Hydrology and Earth System Sciences (HESS) Manuscript Review

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Title:
Opportunities and challenges for the use of scintillometer-based catchment-averaged evapotranspiration estimates as model forcing

General Comments
The objective of this paper is "to thoroughly examine to what extent the results of a rainfall-runoff model can be improved by forcing them with actual evapotranspiration data, obtained using a large aperture scintillometer, instead of using potential rates". The title of the paper also eludes to this. However, the scintillometer derived ET is not dealt with until page 20 out of a total of 24 pages in the manuscript. A large part of the analysis is dedicated to other analyses, i.e. the performance of the RR-model with standard potential ET parameterisation (a modelled yearly cycle) and Penman and Penman-Monteith formulations at various timescales. The authors should either change the title and general research objective to better cover the actual analysis done or leave out all other analyses that do not include the scintillometer data. In addition, the structure of the paper needs to be revised to better follow either of the two approaches.

My main problem with this paper is that the authors are not convincing me that their main result is not a trivial one. They have a model that has been optimised based on ~10 variables and a climatological yearly ETp cycle. If I understand it correctly, out of this modelled yearly cycle a daily average ETp is taken into the optimisation, ignoring the daily cycle. Next, they use the model with ET inputs with different time-scales and corresponding different amplitudes and they get a different result. To continue they use the LAS based actual ET, which has both a different time-scale and it by-passes the positional to actual ET step in the model, which was part of the original optimisation. Isn't it to be expected that that the model won't perform well using input parameters for which the model is not optimised?

The paper is well written. However, the structure of the paper needs to be revised. Also, the paper is very full, there are too many messages. The analyses are generally sound, but the discussion of the results could be better.

All things considered I recommend publication after major revisions

Specific Issues

1. Timescales. There are many time-scales used in the paper (hour-day-month-year) and it is not always clear which one is used in the current analysis. It becomes especially confusing when the ET rates are given in different units AND averaged over different times throughout the paper. ET rates are expressed as mm/hour, mm/day or mm/month and averaged over either a hour, day, or year. I understand

that the RR-model is run with a time step of one hour? It would be helpful to have a short section at the beginning of the paper that explains timescale convections used for rates and averaging periods.

2. Structure-Organization. The paper discusses the following ET input approaches:
    1. standard ETp input (climatological yearly cycle, the same one used for the optimastion) taken as a daily average
    2. P and PM ETp input taken as hourly, daily, monthly and yearly averages
    3. LAS based ETa input taken as hourly average

   It would be very helpful if this lay-out was communicated to the reader at an early stage and organize the sections in such a way that this structure is recognizable throughout the manuscript.

   Seeing that the analysis focusses both on varying the timescales and the type of ET model/measurements. Why not do all three ET approaches with all four of the timescales? In that way you can distinguish between the effect of varying the timescales from the effect of varying the ET input. Now, the two are mixed. For instance, compare first the modelled discharges using hourly vs daily averaged ETp both from standard ETp input based on the climatological yearly cycle before including P, PM and LAS ET's in the analysis.
   Constructing the hourly averaged standard ETp input data may require some work, as one has to superimpose a daily cycle on the climatological yearly cycle taking into account the varying day-length over the year. Ignoring the daily cycle, as is done now in the model, means that ET rates are the same during the day and the night, which is not realistic. Once the climatological, truly hourly averaged yearly cycle has been constructed, it can also be used to optimise the model, which will hopefully yield better model parameters when working with the hourly averaged ETp from P and PM or the LAS ETa. The same could be done for the monthly or yearly average.

3. The discussing of the results displayed in the very crowded tables with regression statistics is poor. For instance on line 24 on page 19: "Comparing the model performance statistics of this approach in Table 3 .., it is clear that PDM performs worse ...". There are hundreds of number given in these Tables; where do you want me to look? Which statistic in particular show the poor performance?

4. In addition to the item above, the statistics Tables 2 and 3 and Figures 3, 4, 6 7 are very busy and some of them are hardly discussed in the text. For instance the peak and low discharge analyses don't seem to add much.

5. LAS based ETact. The LAS doesn't measure ETact, it measure the sensible heat flux, which is used to estimate ETact using the energy balance approach as is done in TOPOPLAST. It is well known issue in micro-meteorology that the energy balance doesn't close (eg Foken, 2008). With the approach followed the energy balance non-closure is accumulated in the LAS ETact estimate. As a result, the daily averaged ET rate in wintertime is even negative (8mm in total in November!). This is not realistic. It is better to keep ETact as zero under these circumstances.
   Also, H will be very small during winter time, typically less 50W/m2. It is questionable therefore, whether it can be considered a LAS based ET estimate as the other terms of the energy balance (i.e. the TOPOLAST algorithm) will be dominant in the ETact estimate.

7. LAS saturation. The authors claim that the LAS will not saturate over 9.5km with a path height of 15m. This is not true. Saturation is to be expected. The BLS2000 software corrects for this effect. This correction is adequate as long as the saturation level is weak. How was this correction for this experiment? (corrected ad uncorrected Cn2 can be found in the BLS2000 software output)

8. Comparing Figs 6a and 7a: In 2007 (Fig6a) Qobs>Qsim_using_Ep for the entire year, whereas in 2010 Qobs<Qsim_using_Ep. How is this possible?

Minor issues:

1. Tables 2, 3 and 4: What are NS and CB? In general, the statistical variables in the tables are not explained in the caption nor in the text.

2. Tables 2, 3 and 4: Check the number of significant digits of the values given. The cumulative Q for instance is given as -2346.307mm. I doubt that the number behind decimal point are still significant.

3. Figures 3-10:
    - Increase the font-size of the tick-marks and axes labels
    - Fig6: part of the y-label is missing
    - Timeseries plot of Figs 3 and 7 only have 1 or 2 tick-marks on time (x) axis; increase this number
    - Timeseries plot of Fig 8 have 35 (!!!) tick-marks on time (x) axis; decrease this number
    - Fig 4: what is the meaning of a negative discharge (y-axis)?
    - Fig 10: too much information in one figure. Present your results in another way (eg Winter-Spring-Summer-Autumn).

4. Page 3 line 17: “However, Oudin ...”; delete “However”.

5. Page 4: line 2: “actual into potential ET” should be “potential into actual ET”.

6. Page 4 line 10-11: change “The impacts of ... are examined.” to “The impacts of ... are examined as well.”

7. Page 7 line 17: A reference is made to Flanders, but in section 2 where the catchment is described it is not mentioned that the catchment is situated in Flanders.

Literature:

Foken T (2008) The energy balance closure problem: An overview. Ecological Applications. 18(6):1351-1367.