

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

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Answer to referee #2

We thank referee #2 for his comments and suggestions.

We basically agree on the comments made by referee #2 on Le Mouél et al. (2010) and Kossobokov et al. (2010) (hereafter LMKC and KLMC). Other comments by the referee are highlighted and followed by our answers.

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

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comment is not clearly about the climate of the past.

In our view, the empirical forcing of climate on decadal scale by solar variations is a topic falling entirely within the subject areas of Climate of the Past as defined in [http://www.climate-of-the-past.net/general_information/journal_subject_areas.html] and the editors seem so far to agree with us since the manuscript has been accepted in the discussion section. We have chosen to submit our manuscript to Climate of the Past and not in the original journal where LMKC and KLMC have appeared for two main reasons: (1) by redoing all the calculations and using new homogenized data not included in LMKC, we go beyond a simple comment and (2) we considered that the matter requires an open review process which is a unique feature of Climate of the Past.

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.

The discussion about KLMC has been removed from the conclusions and organized as a separate section, with some rewriting. There is actually no need (and no wish) of further work to invalidate KLMC. The first four figures of KLMC are identical to the first four figures of LMKC and are thus directly concerned by our sections 5 and 6. The remaining part of KLMC consists in performing statistical tests on the whole series of temperature data. Whether such tests are valid or not is not even important since the tests do not distinguish the anthropogenic forcing from the solar forcing and are irrelevant in any case. We have shown in sections 5 and 6 that the solar shift may be statistically significant over the whole dataset in some cases but is not when the

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last 50 years of the series and the interference with the anthropogenic forcing are removed. The only instance where KLMC removes the last 50 years is contained in table SM3 within their supplement section. But then the statistical test is incorrect as explained our manuscript and does not reach valid conclusions. We have performed plain Kolmogorov-Smirnov tests in the supplementary material and they all agree, as expected, with the results of the Student t-test which. The Student t-test is actually the standard test for statistical significance of the difference between two averages; using the Kolmogorov-Smirnov test only introduces unnecessary complication in the matter.

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..

The role of cosmic rays and its modulation by the solar activity is systematically invoked by LMKC and KLMC and other papers by the same authors as an explanation of their observations. Our conclusion is that there is nothing to explain in this precise case but we think also that it is useful to mention that the link between cosmic rays and climate is itself a matter of debate and cannot be treated as a well established fact, although this is an area of research to pursue. We refer to Gray et al. (2010) for further discussion.

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.

We have done our best to improve the English and readability of the text.

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

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The abstract has been modified as suggested.

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.

We purposely cited the review by Judith Lean (2010), the pioneer and expert of this research field who thinks that it is a major criticism to use in 2010 the raw sunspot counts as a proxy for solar irradiance. There are indeed many alternative solar irradiance curves covering the past centuries that have been published over the past 20 years. These are based on a variety of properties of sunspots including the envelope of the sunspot number cycle, the length of the cycle, the structure and decay rate of individual sunspots, the average sunspot number and/or the group sunspot number, the solar rotation and diameter, sunspot group areas, Greenwich sunspot maps and p-mode amplitudes estimated from sunspot numbers. In order to further underline the problem, we added the citation of another paper by Gray et al. (2010) who review that field. The variety of these TSI reconstructions is illustrated by their Figure 7 compiling 8 different reconstructions for the past 3 to 4 centuries. These TSI curves significantly differ in their long-term trends and structures linked to the 11-yr and longer cycles. Using these published curves would obviously have an impact on the statistical analysis and comparison with temperatures. It is beyond the scope of our work to perform these analyses.

The argument about the presence or absence of trends in the solar irradiance is distracting from the main points the authors want to raise. There are several reconstructions of solar irradiance and there is some on-going debate about which satellite data set is more realistic representing the real trends in solar irradiance in the last decades. I think the authors are misled when they try to dismiss as

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Discussion Paper



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.

We mentioned this issue because both LMKC and KMLC extensively cited Scafetta and Willson (2009) in a way that caricatures the present state of the ongoing debate about the satellite TSI data. The way they refer to this debate is even more misleading in their Comment on our work (see our response to their comment). While discussing about the solar irradiance record based on satellite data and the difficulty in detecting a baseline evolution over the last 30 years of precise data, Kossobokov et al. refer to "*ongoing controversies, such as that between Scafetta and Willson (2009) and Krivova et al. (2009)*". This statement is misleading because there is no ongoing debate between these two particular studies: Scafetta and Willson (2009) used the model developed by Solanki et al. (2005) and Krivova et al. (2007). These authors (Krivova et al., 2009) simply discovered that Scafetta & Willson had made fatal mistakes in using Krivova et al.s model, but that its correct use leads to a stable irradiance baseline. Hence, the study by Krivova et al. (2009) constitutes a clear-cut and definitive refutation of the previous claim by Scafetta and Willson (2009), a paper that should be no longer cited as a valid reference (as done by LMKC and KLMC). Further demonstrations of the errors made by Scafetta & Willson are developed in Gray et al. (2010).

Referee #2 also cites papers by Scafetta & West and by Douglas (Douglass et al., 2008, maybe). These papers do not concern the debate on the satellite irradiance compilation, but deal with the other debate on the attribution of temperature changes to the various forcings and on the comparison between model trends and observations.

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As underlined in our paper, the series of papers by Scafetta & West (Scafetta and West, 2006a,b, 2007) have been assessed by Benestad and Schmidt (2009) who showed that their approach is erroneous and their conclusions is invalid. Very serious errors in Douglass et al. (2008) that invalidate their results are corrected in Santer et al. (2008).

We do not wish to dismiss as many arguments as possible about solar-climate relationships and we actually acknowledge that there are many evidences of such relationships by quoting some papers that we consider as among the most relevant, including the recent review by Gray et al. (2010). The fact that a number of published works in this field have undergone rebuttal over the past (see examples above) is an incentive towards watchfulness but we do not conclude that there is no solar influence on climate.

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.

We do not imply that medicine, sociology or finance are weaker fields of knowledge than other ones but that empirical relations are playing a crucial role in such fields. The mathematical complexity of financial models, that we do not ignore, cannot hide this fact. We have modified the sentence to avoid confusion.

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

We generally avoid the word "indisputable" but we think it is appropriate to use it about the existence of an anthropogenic forcing by increased greenhouse gases.

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

We felt that the sentence "In other word ... but not on the Earth" is necessary as it summarizes the core of the error made by LMKC in estimating the variance of the solar shift. In the third part of their comment to our work, the authors of LMKC demonstrate that they still misunderstand this crucial and rather elementary point. The paragraph

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has been rewritten and separated in two parts to improve clarity.

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

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