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4. Interactive Review

**Regional ombrian curves: a methodology for diverse hydrometeorological regimes**

Theano Iliopoulou\* , Nikolaos Malamos and Demetris Koutsoyiannis

Original Research, *Front. Clim. - Climate Services*

Received on: 22 Sep 2021, Edited by: Giuseppe Mascaro

Manuscript ID: 781539

Research Topic: [A Quest to Fully Understand Precipitation: Novel Methods to Characterize, Model, and Detect Precipitation Processes](#)

Keywords: ombrian curves, Rainfall intensity-duration-frequency curves, Rainfall extremes, regionalization, regional rainfall ...

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History

Editor  
ActiveReviewer 1  
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Date

Updates

21 Dec 2021

Article rejected by Specialty Chief Editor Chris C Funk.

13 Dec 2021

Co-author Demetris Koutsoyiannis posted new comments in the Editor tab.

08 Dec 2021

Interactive review forum activated.

22 Sep 2021

Corresponding Author Theano Iliopoulou submitted manuscript.

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This report was last updated on: 20 Jan 2022-06:25 GMT

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22 Sep 2021-20:21 GMT

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22 Sep 2021-21:51 GMT

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**Language quality** CHECKED

22 Sep 2021-20:22 GMT

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**Ethics guidelines** CHECKED

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**Frontiers manuscript matches** CHECKED

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**AIRA** 22 Sep 2021-20:22 GMT CHECKED

The language quality of this manuscript is suitable for peer review.

**Animal studies statement verification** CHECKED

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This manuscript does not appear to present the results of animal studies.

**Human studies statement verification** CHECKED

**AIRA** 22 Sep 2021-20:21 GMT

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**Identifiable images and data statement verification** CHECKED

**AIRA** 22 Sep 2021-20:21 GMT



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### Face and body detection

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### Controversial themes

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### Controversial keywords (global and journal specific)

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CHECKED

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**Subject:** Giuseppe Mascaro via Frontiers: Major concerns identified in your manuscript - 781539  
**From:** "Giuseppe Mascaro (Via FrontiersIn)" <noreply@frontiersin.org>  
**Date:** 09/12/2021, 01:10  
**To:** dk@itia.ntua.gr

Dear Dr Koutsoyiannis,

I have received comments on your manuscript from two qualified reviewers. Although the paper is overall well written and organized, both reviewers have significant concerns on vocabulary, lack of methodological details, and interpretation of results.

In particular, both reviewers noted that the authors purposely adopted their own terminology to refer to concepts that are widely known in the literature with other words (for example they used ombrian curves to refer to intensity-duration-frequency curves). I agree with the reviewers that, while legitimate, this choice may end up generating confusion to the readers.

Both reviewers also noted that part of the methodology is not properly described and contains reference to papers that are not peer reviewed or available only in Greek language.

Reviewer 3 has also serious concerns on the accuracy and interpretation of the results.

Based on the feedbacks and my personal assessment, I regret to inform you that I recommend rejection at this stage.

Major concerns have been raised over the content of your manuscript, and I have now recommended it for rejection.

The comments provided in the editor's tab, as well as reviewers' assessments, if any, will be sent to the Specialty Chief Editor for their decision. The interactive review forum has been opened to grant you access to these comments.

You can access the forum using the following link:

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Currently no action is required from you. You may nonetheless post a rebuttal for consideration, in the editor's tab, within 7 days of this message together with the resubmission of an updated version of your manuscript addressing the concerns raised.

Please note this does not guarantee that your manuscript will be further considered for peer review, and that no extensions can be granted at this stage.

Do not hesitate to contact the editorial office if you have any questions.

With best regards,

Giuseppe Mascaro  
Guest Associate Editor,  
www.frontiersin.org

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Manuscript title: Regional ombrian curves: a methodology for diverse hydrometeorological

regimes

Manuscript ID: 781539

Authors: Theano Iliopoulou, Nikolaos Malamos, Demetris Koutsoyiannis

Article type: Original Research

Journal: Frontiers in Climate, section Climate Services

Research Topic: A Quest to Fully Understand Precipitation: Novel Methods to Characterize, Model, and Detect Precipitation Processes

Submitted on: 22 Sep 2021

Submitted By: Theano Iliopoulou

Interactive review started on: 08 Dec 2021



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History

Editor  
ActiveReviewer 1  
RejectedReviewer 3  
Rejected**Reviewer 1**

Independent review report submitted: 25 Nov 2021

Interactive review activated: 08 Dec 2021

Recommendation for the Editor: The manuscript should be rejected

## EVALUATION

Q 1 Please list your revision requests for the authors and provide your detailed comments, including highlighting limitations and strengths of the study and evaluating the validity of the methods, results, and data interpretation. If you have additional comments based on Q2 and Q3 you can add them as well.

Reviewer 1 | 25 Nov 2021 | 19:32

#1

The manuscript presents the estimation of Intensity-Duration-Frequency (IDF) curves at regional scale in the Thessaly region (Greece).

The topic is certainly of interest for hydrologists and it is crucial for engineering design of flood protection structures. For such reasons there is a huge literature on hydrological extremes which is mainly based on the concept of homogenous regions, aiming at limiting estimation uncertainty by pooling together ensembles of stations after proper standardization.

