Editor’s decision submission 1

Ref.: "Parametric modelling of potential evapotranspiration: a global survey" (Mr. Aristoteles Tegos)

Dear Mr. Tegos,

I very much regret to have to tell you that publication entitled, "Parametric modelling of potential evapotranspiration: a global survey" (Mr. Aristoteles Tegos) in our journal is not recommended.

We would, however, consider as a new submission for review a substantially revised version of this paper that addresses all of the reviewers' comments. Should you choose to submit such a revised manuscript please refer to the present manuscript number, provide a detailed point-by-point reply to all of the reviewers' comments, and state how the revised manuscript addresses these.

An explanation for this decision is given in the attached review reports (and on https://ees.elsevier.com/hydrol/). I hope that the comments contained therein will be of use to you.

Thank you for your interest in our journal.

With kind regards,

Konstantine P. Georgakakos, Sc. D.
Editor
Journal of Hydrology

COMMENTS FROM EDITORS AND REVIEWERS:

AE:
The author(s) designed and calibrated a global parametric model of potential evapotranspiration (PET) with two parameters (temperature and extraterrestrial radiation) in this paper. The intention of this work is acceptable but the realization of this objective and the writing in current form need significant improvements before you resubmitting it to journals.

Reviewer #1: I cannot see new contribution in this paper.

Reviewer #2:
This paper investigated the relationship between climatic factors and PET with worldwide available data of climatic variables and estimated PET based P-M equation, and estimated the global distribution of two parameters of a two parameters empirical equation for estimating PET only considering temperature and the extraterrestrial radiation. In fact, although P-M formula is still subject to the data limitation, it is the excellent equation for estimating the PET, especially under the background of climate change, which is widely acknowledged. Aerodynamic factors were proved to play the important roles for explaining the PET changes and cannot be neglected (e.g., Rayner, 2007; Roderick et al., 2007, 2009a, b; McVicar et a., 2012; Wang et al., 2012; Li et al., 2013). Meanwhile, the meteorological observation abilities with enough spatial

resolution is continuously in processes of developing. Thus, generally, physically-based methods would be more considered and have wide application. Moreover, the statistic investigations in this study are primary, some of conclusions from them are thus common sense. Meanwhile, the illustrating ways of most figures are need improved. Therefore, although the submitted paper of Tegos et al. is a well written study based a broad data base, I cannot recommend the manuscript for publication in the present form. Some other detailed comments are given below.

(1). Section 3.2, Some conclusions are not rigorous. For example, "Surprisingly, mean annual sunshine duration is slightly less correlated with mean annual PET than extraterrestrial radiation, although the former is expected to be better estimator of the actual solar energy received in the Earth's surface. This is a very important conclusion, which confirms the suitability of radiation-based approaches, using both temperature and extraterrestrial radiation as explanatory variables of PET". The analysis should be conducted under different sub-regions with similar climatic conditions to identify whether the conclusions.
(2). Section 3.3, Why selected five stations in Australia to conducted the investigation rather than global typical stations?
(3). Figure 2-Figure7 can be combined in one Figure
(4). Figure 11. More detailed classification for the stations should be given rather than only two types.
(5). Figure 14 and 15, Please merge the different parts into one world map

Used literatures

McVicar, T. R., et al. (2012), Global review and synthesis of trends in observed terrestrial near-surface wind speeds: Implications for evaporation, J. Hydrol., 416-417, 182-205.

Rayner, D. P. (2007), Wind run changes: the dominant factor affecting pan evaporation trends in Australia, J. Clim., 20(14), 3379-3394.

Roderick, M. L., L. D. Rotstayn, G. D. Farquhar, and M. T. Hobbins (2007), On the attribution of changing pan evaporation, Geophys. Res. Lett., 34, L17403, doi:10.1029/2007GL031166.

Roderick, M. L., M. T. Hobbins, and G. D. Farquhar (2009a), Pan evaporation trends and the terrestrial water balance. I. Principles and observations, Geogr. Campass, 3, 746-760.
Roderick, M. L., M. T. Hobbins, and G. D. Farquhar (2009b), Pan evaporation trends and the terrestrial water balance. II. Enery balance and interpretation, Geogr. Campass, 3, 761-780.

Wang, W. et al. Reference evapotranspiration change and the causes across the Yellow River Basin during 1957-2008 and their spatial and seasonal differences. Water Resour Res 48, W05530, doi: 10.1029/2011WR010724 (2012).

