth, the Z direction). Given previous it is correctly to assume that the total water extraction reflected in the Kc factor (lumps together resistance to transpiration, crop height, crop roughness, reflection and crop rooting) for both is of the same order. This means that crop factors of crops or forests in similar vegetation states maybe vary with less then let us say 20 - 30

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 Kc. More justification is therefore required for Kc values. Perhaps improving the discussion on the previous page will help.

ANSWER: It is inherent on the approach that possible errors in drainage estimations will affect Kc but they also will affect all the other involved model parameters. Kc is a very sensitive parameter in the WAVE model. To quantify the effect of errors in drainage on the crop factor requires a Monte Carlo type error propagation analysis. The authors believe that the development of the latter is far beyond the objectives of this manuscript.

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

ANSWER: Some text to clarify this was added. Essentially a profile pit was dug in the various plots; soil layers were determined and in each soil layer root density was assessed visually.

P771L11 "October 1999 to November 2001"

ANSWER: OK.

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

ANSWER: OK. We changed it.

HESSD
2, S540–S553, 2005

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.

ANSWER: The set up of the throughfall collectors is conform to ICP directives (International Co-operative Programme on Assessment and Monitoring of Air Pollution Effects on Forests. www.icp-forests.org) since these stands are part of the Level II experimental plots. The throughfall collector consists of a funnel positioned 1 m above the surface and connected to a bottle buried in the soil. No radiation reaches the bottle and given the fact that the bottle is buried in the soil it is correct to assume that the bottle remains even in summer cool. As such the amount of water evaporated from the bottle can be neglected.

P772L1 "tube was installed"

ANSWER: OK.

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.

ANSWER: OK. Additional information was added. Not being experts in LAI methods the authors refereed to specialized literature, such as Jonckheere et al. (2004a). It is true that the methodology is calibrated for pine and might be different for broadleaves. However, Joncheere et al. have (2004b, submitted) demonstrated the importance of stand structure on LAI determination using fractal dimensions calculated on the hemispherical imagery. Canopy architecture has more influence on the LAI than whether the stand is broadleaved or coniferous, although canopy architecture is also influenced by the specie type . The applied methodology takes into account clumping and large open gaps and the LAI results are more correct then just using the LICOR-LAI. The

HESSD
2, S540–S553, 2005

correction parameters will be different for broadleaved and coniferous trees, but less important then the stand structure. Thus, the use of correction parameters on hemispherical imagery for coniferous on broadleaved trees is plausible (in absence of other correction functions).

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

ANSWER: OK.

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

ANSWER: Was properly addressed.

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.

ANSWER: (1) The method was used to disaggregate the weekly values to daily values. The method has not been validated (daily throughfall values were not available) and setting the regression line intercept to zero is perhaps not correct. But since the approach is just used as a disaggregation tool (interpolation) it might be correct to do so. If the intercepts are negative, then this approach results in an overestimation of the intercepted water. However the intercepts of the regression line likely does not contribute in a major way. For example we calculated the TF coefficients for Plot 8 (Oak)

HESSD

2, S540–S553, 2005

for the period 15 June to 14 September 2000. During this period the canopy is fully developed (maximal water interception may be expected). The regression coefficients, slope, intercept and R^2 between daily throughfall and total rainfall are 0.972 [-], -0.137 [mm] and 0.78, respectively. The regression with the intercept set to zero [mm] results in a slope of 0.959 [-]. With the average daily total rainfall of 2.29 mm the amount of calculated throughfall water is 2.09 mm in the case were the intercept is not set to zero. With the intercept of zero the amount of calculated throughfall water is 2.19 mm or only 4.6 (2) Indeed with this approach it is possible to inflate the long term values. But since this is the only data available it seemed to us the best estimation approach to assess the canopy interception water. (3) The evaporation from the collectors can be neglected due to the collector set-up as explained above.

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

ANSWER: This reference was in the 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.

ANSWER: Although the structure of forests is different from the structure of croplands this does not necessarily mean that the water consumption of both is different. In this study the water use was quantified and it was demonstrated that for the climate conditions of Flanders the water use of forests is larger than that of cropland. The analysis clearly revealed that forests consume more water than croplands.