Despite the wide and centennial literature on Extreme Value Theory (EVT), and the numerous papers in the leading hydrological journal and books dealing specifically on hydrological extremes, the submitted manuscript does not give the proper credit to previous published and consolidated literature.

The authors rename the term "Intensity-Duration-Frequency curves" with the term "ombrian curves". While referring to "Intensity-Duration-Frequency curve" anybody with a minimum background in hydrology immediately understands the subject, the same does not hold for "ombrian curves". The authors justify the new name since it is the translation of rainfall in the Greek language, but this is dangerous and misleading for several reasons: most of readers of international journals do not speak the Greek language, and even if this is the case a translation of "rainfall curves" does not properly define the curves that indeed relates i) Intensity, ii) Duration and iii) Frequency of rainfall events, so this renaming is not technically sound. Moreover if Spanish, Italian, France, English, German researchers start to publish papers renaming widely adopted terms using new names derived by their national language it become very hard to understand each other.

Similar comments hold for the proposed "K-moments" methods, that is a renaming of the widely known and widely applied "Probability-Weighted-Moments" (PWM) and their combinations known as "L-moments".

The novelty of a work cannot be measured with the number of renamed words and procedures.

Eq. (1) is supported on by two references (Koutsoyiannis, 2021; Koutsoyiannis and Iliopoulou, 2021), the first is a book published in Greece, the second is a book in press, so I could not



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\(csi) which is not a Pareto distribution, and nor a Generalized Pareto distribution (in case the authors forgot to write also the word "Generalized").

Eq. (20) is a masked (and probably wrong) plotting position formula. With some algebra ones obtain the cumulative frequency  $F_i$  of the  $i$ -th ordered statistic:

$$F_i = 1 - \Delta_{(i:n)} = (i-0,035)/(n+0,526)$$

Again, the only reference for this formula is a Greek book (Koutsoyiannis, 2021), so I cannot check the derivation. There are several plotting position formulas published in the literature, derived to minimize certain statistics, but the differences are often negligible.

All plotting position formulas that I know obey the golden rule  $F_i = (i-a)/(n+1-2*a)$ , the only exception is Eq. (20) by Koutsoyiannis (2021).

In conclusion, I cannot recommend the publication of the submitted manuscript, since the authors are presenting well know methods and "new", and in addition there are technical flaws in some assumptions.

## Q 2 Check List

Reviewer 1 | 25 Nov 2021 | 19:32

#1

a. Is the quality of the figures and tables satisfactory?

- Yes

b. Does the reference list cover the relevant literature adequately and in an unbiased manner?

- Yes

c. Are the statistical methods valid and correctly applied? (e.g. sample size, choice of test)

- No

d. Are the methods sufficiently documented to allow replication studies?

- No

## QUALITY ASSESSMENT

Q 3 Rigor

Q 4 Quality of the writing

Q 5 Overall quality of the content

Q 6 Interest to a general audience

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History

Editor  
ActiveReviewer 1  
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## Reviewer 3

Independent review report submitted: 25 Oct 2021

Interactive review activated: 08 Dec 2021

Recommendation for the Editor: The manuscript should be rejected

## EVALUATION

Q 1 Please list your revision requests for the authors and provide your detailed comments, including highlighting limitations and strengths of the study and evaluating the validity of the methods, results, and data interpretation. If you have additional comments based on Q2 and Q3 you can add them as well.

Reviewer 3 | 25 Oct 2021 | 02:27

#1

This is a review of the manuscript "Regional ombrian curves: a methodology for diverse hydrometeorological regimes", co-authored by Iliopoulou et al. and submitted to Frontiers in Climate. The Authors propose a framework for regional modelling of Intensity-Duration-Frequency (IDF) estimates, by employing a smoothing technique that incorporates the surface elevation as an explanatory variable. The language quality of the manuscript is good, with only minor expressional exceptions (see e.g., lines 239-242), while the abstract reflects the study's contents. The Authors, however, resort to the use of strong language and unsubstantiated personal opinions in various occasions throughout the entire manuscript, which should be altered to more moderately toned statements (or better removed) before any future submission attempts. A characteristic example can be found in lines 30-31, where (in a quite forceful tone) the term "intensity-duration-frequency curves" is dismissed (despite its wide use since the 1930's).

Regarding the description of the proposed framework, the methodology section is not fully transparent and difficult to follow. First, all incorporated techniques have been developed in past studies by some of the Authors, and are simply rebranded as new knowledge. In this context, Eqs (1) - (7) have all been previously presented in Koutsoyiannis et al. (1998). A reformulation of existing expressions by redefining its parameters can be considered a mere modification. The bilinear surface smoothing (BSS) models used for regionalization are also developed in previous research efforts of some of the Authors; see also lines 190-192. To put it concisely, the collection and combination of existing tools without any substantial refinement or adjustment cannot be presented as a newly developed framework (see e.g., lines 17 or 72-73).

Then, there are significant inaccuracies and points that could create confusion to the readership:

1. For example, in lines 116-117 the Authors mention the use of a Pareto distribution to model rainfall intensity based on the entire rainfall timeseries. Note, however, that the Pareto distribution can be used only in the context of exceedances above a properly selected threshold, within a peaks-over-threshold (PoT) approach. That said, either the Authors infer the latter case and should rephrase properly for the sake of clarity, or they have a completely different reasoning that needs to be thoroughly explained.

