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Hydrology and Earth System Sciences
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# *Interactive comment on* "How over 100 years of climate variability may affect estimates of potential evaporation" *by* R. P. Bartholomeus et al.

**R. P. Bartholomeus et al.**

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Dear Referees,

Thank you for the positive assessments of our paper 'How over 100 years of climate variability may affect estimates of potential evaporation' (Paper hessd-11-10787-2014). We appreciate the recognition of the relevance of the work and the thoroughness of the analysis.

Herewith, we provide a revised version of our manuscript in which we incorporated all suggestions and remarks made by you. To our opinion, incorporating all valuable suggestions has clarified and strengthened the manuscript, especially because we:



- provide more insight in the errors made in relation to the structure of the evaporation models

- put more emphasis on the quantification of the error that could be made by using crop factors

- give suggestions how to correct for such errors.

Detailed responses to all comments of the referees can be found in the following pages. The revised manuscript, with the changes highlighted, is provided as supplement to this letter.

Yours sincerely,

Ruud Bartholomeus

Detailed replies to the referees' comments

Referee #1

#1_1: General Comments: The authors have done a lot of work characterizing the variability in ET estimates using various calibration periods over the last century. Their overall conclusion is that using calibrated coefficients extrapolated from a short period of time under different climate conditions can lead to systematic differences between empirical and process-based models. This is not a surprising result, and I would be interested to see the results presented in a way that gives readers tangible information that allows them to make the best decision of how to model ET given limited radiation or ET measurements.

Reply 1: We agree that it is widely acknowledged that the application of empirical coefficients is limited to their period of calibration. However, although this limitation of the two-step approach is known among both scientists and practitioners, the approach is still regularly applied in hydrological modeling studies on different spatial and temporal scales without appropriate consideration or warnings. Our study is novel in the sense that we provide quantitative information on the limitations of the two-step approach for different vegetation types and estimation procedures (potential evaporation) using a very long time series, allowing multiple 30-years periods to be assessed. Such an approach can be used in similar modeling studies to i) derive uncertainty ranges for the parameters, ii) quantify the errors that are introduced by a specific method and set of parameters, and iii) correct for the errors when they are predictable.

In the revised version of the manuscript we put more emphasis on the quantification of the potential error and how one could correct for this error, rather than simply stating that applying empirical coefficients for extrapolations may introduce errors. We added a section in which we provide more guidelines on the choices one should make in evaporation modeling. See p.1., l. 10-11 and l. 21-24; p. 6-7, l. 31-12; p. 18, l. 8-29; p. 21, l. 17-23.

#1_2: I think the paper would benefit from an additional section examining the reliability of published crop coefficients and commonly used parameters for ET estimations over the period of record, and draw some general conclusions about that. At the very least, the authors should include more context for the estimated parameters generated in this study in terms of how they compare to already published values (they cite Feddes, 1987 and Allen, 1998 – others to look at could include Shuttleworth, 1992 or other ET factors in hydrology reference texts).

Reply 2: We agree that comparing crop factors that are being used for the meteorological conditions in the research area, i.e. the Netherlands, could be a valuable addition to our analysis. Therefore we added a comparison of crop factors to the revised manuscript (see p. 17, l. 6-19). It should be realized, however, that comparison with published crop factors is misleading, as these are obtained for the non-calibrated Makkink reference evaporation. Nevertheless, besides comparing model derived crop factors with measured ones, we show the variability in crop factors, caused by changing climatic conditions. A comparable variability can be expected for published crop factors. The analysis thus provides insight into the uncertainty ranges that can be ex-



pected for published empirical coefficients and this information can be used to better judge the uncertainty in the results of a modeling exercise (see p. 17, l. 19-23).

#1_3: In addition, a number of the figures are difficult to read – I'd suggest presenting a representative figure or few figures from some of the multiple-pane plots and explaining the differences between groups in the text.

Reply 3: We have already limited the number of panes by not showing all results, for all crops and all evaporation components. In our opinion, further limiting the number of panes might hamper the clarity of the results. We therefore decided not to remove figures from the multiple-pane plots.

#1_4: Specific comments: Figure 2: What is the significance of the dashed lines compared to the solid lines?

Reply 4: These lines were only dashed to clarify the specific connection; we clarified the connection in the revised manuscript. See Figure 2.

Referee #2

General comments

#2_1: This paper investigates how non-stationarity in climate data can influence the estimates of potential Evaporation using the "two step" or crop factor approach. Overall this is a timely discussion to have. It is more and more clear that there is a large amount of variation in the climate and this affects the performance and behavior of hydrological and climate modelling if parameters in the model are considered stationary. Simply put, non-stationarity is unexplained variance. On the one hand, it is good to indicate these issues and to warn practitioners, but on the other hand, do we really believe we can make accurate predictions outside a calibration period? This is not even true for simple regression models, so why would it be true for calibrated hydrological and climate models. Any extrapolation outside calibration data is going to suffer from increased uncertainty. This has been known for years. The question might be more, why is this easily forgotten, and how do we deal with it? The probable reason why it is easily forgotten, is that we believe that our models, because we are attempting to represent real physical processes, are not regression models.