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

ANSWER: OK.

Table 2 The units presented are inconsistent: choose whether PAI is m2/m2 or unitless.

HESSD

2, S540–S553, 2005

ANSWER: OK. Indeed, we were inconsistent in the manuscript. We choose for m^2 $m\text{-}^2$.

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 Kc? A two-season Kc?

ANSWER: We used a two season Kc with minimum values during winter and maximum values in the summer. The LAI increases linearly from the winter minimal value to the maximum value in the summer. A linear interpolation between the minimum and maximum Kc is applied to infer daily Kc values. This is explicitly included in the section on the “Model Description” since it seems to be confusing; although being a common practice in crop modelling. The effect of using only one Kc values (for instance the maximum value) is that more soil water is extracted then observed and hence the model underestimates the observed soil moisture content. The CD value will be larger than 1 (since the predicted SMC is larger in Eq. (5) CD will increase: or when the observed variance is larger then the variance between the prediction and mean observation). Only if i.e. the sum of the observations is larger than the sum of the predicted values, then the CD > 1. It is underestimation of the model but overestimation of the observations. Since these are model calibration and validation statistics, we expressed all results versus the model as reference. With CD > 1, the model SMC is underestimated. More on CD can be found in the reply to comment P769L13-14.

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.

ANSWER: Indeed the ME values are low for those plots. Yet, they are larger than zero and thus acceptable. Those plots with ME smaller than zero are not acceptable.

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

HESSD

2, S540–S553, 2005

modeling bottleneck.

ANSWER: (1) The statistics of the individual layers are not presented for the reason that it would require a very large table or multiple tables. Table 1 is a summary table. Statistics for the individual layers were however included when presenting the validation results. (2) Since the modelled SMC is higher than the observed SMC, it may indicate that more water is extracted (root uptake) from this layer than observed. It is plausible that fine roots (not detected in the visual characterisation of the root profile) are present in this soil layer but not taken into account in the model.

P780L1 "which may indicate"

ANSWER: OK.

P780L3 "ratio of ETact to ETc"

ANSWER: OK.

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

ANSWER: OK. But the goal was to take into account differences in stand characteristics (LAI) to situate differences in water use. We moved this to the discussion of LAI in Section 3.3.

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.

ANSWER: Since forest due to their deep rooting system can extract more water from the soil, forest are able to satisfy higher water demands of the atmosphere compared with cropland and better follow atmospheric peaks. Agricultural crops, due to their dense shallow root system, may deplete much sooner the plant extractable water storage capacity.

P783L2 and P784L18 If we know crop interception is 3-10 percent, why not take it into

HESSD

2, S540–S553, 2005

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.

ANSWER: In previous studies with WAVE interception of croplands was never taken into account. However for forests Ep should be calculated from ETc reduced with the evaporation of the canopy interception to finally asses Tp, the overall water balance does not change. For croploands the evaporation of the intercepted water is then part of Tp and part of EP. The soil water availability will affect the computed Tact. Consequently, the ratio Tact to Tp is smaller then when interception is taken into account. The average difference in water use between forest (ETact = Eact + Tact + INT) and crops (ETact = Eact + Tact) however stays the same.

P783L10-11 "not normally distributed"

ANSWER: OK.

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.

ANSWER: In Flanders, as stated in the Introduction, limited research has been conducted on the hydrology of pure forest stands. Including watershed scale results is only feasible for (small) catchments with the same (forest) vegetation cover. Since the climatic conditions in Flanders are similar to those of the Netherlands, where data for comparison are available, the discussion was focused on confronting the data published in the Netherlands and the data published in Flanders and derived in this study. However, we have included a line with a feedback of our results to the results mentioned in the Introduction. The fragmentation of the Flemish forests was not studied at all (beyond the goals of the project). WAVE was chosen because of the fragmentation

HESSD

2, S540–S553, 2005

Interactive Comment

and not as a tool to study it.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 761, 2005.