

Interactive comment on “Statistical issues about solar-climate relations” by P. Yiou et al.

Anonymous Referee #1

The figure numbers have been checked and corrected. The corrections in the manuscript have been taken into account.

We shall provide the R code (with the data files) we used to obtain the figures of the paper.

Anonymous Referee #2

The manuscript is basically a rebuttal of a previous papers on the solar influence on European temperature by Le Mouel et al. and Courtillot et al. The authors of the present manuscript try to estimate the significance of the correlation between temperature and solar activity under a more realistic null-hypothesis, encompassing the autocorrelation structure of the temperature series and the geomagnetic series, used as a proxy for solar activity. The main conclusion is that the statistical significance of these relationships under this null hypothesis is very low.

Although I think that the contents of the manuscript are correct, I found it difficult to read. The structure is confusing and the language very imprecise in critical passages.

The English formulation does not contribute to bring the message across clearly. In my view, one critical problem in the manuscript is that it is aimed to rebute a former analysis without saying it explicitly. The introduction thus appears unfocused, criticizing in general statistical studies that analyze the solar-temperature relationships. Only after having read half of the manuscript would a reader realize that the criticism is directed to a particular methodology exposed in a particular paper. I think the manuscript would be much more clear if it would state this from the very beginning, then summarize the methods employed by Le Mouel and Courtillot, state their conclusions and the set off to unveil their flaws or deficiencies, essentially the lack of a proper analysis of statistical significance.

OK. We reformulated the introduction and abstract to make it clearer right away that we focus on the analyses and methodologies of Le Mouel et al. (2008, 2009) and Courtillot et al. (2010). We kept the first paragraph of the introduction “as is”. The second paragraph introduces the context for methodology and data.

We point out that this is not a simple “comment” or “rebuttal”: we redid all their analyses and complemented them with statistical tests. Hence we believe that this manuscript conveys a true added value with respect to the original three papers.

The abstract is too general and non- informative: 'The goal of the paper is to provide a framework to control the spurious results that statistical tools can generate'. Do the authors refer to *all* statistical methods ? do they refer to *all* statistical studies about the solar-temperature relationship? I do not think so.

The abstract is rewritten for clarification and focus.

The introduction is devoted initially to a short review of the physically based studies of the solar-climate relationship. This review is quite biased, citing only papers that tend to be critical to this relationship. As this manuscript is mainly concerned with statistical issues, I do not see the need for this part of the introduction, unless the authors would like to go into

deeper detail, which they explicitly do not wish to.

The papers we cite are essentially review papers. To the best of our knowledge, they have never been “rebutted”. The IPCC report (Jansen et al., 2007) does not deny a solar influence on climate.

After briefly mentioning the papers by Le Mouel et al., the introduction turns too general again, and states that the goal of the manuscript is ‘to explore the statistical consequences of transformation of data’. But the manuscript only deals with one particular data filtering, that apparently applied by Le Mouel et al. The expression ‘transformation of data’ is in my view also too unspecific. The Le Mouel et al. study was previously unknown to me, and I think any reader in my situation would find this introduction quite confusing.

As written before, the introduction has been rewritten, according to those recommendations. The reviewer might be interested to learn that those papers were recently mentioned in the general press in France as major scientific discoveries (Le Figaro, Libération, Le Monde).

The method section starts with a fairly basic description of autoregressive processes, to settle down later on the most simplest of those as a null-hypothesis.

Not quite: we discuss AR(1) processes from the beginning, then introduce a generalization with time-varying coefficients.

From this, several data filters are derived (the mean interannual squared variation and the mean squared daily variation). It turns out that some functional forms of the expectation of these filters provide an estimate of the lag-1 autocorrelation of a series. The reader would try to scour the manuscript for the reason of this detour. Couldn’t one just estimate the lag-1 autocorrelation from the standard Yule equations or least-square estimators (in this case, just the correlation at lag 1?) Is this done because Le Mouel et al. also applied these filters? Is this done because these estimators are better by some unspecified measure?

We do give the “classical” Yule-Walker estimate for the estimate of the parameter a (Eq. (2)), and compare it with the more “elaborate” one from the $L(t)$ transform introduced by Le Mouël et al.

If one only wants to estimate the a parameter (of an AR(1) process), then the Yule-Walker (or maximum likelihood) estimator are obvious choices. The use of $L(t)$ is justified by the “hope” that if the parameter a varies with time, then $L(t)$ can capture those variations. A large part of the paper is spent to show that it is not true when the variations are continuous.

Data: why are daily local data of the magnetic field used as a proxy for solar activity?

There are several published and assessed reconstructions of solar activity around.

What is the reason to use local data ? solar activity certainly does not depend on the location of the Earth where the magnetic field is measured? is it necessary to use daily data of the magnetic field? Few people would believe that that daily variations in solar activity can influence local daily temperature in a noticeable manner. Is this again because the UK series is deemed more reliable or because Le Mouel et al used it ?

The choice of data confirms that the purpose of the manuscript is not to explore the sun-temperature relationships per se -this choice of data is certainly not optimal-, but to rebut the Le Mouel et al. study. I have therefore some concerns that Climate of the Past is the right framework for this.

As already stated, this paper indeed contains a rebuttal and information

complements for three papers that were recently published (in two different journals). This implies that we stay as close as possible to the data and methodology framework of those papers, although we suggest alternate ways of tackling the problems of data processing and parameter estimates.

