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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the Suitability of Current Atmospheric Reanalyses for Regional Warming Studies over China" *by* Chunlüe Zhou et al.

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Reviewer #2 General Comment: The study attempts to assess the value of various global reanalysis products over the Chinese domain by comparison to a homogenized set of station data. The overall approach is logical. The findings with regards to which reanalyses products are high quality are in line with existing understanding. Some effort to understand the potential thermodynamic and boundary condition causes of differences are interesting and novel although could be presented much more simply. It is clear that a huge amount of effort has been undertaken to access and analyse a wealth of data. As such, the authors are to be commended on a substantive body of

work. However, I have some concerns around aspects of the analysis and presentation. The work may be publishable in ACP following revisions if they satisfactorily address my concerns.

Response: Thanks for your effort to evaluate our submission and high recommendation. Below please find our point to point response to your comments.

Specific Comments: 1) Comment: Treatment of observations The main issue with the analysis is the treatment of a single homogenized series of temperature observations as constituting a 'truth' against which it is possible to make definitive resulting assessments of the reanalyses products. In reality no single approach to homogenization of observations can ever yield a perfect reconstruction of the true evolution of the observed variable. Therefore the observations even after homogenization cannot be treated as a demonstrable truth against which definitive statements of reanalysis quality can be made. In cases where offsets between the observations and reanalysis are substantive it is relatively simple to diagnose that there must be an issue in the given reanalysis product as, although imperfect, the uncertainty in the observations can be reasonably bounded. However, many of the differences between candidate reanalysis products and the homogenized reanalyses instead fall into the grey zone whereby the difference is smaller than, or of comparable magnitude to, the potential residual uncertainty in the homogenized series. The authors could address this point by collecting the substantive family of homogenized temperature station series that have been created over China over the past decade or so and comparing the full family of homogenized series to the full family of reanalyses products. This would serve to substantially strengthen their overall analysis under the assumption that the family of homogenized products and the family of reanalysis products both consist of random draws from the parent distributions of possible homogenized / reanalyzed series. Without undertaking such a step, although the work may just about be publishable, its utility will be substantively compromised. Similarly, the observations of the studied covariates (cloudiness, rainfall, radiation etc.) must be uncertain. Again, when differences are substantive

inferences can be made without issue. It is when distinctions between the reanalyzed and observed fields are small that interpretation becomes difficult. In such cases the remaining uncertainties in the observations of the covariates limits what inferences can be made.

Response: Thanks for your comments. We agree with the reviewer on the usage of reference dataset. Single homogenized series is imperfect. We are keeping cooperating with several Chinese groups, each of which conducts homogenization of surface air temperature data. These datasets have different spatial and temporal coverage and are not freely exchanged, we therefore did not include the homogeneous datasets in this study. In the revised paper, we discussed the impact of the uncertainty of the observed data. The Student's t-test was further conducted to difference between reanalysis and homogenized series for considering both uncertainties at the significance level of 0.05. This information was added in the lines 292-294. Furthermore, only using the homogeneous time series (not including adjusted time series) at the significance level of 0.05, it can show almost the same results as those from all the time series in the revised paper (see Fig. 5).

2) Comment: Clarity of analysis In many places the text is hard to follow. This mainly arises through choices as to how to structure the sections and individual paragraphs and this makes it hard as a reader to follow the logical arguments being made by the authors. The abstract, in particular, is hard to follow as submitted. Efforts at restructuring to make more clearly the arguments the authors wish to put forwards would increase the value of the piece. The methods section isn't entirely clear and in some places it is questionable whether sufficient detail is given to allow replicability. In particular the set of seven equations is given without sufficiently clear justification and without detail as to whether these are applied gridpoint-wise, smoothed etc. In the results, the continual listing of regions and reanalyses in different contexts is confusing and hard for a reader to unpick. Greater use of figures and / or tables may serve to improve the messaging aspects here. I find myself trying to connect 12 sets of dots to get a feeling

