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Hydrology and Earth System Sciences Discussions

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.

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We would like to thank reviewer 2 for his/her critical review. First of all we would like to point out that we slightly adjusted the title of the manuscript to emphasize our interest in the influence of the differences in PET on final modeled discharge. We do not only want to create a global historical daily PET dataset, in this paper we also test the suitability of these time series for use as hydrological model input, in this case the hydrological model PCR-GLOBWB. Therefore we also included an analysis of the translation of the differences between PET methods to actual evaporation, runoff and discharge and in

addition we investigate the necessity of selecting the correct method.

In the revised manuscript we included discussion and analysis of seasonal results. In addition we have re-written several sections. Please find below our replies to the specific review comments. Changes have been made to the manuscript accordingly.

> Many statements are arguable and possibly not true. To quote only a few found in the introduction:

> E.g. 'Penman-Montieth is (: : :) preferred over simpler temperature based methods (: : :) because it includes the effects of changes in multiple atmospheric variables'. While it is true PM uses several atmospheric variables, its main advantage is to be physically-based rather than being a purely empirical method.

We re-formulated this sentence in the manuscript. In the end we did mean the same as the reviewer is here proposing. PM is indeed the only fully physically-based method and therefore it can consider changes in multiple atmospheric variables under changing climate conditions.

> E.g. '[Global Hydrological Models (GHM) using PET as input have Actual Evapotranspiration] (: : :) processes schematized in more details [than Global Climate Models]'. With the majority of current GCM being coupled/ including complex land-surface schemes, it is doubtful that AET is better estimated through a (non land-surface) GHM. Because the GHM do not have dynamic vegetation growth and energy exchange schemes, they have been developed to use as input the PET, as limit of the maximum loss through transpiration, and model parameters are calibrated so that the resulting runoff matches as much as possible observations. AET (and soil moisture) are internal variables rarely checked for consistency with observations, and as such, cannot be claimed to be more accurate 'as a principle' that when calculated from a modelling scheme.

We did tone this statement down in the manuscript. Yet, we do not completely agree with the comments on a 'non land-surface' GHM and "the lack of dynamic vegetation growth". As mentioned in section 2.4, PCR-GLOBWB includes two soil layers, one groundwater layer and a vegetation cover existing of two vegetation types (short and tall vegetation, i.e. grasses / low crops and trees) Although only two cover types are used to represent land cover at the cell level, each type represents the sub-grid variability in land cover conditions by incorporating the GLCC version 2 land cover types (Olsen, 1994a, b; USGS Eros Data Center, 2002) at a resolution 30 arc seconds. Moreover, each land cover type complies to a monthly climatology of phenology that influences hydrology, including evapotranspiration. While this scheme is more parsimonious than those used in LSMs, it is capable to mimic soil moisture dynamics in detail in response to climate and land cover conditions.

> E.g. 'Influence of biases and uncertainties in PET usually decreases in the hydrological modelling chain'. This statement should be toned down as this would be extremely dependant on the season and location. In fact, when looking at monthly or daily time step, there is an argument that the uncertainty in PET generates significant uncertainty in runoff – in particular when the fine balance between rainfall and PET becomes different from expected from observations.

We modified this statement in the manuscript. We should not have included the uncertainties in PET in this sentence. What we meant to say was that the biases in PET, or in other words the differences between the different methods, decrease throughout the modeling chain while PET is converted to AET and runoff. This decrease is related to the fact that the PET demand can not always be met by soil moisture conditions (mainly in arid regions) and that precipitation also highly influences the final runoff amount (mainly in humid regions). For example the absolute difference in runoff is much smaller than the absolute difference in PET in northern wet regions or for example in Asia, where Monsoon rainfall highly influences total runoff amounts. Although the reviewer questions the value of the CV maps in fig. 9, we do believe that these maps illustrate the decrease in difference (i.e. the decrease in CV values) between

the six methods while moving through the hydrological modeling chain. Potential evaporation amounts are reduced in actual evaporation in many locations for at least part of the year due to limited soil moisture variability (for example in arid and semi-arid regions). In addition, for this application of potential evaporation within a hydrological modeling study we are also interested in changes in runoff and eventually discharge. For runoff not only limited soil moisture availability reduces the difference between PET methods, rainfall reduces the variability between methods as well, as it is also a major component of the water balance at many locations (for example in Monsoon influenced basins, the tropics and other humid regions).

> The analysis is somehow weak and should be strengthened: most assessments are made at the annual level, while it is the seasonality of rainfall/PET and their relative value which is important for the water balance. The authors focus too much on discussing annual spatial patterns of PET, of little interest, and omit the spatial-temporal aspect. More hydrologically-relevant analysis and discussion must be added.