We chose *Climate of the Past* because of its open review process, which allows for a clarification of the reviews, i.e. the readers have access to what all reviewers write, and see how we respond.

The manuscript goes on to apply the quite entangled estimators for the lag-1 auto-correlation of a time series to the long temperature and magnetic field observation series, obtain values for the lag-1 autocorrelation, and generate surrogate series are generated with the same lag-1 autocorrelation. Then the same filters are applied to the surrogate series and a distribution of the correlations between the filtered series under the AR-1 null hypothesis is derived. The reader would think if this long detour is really necessary. For which reasons in particular are these filters applied? What information do these filters provide? why are they interesting? The justification is that they seem to be the used by Le Mouél et al. If this is the case, it should be stated up front, otherwise the reader will be confused.

As stated before (and hence in the rewritten introduction of the paper), we present and redo the computations of Le Mouël et al. The $L(t)$ and $Q(t)$ filters are motivated by the hope that they give access to time variations of the a parameter in an AR(1) process. Hence, if a given time series is modelled by an AR(1) process, the $L(t)$ or $Q(t)$ filters potentially give access to time variations of the memory parameter. We use surrogate time series to derive confidence intervals of L and Q bounds for AR(1) processes.

I found the last section 4.4 particularly unclear. This section deals with the general of problem of causality between two time series.

This is not completely true. As stated in the subsection title and the first paragraph, we deal with the identification of causality using L . We are not more general than that.

According to the manuscript, a possible proof of casualty of series x on series y is if the lag-1 autocorrelation of y , which may slowly evolve in time, depends on the values of x .

No, we do not prove (or disprove) causality. We assume it in the way Le Mouël et al. do it (i.e., with an AR(1) interpretation), then show that the L transform cannot detect it.

Why is this really convoluted model of causality being addressed? The reader will remain baffled until some paragraphs later he is informed that this is what Le Mouél et al. did. Again, I found this very confusing.

In that section, we play the Devil's advocate by granting that solar activity on daily time scales can affect the daily "year-to-year" variations of temperature, and propose an ideal stochastic model connecting the two processes (Eq. (9)). We then test whether the L transform of simulated temperature can detect this solar modulation through correlation as done by Le Mouël et al. 2009. The answer is generally "no". Note that we do not prove (or disprove) causality. If we assume it in the way Le Mouël et al. do it (i.e., with an AR(1) interpretation), then the L transform cannot detect it.

In the following I list the passages that remained unclear to me:

Page 464 line 7: 'we computed the daily mean' -> spatial average for each day
Done.

line 17: this problem could be circumvented by using centered data' ?

The sentence is changed to be more explicit.

line 25: 'most of the stations are tagged as suspect in the EC&D data base, but the authors would deal with this problem in a forthcoming manuscript'. I found this very striking. Shouldn't first the quality of the data be checked, previously to any analysis, specially if they are tagged as suspicious?

We used a dataset that is close to the one of Le Mouël et al. (the selection criterion is given in the "data" section). Note that some of the stations they used are no longer available in the ECA&D database (e.g. Uccle). If we had had to assess the relationship between European temperature and solar variability, we would of course have checked the quality of the data. An evaluation of this quality is provided in the CPD manuscript of Legras et al. (2010), which was not submitted when the present paper was submitted to CPD.

Page 467 line 4 : 'mathematical expectation'

→ expected value

OK, the change is made. Note that W. Feller (and other American statisticians) also uses the term "mathematical expectation" in his seminal textbook.

Feller, W., An Introduction to Probability Theory and its Applications (3rd ed.), vol. I, Wiley, New York, 1968

line 5: ' for a sufficiently large Zeta'. I think the equation after this line is valid for any value of the window width.

OK. We rewrote the sentence to: The expected value of zeta converges to [...] for all Theta.

Equation 5. Again I think the equation is valid for any window width, provided that the correlation at lag 365 is neglected, which I think is what the authors are doing

Same as above.

Page 468 line 22, 'the probability of failure when one rejects the null hypothesis'

→ the risk of wrongly rejecting the null hypothesis.

OK.

Page 469

line 18 : 'N=300000 increments'

→ time steps

OK.

Page 470

line 21: 'hence the large variations of Q(t) are meaningful for temperature and could be interpreted as such'? Imprecise language. what does 'meaningful' mean ?

The sentence is removed, as it is redundant with the previous one.

line 24: ' their significance with respect to a AR1 process has the same feature as Q'. Imprecise language. what does feature mean here ?

OK. The sentence is simplified.

Page 475 line17: 'the analysis and interpretation of L(t) are potentially irrelevant' ? I cannot understand this sentence.

The sentence is changed to: the interpretation of $L(t)$ in terms of process memory is impossible.

In general, I found the language imprecise. I would recommend to avoid buzz words such as 'feature', 'characteristic' and words of the same kind. Last but not least, as a non-native speaker I think the manuscript would benefit from copy-editing by a native speaker.

Words like "feature" are used to present a general context, and are immediately given a precise meaning (with more specific definitions) in the text.

All the authors of the paper, although French, have rather long lists of publications in respected international scientific journals. Our experience is that copy-editing by colleagues who are not directly involved in the subject can produce subtle misinterpretations with devastating effects. As long as grammar and syntax are respected, we prefer using a possibly awkward English that truly reflects what we did and think, than using more elegant formulas that could convey a message that we do not mean.