Demetris Koutsoyiannis

From: grlonline@agu.org
Sent: 5/6/2006 16:15
To: dk@itia.ntua.gr
Subject: GRL: 2006GL026709 (Long-term persistence and uncertainty on...)

Dear Dr. Famiglietti,

Two weeks ago (on May 22, 2006) we sent the following email questioning the decision for our manucript. Since then we received no reply. So we are repeating our message, this time using the option "Send Manuscript Correspondence" of the online system, in order to make sure that our message is received.

Thanks for your consideration; we look forward to your reply,

Demetris Koutsoyiannis and Alberto Montanari

-----Original Message-----
From: Demetris Koutsoyiannis [mailto:dk@itia.ntua.gr]
Sent: Monday, May 22, 2006 7:04 PM
To: 'grlonline@agu.org'
Cc: 'grl@uci.edu'; 'Alberto Montanari'; 'Demetris Koutsoyiannis'
Subject: RE: 2006GL026709 Decision Letter

Dear Dr. Famiglietti,

First of all we wish to thank you very much for the prompt processing of our manuscript. It is for both of us our first experience with GRL (as authors) and we were positively impressed by the quick reaction of the journal.

Simultaneously, it is the first time for both us to have such an interesting yet so negative experience: One of the reviewers (Reviewer 1) reprobates our manuscript from the first sentence and the other (Reviewer 2) says that it is superb. With all respect to your decision, which was shocking for us, we think that this unique situation justifies a reaction, so we took the liberty to write this letter.

We appreciate the effort of Reviewer 1 to write this long discussion of our paper and in a different situation, for instance if our paper were published and this discussion were done publicly, we would be very pleased to continue it (such a pleasure is perhaps related to Charles Darwin's quotation: "false views, if supported by some evidence, do little harm, for every one takes a salutary pleasure in proving their falseness"). But as an anonymous and confidential text, we strongly believe that this is a disappointing and not useful review for many reasons. We have already proceeded to compile a detailed reply to this review but in this letter we give only a summary of the main reasons why we regard this as a disappointing and not useful review.

1. The arguments it provides are based merely on interpretations and beliefs, and not on the real content of our manuscript.

2. It gives away bad understanding (if not deliberate distortion) of our manuscript. For example when the reviewer says ".. the claims of existence of LTP in this and related previous studies ..." he/she perhaps did not understand our point (written even in the conclusions) that "even the presence of LTP can be disputable on purely statistical grounds".

3. It indicates a confusion of physics and dynamics with mechanistic explanations thereof, which brings us back to the 19th century, before statistical thermophysics and quantum physics (which rely on the concepts of probability and statistics and depart from mechanistic physics) had been developed. Specifically, we believe (and discuss in our manuscript) that LTP is related to the maximum entropy principle. Entropy is a probabilistic (rather than a mechanistic) concept yet a peak research topic even today (as indicated for instance by the fact that Gell-Mann, the Nobel laureate who discovered the quarks, has published recently a couple of papers on it).

4. It promotes anachronistic or even illegitimate ideas about research. When the reviewer says, "Five of the citations are to web pages!! This is thoroughly inappropriate ..." (his/her exclamation marks β?? both of them) he/she seems to exorcise the internet, which for most of us (cf. the Berlin Declaration of 2003) is a global and interactive representation of human knowledge and scientific communication. We recognize that our citations to web pages was not in accordance to the AGU reference style (we should have rather put them in the text and not in the reference list), but simultaneously we cannot accept the exorcising of the internet. We have always followed the practice to cite even personal communications, a practice that it is typical in the scientific community aiming to give appropriate credit where credit is due, and we cannot imagine that public communications on the internet should be excluded from this practice.

5. It gives away a dislike of scientific research per se. When the reviewer attributes to our manuscript "... no clear recognition of the extensive body of existing literature on statistical modelling of climate behaviour...", in our opinion he/she expresses the idea that our research should comply with this "body" he/she has in mind. But if "research" is to comply in principle with any "body of literature", it will not have the ability to be novel and innovative. And if "research" has not the element of novelty and innovation, it is not research at all.

