

Demetris Koutsoyiannis

From: grlonline@agu.org

Sent: 29/8/2006 22:58

To: dk@itia.ntua.gr

Subject: 2006GL027652 (Editor - James Famiglietti): Decision Letter

Dear Dr. Koutsoyiannis:

We have had your manuscript, 2006GL027652, "Statistical analysis of climatic time series: uncertainty and insights," 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. 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

Reviewer #1 Evaluations:
Science Category: Science Category 4
Presentation Category: Presentation Category C

Reviewer #1(Summary comment):

... based on a lot incorrect assumptions. The literature has not been read thoroughly. This is essential for someone changing fields.

Reviewer #1(Formal Review):

First, I should say that I would be slightly annoyed if GRL went to new reviewers for a paper that is clearly a modified submission in the light of an earlier set of reviews. That said I have not looked at the earlier responses and modifications only to ascertain that this paper is a resubmission.

I have several major comments on this submission. They reflect two main areas, detection and attribution studies and the use of the paleoclimatic time series. On the latter, I am going to recommend rejection as the literature cited has not been read, as there are several statements which are incorrect and just plain wrong. These could have been found in the papers cited, but it seems clear to me that several of the papers have not been read. They make some of the assumptions made in this paper incorrect.

In Detection and Attribution (D&A) studies there is a clear need to incorporate the results from

modelling studies. Anthropogenic forcing (from increases in greenhouse gases and sulphate aerosols) is patently a new forcing that was not present in centuries prior to the 19th century. What D&A studies seek to do is to see if a signal from modelling studies is present in observational data, i.e. there is an expectation (from the GCM) of what should have happened. There can be some argument as to whether the GCM is adequate and/or whether the past history of natural forcing (solar output changes and large volcanic eruptions) is correct, but this aspect is immaterial here. The point is that the boundary conditions from the past have changed, so looking at the long paleoclimatic record isn't that appropriate. The standard deviation of the instrumental record should increase in the 20th century because of the warming which has occurred.

How you might overcome this is to use some of these long integrations with your approach, comparing millennial-scale runs of GCMs with natural forcing and then with natural and anthropogenic runs. Some of the available runs that could be used are given in Jones and Mann (2004).

With the paleoclimatic series used it is clear on p5 (line 106) with the statement that 'These series are annual and therefore are not affected by seasonality' is just plain wrong. This shows that the papers have not been read in detail. For example, the reconstructions referred to as J98 and B00 are not annual (calendar year) but summer reconstructions. So comparing these two with instrumental calendar year data from the 1850s is inappropriate. Also these two reconstructions are scaled to have the same standard deviation as the instrumental summer NH temperatures by their reconstruction technique. They are given the SD of the instrumental series of a relatively long period of data. The SD of the NH instrumental data (from land and marine series) for summer is smaller than that for calendar-year averages. These two series are developed using the technique referred to by Jones and Mann (2004) as CPS and differ from M99 (which is a CFR approach).

The other two approaches used (E02 and M05) are also basically CFR techniques where the 'average' paleo series for the NH are given the instrumental standard deviation. They differ from J98 and B00, by giving the series the SD of decadal timescale instrumental data. So, using SD as a metric isn't that appropriate, well not unless all the caveats given in the original papers are brought into the discussion.

I am aware that all this discussion applies to Rybski et al. (2006), but I'm not reviewing that paper.

Some specific points

Lines 28-31, All the papers I've read on the Hurst phenomenon refer to hydrological series (runoff, drought, long tree-ring series). There is an assumption of non-stationarity in the process. It would be useful to add a few more of these references. Has anyone, for example, applied this type of approach to temperature data? Also has anyone applied the approach to global-average data, rather than regional or site-specific data?

Line 38. In the discussion of past forcing, you cannot ignore anthropogenic forcing in this list, nor that it is a new forcing, so almost certainly its imprint will not be on past records.

Line 56. What are the physical principles that LTP can result from? These are included in the GCMs, as best as is currently understood. The distant past is almost certainly not going to be relevant to recent past nor the future, as a new factor (anthropogenic forcing) is acting.

Lines 88-95. This discussion is meaningless unless it is together with an appreciation of the new anthropogenic forcing factors. Dams on rivers change the flood characteristics, and this new forcing is changing the character of regional and large-scale temperature series. GCMs give us our best guide as to how this may be happening. This understanding may be wrong, but it is ignored totally within this paper. In simple terms, I'm saying that the past may not be the best guide to the future.

Lines 99-100. It is incorrect to refer to the CRU data. There should be a reference. There are a number of CRU series (for land only, marine only and combined). I reckon you are using the

combined series, but this is just a guess. The land only series may be more appropriate as most the proxy records come from marine domains.

Lines 101-109. I have discussed this earlier. It is not appropriate here to refer to a web site for Stockwell.

Lines 217-220. The CRU series again, and some very brief discussion. There is a new series (Brohan et al., 2006). This provides an in depth discussion of the data series and it or earlier papers should have been referenced. My reason for referring to this new paper is that it provides error estimates on the NH temperature. These error estimates are available on a web site (www.hadobs.org) and click on HadCRUT3. There isn't a need to get all the gridded data, as hemispheric/global series are there with their errors (from site moves, large-scale biases and the effects of reduced spatial sampling in the years before 1950). These errors are on two different time scales (annual and decadal). You'll see that going from one to the other isn't trivial. Some of the errors are site specific so cancel readily, but the biases and sampling issues are more pervasive, so don't cancel as would be expected from a statistical viewpoint.

Lines 221-227. You've been reading too much of the wrong literature on proxy data. Read Jones and Mann (2004) for a more balanced view. Subjectivity isn't related to which samples are used in dendroclimatology. All samples from each of the sites are used, generally. Subjectivity relates to how the individual series are standardized (RCS techniques etc). I know I sound to be preaching, but if you are going to use data from other fields, at least read what the issues are, and don't base arguments on what you might read on web sites. Another good book on paleo series is Bradley (1999) referred in Jones and Mann (2004). My notes on reading these few lines, just say wrong, wrong, wrong. I know there is a lot of literature to go through, but this is always the case when one shifts fields. You will know of all the issues that can affect runoff and precipitation series, but this knowledge is different in each paleoclimatic field.

You will find dendroclimatology the most numerate of the proxy sciences.

Lines 239-241. As stated earlier the comparison with the CRU data is inappropriate, if the seasons are different. You can't make the assumption that the seasons in the past all behave the same in their long-term characteristics. Winters could have changed differently from summers, for example. The calendar-year average masks any possibilities of knowing if this has happened, and almost all proxies (with the exception of corals) are almost certainly seasonally specific. There is a discussion of this in Jones and Mann (2004).

Lines 259-264. Your conclusions aren't valid due to the way the various series have been produced.

Finally, as I've said earlier all these conclusions apply to Rybski et al. (2006) and also but to a lesser extent to Cohn and Lins (2006), so don't take the comments personally. The field is complex, and there is an awful lot of misinformation out there. Long GCM runs will enable you to test assumptions about recent new forcing and how it impacts large-scale temperature averages.

Brohan, P., Kennedy, J., Harris, I., Tett, S.F.B. and Jones, P.D., 2006: Uncertainty estimates in regional and global observed temperature changes: a new dataset from 1850. *J. Geophys. Res.* 111, D12106, doi:10.1029/2005JD006548.