

Review of “Comparisons of Arctic upper-air temperatures from radiosonde and radio occultation observations” by Chang et al., submitted to AMTD

General comments:

I have already served as a reviewer of this paper for a different journal where I, during three rounds of review pointed out various, in parts major issues with the paper.

Unfortunately the version of the paper published here does not include some of the major comments I already provided for the other journal. This makes me question if the authors actually value the hours of time reviewers are spending to provide feedback on their work. Here I am providing another review of this paper, but I marked all the parts I could copy from my last review in red.

Unfortunately this paper is not very well written and in parts hard to read. In addition some sentences can be misunderstood, which is very undesirable for a scientific paper.

I also realized that some sections are missing/deleted. The introduction states that section 2 will describe a comparison with the GCOS Reference Upper-Air Network, but this comparison is not in the paper. I do know that this comparison was done in earlier versions of the paper, but the authors seem to have taken it out without adapting the introduction and conclusion. I don't think enough care is taken in during the preparation of this paper.

After recognising that Fig.1 has issues pointed out in the specific comments I stopped reviewing this paper. Although I had pointed out to the authors that something is wrong with Fig.1 during a review of the same paper in a different journal, the issues are still not fixed. I have lost the confidence that the authors provide a carefully investigated study here.

While the final decision should be up to the editor, I would advise to reject this paper due to a wrong figure, poor writing and because this paper is not providing outstanding new findings.

Specific comments:

The authors point out many times that they aim to understand the performance of both radiosonde and radio occultation measurements/retrievals in describing the Arctic upper-air temperature. I don't understand how this is possible. How can you make a statement about the performance of two instruments when comparing them given that non of them is used as a reference.

Another point is, that in general, lower atmospheric RO temperature (wet temperature) profiles contain a priori information from a model or from a climatology like ERA Interim. In order to get a temperature in lower Arctic levels, an estimate of the Arctic humidity will be used in the retrieval and this humidity estimate will be not always be correct (and depend on the reanalysis/model used). I am not sure how much is really gained when using the RO wet temperatures compared to using the reanalysis itself. Also the radiosondes are included in this reanalysis, thus being included in the a priori for the RO retrieval.

When having a close look at the figure 1, I realised that the positions of the GRUAN and radiosonde stations do not agree with the 5 by 5 degree grid that you describe about in the text.

- **Figure 1: Is the blue dot in the middle the pole? Is that 90degrees? In figure 9 you make this inner circle white as it is only the pole, so why not doing this here?**
- **But anyway there is a bigger issue with this figure. There should be 5degree by 5degree grids. So the most inner circle (after the blue dot that I think should be the pole) would go from 90-85, the second from 85-80, then third 80-75, forth 75-70 and fifth 70-65. But comparing this expectation with the position of three sites shows me that something is not**

correct. The bins in the following description are counted from inside (but the most inner circle is ignored as I think its the pole, if its not the pole, this would not explain the positions either.)

- Sodankyla (WMO station ID 02836) 67.37° , 26.63° at the middle of the fifth bin (I am not sure which of the two possible dots should be Sodankyla, but both of them are at around 70N)
- Ny-Ålesund (01004) 78.92° , 11.92° should be at the inner end of the third bin. This is not the case. I don't know why this is not the case, but e.g. NYA actually appears in the second bin.
- Barrow (70026) 71.28° , -156.78° should be at the outer end of the fourth bin, but is at the beginning of the fourth bin

Unfortunately this major issue was pointed out by myself during the review of this paper for another journal. Still the authors have not fixed it, or clarified (in case I did misunderstood something here). At this stage I decide to finish the review, as I lost the confidence in the carefulness of the authors. I am not sure if the text is simply incorrect or if the figure is wrong, though I have the feeling that in general this work is not done with the needed care to produce high-quality scientific publications.

technical corrections

In my opinion this paper is not well written and I will only point this out in parts.

page 1, line 12: “introduced”, I would probably write “used” instead

page 1, line 15: “matched” I would probably use the word “agreed” instead

page 1, line 20: “widely covered”? I know you mean that they have a good spatial resolution, but I wouldn't say it like this

page 1, line 27-28: this sentence is not nice. I don't like “traditional tool” and also the radiosonde itself does not have a poor temporal resolution (it measures data every e.g. every second). What you mean is that the RS time series only has one/two profiles every day.

This comment might seem fussy, but this is a scientific paper which is going through peer-review and should be as correct as possible afterwards.

Page 2, line 9: Please rewrite. “The RO techniques is based on” ... and also the signal propagates from the GNSS satellite to the LEO satellite (and never the other way around, which I think between implies).

Page 2, line 13-14: This does not only count in the lower troposphere, but at all levels where humidity is not negligible. Please make this clear.

Page 2, line 15-16: This is not correct. You can derive dry temperatures throughout the whole atmosphere, though in the lower levels they will not agree with the physical temperature as they attribute the influence of the water vapour to the temperature. I would suggest changing the whole paragraph from line 13 onwards to actually clarify what dry and wet temperatures are and where they are valid.

Page 2, line 19: You are saying you compare the RO wet temperatures with the GCOS Reference Upper-Air Network in section 2, but indeed you have cut the section about this comparison out of the paper in this version prepared for AMTD. You mention it again in the conclusion though. I am not sure how that can happen, but it makes me wonder if enough care was taken when preparing this publication.

page 2, line 21: “were used to estimate” are you estimating the temperatures? I thought the CDACC product provides the temperatures? As far as I understand you estimate temperature anomalies.

Page 2, line 24-25: I don't understand how the performance of both measurement systems can be understood. If you are not using any of them as a reference, what can you say about the performance?

Page 14, line 24: “Additionally, the temperature profiles from RS and COSMIC observations were

compared to understand their performance in describing the Arctic atmospheric temperature.” again I don't think you compare the performance, how do you know about the performance? what is your reference? I think you actually compare the the COSMIC with the RS observations.

Page 3, line 4-6: Too long sentence, break up in two.

Page 3, line 8: why only lower troposphere?

Page 3, line 12-13: the abbreviation fr CDACC is wrong. It is the COSMIC Data Analysis and Archive Center

Page 3, line 22-26: This is not essential for the study. Please shorten.

Page 3, line 28-33: I think this is not needed. Just state how many profiles you can use, or just the number per grid might be sufficient.