Reply 5: Thank you for your comments, which are related to the comments made by reviewer #1 (see Reply 1). We fully agree that although experts know that the two-step approach introduces errors, many seem to not be aware of which methods they are actually using and what the consequences may be for their modeling studies. The limitations of the two-step approach are often neglected, and apparently there is a need to demonstrate its limitations. We included additional text on this topic in the revised manuscript.

We agree with the reviewer, that applying coefficients outside their calibration range is a major flaw in research. We believe a primary reason for this recurring problem is that warnings about extrapolation are often qualitative and therefore extrapolation sensitivity can be disregarded as "noise" within a larger "signal". This shows the importance of this research – by quantifying the sensitivity of evaporation to this commonly overlooked assumption, we hope to stop the propagation of this error in future studies.

With our analysis we now quantify the potential errors, which provides insight in the reliability of the method. Such analysis supports both scientists and practitioners to decide which method is appropriate for their analysis. See Reply 1 for the corrections that are made in the revised manuscript.

#2_2: What I really missed in the paper is a solution. We could define the uncertainty and attempt to adjust the management to deal with the uncertainty, but this is rather unsatisfactory as a scientist. The other, more important approach, is to find a way to modify the model to deal with the issue. Are you suggesting we throw out the two-step approach? Or can we adjust the two-step approach? In the end, Figure 10 actually indicates that there is some pattern in the over and underestimation, both between models and in time periods. So there is some predictability in the actual deviations.



This would have been nice to explore.

Reply 6: Thank you for this valuable suggestion. We extended our analysis to provide guidelines to estimate the potential error that is made in extrapolations, based on the differences in climatic conditions between reference period and application period. See p. 18, l. 8-19.

#2_3: The other issue of interest that emerges from the paper is the comparison between models. While this is highlighted (Hargreaves and Blaney-Criddle versus Makkink and Priestley-Taylor), it is not really analysed in relation to the structure of these models. Why do the temperature models fail more than the radiation driven models?

Reply 7: We explain the differences in the revised manuscript, supported by statistical correlations between parameters used in the different Eref methods and Eref_PM trends. See p. 12-13, l. 28-6.

#2_4: Finally there is the difference between vegetation. While this is just synthetic data, this incorporates the "current knowledge" about the evaporation from these vegetation types. In addition, the variation between veg types appears to be lower than between models. Is this interesting?

Reply 8: The last paragraph of section 3.2 explains differences between vegetation types, based on their structure. We would like to note that Figure 7 actually shows that the variation between vegetation types is larger than the variations between models.

#2_5: So, while I think the analysis is tidy and neat, and the topic of interest, I miss depth in the article to actually progress the science and the application.

Reply 9: We believe that the suggestions of the referees and the additional analysis included in the revised manuscript strengthened the scientific aspects of our analysis and now provides sufficient guidelines for both scientists and practitioners to quantify and minimize potential errors induced by simple regression models and empirical coefficients in the two step approach for estimating potential evaporation.

Specific comments

#2_6: I have a few specific comments P10792 line 27: no-analogue? Is this a typo, I wasn't sure, should this be non-analogue?

Reply 10: no-analogue is the correct term. See e.g. http://www.sciencedirect.com/science/article/pii/S0921818112002299; http://www.plosone.org/article/info%3Adoi%2F10.1371%2Fjournal.pone.0006825#pone-0006825-g003

#2_7: P10795 line 5 & 6: The accuracy of SWAP, It is not really irrelevant. I think you need to at least identify whether the choices of parameters in SWAP would affect the variability and the relative proportions of the calculated E components. So has your choice of crop, soil depth etc affected the different E component variation in time. You are assuming that the relative relationship between Ei and other E components is invariant of your crop choice and soil depth. Page 10797 line 14, this might cover my previous comment, but still worth checking.

Reply 11: We selected vegetation types ranging from grasses, to shrubs and forests to demonstrate that our findings hold for different vegetation structures. Each of these vegetation types has its own specific parameter values. Therefore, these different vegetation types already include different choices of parameters in SWAP that affect the variability and relative proportions of the calculated E components. We clarified this in the revised manuscript (see p. 8, l. 21-24). Additionally, Ei is not invariant of crop choice, as the simulated interception is vegetation dependent. As already indicated by the referee, we took standard values for these vegetation classes as used for the National Hydrological Instrument for the Netherlands. Soil depth is not relevant, as we only consider potential evaporation.