2. Along the same lines, for Eq. (7) solely the case of  $\xi > 0$  is presented, with a description



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3. It would be really helpful if the Authors could explain why they resort to a less parsimonious approach (i.e., 6 parameters) than that presented in Koutsoyiannis et al. (1998) for IDF estimation, given that the latter also included fitting of model parameters independent of the temporal scale (i.e., duration). The increase in the number of parameters simply increases estimation uncertainty, while it has been proven that there are more robust and parsimonious approaches in the literature.

4. Additionally, an explanation of the statement in lines 145-147 needs to be provided. While the determination of  $\alpha$  and  $\eta$  would indeed require sub-hourly data (not sub-daily), why would  $b(T)$  be better inferred from daily records? Since data in sub-hourly temporal scales exist (which does not seem to be the case according to lines 170-171) and the Authors pool them during their approach based on annual rainfall maxima (see also lines 271-274), why would there be a need for aggregation to daily scales?

5. It should be clear that six (6) parameters for the IDF estimation, and  $4u$  or  $6u$  (where  $u$  is the number of incorporated locations) parameters for the regionalization, are not a good reflection of a parsimonious framework. In my understanding, the Authors attempt to satisfy the parsimony requirement by choosing which parameters they would spatially model. But, under what criteria is this procedure conducted? Commenting on the statement of lines 164-165, all parameters should be reliably (to an acceptable level) estimated by data. The vague choice of the scale parameter,  $\lambda$ , as the only one varying spatially (see lines 165-168 and 179-180) is most probably carried out simply because  $\lambda$ : (i) varies predictably in space (i.e., with the elevation), and (ii) is not very sensitive to the sample length. Note, however, that while the shape parameter  $\xi$  is the most influential one when interest is in modeling the frequency and intensity of rainfall extremes, the Authors select not to model its spatial variation explicitly, and adopt a constant value over the whole domain. How does this assumption affect the accuracy of the obtained results?

6. Could the Authors please provide some information on the “diagnostics checks” mentioned in line 178?

7. Why would the Authors choose elevation as the explanatory variable of the regionalization framework (see lines 231-235), when some of the model parameters (and especially the shape parameter  $\xi$ ) are not highly influenced by it? Is there any evidence that supports the opposite and, if yes, could the Authors please include it?

8. How would the suggested regionalization framework be generalized to other regions, or how could the user actually regionalize other (more crucial) model parameters? Shouldn't the performance of the proposed regionalization techniques be demonstrated when including other parameters as well?

9. The Authors' reasoning, as well as their assumptions, throughout the entire manuscript are generic and not generalizable, thus pointing to a case study for the area of interest in Thessaly, Greece (see e.g., lines 176-177). This does not make the suggested framework a widely applicable methodology.

10. Given that the scope of the manuscript is focused more on the regionalization of IDF estimates, why do the Authors present so limited (and general) information on the Bilinear surface smoothing (BSS) models (i.e., Eqs (8) - (9)), while expanding so much on IDF estimation (i.e., Eqs (1) - (7))? Note that both techniques have been developed by some of the Authors in the past.

11. The Authors mention that BSS is based on the minimization of the total squared error. But is the technique unbiased? Does it take into account possible heteroscedastic behaviors of the data? In other words, apart from the questionable parsimony (there are indications of questionable rigor as well; see also lines 229-230) of the BSS approach, what is the advantage compared to Kriging? Simply the number of available data points (see lines 64-66) is not sufficient to dismiss Kriging, given that the spatial pattern and density of the data locations are the crucial factors (see e.g., Warrick and Myers, 1987).

12. This comment is minor, but there are no “distributional” parameters (see line 270). The correct terminology is “distribution” parameters.

13. The Authors confusingly call the method K-moments and state that it “shares the merits of L-moments” (see lines 274-278), when it practically is a reformulated version of the L-moments approach; see also Hosking and Wallis (1997). This is a totally unacceptable practice in science and engineering, as rebranding methods that exist for more than 20 years may largely confuse the readership. By the way, can the Authors include the full reference of the study of Koutsoyiannis (2019) in the reference list?

When it comes to the results, there are certain points that require some clarity. For example, in lines 468-473 the Authors mention the exploration of the suitability of the surface elevation as an explanatory variable for the spatial analysis. Yet, the following reasoning (i.e., lines 474-483), as well as Table 1 (which contains the performance of BSS and BSSE), do not necessarily support the inclusion or exclusion of the aforementioned variable, given that the differences are really small (i.e., on the order of 1 mm for annual maxima) considering the addition of two extra parameters (thus, additional estimation uncertainty) and that regionalization takes into account solely the scale parameter  $\lambda$ . Some





511-512)?

2. The fit of Figure 6 seems to deviate towards larger return periods TK (i.e., for the validation set). Could the Authors please plot the theoretical line above the empirical points?

3. Figures 8-10 display significant deviations that are not fully reflected by the Authors' statements (see lines 540-555). Evaluating the results qualitatively, it is quite obvious that the variance of error is quite significant. Figures 8 and 9 are definitely not in good agreement with empirical evidence (see also lines 546-550), as the theoretical estimates deviate even for return periods on the order of 10 years, regardless of the characteristic temporal scale. Could the Authors please elaborate on this model behavior? Are these deviations acceptable in the context of their (or any other) work? Could these curves be used in any design that would incorporate extreme rainfall estimates?