6. It gives away bias and unawareness of other "bodies of literature" related to persistence in climate, some of which are cited in our manuscript. We can provide more information on this in a more detailed reply. Here we wish only to point out that, if the information we have is correct, the effects of persistence in climatic statistics will be mentioned for instance in the IPCC WG1 Fourth Assessment Report (in preparation).

7. It depreciates already published papers (and recent GRL ones) saying "The fact that GRL has published other poor papers in this subject area e.g. Cohn and Lins (2005) and Rybski et al (2006) is not a justification for the continued publication of such papers." Of course, the reviewer has the right to have this opinion for these papers, but we believe that the appropriate vehicle for expressing opinions on published papers is the public dialogue (e.g. journal discussions) rather than confidential and anonymous reviews.

8. It depreciates scientific disciplines and particularly the interdisciplinary dialogue, saying "citations of past work the authors here use to motivate their application of LTP to climate data come entirely from the hydrology and biological literature". The reviewer extends his/her criticism to "the various attempts to calculate fractal dimensions, scaling laws, Hurst coefficients, and various measures of long-range or 'long-term' persistence" in these disciplines, which he/she considers inappropriate for climate, thus promoting a strange idea that hydrological processes are irrelevant to climatic processes.

With these thoughts we wish to kindly ask you to consider giving a second chance to our manuscript and to allow us to proceed to a resubmission, in which we will address all useful review comments, both the formal ones as well as others kindly offered by colleagues to whom we communicated our manuscript, and we will give detailed replies to other comments that we disagree with.

Such a resubmission will be beneficial for our manuscript and the ideas we try to express. But please allow us to think that it will be beneficial for the journal and its editor, as well. Because, it will reduce the probability of an editorial "error of the second kind" (this is a statistical analogue term that has been introduced by the HSJ editor Z. Kundzewicz to describe the rejection of papers that deserve publication). But even if there is no such an error in your decision, being aware of some of your own research, we hope that you may find some of our above arguments, as well as those we will express in the resubmission, convincing enough to justify a reformulation of your statement "I am in full agreement with the comments of reviewer #1".

If, despite our hopes, you disapprove a resubmission of our manuscript, then we will follow your initial suggestion and submit it somewhere else. In this case, may we have your permission to include in the new submission all material of the first submission including this exchange? It is a practice that we have generally followed in resubmissions in other journals as we feel that, in one in another manner, previous discussions may be useful information for the editors handling the resubmissions.

Kind regards,

Demetris Koutsoyiannis and Alberto Montanari

PS. We started this letter saying that it is our first experience with GRL as authors. However, we would like to clarify that we have some experience in scientific publishing in other journals; also we have served GRL as reviewers and provided several services to AGU journals, for which AGU has rewarded and honoured us (DK received WRR editorβ??s citation for excellence in refereeing in 2000 and AM is currently an associate editor of WRR). This experience, extended to other international journals, allowed us to develop some opinions and views on scientific publishing procedures and to strongly believe that the dialogue on these issues is very significant for any improvement. DK has expressed some of his opinions co-authoring a couple of recent journal articles. Hoping that you may find them related to the present discussion, we took the liberty to attach them herein.

> -----Original Message-----
> From: grlonline@agu.org [mailto:grlonline@agu.org]
> Sent: Friday, May 19, 2006 5:22 PM
> To: dk@itia.ntua.gr
> Subject: 2006GL026709 Decision Letter
>
>
> Dear Dr. Koutsoyiannis:
>
> We have had your manuscript, 2006GL026709, "Long-term persistence and
> uncertainty on the long term," reviewed for both scientific content
> and GRL-specific criteria. Based on this evaluation, I cannot
> consider your manuscript further for publication in Geophysical Research Letters.
> Although one of the reviews is supportive (reveiwer #2), I am in full
> agreement with the comments of reviewer #1. Attached below are the
> review comments, which you may find helpful if you decide to revise
> the paper and submit it to another journal. I am sorry I cannot be
> more encouraging at this time.
>
>
> Thank you for your interest in GRL.
>
> Sincerely,
>
> James Famiglietti
> Editor
> Geophysical Research Letters
>