First of all it should be noted that the RMSD values in figure 6 are derived from cell-specific CFSR and CRU based monthly PET time-series and not annual times series, thus providing preliminary information about the inter-annual variability related to the PET methods. In addition, seasonal PET maps have been added to the revised manuscript (Fig. 4). Discussion on the seasonal PET maps is now given in section 3.2.2.

> A large part of the paper discusses the sensitivity to climate of the particular GHM used (here PCR-GLOBWB) – not PET as suggested in the title. This is arguably outside the paper's aims, as the results showed regarding runoff (and AET) are strongly dependant on the GHM used (and its way to estimate AET, in particular its vegetation scheme and soil parameters). Suggest to remove, or if stays, the fact that the results are entirely dependant on the GHM used must be strongly emphasised in the text.

We do now clearly state that the results for AET and runoff are influenced by the GHM used (2.4): "It should off course be noted that the influence of biases in PET on modeled actual evapotranspiration, runoff and discharge also depends on the GHM used. Therefore the results of this study can not be generalized to all GHMs."

Yet, as we performed this analysis also to ensure that PET time-series used for hydrological modeling will not deteriorate modeled runoff, this analysis is to our opinion an important part of this study. We state this clearly in the introduction: "In addition the dataset should be a reliable reference for the statistical downscaling of daily PET time series calculated from raw GCM data which can be used as input for hydrological models within climate impact studies".

> Section 2 and section 3 are very heavy and difficult to read. They would strongly benefit to be merged. In section 2, there is little justification in the choice of the analyses. As already mentioned, why is the evaluation done at an annual and not monthly time scale? The purpose of the tests and the information they would provide is not given, and some tests non understandable.

Within section 2 a justification for the use of the different methods is now given. In addition redundant text has been removed. The visualization of the comparison, within the main part of the manuscript, focused on annual results just because of the large number of pictures that would otherwise have been required. RMSD values have been calculated from cell-specific monthly time-series. In the revised manuscript seasonal PET maps are included as well. These maps are discussed in section 3.2.2. We decided not to merge section 2 and 3, because some methods explained in section 2 apply to multiple parts of section 3 and because we want to maintain the conventional division of a research paper with one section on data and methdods (including statistical analysis methods) and one section with results.

> E.g. it is not clear what variables are used in the Welsh test – the text mentions '24 numbers' but it is not clear if they represent different variables (in which case, PET is not what is evaluated) Is that calculated on annual averages?

Indeed, we have clarified this in the revised manuscript: "Significance of differences between annual average CRU and CFSR derived time series has been quantified with the Welch's t-test." Significance of differences is calculated for: "the annual average PET, AET or runoff value".

> Presumably, this test aims to quantify the sensitivity of the GHM to error in the input data. Again as mentioned previsously this is out of the scope of a paper on PET daily time series, but and would be better in an hydrological model paper.

This analysis is of interest to our study, as on of the goals is to select the most suitable PET methods for GHM applications, the Welch's t-test (having the same philosophy as the t-test) highlights which PET method is likely to provide the highest agreement with CRU-derived simulations. To emphasize our interest in the impact of the different PET methods on modeled discharge we slightly adjusted the title of the paper.

> Same comments of lack of clarity/ justification/rationale can be made for RMSD: there, spatial biases must be discussed, not only global averages of seasonal bias and really maps of seasonal biases should be provided: : :

Seasonal PET maps have been included in the revised manuscript, see also section 3.2.2. Seasonal maps with AET and runoff are included in the supplementary information.

> Additional analyses and discussions should include : whether biases in PET are larger for some months/seasonals for particular formulations, or if they all behave similarly throughout the year, and have a 'systematic bias' everywhere; whether some formulations perform better under specific climates (e.g. temperate humid, temperate dry, arid, tropical etc: : :) – this could for example be the case for temperature-based methods which will account for the limiting factor of humidity in the estimation of PET. The differences in input data (CRU and CFSR) ought to be compared, as PET is estimated with different combinations. This would give a possible idea of where the main biases in CFSR exist, and the sensitivity of the different PET formulations to different

input data. The introduction somehow suggested this might be treated in the paper but it is not: understanding the relative consequences of biases in different variables used for PET would be a valuable information to climate modellers.

Here we followed up on a suggestion given by the other reviewer and analyzed the influence of the difference in input variables of the Penman-Monteith equation from the CFSR and CRU datasets. See section 3.1 for a sensitivity analysis in which different CFSR input variables are replaced one at the time with CRU equivalents. Section 3.2.2 discusses the seasonal differences in PET derived with the different methods.