Li, Z. et al. Analysis of changing pan evaporation in the arid region of Northwest China. Water Resour Res 49, 2205-2212, doi: org/10.1002/wrcr.20202 (2013).

Reply to the review comments for the paper entitled "Parametric modelling of potential evapotranspiration: a global survey" by A. Tegos, N. Malamos, A. Efstratiadis, Y. Tsoukalas, A. Karanasios and D. Koutsoyiannis (HYDROL23973)

Response to Dr. Konstantinos Georgakakos, Editor

We are pleased to submit the new version of our study entitled "*Parametric modelling of potential evapotranspiration: a global survey*", to be considered for publication in the Journal of Hydrology. We are grateful for your previous evaluation, as well as the comments of the two reviewers. Following your recommendations, as well as the second reviewer's comments, we implemented a major overhaul of the paper, attempting to address the remarks, as outlined below.

Response to Dr. Jianzhong Wang, Associate Editor

> *The author(s) designed and calibrated a global parametric model of potential evapotranspiration (PET) with two parameters (temperature and extraterrestrial radiation) in this paper. The intention of this work is acceptable but the realization of this objective and the writing in current form need significant improvements before you resubmitting it to journals.*

We appreciate your kind evaluation and your effort in organizing the review process.

All review suggestions that we received have been implemented in the revised article, as explained herein. Please, refer to our detailed responses to the editor and the reviewers. We believe our revised manuscript is significantly improved especially with the new section 5.2 where an extended statistical analysis of the model outcomes is presented.

Response to Reviewer #1

> *I cannot see new contribution in this paper*

The recently presented concept of the parametric approach has been recognized and cited by several publications in well-known peer-reviewed journals, following the scientific efforts of several researchers during the last decades to produce simplified models for the quantification of PET.

Outright rejection of our findings without any scientific justification, does not allow us to present any further response to the anonymous reviewer.

Response to Reviewer #2

General comments

> *This paper investigated the relationship between climatic factors and PET with worldwide available data of climatic variables and estimated PET based P-M equation, and estimated the global distribution of two parameters of a two parameters empirical*

equation for estimating PET only considering temperature and the extraterrestrial radiation. In fact, although P-M formula is still subject to the data limitation, it is the excellent equation for estimating the PET, especially under the background of climate change, which is widely acknowledged. Aerodynamic factors were proved to play the important roles for explaining the PET changes and cannot be neglected (e.g., Rayner, 2007; Roderick et al., 2007, 2009a, b; McVicar et a., 2012; Wang et al., 2012; Li et al., 2013). Meanwhile, the meteorological observation abilities with enough spatial resolution is continuously in processes of developing. Thus, generally, physically-based methods would be more considered and have wide application. Moreover, the statistic investigations in this study are primary, some of conclusions from them are thus common sense. Meanwhile, the illustrating ways of most figures are need improved. Therefore, although the submitted paper of Tegos et al. is a well written study based a broad data base, I cannot recommend the manuscript for publication in the present form. Some other detailed comments are given below.

We are grateful for finding our paper well written based a broad data base. Your review helped us to improve our paper by taking into account the two significant issues suggested, i.e.: (1)The global applicability of the Penman- Monteith and the need for new approaches, and (2) The physical structure of these alternative frameworks based on the required meteorological data.

It is obvious for many decades from numerous scientific works that the Penman- Monteith is still the most appropriate and physical consistent framework for the PET estimation. Nevertheless, the requirement for four measured meteorological variables (temperature, radiation, humidity, wind velocity) led a lot of researchers to study parsimonious expressions. This scientific issue remains a challenging task, as the meteorological network in some areas of the world is still poor (Africa, Asia, South America). Our effort is to produce useful and applicable estimates for these data-scarce areas.

The modified version of the parametric model, as we explain thoroughly in our paper, appeared to have decreased performance in energy-limited and water-limited locations, mainly due to absence of the humidity and wind velocity as explanatory variables. To ensure this hypotheses and findings we added a new section (i.e. *5.2 Residuals analysis for stations with negative NSE*) where we performed an extended statistical analysis of the model's results at locations with poor performance. Also, at this new section we incorporated a new map and 3 new figures in order to perform the investigation of the model's performance, in those cases. We accordingly modified Section 3.3 of our manuscript based on the proposed references, which were also included in the References section.

Furthermore, we modified accordingly the Conclusions section, in order to address the reviewer's concerns.

Also, we made several changes throughout the text in order to address the concerns raised.

