

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

Anonymous Referee #3

Received and published: 13 November 2015

I am sorry to say that I am critical on this Discussion paper.

First, I find the mathematical approach pretty old-fashioned and rather complex. There are numerous articles using the approach of Box-Jenkins, including transfer function modeling, etc. Thus, my first critical point is: what is new here? There is no mentioning or application of more modern techniques such as Structural time series models, next to the ARIMA approach chosen here.

Second, the title and abstract suggest a general approach for 'time series in climatology'. But the only concrete example is on sunspot numbers and TSI. Dendroclimatology is named by one of its founders Douglas, 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

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



substantiated here. A sentence like 'is mathematically incorrect' (page 4702, line 3) is much too strong.

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! 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 standard method in dendroclimatology is by calculating RE and CE values. See for example "Surface temperature reconstructions for the last 2000 years", National Research Council 2006, page 93. And for example, H. Visser et al. 2010: Detecting instabilities in tree-ring proxy calibration, CP 6, 367-377. Nothing on validation is shown or discussed in this paper. I find this is a serious omission. The final sentence in the Abstract 'The results of the reconstruction are in statistical agreement with observations' is not substantiated by this paper. It is fitting at best.

Fourth, I would be interested to see a study on TSI/sun spot numbers which is capable of modeling cyclic behaviour where the wave length is flexible, here between 8 and 15 years or so. This problem is not treated here. ARIMA modeling assumes fixed cycle lengths (as many other time series methods). 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.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