> It is not clear how useful the analysis using 'CV' is, nor how relevant comparisons of 'actual evapotranspiration' or runoff [the statement of 'correct' must be removed from page 7367]. Suggest to remove.

As mentioned before, although the reviewer questions the value of the CV maps in fig. 9, we do believe that these maps illustrate the decrease in difference (i.e. the decrease in CV values) between the six methods while moving through the hydrological modeling chain. We admit that this analysis was performed on annual averages, yet within a global analysis there are too many results that need to be summarized and therefore here only maps with CV values of the annual averages of the different methods are given. Moreover, especially annual average AET can be interpreted as a summary of the seasonal varying values in PET and runoff as it influences, and is influenced by, the long-term water balance through amongst others soil moisture storage and depletion.

> There are some contradictions in the text that must be removed: e.g. page 7374 lines 23- 'selection of PET is of minor relevance for modelled discharge' and same page, lines 12-13: 'contrary to Oudin (: : :) the selection of a PET equation does influence modelled discharge'. Another (pages 7377 and 7378): 'PET is globally best calculated with the BCrecal equation' and 'daily BCrecal PET spanned a relatively small range of daily PET values (: : :) and discharge derived from BCrecal PET is too low compared to other methods for most basins'.

Part of this text has been re-written. Note that we state that: "Overall, the re-calibrated forms of the Blaney-Criddle and Hargreaves equations applied to CFSR data seemed to be best suited to derive daily PET times series." and pose the critical remarks listed in this comment of the reviewer.

> The discussion and conclusion would benefit from a better structure and complete re-writing to avoid mis-interpretation of the results

Section has been re-written, merged and reduced

> Some minor comments:

> CFSR must be written in letters when first used

Changed

> Very wordy introduction. No clear rationale for the choice of PET methods, nor which ones will be tested.

The introduction has been reduced and the motivation for the selection of the PET methods is clarified.

> No justification why NCEP/NCAR is preferable to the ERA-40 re-analysis.

Indeed no justification for the use of NCEP/NCAR instead of ERA-40 re-analysis data is given. Yet on page 7359 line 18 and further we do motivate why we are using the CFSR dataset. This is subjective a choice, the ERA-40 or ERA-interim re-analysis dataset could have been used as well.

> Imprecision/inconsistency in the text: spatial resolution of CFSR given to be 0.3_ page 7359, but 0.5 _ page 7360

The resolution of the CFSR dataset is described in section 2.1: "a resolution of approximately 0.25 degrees around the equator to 0.5 degrees beyond the tropics" in the introduction its resolution is now indicated as ∼0.3 degrees. The original CFSR dataset is interpolated to a regular 0.5 degrees grid as required by the hydrological model. On page 7360 we state that the main goal of this study is “the construction of a global gridded dataset of reference PET at high spatial (0.5 degree) and temporal (daily) resolution from CFSR reanalysis data”. As stated in section 2.1 “These daily (CFSR) time series were then interpolated to a regular 0.5 degrees grid using bilinear interpolation”

> Need for generating a new PET series different from CRU not clear. Presumably because the GHM requires daily, rather than monthly, data.

The GHM can handle both daily and monthly fields. It should be clarified that in principle the model complies with the FAO guidelines (Allen et al., 1998) and computes PET with the Penman-Monteith method on a monthly basis, from monthly values. Within the model, this monthly potential evapotranspiration is than broken down on the basis of the daily anomalies in temperature relative to the actual monthly mean so that the monthly total remains unchanged. This method of downscaling is now also mentioned in the introduction “Within hydrological model studies monthly PET time-series or monthly PET time-series downscaled to daily values (for example based on temperature) have frequently been used (Van Beek, 2008; Sperna Weiland et al., 2010; Arnell, 2011).” Yet, as the daily fields of the required input variables of the PET equations increasingly become available, using daily PET time-series can be more convenient than upscaling the daily input variables to a monthly timestep, calculate monthly PET and downscale this monthly PET to daily values using daily temperature alone. This information is added to the introduction.

> Rationale for a local calibration for BC but a global calibration for HC must be clearly mentioned.

This is now more clearly mentioned in the last paragraph of section 2.3 where al PET equations are described.

> Quite a few references are missing from the list.

Checked, indeed several references had to be added to the reference list.

> Often, it is not clear which combinations of climate variables are used in the GHM, and why (e.g. 3.1.3: is the hydrological modelling done with CRU precipitation?)

We have clarified this aspect in the revised manuscript.

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