

***Interactive comment on* “Selecting the optimal method to calculate daily global reference potential evaporation from CFSR reanalysis data” by F. C. Sperna Weiland et al.**

**F. C. Sperna Weiland et al.**

frederiek.sperna@deltares.nl

Received and published: 30 November 2011

We would like to thank reviewer 1 for his/her comments which helped us to improve the quality of our manuscript. Please find below a step-by-step response to the individual comments. The revised manuscript includes the required improvements, all minor comments have been included as well.

> General comments:

> 1. P7357, line 21: bias in radiation would also influence ‘offline’ calculation of PET

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Indeed, we do agree that bias in radiation may also influence off-line estimates of PET derived with Penman-Monteith and Priestley-Taylor methods. Yet, the more empirical relations, which do not use radiation as input, are unaffected by biases in radiation. We have added a statement on this to the introduction. “Of course, it should be noted here that off-line calculation of PET and AET can also be biased by deviations in GCM radiation used as input for some of the PET equations.”

> 2. P7358, line 17: however, local calibration has been undertaken successfully previously – and as is also carried out in this paper.

Added to manuscript: “Yet, Jensen (1966) showed that the climate dependency of the Blaney-Criddle equation disables its application in multiple different climate zones. To overcome this problem we tested the local-recalibrated Blaney-Criddle method proposed by Ekström et al. (2007).”

> 3. P7361, line 23: net incoming radiation is not included in the CRU TS 2.1 data set. Please explain how this variable was derived.

Added to manuscript: “As radiation is not included in the CRU datasets, a standard climatological maximum radiation cycle was calculated using the day-number and latitude as input (Allen et al., 1998). This maximum radiation was reduced to incoming radiation at the surface with monthly CRU cloud cover time-series.”

> 4. P7361, line 25: cloud cover is included in CRU TS 2.1, so why did you use the non-time varying CRU CL 1.0 data?

This is a mistake in the manuscript. Indeed the monthly CRU TS2.1 cloud cover time-series were used. This has been corrected in the manuscript.

> 5. Please comment on the implications of CRU-PM calculation procedure for PET accuracy, and subsequent comparison with CFSR – e.g. use of climatological wind-speed, given importance of windspeed for calculation of PET shown by other studies (e.g. Roderick et al. 2007)

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

We added a sensitivity analysis in section 3.1. Here we analyze the influence of the difference in (monthly) input variables of the Penman-Monteith equation from the CFSR and CRU datasets by replacing, one at a time, CFSR input variables with CRU equivalents.

> 6. Please comment on why differences occur in PM PET between CFSR and CRU – can this be pinned down to the influence of one particular meteorological variable (e.g. use of average rather than time varying wind, procedure for calculation of net radiation)? It would be highly informative to see a systematic analysis of this.

See comment 5 and section 3.1 of the manuscript. Within a sensitivity analysis we show that particularly radiation has a large influence. The influence of windspeed appears to be less important. As now also mentioned in the manuscript in section 3.2.1, the use of the annual radiation cycle with the CRU dataset partly explains the agreement between CRU derived PM PET and CFSR HG PET, where this cycle is used as well.

> 7. Additionally, it should be acknowledged that you are validating CFSR PM PET against a data set (and calculation procedure) that is itself of varying quality and subject to uncertainty.

We agree and are ourselves aware of the limited quality of the reconstructed CRU dataset. We now make this point in section 2.2 and section 3.1: “It should be noted that the measurement based CRU dataset is subject to inaccuracies as well.” Yet, CRU is one of the few available standards of observed climate and it should be noted that other studies have considered CRU derived PET as a reference before (Allen and Droogers, 2002; IPCC, 2007).

> 8. P7362, line 4: Were CRU data downscaled to daily resolution, and if so how?

In line 4 we refer to daily PET time-series derived from the CFSR dataset only, this has been clarified in the manuscript: “Within this study, daily PET time series, derived from

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

the CFSR re-analysis dataset with six different PET equations, were compared.” For the downscaling of the CRU dataset to a daily time-step, which was only required for running the hydrological model, we added a reference to the manuscript: “For application in the hydrological model the CRU time-series were downscaled to daily values using the monthly CRU precipitation and temperature quantities and their daily distribution as in the CFSR dataset (Van Beek et al., 2008).”

> 9. Figure 4 is too small. In general, all of the figures would benefit from being a little larger.

Fig. 4 is now divided in two separate larger figures. We will check in the final prints whether figures are clear enough. Their quality is sufficiently high that they can easily be enlarged (600 dpi).

> 10. P7362/7363: The stability of the BC calibration unlikely to be satisfactory under a changing climate – whether over the historical period or for scenario climate – therefore the validity of including modified BC in this study can be questioned. To a lesser extent, the same argument applies to the Hargreaves calibration.

Selecting the optimal PET method also means balancing between different needs. On the one hand a physically based equation like the PM equation considers changes in all variable, yet as also stated in the manuscript this makes the equation suspect to uncertainties in many input variables and less computational friendly. Therefore we also included some more empirical equations in our analysis.

We are aware of the fact that the stability of the modified BC equation may not be guaranteed under changing climate conditions. This is mentioned several times in the manuscript (abstract, introduction, section 3.1.4, conclusions). Still it was decided to include a local re-calibrated form of the BC equation, as several attempts exist in literature. Our analysis again shows that the equation is likely to be unstable under changing climate conditions due to its spatial varying re-calibrated coefficient values.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

In the conclusion we mention that the Hargreaves equation is less sensitive to changing climate conditions, as the equation is globally uniform modified by increasing the multiplication coefficient from 0.0023 to 0.0031. This modified version of the Hargreaves equation shows reasonable performance over the whole globe and is therefore expected to be more stable under changing climate conditions.

> 11. On the other hand, given that both BC and HG are calibrated it is a little strange that calibration of the alpha parameter in the PT equation was not at least discussed.

The PT equation could have been re-calibrated as well. Yet, we found more references of local or large scale re-calibrations of the Hargreaves and Blaney-Criddle equation and therefore decided to focus on these two equations in our global analysis.

> 12. Phrasing is awkward in a number of instances, although it remains possible to understand the MS (some examples included in Minor Points, but too many to mention individually)

The manuscript was re-checked and several sentences were reformulated.

> 13. Although reference is made to previous papers which have described the PCR-GLOBWB, some further information is required in this paper – including the extent to which the limitations of this model influence this study.

We included discussion on the representation of crop types in the hydrological model, the method that describes the saturation of the soil which influences actual evaporation and the relative coarse resolution which influences the quality of modeled discharge.

> 14. Whilst able to follow the general results, I got a little bogged down reading through the various different analyses described in Section 3. Is it possible to simplify this section?

We removed the analysis of minor details to clarify the red line and main messages. In addition the comparison of global average RMSD values for the different methods has been removed and several sub-sections have been merged.

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 7355, 2011.

**HESD**

8, C5110–C5115, 2011

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C5115

