

Editor Decision: Publish subject to minor revisions (review by editor) (10 Mar 2021) by Stelios Kazadzis

Comments to the Author:

Please take into account the second round of comments from the reviewer.

Non-public comments to the Author:

I think the authors have to include the second round of comments from the reviewer.

This is a very interesting time series that has the unique feature of two negative linear series and a "jump" in between.

So I agree with the reviewer that the total change should be mentioned and this sudden change during the 1983 which can be partly linked with cloud changes and with DTR ones have to be explained. Or at least to be mentioned and the authors can provide some explanation that could be revisited in the future.

Dear Dr. Kazadzis, thank you for your comments. We agree with the comments of the reviewer and changed the manuscript accordingly.

The authors are again grateful to the anonymous referee for taking his/her time reading and making important suggestions in the revised version of the manuscript.

The present version of the manuscript is much better than the previous version. However, I still have some objections regarding the analysis of the data, and especially the discussion about the results shown in Figure 2.

As I understand from the manuscript, and from the reply of the authors to one of my previous comments, they used the methodology of Rodionov (2004) and detected a statistically significant change in the direction of the trend of DTR in 1983. Based on this result, as well as the results of other studies, they decided to perform the analysis for two sub-periods: 1961 – 1983 and 1983 – 2016.

Authors reply: Using Rodionov (2004) methodology, based on mean values, the observed regime shift in 1984 was for Tmax, not for DTR. As discussed next, we agree with the reviewer that a piecewise linear regression model should be more appropriate than comparing mean values before and after the shift if it is detected.

First of all, is 1983 the first, and the only year that the change in the direction of the trend in DTR is significant? For example, I would expect that the change would be significant for a range of 3-4 years around 1983. Some relative discussion should be added in the document. There should also be some similar discussion regarding all parameters shown in Figure 2.

Authors reply: The change in the trend direction of both SD and DTR was detected using the piecewise linear regression model proposed by Muggeo (2003), as discussed in the previous round of replies to the referees. For SD, the change was detected in 1982 with an uncertainty of 4 years, with $p = 0.008$ and for DTR, the change was detected in 1979 with an uncertainty of 4 years ($p = 0.017$). The authors apologize for the lack of clarity and for the incomplete information in the previous reply. The applied model to detect trend changes and respective reference were included in the methodology, at lines 144 to 145: “We also applied a piecewise linear regression model, proposed by Muggeo (2003) to detect any trend changes”. The results of the application were included in the revised version of the manuscript, in many parts, as in lines 223 to 228; 231; 237 to 238; 243 to 248; 262 to 264; 382 to 388, including in the Abstract, lines 29 to 34 and Conclusions, lines 430 to 440.

Secondly, I would recommend to the authors to use a piece-wise linear regression model when they detect a significant change, instead of studying the two trends independently. This way the authors will be able to discuss the trend in e.g. the first period with respect to the trend in the second period. Else, the discussion may be misleading. For example, the discussion about the results for SSR in Figure 2 is misleading. Based on what is discussed in the document (especially in the introduction), the reader would expect that the SSR was decreasing by 0.4% per decade in 1961 – 1983, and then by 0.39% per decade in 1983 – 2016. Thus, an overall decrease of ~2.2% for the whole period (1961 - 2016). This is not of course truth since the SSR is relatively stable during 1961 – 2016.

Authors reply: As mentioned in the previous comment, a piecewise model was in fact used. And yes, the authors agree with the referee comment about the misleading interpretation given by such numbers, what reinforces the importance of analysing the longest timeseries possible in this kind of study.

Third, I strongly recommend adding some discussion about the average change throughout the whole period, at least for the parameters for which the regime of the trend is not changing significantly (e.g. SSR).

Authors reply: The authors agree with the recommendation and included the trends for the whole period for all the analysed variables. The tables 1 (starting in line 249) and 2 (390) incorporate the results.

In any case, and even if the authors decide not to follow my advice regarding the methodology of the analysis, they should discuss in a much clearer way what has happened in the period of study, and in which cases the discussed results are significant.

I also recommend that the following corrections should be applied.

L23: Replace “up to” with “and” – **replaced.**

L25: Delete “of”. Furthermore, the significance level is 89.9%. The p-value is 0.101. – **This part of the text was deleted.**

L26-29: “A similar ... 0.013)”. Please re-write this sentence because it is not clear.

Authors reply: In this revised version, the text was rewritten to: “Sunshine duration and the diurnal temperature range also presented negative trends, in opposition to the positive trend observed in the cloud cover fraction”.

L39: “each possible cause” instead of “each possible causes” – **The text was corrected following referee suggestion in line 38.**

L132: “were monitored since” instead of “started to be monitored in” – **The text was changed in the line 131 of the present version of the manuscript.**