<u>Specific comments</u>

1. *Section 3.2, Some conclusions are not rigorous. For example, "Surprisingly, mean annual sunshine duration is slightly less correlated with mean annual PET than extraterrestrial radiation, although the former is expected to be better estimator of the actual solar energy received in the Earth's surface. This is a very important conclusion, which confirms the suitability of radiation-based approaches, using both temperature and extraterrestrial radiation as explanatory variables of PET". The analysis should be conducted under different sub-regions with similar climatic conditions to identify whether the conclusions.*

Response to comment 1:

We would like to mention that these conclusions were based on the outcome of the performed analysis presented in Section 3.2. The correlation between the model's parameters and the different factors affecting PET, across different global sub-regions, constitutes a research topic that has to be examined separately because of its importance.

In this study we emphasized on the first global application of the parametric model and focused on the main explanatory variables that affect PET. An extension of the paper in order to include analyses across different global sub-regions would alter the objective of our study. In this context, we altered accordingly the Conclusions section to incorporate this analysis in future research.

2. *Section 3.3, Why selected five stations in Australia to conducted the investigation rather than global typical stations?*

Response to comment 2:

Such an analysis can be performed using data from any location across the globe. We chose those ten stations in Australia (five stations exhibiting regular loop-type relationships and five stations exhibiting irregular relationships - Figures 3 and 4), because they belong to an area where different climatic conditions prevail. In this way, we aimed to present that in some cases where non-smooth evaporation loops are formed there is no correlation between PET and extraterrestrial radiation or temperature.

3. *Figure 2-Figure7 can be combined in one Figure*

Response to comment 3:

We agree and revised as proposed.

4. *Figure 11. More detailed classification for the stations should be given rather than only two types.*

Response to comment 4:

We agree and revised as proposed (now Figure 10). Furthermore, we detailed the classification of the figure that presents the global distribution of BIAS (now Figure 11).

5. *Figure 14 and 15, Please merge the different parts into one world map*

Response to comment 5:

We agree and revised as proposed (now Figures 13 and 14)

Final response:

In order to address the comments made by the Editor, the Associate Editor and the second reviewer, we added a new section:

- **5.2 Residuals analysis for stations with negative NSE**

Also, a number of changes were made in Sections:

- **3.3 How well do extraterrestrial radiation and temperature explain the seasonal patterns of PET?**
- **5.2 Evaluation of model performance across geographical zones**
- **6.2 Spatial distribution of parameters *a´* and *c´***
- **7 Conclusions**
- **References**

Editor's decision submission 2

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Thank you for your interest in our journal.

With kind regards,

Marco Borga
Editor
Journal of Hydrology

COMMENTS FROM EDITORS AND REVIEWERS:

This revised paper made noticeable improving in its content and writing. Lack of comparison with other existing simple parametric models of PET is probably a weak link of this paper. My opinion to this paper: Major revision.

Reviewer #1: The paper fails to stress a very important point, i.e. that it is based on average monthly variables. The readers need to be warned early (starting in the summary/abstract) and explain well the CLIMWAT 2t database . The averages over very long time intervals there may be reversals of the sign of the correlations between variables, the authors should bear in mind that the monthly timescale usually provides higher accuracy than smaller window sizes, due to a reduction of the variability derived from using averaged values. So, this should be taken into account in the conclusions.

Another very important point is the used PET concept. PET provides a good representation of the maximum possible water loss from a reference surface ("52 Since PET depends on soil properties, 53 a better defined term is the so-called reference crop evapotranspiration, introduced by"…) and not depend of soil. The PET concept, introduced by Thornthwaite, aimed at defining the maximum evaporation demand for a given climate. PET had widespread usage from the 1940s through the 1970s (Thornthwaite, 1948, Penman, 1963, Jensen, 1974).   Although the PET concept is very applied, this concept is considered as a source of confusion. In order to overcome this problem, PET has been gradually replaced in the past decade by other more narrowly defined terms, such as reference evapotranspiration (ETo) (Jensen et al., 1990), for which the characteristics of the vegetated reference surface have been standardized. The ETo FAO 56 method estimate ETo. (reference evapotranspiration and not PET Potential), "which refers to the evapotranspiration from a 55 standardized vegetated surface (i.e., actively growing and completely shading grass of 0.12 m height, surface resistance 70 s m-1, and albedo 56 = 0.23)". In the text you say "The globally accepted 57 method for consistent estimation of ETo (NOT PET) is the Penman-Monteith (herein referred to 58 as PM) equation, as formalized by FAO, which is physically-based and is therefore 59 used as standard for comparisons with other, more simple approaches (Allen et al., 60 1989)"

Other comments:
Equation (2) is a dimensionally incorrect equation. You must explain more the formulaion, and why you do not use others more applied TET (temperature based models)..

