

Interactive comment on “On reconstruction of time series in climatology” by V. Privalsky and A. Gluhovsky

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We are grateful to the Anonymous Referee#3 (the Referee in what follows) for his or her comments.

“... I find the mathematical approach pretty old-fashioned and rather complex.” Yes, in comparison, the traditional correlation/regression approach is brand new (just about 130 years old against about 40-60 years of time series analysis) and is certainly much simpler. In fact, the traditional approach is way too simple for time series analysis and the problem at hand (as shown in the paper).

“There are numerous articles using the approach of Box-Jenkins, including transfer function modeling, etc.” Please show a publication of time series reconstruction based

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upon multivariate time series analysis in both time and frequency domains with an example of its comparative efficiency.

“... what is new here?” Methodologically, practically everything is new for climatology. In particular, the multi-variate approach in the time domain and the parametric estimation in both time and frequency domains. And the reconstruction results are new as well.

“There is no mentioning or application of more modern techniques such as Structural time series models, next to the ARIMA approach chosen here.” This is not a review of time series methods.

“... the title and abstract suggest a general approach for 'time series in climatology'. But the only concrete example is on sunspot numbers and TSI.” This is not true. The title and the abstract say “reconstruction of time series in climatology”. And the example is an example of reconstruction.

“... but nothing is shown from this important field of research. There are many top quality papers on this topic in recent years but not named in this paper. Thus, the claims in the title, abstract and other texts are not substantiated here.” Again, this is not a review. At the same time, we regret that the Referee regards the earlier publications referred to in our paper as not significant. We disagree. We showed that the previous publications discussed in our article are important for the entire field of research. They include the earlier publications by A. Douglass who, besides his other achievements, noticed the inadequacy of the cross-correlation approach at the time when the dendrochronology was at its birth stage. (It is interesting that none of the three anonymous referees of this paper has a comment on that visionary opinion of one of the founders of dendrochronology.) We included references to more recent research by H. Fritts and J. Guiot (who took the task of reconstruction to the frequency domain and tested some parametric models). As we say in the paper, we regard our method as a “further development of previous efforts taken by a number of authors”, with proper references.

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We thoroughly reject the accusation by the Referee that “nothing is shown from this important field of research”.

A sentence like ‘is mathematically incorrect’ (page 4702, line 3) is much too strong.” No, it is absolutely correct.

“Third, a topic of utmost importance, that of calibration and validation, is not treated at all here. The sunspot numbers and TSI share only 3 1/2 cycle. But the historic predictions extent over 21 cycles; There are no “predictions” in the paper and, moreover, the term “historical predictions” is an oxymoron. We used the latest and most reliable data on SSN and TSI available at this time. In particular, the TSI data are the first in the history of direct observations of TSI outside of the atmosphere. And no further validation is required because there is no other reliable TSI data and because this is a comparative study of two methods.

“If the authors want me to believe that these predictions are accurate, they have to show the patterns and correlations over this 3 1/2 cycle are very stable.” The requirement “to show. . .are very stable” is vague. What “patterns”?What “correlations”? If the Referee means that the statistical properties of the process are time-dependent, there can be two reasons for it. First, it can be caused by the sampling variability. This would be normal for any time series of finite length. Second, the process is nonstationary. Then, you cannot study it on the basis of just one short sample record (see Bendat and Piersol, 2010).

“The standard method in dendroclimatology is by calculating RE and CE values. See for example . . .” This is not a standard method. And it is a comparative study. The higher efficiency of our approach as compared to the correlation/regression approach, which is indeed standard for climatology, is sufficient to prove the advantages of the time series analysis applied to time series against the random variables analysis applied to time series.

“ . . . I would be interested to see a study on TSI/sun spot numbers . . .which is capable of

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modeling cyclic behaviour where the wave length is flexible. . .”. This is not the subject of the paper. Also, see the comment above on the non-stationarity.

“By the way: the authors fail to point out why this example of reconstructing TSI is important anyway. They should explain that to the reader.” We regret that the Referee does not see the importance of studying TSI. The acronym TSI stands for Total Solar Irradiance (of the Earth). TSI is the main source of life on the Earth. This is a serious enough reason for us to study it, including examples of its reconstruction over the time when TSI could not have been measured. We also believe that the CP readers, with very few exceptions, understand it as well and do not need it to be explained.

Last but not least, we would like to stress a point regarding attempts by the Anonymous Referees#1 and #3 to prevent the readers of CP from learning about the mathematically correct approach to time series reconstruction in climatology (as compared to the mathematically deficient approach through the cross-correlation coefficient that is standard in climatology). Both referees tried to distort what the paper says about the applicability of our research results to time series reconstruction. Both tried to ascribe to us something that we did not say by either adding something to the original text (Referee#1 added a period in the middle of sentence, see our reply to the Referee#1) or pretending not to see what is written in the original text (Referee#3 “missed” an important word in his or her quote of the paper’s title and abstract, see this reply). We consider this behavior unethical.

The Authors

Interactive comment on Clim. Past Discuss., 11, 4701, 2015.

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