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Climate of the Past Discussions

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

Anonymous Referee #2

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This manuscript would represent a rebuttal of a paper by Kssobokov et al. publish in the Journal of Atmospheric and Terrestrial Physics about the statistical detection of the solar influence on the temperature evolution in three European stations in roughly the last 200 years. The authors of the manuscript would reveal three major errors in the paper by Kossobokov et al. :that the statistical analysis would not be able to detect the solar influence because it does not take into account other forcings in this period, that the level of significance in the statistical tests are inflated due to the lack of consideration of autocorrelation of daily temperature series; and that the temperature series of some of the stations used in the analysis are affected by inhomogeneities that make them, if not corrected, unsuitable for this type of analysis.

In my opinion the authors are basically correct in the errors they point out in the paper by Kossobokov, or at least that their criticism is worth being published. The significance tests do not consider the autocorrelation of the daily series and Kossobokov

et al should have considered it. The present manuscript evaluates the effect of the autocorrelation in a proper way, in my opinion. The issue of the homogeneity of the Bologna series seems also clear to me, and I am not convinced by the defense that Kossobokov et al present about the prevalence of original data. Their argument may be valid for data obtained in experiments under controlled conditions. Unfortunately, meteorological data were not measured and archived with climate applications in mind and therefore they have to be corrected. Another question is how this should be done, but in my view it is out of question that the Bologna series, uncorrected, cannot be used for climate analysis. The issue of the col-linearity of the solar and greenhouse forcing is also quite clear to me. The composite method used by Kossobokov would be valid in case that other forcings vary independently of the solar forcing. The fact that 50 years of the high solar composite occur in the second half of the 20th century is a clear warning that solar and greenhouse gases cannot be so easily separated. Correlations between trendy time series are known to be dangerous since the relationships between number of births and stork populations were first reported. This is unfortunately an error that can be also found in papers by authors sitting squarely in the other side of the climate debate.

However, I have also a few considerations about the present manuscript. First, I do not clearly see the connection between topics dealt with in this manuscript and the overall area of science that Climate of the Past would usually cover. Although this is clearly a question for the editors to decide, I would nevertheless point out that there a risk of misusing CP as an outlet of comments to papers that would better in the original journals, even more so when the topic of the comment is not clearly about 'the climate of the past'.

The conclusion section is weak in my view. The authors could have summarized their main results, but instead chose to summarize the results of another hypothetical manuscript that they will submit 'elsewhere' . This is confusing, and the readers are asked to accept at face value these new results which have not been presented in the previous sections. If the authors deemed that the bootstrap methods used by K are in error, they should deal with them here as well. If they do not wish to discuss them here, they could just mention that 'further work in progress' without opening new discussion points. By the same token, it is striking that the issue of cosmic rays as a vector for solar forcing is climate appears at the end of the discussion. I think this is distracting. The present manuscript is not about solar forcing of climate, it is about the methods applied by Kossobokov et al.to detect it. The conclusion section as it stands now, bears little connection to the results presented in the manuscript. It reads rather like an afterthought : ' for the case that this rebuttal is not enough, the physical theories about the climate-sun connection are also wrong..'

I also think that this manuscript would need a revision to improve its clarity and readability. The English needs certainly a copy-edit revision by a native speaker.

I have some other particular points, some of them are clearly a matter of opinion, but which nevertheless the authors may be willing to give a second thought to.

Page 767 abstract. The abstract should mention the period covered by the analysis and mention the three time series.

line 9, 'sun spot counts are a poor indicator of solar irradiance'. I do not think this is a major criticism and it can be argued that sun spots are perhaps not the best indicator, but certainly the longest. Why open another front of debate in the present rebuttal? If the authors think that sun-spot numbers are not adequate for the analysis they should then present another index and use it for their own analysis.

Introduction. The argument about the presence or absence of trends in the solar irradiance is distracting from the main pints the authors want to raise. There several reconstructions of solar irradiance and there is some on-going debate about which satellite data set ts more realistic representing the real trends in solar irradiance in the last decades. I think the authors are mislead when they try to dismiss as many arguments as possible against the solar influence of climate. They may be correct, but this

is not the right place to do it, even more so when in the abstract they state that their main goal is to rebut Kossobokov et al and Le Mouel et al.. If they want to enter the debate about recent trends in solar irradiance they should then cite also the papers that do not agree with their view, e.g.. by Scafetta and West or by Douglas, and then engage in a deeper discussion of this issue. As I wrote before, I think this would be distracting, as their three arguments mentioned at the start are clear and sufficient.

Page 768 line 5. the comment on the the weakness of other sciences is out of place and merely shows that the authors are not aware of the complex mathematical models used in financial prediction.

Page 771 line 6 'of the order of '

Page 774 line 26 distant 145 km

Page 776 line 6 'comparisons series' reference series

Page 777 line 23. I would tend to avoid the word 'indisputable' in scientific text

Page 780 line 15 'being meaningful'. With this expression , which is used also in other paragraphs, the authors really mean statistically significant. Why not use the more accurate expression 'statistical significant' ?

Page 781 line 13 'In other words,.... but not on the Earth' This sentence is unnecessarily dismissive.

Page 784 line 1 'under-evaluated' underestimated

Page 785 line 1, the formulation of this paragraph is confusing. I would rather separate both issues, homogeneity of the temperature series and col linearity of the solar and anthropogenic forcing, in two clearly stated sentences.

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