#2_8: Page 10799 line 24: Would it worth highlighting what in these models causes



this? They are both calibrated on the same data, both temperature based, but given the same temperature series one deviates downward (under climate change) and one upward, even though the temperature series has the same direction for both. Looking at the equations in Table 1, both use average temperature (which is supposedly increasing), but Har also uses Radiation and the difference between Tmax and Tmin, which might be stable

Reply 12: This is an interesting observation; the different directions in change for BC and Har are caused by a general decrease in Tmax-Tmin, while the mean temperature increases. This is added to the revised manuscript (see p. 13, l. 7-9).

#2_9: Page 10805 line 7: advance in the ability

Reply 13: This has been corrected to advance in the abilities (see p. 20, l.1).

#2_10: Page 10805 line 12: assumptions (plural)

Reply 14: Corrected (see p. 20, l. 6)

Referee #3

#3_1: This paper evaluates the sensitivity of the two step approach to calculate evaporation to the length of the calibration period and the chosen reference years. It compares four different two step evaporation methods with the Penman-Monteith method and compares these five methods with potential evaporation obtained with the process based SWAP model for four vegetation classes. The analysis shows that the empirical equations are highly sensitive to the length of the calibration period and the timing of the selected period and are therefore hard to transfer in time to use for example in climate impact assessments.

General comments: The paper is written very clearly, especially the introduction that provides a very good setting for the paper. To my opinion the description of methods and results misses some background information which I will further detail below. The lengthy dataset used is very valuable for this demonstration, yet this is also an ideal situation where all atmospheric variables are available. The authors could maybe elaborate a little more on what one could do when this information is not available, i.e. the Makkink and Priestley-Taylor methods seem to be doing relatively well.

Reply 15: Thank you for the positive response on the manuscript. In the revised manuscript we provide guidelines to predict the error that is made by using different methods, which have different data requirements (See Reply 1 and Reply 5 for the corrections that are made in the revised manuscript).

#3_2: Moreover, this paper only discusses a Dutch site, can this information be transferred to other locations on the globe or would the results be different for other climate zones?

Reply 16: The absolute values and differences are case specific and thus not applicable to other regions. Nevertheless, the sensitivities identified in this study are related to the models themselves and how they are affected by different climate fitting parameters. While projected changes in radiation and temperature vary globally, the general trends are consistent, and it is reasonable to expect that similar differences identified for this specific case can be expected for other climatic regions. We extended the description of the site and how it resembles global trends in the revised manuscript (see p. 9, l. 17-23).

The chosen site is unique in that it has a long enough historical record to allow for comparisons across different sub-periods. The majority of climate stations have much shorter records, which would not show the change in extrapolation errors through time.

#3_3: The discussion of SPEI values is very good, interesting to see the influence of the calculated evaporation on a relevant indicator. Overall the only drawback is that the results and conclusions are not really novel information.

Reply 17: This comment is similar to those raised by Reviewer #1 and #2 and addressed in Reply 1 and Reply 5).



Specific comments:

#3_4: - The paper provides figures and information of the newly calibrated two step approaches. It is unclear how the results compare to the un-calibrated equations with default values from literature. The same applies for the calibration of crop factors. How do these compare to crop factors from literature and how does the calculated evaporation compare to evaporation calculated using these standard values?

Reply 18: We now provide information on calibrated Eref parameters and compare obtained crop factors with those from literature. See Reply 2 and p. 17, l. 6-23.

#3_5: - The variables involved in calibration are very briefly mentioned in section 2.3 for the reader it is hard to see to which equation these apply. Maybe also mark the variables bold in the equations in Table 1.

Reply 19: The calibration variables are also presented and explained in Table 1, which we now clarified by marking them bold. See Table 1.

#3_6: - In the introduction the authors mention a multiplication factor of 1.1 – 1.3 if interception is involved – has this factor been considered in the remainder of the study? Could the (non)-use of this factor influence the results?

Reply 20: This factor should only be used in combination with Kt and if interception is not simulated explicitly. Therefore, because we simulated interception explicitly, in our study the multiplication factor has not been used. We clarified this in the revised manuscript (see p. 5, l. 30).

#3_7: - Can the calibration or set-up of the SWAP model be considered stationary over time and does this influence the analysis?

Reply 21: Considering stationary vegetation does not affect the results, as we study potential evaporation in time for different vegetation classes instead of specific sites with a dynamic vegetation. Additionally, simulating dynamic vegetation and herewith succession, would unnecessary complicate the analyses.

#3_8: - Section 4 is structured in a non-logical order. I would suggest to either add section 4.2 and 4.3 to the results section or move 4.1 to the end of section 4.

Reply 22: We moved section 4.2 to the results sections, but kept section 4.3 in the discussion section, as 'implications' fit best there. See p. 15, l. 11-30

Corrections:

#3_9: - Both data sets and datasets are used

Reply 23: the occurrence of data set has been corrected to dataset (see p. 9, l. 29).

#3_10: - Section 3.1 Deviation deceases should read Deviation decreases

Reply 24: Corrected (see p. 13, l. 21).

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C5947/2015/hessd-11-C5947-2015-supplement.pdf

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