4. Could the Authors please include their map, with the estimates of their proposed framework, for the storm of Figure 12? The maps of satellite data and rain gauge measurements could then serve as a true benchmark. Or at least, could the Authors please include a table of quantitative results (even only RMSE, or relative RMSE) so that the results can become more tangible? The current description in lines 603-621 seems rather counterintuitive and biased, given that the Authors simply chose a point of interest.

Based on all the foregoing, it is reasonable to say that the current version of the manuscript does not support any of the outlined conclusions. The Authors, in summary, should (a) alter, remove, and avoid the use of strong language, (b) reformulate the entire methodology for clarity, (c) strictly avoid rebranding past knowledge as new, or re-deriving existing and well-established techniques with the goal to present them as novel or advanced (the latter is a reference to the K-moments), (d) reconsider their statements on parsimony and rigorously, (e) reexamine and reassemble the results section, which is currently rather incomprehensible and contains inaccurate statements and results of low accuracy, and (f) evaluate the incorporation of more important distribution model parameters, such as the shape parameter  $\xi$ , as well as (g) attempt to found their study on assumptions that would support the potential generalization of the proposed techniques (i.e., not only for Thessaly).

In view of the above, I would recommend that the study should not be considered for publication in Frontiers in Climate, under its current format.

For a refined version of the comments (that includes the proper formatting for variables and line numbers), please see the accompanying attachment.

## References

- Hosking, J. R. M., & Wallis, J. R. (1997). Regional Frequency Analysis: An Approach Based on L-moments. Cambridge University Press, UK. <http://dx.doi.org/10.1017/cbo9780511529443>
- Koutsoyiannis, D., Kozonis, D., & Manetas, A. (1998). A mathematical framework for studying rainfall intensity-duration-frequency relationships. Journal of Hydrology, 206(1-2). [https://doi.org/10.1016/S0022-1694\(98\)00097-3](https://doi.org/10.1016/S0022-1694(98)00097-3)
- Warrick, A., & Myers, D. E. (1987). Optimization of Sampling Locations for Variogram Calculations, Water Resources Research, 23, 496-500. <https://doi.org/10.1029/WR023i003p00496>.

[Review supporting file - 203086](#)

## Q 2 Check List

Reviewer 3 | 25 Oct 2021 | 02:27

#1

- a. Is the quality of the figures and tables satisfactory?  
- No
- b. Does the reference list cover the relevant literature adequately and in an unbiased manner?  
- No
- c. Are the statistical methods valid and correctly applied? (e.g. sample size, choice of test)  
- No
- d. Are the methods sufficiently documented to allow replication studies?  
- No

## QUALITY ASSESSMENT

Q 3 Rigor

Q 4 Quality of the writing



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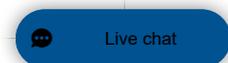
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Received on: 22 Sep 2021, Edited by: Giuseppe Mascaro

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Research Topic: [A Quest to Fully Understand Precipitation: Novel Methods to Characterize, Model, and Detect Precipitation Processes](#)

Keywords: ombrian curves, Rainfall intensity-duration-frequency curves, Rainfall extremes, regionalization, regional rainfall ...

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History

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Rejected

Handling Editor: Giuseppe Mascaro

Received date: 22 Sep 2021

Editorial assignment start date: 22 Sep 2021

Independent review start date: 01 Oct 2021

Interactive review activated date: 08 Dec 2021

## Feedback by reviewers and AE

**Guest Associate Editor: Giuseppe Mascaro | 08 Dec 2021 | 23:08** #1

Dear Dr. Iliopoulou,

I have received comments on your manuscript from two qualified reviewers. Although the paper is overall well written and organized, both reviewers have significant concerns on vocabulary, lack of methodological details, and interpretation of results.

In particular, both reviewers noted that the authors purposely adopted their own terminology to refer to concepts that are widely known in the literature with other words (for example they used ombrian curves to refer to intensity-duration-frequency curves). I agree with the reviewers that, while legitimate, this choice may end up generating confusion to the readers.

Both reviewers also noted that part of the methodology is not properly described and contains reference to papers that are not peer reviewed or available only in Greek language.

Reviewer 3 has also serious concerns on the accuracy and interpretation of the results.

Based on the feedbacks and my personal assessment, I regret to inform you that I recommend rejection at this stage.

**Co-author: Demetris Koutsoyiannis | 13 Dec 2021 | 08:52** #2

Dear Editor,

I have tried to upload my response as a pdf file, but I could not find this option in your system.

Please see my [reply](https://www.researchgate.net/publication/356981706) in <https://www.researchgate.net/publication/356981706>

With kind regards,

Demetris Koutsoyiannis

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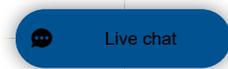
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## An open letter to the Editor of *Frontiers*

by Demetris Koutsoyiannis

2021-12-13

After invitation (to one of my two younger coauthors) by *Frontiers*, which, according to its [own statement](#) “is a leading Open Access Publisher and Open Science Platform”, we submitted there our article “Regional ombrian [1] curves: a methodology [1] for diverse hydrometeorological [1] regimes”. The invitation was for the article collection “A Quest to Fully Understand Precipitation: Novel Methods [1] to Characterize [1], Model, and Detect Precipitation Processes” (*Frontiers in Climate* [1] – section *Climate* [1] Services).