The statistic in this study are confusing, some of figures and conclusions from them are thus common sense.

Figures 3 and 4 take up space and very inefficiently pack the information

In 583 line you wrote " regarding the local validation set (Table 6), the model predicts monthly PET 583 with significant accuracy, thus exhibiting an average efficiency up to 0.855, and an 584 average bias of only -0.07. Except for three stations …, the NSE exceeds 0.70, while in 17 out of 37 stations it exceeds 0.90" I think that the performance of the temperature model vary significantly according to the climate under consideration (Almorox et al, 2016 Theor Appl Climatol.). You must study the climate type for the conclusions.

Reviewer #2: AA should make clear which are the advances reported in this paper relative to previous studies on parametric estimation of PET already published:
Tegos, A., Efstratiadis, A., Malamos, N., Mamassis, N., Koutsoyiannis, D., 2015. Evaluation of a parametric approach for estimating potential evapotranspiration across different climates. Agriculture and Agricultural Science Procedia, 4, 2-9.
Tegos, A., Malamos, N., Koutsoyiannis, D., 2015. A parsimonious regional parametric evapotranspiration model based on a simplification of the Penman-Monteith formula. J. Hydrol. 524, 708-717.
In the former paper review, the Rev#1 did not find novelty in this MS. AA, instead of showing that novelty claimed against the reviewer. In this review I therefore regret that AA did not use the introduction to clearly show that innovation as well as the innovation relative to various parametric approaches already published. Moreover, in results section, the performance of the parametric model referred herein should be compared with those of previous as well as other modeling approaches.

Moreover, the MS lacks a proper definition of objectives.

The concept of PET does not fully correspond to grass reference ETo using the PM equation. It would be better to not only provide an old definition of PET but also of the

grass reference ETo and likely showing the supporting PM equation.
There are several mistakes in Introduction such as Lines 77-79: a study of 1986 cannot relate to the PM method which was developed only by 1991 and first published by 1994 (Allen et al. ICID Bull 43(2), 1994)
Unfortunately, AA followed the majority of studies relative to using Hargreaves eq. (L 81-87) as alternative to the PM equation. Instead, AA should have also referred to the procedures proposed in FAO56 for estimating missing variables and to various studies analyzing them, e.g., Todorovic et al. J Hydrol 481, 166-176, 2013 applied to the Mediterranean area, effectively covering a range of climates.
Recent papers have shown that the estimation of solar radiation using Tmax and Tmin performed quite well. This type of studies should not be omitted. Good results were also reported by other studies on the estimation of Tdew or the actual vapour pressure. These studies should not be omitted.
As referred by Pereira et al. 2015 (wrongly/contradictory quoted line 307), adopting those or other estimates of the short wave solar radiation and of the actual vapour pressure allows using the PM equation and therefore approaching the Physics based ETo equation which does not really occurs when using other equations, or heuristic and parametric methods. A proper realtion from the used model to the PM equation is not discussed in the current MS.
When computing ETo using reanalysis products allows to directly use the gridded derived weather variables in the PM equation after bias correction (also omitted in this MS). Do we really need a parametric equation when future points out to better explore reanalysis products?
Notice that eq. 1 refers to any type of vegetation with or without stress. The PM equation, generally used for PET, is obtained after parametrization of Eq. 1 for grass as described in FAO56. Thus Eq. 1 is not usable as such to support PET definition
Lines 178-181 assume a correspondence between terms of th PET formula to the PM equation but there is no demonstration that this may be true
Lines 192-196 have no sense because comparing the dynamics of temperature with Ra when it is known that if a comparison is intended is between 2 dynamic variables T and Rs. Please avoid such approaches
L 196-201. This reasoning is misleading because the way how T is used in parametric eqs is not favored by eq. 4.
Eq. 5 is just eq. 2 making b=0. Thus there is no innovation in that approach relative to eq. 2. This is a supplementary reason to any reader to consider that this MS is poorly or no innovative.
Previous uses of the CLIMWAT 2.0 database should be mentioned. Moreover, what for ETo from this data base was used? This should be explained in Section 3.1
Why to answer to "Which meteorological drivers explain mean annual PET over the globe?" by plotting PET data against the four meteorological variables required in the Penman-Monteith equation? we already know that, on the one hand weather variables interrelate and, on the other hand, that a variety of external factors interfere. To perform that analysis using climate clustering could give some results if clusters would be well selected. I'm sorry, but this type of approach does not add to actual knowledge.
Section 3.3. "How well do extraterrestrial radiation and temperature explain the seasonal patterns of PET?" is also a dubious question and the approach used of plotting a loop-type and non-loop-type shapes just demonstrate that relations Ra vs. T are effectively not linear because other climate factors characterizing the seasonality of the interfere in that relation. this approach does not add to the (supposed) objectives of the study.
Section 4: how was calibration performed? For all 4300 stations? How were selected

