

Interactive comment on “On misleading solar-climate relationship” by B. Legras et al.

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Answer to referee #1

We thank referee #1 for his comments and suggestions. Comments by the referee are highlighted and followed by our answers.

The title of the study (On misleading solar-climate relationship) does not quite relate to the arguments the authors make. The authors argue that a solar-climate relationship does not really exist - at least that it cannot be convincingly shown that it differs from randomness. The point I like to make is that a non-existing relationship cannot be misleading. My suggestion would be to change the title to something like A critical look at solar-climate relationship (although I realize that this might remind readers to the work of Pittock (1978)).

The referee is right is pointing out a semantic ambiguity in our title. The title has been actually shortened from a previous more explicit version upon a request by the editor. We propose to change it to "A critical look at solar-climate relationships from long temperature series".

The point I would like to make here is that I found it hard to arrive at equation (3) of the current paper. It seems it is possible to reproduce this formula, but not without a strong assumption. This concern relates to the appearance of the factor $(1/N^H + 1/N^L)$. The authors need to make this step clear so that even the less statistically-inclined reader is able to follow and reproduce the steps.

There is no strong assumption needed to obtain equation (3). The only requirement is that the 21-day averaged temperatures for a given calendar date and over the years included in the dataset are independent random variables with the same variance σ^2 . The true variance of the solar shift is then $\sigma^2(1/N^H + 1/N^L)$ and the first factor on the r.h.s of equation (3) is an unbiased estimate of σ^2 . This formula, known as the "pooled variance" is derived in many textbooks (Weatherburn, 1961, sec. 88)(Dekking et al., 2005, sec. 28.2) and is also commonly used in standard numerical software (Press et al., 1992, sec. 14.7). We have added a couple of sentences about the derivation of equation (3) in the revised version of the manuscript.

A strong point of the current paper is that the estimates of confidence intervals is done by a boot strapping method as well. The authors should be more specific in the actual paper concerning the precise procedure taken in this boot strapping.

We have also rewritten the description of the random permutation test procedure to improve clarity.

page 768, lines 24, 25: The variations in the 10.7 cm solar flux, an index often used for the solar cycle [4], are much more than suggested. The variations range from ca. 70 units for solar minima to ca. 200 units for solar maxima (Labitzke, 2001).

The solar flux varies much more at 10.7 cm than in other parts of the electromagnetic spectrum but this is not relevant here since our sentence was focusing only on the domains of the solar spectrum which are playing a role in the Earth radiative budget and are likely to influence its climate.

A reference to Labitzke (2001) has been added in the text.

page 774, line 12: consulting the ECA&D staff made clear that the policy changes were at the side of the Belgian Met. Office (KMI) rather than ECA&D. This point also relates the remark made at page 778, line 8.

It is true that the Uccle daily temperature series has been removed from ECA&D dataset upon request of the Belgian Met. Office who considers that ECA&D free distribution scheme is interfering with its commercial policy. Although the data are plausibly still available for research purposes, we have decided not to require them since it would not have been possible to redistribute them with our supplementary material. We have added a few words to make clear that the policy change is not due to the ECA&D team who, on the contrary, strongly supports open access to data. In any case, we do not think that our demonstration suffers from the lack of analysis of Uccle data.

page 776, lines 15-18: in order to assess the homogeneity of the Bologna series, the authors need to make clear that their reference series do not suffer from inhomogeneities. One option to do this would be to give the homogeneity information provided by the ECA&D webpages, but other approaches are possible as well.

None of the temperature series can be assumed to be free of inhomogeneities. We do not use "homogeneous references" to assess homogeneity, but direct pairwise comparisons, of a candidate versus its neighbours. If a change remains constant throughout the set of comparisons, in sign and amplitude, it is likely due to the candidate. Metadata can often resolve ambiguities and provide consistency check when discontinuities are detected, but past changes have not always been recorded and ECA&D has not

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collected all the existing metadata.

Other minor points and typos have been corrected according to the suggestions of the referee.

References

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