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.

Anonymous Referee #2

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General

The paper investigates the role of different formulations in the uncertainty in Potential Evapotranspiration when calculated from Global Climate Model outputs (here a reanalysis product, CFSR) and the resulting uncertainty in runoff calculated using a Global Hydrological Model. Uncertainty in PET has been long ignored in the chain of uncertainty of global (and regional) hydrological modelling but has lately seen an increase in interest. This is an important issue in hydrological modelling and is of interest for the HESS journal.

However, the paper in its current form is not ready for publication and would need a major revision (mostly re-writing, but more focused, including analyses more hydrologically-relevant than is currently the case) before being acceptable. Generally, the structure and text of the paper are rather heavy, and some of the sections difficult to follow and some statements erroneous or open to debate. The maps are too small, and generally based on long term annual statistics rather than a more hydrologically relevant seasonal analysis. The general feeling of the paper is of deception. Some particular points worth noting [in no particular order]:

- 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. 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. '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.

- 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.

- 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.

- 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. 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? 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. 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. . .

- 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.

- 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.

- 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'.

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

Some minor comments

- CFSR must be written in letters when first used

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

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

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

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

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

- Quite a few references are missing from the 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?)

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