We received a rejection based on “comments ... from two qualified reviewers”, as the Editor calls them. As summarized by the Editor, the rejection is justified as follows:

*... the authors purposely adopted their own terminology to refer to concepts that are widely known in the literature with other words (for example they used ombrian [1] curves to refer to intensity-duration-frequency curves). I agree with the reviewers that, while legitimate, this choice may end up generating confusion to the readers.*

*Both reviewers also noted that part of the methodology [1] is not properly described and contains reference to papers [1] that are not peer reviewed or available only in Greek language.*

This letter is my personal one (I am the third—and last—author of the paper [1]). I do not wish to involve my coauthors in this because I refer to personal experiences and opinions.

I personally have rich editorial experience and I have written a lot of [editorial articles about the peer review process](#), mostly jointly with other editors, which I would recommend for reading by the young editors. In addition, I have a [very rich record of rejections](#), mostly for the papers [1] that later became my most cited. Therefore, I have developed mithridatism [1] and I personally feel rather safe, as I approach my end of my academic [1] career. Yet I feel I have some responsibility for my younger colleagues and the improvement of the peer-review system [1]. I believe this case is a prototyping [1] example of system [1] failure and therefore, in addition to uploading this letter to the journal’s system [1], [I am making it open](#). The Editor, the reviewers and anyone interested are invited to add their critical [1] comments openly in the [ResearchGate platform, where I have published the letter](#). An additional reason for making it public is that this rejection is the most amusing I have ever received. I hope some readers may have fun with it.

Most amusing I found the fact that the “two qualified reviewers” who make review for an open access journal for an “Open Access Publisher and Open Science Platform” seem not familiar with what open access is. They also have limited knowledge about what peer review is and therefore they misled the Editor. They treat my book *Stochastics [1] of Hydroclimatic [1] Extremes*, to which the paper [1] heavily refers and which has a Greek publisher, as a book written in Greek and available only to Greeks. However, the book is written in English and is open access. If they googled just two words of its title (let alone if they copy-pasted its title), they would locate and download the book. As seen in Figure 1, Google lists it first among all entries it locates, so they would have no difficulties.

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[1] I admit that we used several Greek words in the paper, which apparently annoyed the reviewers and the Editor. Here, I have put this footnote as a notification for any Greek word that I use and I beg their tolerance.

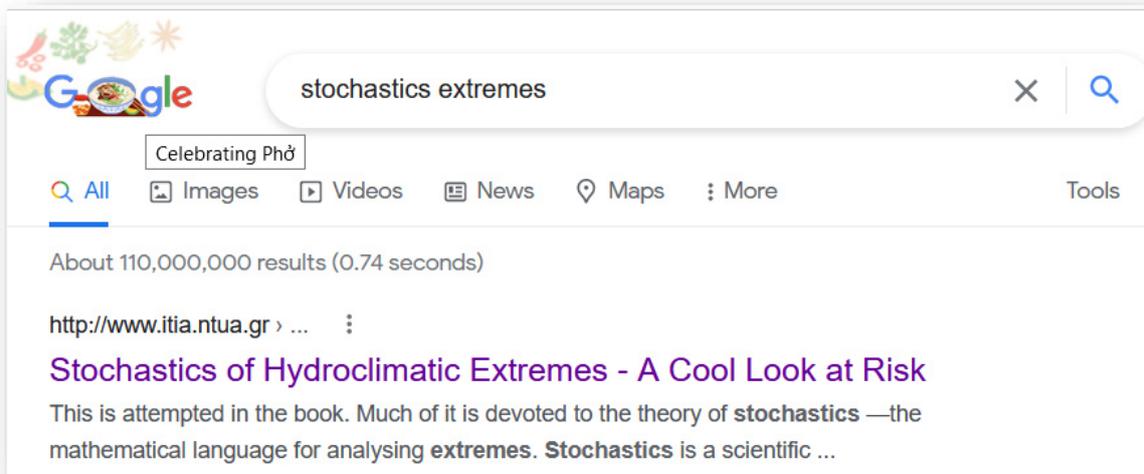
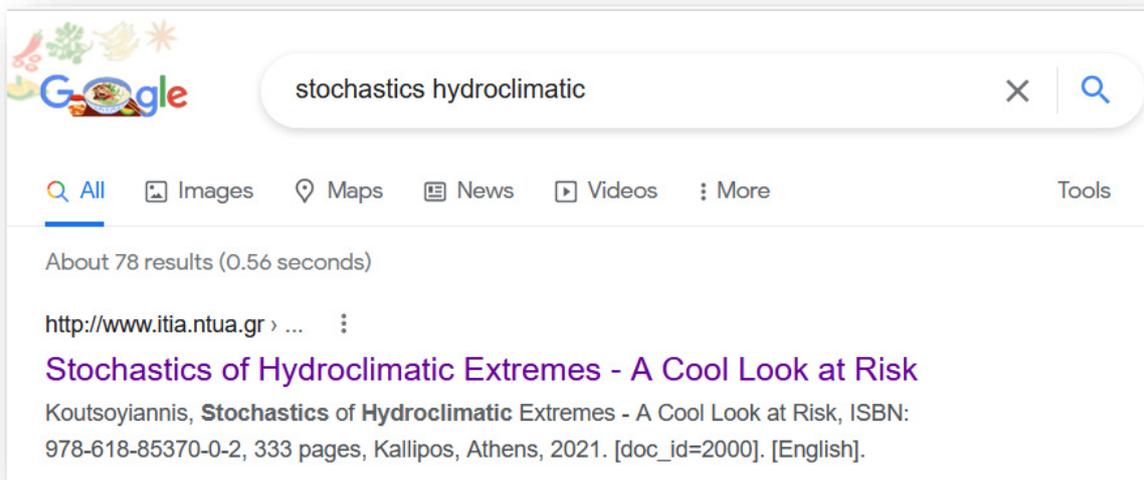


