

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also noted that part of the methodology [1] is not properly described and contains reference to papers [1] that are not peer reviewed or available only in Greek language.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

[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.

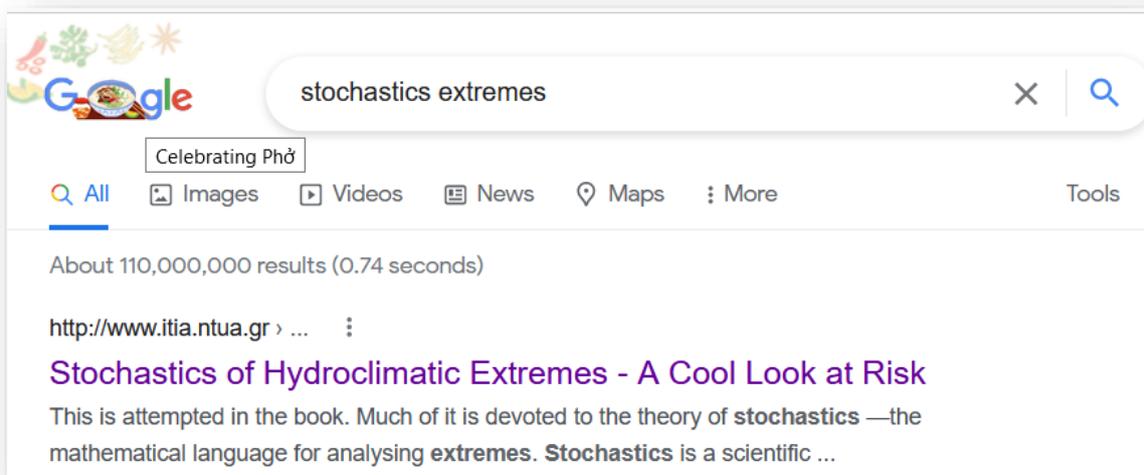
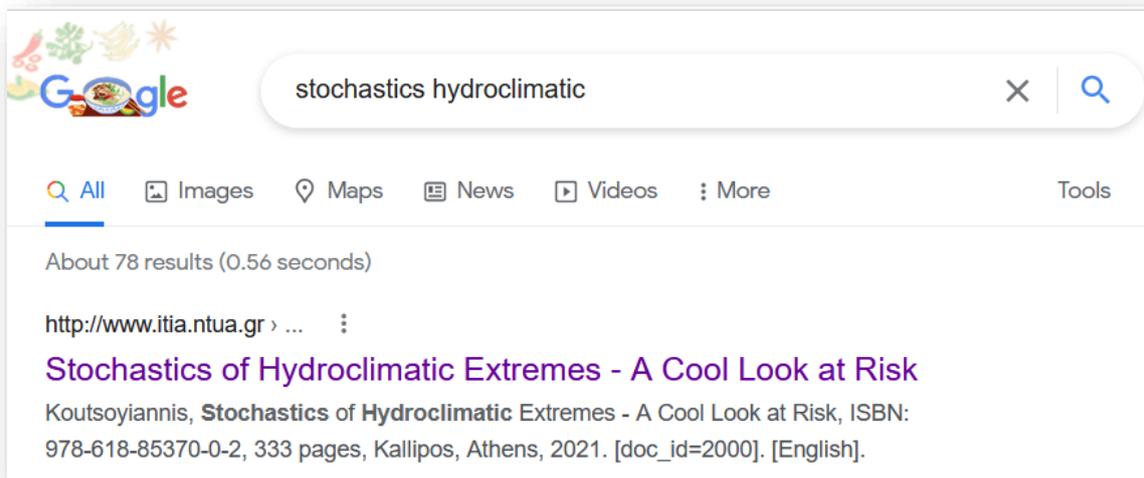
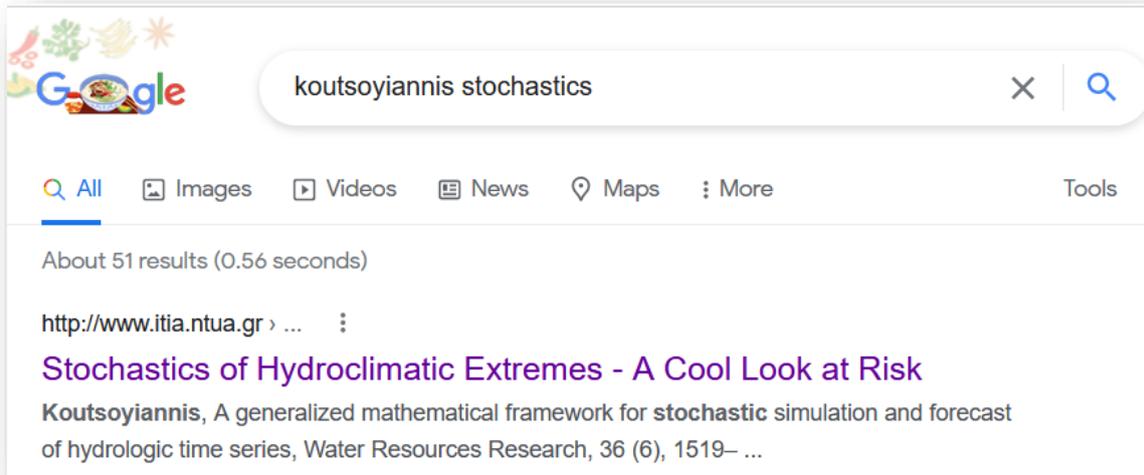


Figure 1: My open access book listed first by Google searches using only two words of its title (three combinations).

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,

[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.

I'm The Referee	
David J. Pannell*	
You've posted in your paper To a journal of repute And you're hoping that the referees Won't send you down the chute	This power's a revelation I'm so glad it's come to me I can be a total bastard with Complete impunity
You'd better not build up a sense of False security I've just received your manuscript and I'm the referee	I used to be a psychopath But never more will be I can deal with my frustrations now that I'm a referee
* from: Pannell, D. J. (2002) Prose, psychopaths and persistence: personal perspectives on publishing. <i>Can. J. Agric. Economics</i> 50(2), 101–116.	

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

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>.