how each reanalysis performs in each aspect in each region and then compare all the joined dots in my head but the problem gets way too big to do so very quickly. The authors have done a huge amount of analysis but the choice of primarily describing in text without tabular and / or visual ways of summarizing the interconnectedness arguments being made is an impediment to reader understanding. I find the results, discussion and conclusion sections to be substantively overlapping. These sections would benefit from substantive redrafting and reordering. The results should outline what is found. The discussion should highlight the principal findings and implications. The conclusion should be at most 2-3 paragraphs of key take away messages. Presently the results and discussion feel repetitive and the current conclusions feel to me more like a discussion. Finally, the text would benefit from substantial input from a native English speaker if available. I am always in two minds over such a comment because I am acutely aware I could write to nothing like the standard in any other language. The authors therefore have my greatest respect for not only undertaking the science but writing it in a second language. But, equally, if the authors wish to have impact they would be served by careful input from a native speaker and it would be remiss of me not to suggest this.

Response: Thanks for your detailed comments. Following your constructive suggestions, we have carefully checked the revised paper and made the logic of Abstract concise and the logic of the revised paper smooth. Especially, we re-edited the Sections Discussion and Conclusions to make them non-overlapping. We added more details in the Section Method 2.4 to make it easier to follow: To further investigate the relationship of spatial distributions of the Ta trend biases with the relevant parameters among the twelve reanalysis products, the weighted total least square (WTLS) was adopted, in which the spatial standard deviations and correlations of both variables at $1^\circ \times 1^\circ$ grids were included (Reed, 1989;York et al., 2004;Golub and Van Loan, 1980;Hyk and Stojek, 2013;Tellinghuisen, 2010) In the Monte Carlo method, the grid index is generated as random number, i.e., the 1-691 grid index for the $1^\circ \times 1^\circ$ grids over China, based on which we could sample the spatial pattern in trends biases in Ta, Rs, Ld and precip-

itation frequency. We re-plotted the Fig. 3 to be clearer for reader, especially using different markers for both grouped reanalyses. Again, following Comment #1 and your suggestions, we have sent out the revised paper for Professional English editing and we have carefully made some language editing in the revised paper.

3) Comment: Figure suggestions For the reader it is important that you explicitly define the regions. I would add a new Figure 1 consisting of a map of China in which the different regions are clearly demarcated. The regions are listed in the caption of Figure 1 but there is nothing I can see in Figure 1 which actually denotes this. Almost all figures use a rainbox colour scale which is inaccessible to those who are colour-blind, which is a not inconsiderable proportion of the population. Numerous colour-blind friendly colour schema are available and consideration should be made as to their use to improve accessability. I find many figures hard to understand. The authors are trying to pack a lot of information into these and in many cases because they are postage stamps this is hard to see and interpret. I find Figure 8 particularly difficult and, if I am honest, even after spending 10 minutes trying to understand it suspect that I do not. If you make the reader work this hard they will give up and move on. In general work on making the figures more intuitive and accessible would help enormously.

Response: Thanks for your constructive comments. Please see the region division in the Fig. 1c and its corresponding figure caption. We have made much efforts to try but fail to adopt various colorbars for three variables (RGB composite) instead of rainbox colourbars. We also found quite a few literatures used the same rainbox colorbars to plot three variables, e.g., published in Science (Nemani et al., 2003) and Nature (Seddon et al., 2016). We re-plotted the Fig. 3 to be clearer for reader, especially using different markers for both grouped reanalyses. We re-wrote all the relevant figure captions including the caption of Figure 8 to be concise for easy getting main information.

Reference: Nemani R R, Keeling C D, Hashimoto H, et al. Climate-driven increases in global terrestrial net primary production from 1982 to 1999[J]. Science, 2003, 300(5625): 1560-1563. Seddon A W R, Macias-Fauria M, Long P R, et al. Sensi-

tivity of global terrestrial ecosystems to climate variability[J]. Nature, 2016, 531(7593): 229.

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