Figure 1: My open access book listed first by Google searches using only two words of its title (three combinations).

Well, if we identify peer review with what these “two qualified reviewers” have made, then, indeed my book is not peer reviewed. However, the Editor may wish to see the critiques [1] published along with the book—two in the beginning (Foreword and Prolegomena [1]) and two in the back cover. (In the acknowledgments I also name several other colleagues who provided comments and suggested corrections.)

To keep the letter short and focused on editorial issues, I am not going to discuss the review comments in detail. However, I will discuss two more issues mentioned or implied in the Editor’s summary, related to renaming customary concepts or repeating them with other names.

Apparently, the reviewers did not read our first statement in the Introduction, where we clarify that the common term ‘intensity-duration-frequency’ curves is a misnomer. One reviewer insists that:

*the curves ... indeed relates i) Intensity, ii) Duration and iii) Frequency of rainfall events, so this renaming is not technically [1] sound.*

As we clarify in the paper [1], duration is different from time scale and what is described by these curves is not duration but time scale. Also, frequency is different from return period [1] and what is described by these curves is not frequency (dimension  $[T^{-1}]$ ) but return period [1] (dimension  $[T]$ ). It is a pity that such an elementary scientific knowledge is still unknown to some hydrologists [1]. We are glad that the Editor finds our renaming “legitimate”, but we disagree that “this choice may end up generating confusion to the readers”. Rather we hope to contribute to dispelling the existing confusion. I regret to say that, being a fan of Aristotelian [1] [saphenia](#) [1] [2], I refuse to follow the reviewers’ and Editor’s suggestion. And the Editor is right: we are doing this “purposely”.

The reviewers also opine that in our paper [1] we rename other terms, such as L-moments to K-moments. I am inviting the Editor to see the 60 pages of Chapter 6 in my aforementioned book to check whether the two concepts are identical—in particular in its relevance to our subject of ombrian [1] curves. Also, the reviewers find repetition with my 1998 paper [1], which one reviewer cites in her/his review. Again, I am inviting the Editor to read the 30 pages of Chapter 8 in my aforementioned book to check whether the new framework, described in detail in the book and followed in the paper [1], is a remake of the old one.

I had notified my young coauthor who received the invitation from *Frontiers* about the following [policy](#) [1] of *Frontiers*:

*When a manuscript is accepted for publication, the names of the reviewers who endorsed its publication appear on the published article, without exceptions. If a reviewer recommends rejection or withdraws during any stage of this process, his/her name will not be disclosed.*

Specifically, I expected that, as reviewers are becoming more and more fearful in being transparent, using their names and, hence, assuming responsibility about what they say, rejection is their most likely verdict. It appears that I was right. Indeed, both reviewers like to wear the mask of anonymity [1]—and, indeed, masks have become so fashionable nowadays. But I believe there is no hope for improvement if the peer review system does not move towards eponymity [1] [3]. As a coauthor and [I have stated](#) in a related case,