the ranges of variation of the parameters?
Why Fig. 5 deals with two linear regression models and the nonlinear parametric model if the latter is the focus of the study? Apparently there is no reason for that.
Why the assessment of model performance was performed across very large geographical zones and not by climatic zones? The approach used is not providing appropriate results.
From the considerations above, it is not evident the innovation of a parametric model comparatively to other approaches currently studied, nor is evident that the model performs well comparatively to other approaches. Therefore, the MS is not appropriate to be published in an academic ISI journal

Reviewer #3: The authors are trying to present an efficient method to estimate PET through modifying a current parametric model. I do not recommend publication of this paper in J Hydrology because of the novelty.

The authors' intention is to replace Tmean in the original model with (Tmax+Tmin)/2. However, (Tmax+Tmin)/2 has been widely used in most of the current PET models. If the original model does not use this variable, it means it does not fall in the scope of 'popular' models. Through your modification, it might be more popular; however, after modification, it looks similar as some of the current models. Therefore, the modification of the parametric model has nothing new.

It is not necessary to validate the correlations between PET and solar radiation or PET and temperature. Obviously, you want to say these two variables are mostly correlated with PET, and then your proposed method is reliable. This kind of work has been done by a lot of previous studies. Further, higher intervariable correlation does not imply that PET changes are mostly attributed to the changes in those variables, which has been quantified by studies for attribution analysis.

It seems that the advantage of this study is the employment of big data, which helps further discuss the spatial pattern of the parameters. Although the authors calibrated the model and analyzed the calculated PET in details, the poor innovation underscores the importance of these steps. And the procedure for the model parameter estimation can be finished in a few days by someone good at programming.

Introduction:
The authors talked a lot about the definition of ET or PET. Although the definition is very important for beginners, it is not so necessary to give detailed information in your study since this is not so necessary for readers who are interested in your study.

The authors did not present the scientific questions about what the problem is and why your work is very important. Please reorganize the introduction.

What is the difference between this study and your previous studies? This should also be clarified in the introduction.

Methods:
It seems that the innovation of this study is to replace the mean temperature in the original formula with (Tmax+Tmin)/2. Is this really very new? The calculation Tmean=(Tmax+Tmin)/2 is always used in different equations for PET estimation.

The correlations between PET and climatic factors cannot be used to determine the main controlling factors. According to a lot of current quantification based on analytic formula, wind speed contributed the greatest to PET changes. However, in your study, you found that PET is uncorrelated to wind speed. Therefore, you assumption based on correlations between PET and solar radiation, PET and temperature might be a problem.

Final Response to JoH by the representative author

Dears Editor Professor Borga, former Editor Professor Georgakakos, Associate Editor Mr. Wang,

We appreciate all of you for the organization of the both round reviews of our paper and for giving us the opportunity to re -submit it for another one time. I'm very proud for the double rejections from the three anonymous reviewers as I'm sure that this manuscript is great. Since, I'm just a civil engineer with a hobby to explore new applicable engineering ideas during my free time, a researcher without any funding, I have the opportunity to feel the freedom and incredible arrogant as well. Probably these feelings are foreign for the anonymous reviewers, since the anonymity is a safe resort for useful bins. From my point of view, below are the following key reviewers points:

Reviewer #1: I cannot see new contribution in this paper. (8 words for his/her first review)

Reviewer #2: In the former paper review, the Rev#1 did not find novelty in this MS. AA, instead of showing that novelty claimed against the reviewer (Probably he/she agree with the thoroughly review of the Reviewer #1 during first round).

Unfortunately, both reviews were for minor changes, hope that you understand that. We tried through extended new analysis to support our findings and the second review/decision was just a "dogmatic deny" to publish our findings.
In this light we cannot continue to a perpetual review process.
Our and mainly mine non-funding research work will find the necessary recognizition-path, somewhere elsewhere.

Thanks in advance,
Aristoteles Tegos