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Interactive comment on "Comparison of the Arctic upper-air temperatures from radiosonde and radio occultation observations" *by* Liang Chang et al.

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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. Answer: Thank you very much again for agreeing to review the manuscript after previous three rounds of review. We highly appreciate the comments from the Reviewers, and value the spent

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time from the Reviewers on our manuscript. In fact, the previous three rounds of review (especially the comments from the third round) depressed me so much. However, my co-authors and I will never give up and will make our effort to improve the manuscript. Here on behalf of all co-authors, I am continuing to response the comment from the Review, please see details below. 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. Answer: Sorry for the carelessness! The comparisons of COSMIC and RS temperature with GCOS observations were deleted in the manuscript. Because only three GCOS sites are available in the Arctic, it may be insufficient to evaluate the performance of COSMIC and RS observations in the Arctic. 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. Answer: After going over the results in Fig. 1, we realized that the results were illustrated incorrectly due to the mapping software. In the original manuscript, we got the results in Fig.1 with Matlab software first and then generated the Fig. 1 with the Generic Mapping Tools (GMT) software. However, the results from GMT software showed something strange and we do not know how to solve it. Therefore, we regenerate the Figs. 1, 7 and 8 with Matlab software in the revised manuscript, and the shifts of RS position were fixed. 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. Answer: We thank the Reviewer for giving the direct suggestion for the Editor. We admit that many works have been done on comparing the temperature measurements between radiosonde (RS) and COSMIC observations on global scale. However, the Reviewer is invited to pay attention to the

explanations of Arctic sea ice extents in 2007 and 2012 with COSMIC observations in the manuscript. As the number of observations in the Arctic is an important issue for the Arctic climate change research, it is demonstrated in this manuscript that the COSMIC data can be a complementary source of information in understanding the Arctic upper-air temperature variations and related climate change. Specific comments: The authors point out many times that they aim to understand the performance of both radiosonde and radio occultation measurements/retrievals in 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. Answer: The eventual purpose of the manuscript is to demonstrate the possibility of COSMIC data in reveal the Arctic climate change, rather than the comparison between COSMIC and RS temperature. In the revised manuscript, the COSMIC and RS temperature measurements were compared, and results show large temperature discrepancies between RS and COSMIC observations were observed at the lower and upper levels, while a minimum RMS of 1.51 °C was detected at 400 hPa. In addition, considering the operational RS observations for climate monitoring were strongly hindered by numerous and poorly documented changes in instrumentation and operational procedures, the homogenized RS data were used for further analysis. 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. Answer: As analyzed in Section 4 of the original manuscript, seasonal temperature anomalies from RS and COSMIC observations at 400 hPa during 2007 and 2012 were used to explain the Arctic sea ice extents in 2007 and 2012. At the layer of 400 hPa, the temperature measurements from reanalysis and RO data are

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different from each other. 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. âĂć 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? âĂć 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.) âĂć 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) âĂć 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. âĂć Barrow (70026) 71.28°, -156.78° should be at the outer end of the forth 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. Answer: As described above, the analysis during the data processing in the manuscript are right. The discouraging performance in Fig. 1 was mainly due to the mapping software. After the results were regenerated with Matlab rather than GMT software in the revision, the above problems were solved. We are sorry for the careless descriptions in the original manuscript. 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. Answer: We appreciate the Reviewer for his/her carefulness, but I think some of the descriptions in the original manuscript is also suitable (e.g., ‘introduced’). Despite I am not fully agree with the Reviewer’s comments in English written, I revised above 4 comments in the revised manuscript. 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). Answer: We have rewritten the sentence, i.e., “The RO technique was based on the time delays of the radio signal propagating from the GNSS satellite (e.g., Global Positioning System (GPS)) to the receiver placed on a low earth orbit (LEO) satellite (Kursinski et al., 1997). The radio signal was bent by the atmosphere, and the bending angles of the RO signal are derived from the propagation time, which can be precisely measured with atomic clocks.” 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. Answer: We have rewritten the sentences. The description on ‘lower troposphere’ was deleted, and the ‘a priori information’ was added. 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. Answer: Sorry for the carelessness! The comparisons of COSMIC and RS temperature with GCOS observations were deleted in the manuscript. Because only three GCOS sites are available in the Arctic, it may be insufficient to evaluate the performance of COSMIC and RS observations in the Arctic. 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. Answer: We have corrected the description and similar expressions in the revised manuscript. 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 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. Answer to the above two comments: In the revised manuscript, we have corrected the expressions. The comparison of COSMIC and RS temperature was implemented in the original manuscript to estimate their agreement, and what we are more concerned about is the possibility of COSMIC data in reveal the Arctic climate change. In the revised manuscript, considering the operational RS observations for climate monitoring were strongly hindered by numerous and poorly documented changes in instrumentation and operational procedures, the homogenized RS data were used for further analysis.

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 Answer to the above three comments: We have made the corrections in the revision. Page 3, line 22-26: This is not essential for the study. Please shorten. Answer: We have deleted the sentence. 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. Answer: We have deleted the sentence.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/amt-2016-232/amt-2016-232-AC5-supplement.zip

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