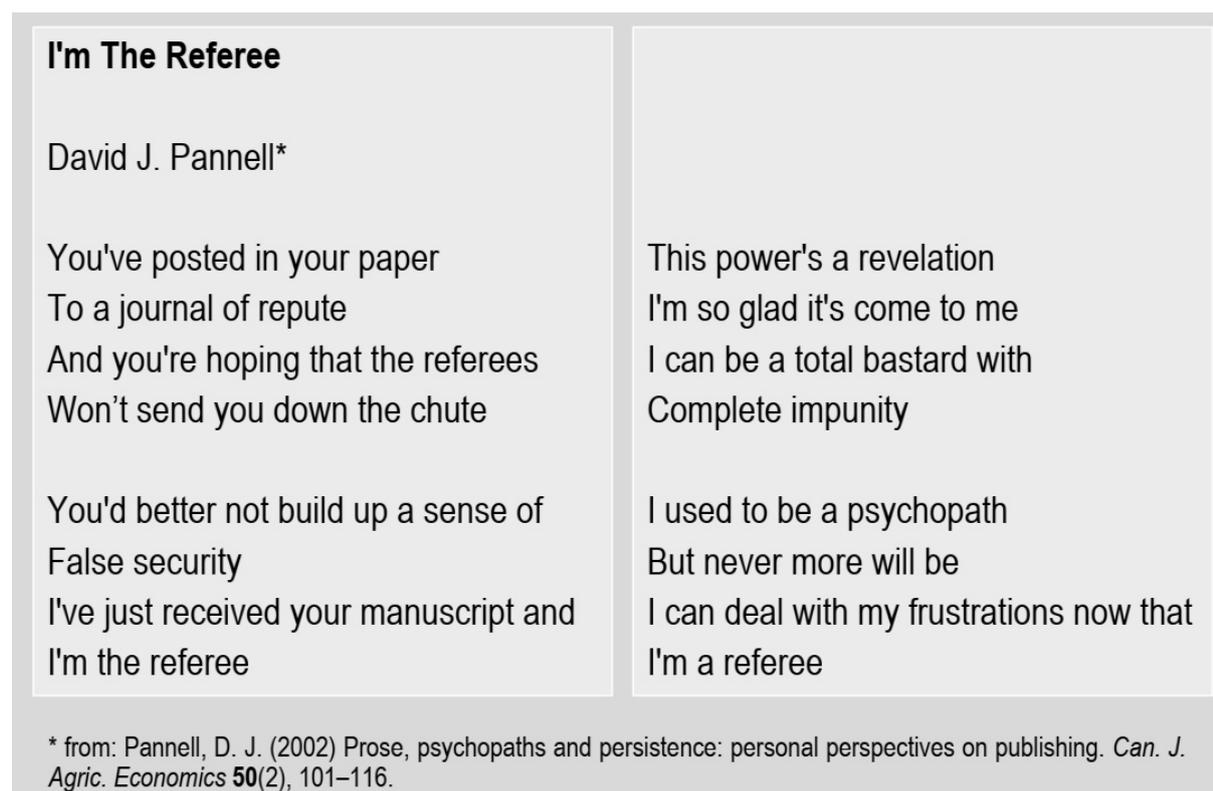
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[2] Leshner, J.H., 2010. Saphêneia [1] in Aristotle: “Clarity”, “Precision”, and “Knowledge”. *Apeiron* [1], 43, 143–156.

[3] I am doing only eponymous [1] reviews and in each of them I include the following statement:

... in an era where the quest for transparency has become extremely important, it is time for a radical change in scientific ethics [1]. Thus, when we are tempted to submit an anonymous [1] review, a good question to ask ourselves is this: If I cannot be an eponymous [1] reviewer, is it accurate to be called a reviewer? (And if yes, who is actually that reviewer? Myself or my anonymous [1], perhaps frightened, clone [1]?)

In closing, I dedicate the poem [1] shown in Figure 2 to the *Unknown Anonymous [1] Reviewer* (I use the latter term as a general category [1], like in the case of the *Unknown Soldier*). The poem [1] is not mine; it's written by [David J. Pannell](#). But I very much like it and I find it quite relevant.



**Figure 2: *I'm The Referee*; poem by David J. Pannell [4] (image copied from Kundzewicz and Koutsoyiannis [5]).**

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**Reviewer's assertion:** It is my opinion that a shift from anonymous [1] to eponymous [1] (signed) reviewing would help the scientific community to be more cooperative, democratic [1], equitable, ethical [1], productive and responsible. Therefore, it is my choice, consistent with my aesthetic [1] attitude, to sign my reviews. Furthermore, I believe that the current trend in the review system to seek credit for anonymous [1] transactions (by asking recognition for anonymous [1] reviews through Publons) is problematic [1] on ethical [1] and aesthetic [1] grounds.

[4] Pannell, D.J., 2002. Prose, psychopaths [1] and persistence: Personal perspectives on publishing. *Canadian Journal of Agricultural Economics* [1], 50(2), 101-116.

[5] Kundzewicz, Z.W., and Koutsoyiannis, D., 2006. The peer review system revisited. *Hydrology [1] Journal Editors Meeting, Vienna (Advances in Water Resources, Hydrological [1] Processes, Hydrological [1] Sciences Journal, Hydrology [1] and Earth System [1] Sciences, Journal of Hydrology [1], Journal of River Basin Management, Nordic Hydrology [1], Water Resources Research)*, [doi: 10.13140/RG.2.2.32180.65920](https://doi.org/10.13140/RG.2.2.32180.65920).

### Update 2021-12-15

For additional saphenia [1], I have added clarification and a relevant reference [2] about what saphenia [1] is. In addition, I am including here my reply to a comment by a reader who wrote:

*I wonder was this really the reason they rejected your manuscript.*

My reply (copied from the ResearchGate comments) is this:

*I do not think the real reasons for rejection were those stated. Interestingly, both reviewers chose the following option among those the journal offers as Reasons of Rejection:*

*“There are serious concerns about ethical [1] issues in the manuscript that cannot be rectified through author revisions.”*

*I guess this needs an expert in psychology [1] to interpret—unfortunately, I am not one.*

### Update 2021-12-16

Rereading the text, I discovered that I had missed to mark a lot of Greek words as such, which I have now corrected.

### Update 2021-12-22

The Specialty Chief Editor sent us yesterday an email, mentioning this open letter and confirming the rejection. He says he is “in agreement with the editor and reviewers in this matter.” To confirm this agreement and make the case more fun, he changed the reviewers’ *Reasons of Rejection* shown above (see Update 2021-12-15) to this one:

*“Objective errors in the methods [1], applications, or interpretations were identified in this manuscript that prevent further consideration.”*

### Update 2022-03-01

After a comment by Marianna Loli, I added “method” [1] and “methodology” [1] to the Greek words.

