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Climate of the Past Discussions



# *Interactive comment on* "On misleading solar-climate relationship" *by* B. Legras et al.

**Anonymous Referee #3**

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The manuscript is devoted to a critical re-assessment of a series of recent publications by the Paris group (Le Mouel et al. LMKC and Kossobokov et al. KLMC) published in J. Atmos. Solar-Terr. Phys. (JASTP). This is indeed an important topic since the field of empirical studies of sun-climate relations is highly controversial and obscure. Since the main tool in this field is based on statistical tests and correlations, proper evaluation of statistical significances is crucial. The criticized papers LMKC and KLMC were published in JASTP. Accordingly, it would be natural if criticism was submitted to the same journal. This question is left up to the Editor, and here the Reviewer concentrates on scientific issues.

The authors raise two main concerns against the results obtained by LMKC and KLMC: (I) the claimed statistical significance is based on an improper analysis, while the correct test suggests that the result is not significant; and (II) the found relation between high solar activity and warmer times is largely based on the last 50 years when the an-



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thropogenic warming effects is important, while the relation looses its significance if the last 5 cycles are removed. These arguments are serious and worth to be presented to the community. The third argument concerning the low quality of the data used is less constructive – we all want perfect data sets but have to use what we have, especially on the long time scale.

However, some comments listed below should be addressed before the paper can be recommended for publication.

1) The title would sound better if replacing “misleading” by “questionable”.

2) Page 771-772: Modern minimum is not considered as a grand minimum (unlike the Maunder minimum). Even the Dalton minimum is not similar to the Maunder minimum – the dynamo was somewhat reduced in strength but not suppressed.

3) Last para of section 2: It would be worth mentioning that the trend in TSI is still a subject of debates – other TSI composites do not depict a decreasing trend.

4) Same para: even though the sunspot number per se is not a good proxy for irradiance, it is used as the basis for TSI reconstruction in the past, in particular by Judith Lean. I suggest removing the confusing quotation here.

5) Anyway, this reviewer see not much use of Section 2. It is not clear what is proposed by the authors to replace SN (TSI reconstructions by, e.g., Lean or Solanki or Froehlich?) and how it can affect the studied relation.

6) Page 781, line 11: from bottom: the last sentence “In other words...” is recommended to be removed.

7) Page 781-782: the last part (on the Student’s t-test) needs to be given in more detail. The significance level (0.05?) should be defined.

8) Page 784, line 22: the end of the paragraph “This error ...” needs to be given in more detail. It is hard to understand what the authors want to say here.



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9) Page 785, line 20, last para: a more balanced view on cosmic ray effect ought to be given – e.g., a mechanism alternative to "Svensmark's" one was proposed by B. Tinsley (electroscavenging). The authors may want to refer to a recent review of empirical evidences of cosmic ray effects.

10) It would be worth stating that sunspot activity was indeed very high since 1950's (accidentally concurrent with the anthropogenic effect) – e.g., Solanki et al. (Nature, 2004).

11) Special comment regarding the Supplementary material. This reviewer can hardly understand it since he/she is not familiar with Mathematica. Presently it is a mixture of pieces of code and texts that is difficult to follow. A simple plain description of the procedure followed by the code would be useful.

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Interactive comment on Clim. Past Discuss., 6, 767, 2010.