### Update 2022-03-29

1. After a comment by Nikos Theodoratos, I added “paper” [1] to the Greek words.
2. We have now submitted the paper [1] to [Hydrology](#) [1] with slightly different title, where we also

*“acknowledge comments by anonymous [1] (Greek for nameless, unspeakable, inglorious or, in more modern terms, masked) reviewers on a previous version of the manuscript submitted elsewhere (cf. [51]) that motivated us to strengthen the paper against their criticism [1] and highlight its contribution.”*

The reference [51] is the present Open Letter.

We have also taken the option offered by [Hydrology](#) [1] to publish a preprint in their platform. The preprint can be found here: <https://www.preprints.org/manuscript/202203.0383/v1>.

The platform allows comments by anyone interested. Thus, the rejecting anonymous [1] reviewers of *Frontiers* may consider becoming eponymous [1] and posting their comments there. Certainly, we will welcome their eponymous [1] comments, as well as those of the Editors or any other colleague, and we will be glad to respond.

**Update 2022-04-23**

The paper [1] has been accepted and published, with full acknowledgment of the “qualified reviewers” discussed here, as quoted in the previous update. The paper [1] can be found here: <https://www.mdpi.com/2306-5338/9/5/67>.

**Subject:** Chris Funk via Frontiers: Decision on your manuscript  
**From:** "Chris Funk (Via FrontiersIn)" <noreply@frontiersin.org>  
**Date:** 21/12/2021, 22:25  
**To:** dk@itia.ntua.gr

Dear Dr Koutsoyiannis,

Dear Drs. Iliopoulou, Malamos and Koutsoyiannis,

After having reviewed the reviews from the reviewers, the comments of the lead editor, and the open letter to the editors of Frontiers I am afraid that I must support the independent assessments of the reviewers and editor for rejection of this manuscript. The reviewers, editor and myself all believe that this manuscript did not effectively place this research in the context of the larger literature and past research by the hydrologic extremes community. In writing a research paper, it is incumbent on the authors, not the reviewers, to explain the relationship between the work presented and past research by prior authors. This paper does not seem to have established clearly the need-for and novelty-of ombrian curve-based analyses and K-moments versus L-moments. While the quality of the writing is overall good, the paper does not credit prior research adequately, and explain the relationship between what is presented here and prior innovations by the hydrologic science community. This omission makes it extremely difficult for readers to assess these efforts, and limits the interest to a general audience. Reviewer 3 also had several serious methodological concerns.

On the basis of the criteria, I am recommending this paper for rejection. I am sorry for this unfortunate conclusion, but am in agreement with the editor and reviewers in this matter.

Best Regards,

Chris Funk

Unfortunately, I have to inform you that your manuscript "Regional ombrian curves: a methodology for diverse hydrometeorological regimes" cannot be accepted for publication in Frontiers in Climate, section Climate Services.

The reason for this decision is:

Objective errors in the methods, applications, or interpretations were identified in this manuscript that prevent further consideration.

Dear Drs. Iliopoulou, Malamos and Koutsoyiannis,

After having reviewed the reviews from the reviewers, the comments of the lead editor, and the open letter to the editors of Frontiers I am afraid that I must support the independent assessments of the reviewers and editor for rejection of this manuscript. The reviewers, editor and myself all believe that this manuscript did not effectively place this research in the context of the larger literature and past research by the hydrologic extremes community. In writing a research paper, it is incumbent on the authors, not the reviewers, to explain the relationship between the work presented and past research by prior authors. This paper does not seem to have established clearly the need-for and novelty-of ombrian curve-based analyses and K-moments versus L-moments. While the quality of the writing is overall good, the paper does not credit prior

research adequately, and explain the relationship between what is presented here and prior innovations by the hydrologic science community. This omission makes it extremely difficult for readers to assess these efforts, and limits the interest to a general audience. Reviewer 3 also had several serious methodological concerns.

On the basis of the criteria, I am recommending this paper for rejection.

Best Regards,

Chris Funk

You can access the review forum with the manuscript and comments using the following link:

<http://www.frontiersin.org/Review/EnterReviewForum.aspx?activationno=1bf524bc-33cb-46ce-9211-f614bc3da53c>

Please note that should you choose to resubmit your manuscript to a Frontiers Journal/Specialty, it must be accompanied by a statement of resubmission inserted in the relevant textbox of the submission platform, and addressing the reasons for previous rejection or withdrawal, as well as highlighting any subsequent changes.

With best regards,

Chris Funk  
Specialty Chief Editor,  
[www.frontiersin.org](http://www.frontiersin.org)

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We want to hear about your experience with Frontiers.

We are constantly striving to improve our Collaborative Review process and would like to get your feedback on how we did. Please complete our short 3-minute survey and we will donate \$1 to Enfants du Monde, a Swiss non-profit organization:

[https://frontiers.qualtrics.com/jfe/form/SV\\_8q8kYmXRvxBH5at?survey=author&t=rej&aid=781539&uid=293774](https://frontiers.qualtrics.com/jfe/form/SV_8q8kYmXRvxBH5at?survey=author&t=rej&aid=781539&uid=293774)