Anonymous Referee #1
Received and published: 12 October 2015

1 General comments

This paper investigates atmospheric variability over a specified region of South Asia, using radiosonde, GPS RO COSMIC, and MERRA reanalysis data. Different radiosonde types used in this region are compared to COSMIC. ENSO, QBO, and IOD modes and their correlation with temperature and tropopause variability are discussed.

We would like to thank the reviewer for thoroughly reviewing the manuscript and for providing very useful comments and suggestions. We have carefully considered each of the comments in our revision. Our responses are provided below:

Major points:

1. While there is certainly quite some work included in this paper, an in-depth discussion of its results is missing in large parts of the manuscript. In my opinion, the research question behind this work should be defined much more explicitly. It should be discussed in more detail what we can learn from the results.
Thanks for the comments. The primary objective of this work is to assess the interannual variability of UTLS temperature over the GBM river basin in South Asia (see, lines 131-135). We agree with the reviewer that a more in-depth discussion is required to quantify the impact of ENSO/IOD and QBO on the variability of UTLS temperature. In this regard, to emphasize more on the interannual variability, we have summarized the comparison study in the supplementary material. We have also modified the introduction and clearly defined the objectives.

2. Comparison studies of radiosondes versus GPS RO, including the types of radiosondes used in this work, have been done in the past, and are partly included as references in the manuscript. I do not see significant new results in this comparison. While I think that it is important to monitor the quality of radiosondes especially in this region with known deficiencies in the quality of some of the deployed radiosonde types- I do not see the relevance of this comparison with regard to the topic of the paper, the variability of the UTLS.
We agree with the reviewer that there are some studies which compare radiosondes and GPS RO in the region that have been included in the references. Our motivation for including such comparison in this study was to assess the updated version of radiosonde observations especially over India, the results which are worth highlighting. Nonetheless, we have summarized the comparison study and moved them to the supplementary material to create space for an in-depth discussion of the results.

In summary, I cannot recommend publication of this work before a major revision of some key aspects. The results should be interpreted and discussed in more detail; the research questions should be stated more clearly and the conclusions should discuss these; new literature should be referenced; the section about the comparison of radiosondes should be either removed or its relevance to the research question should be justified; and the information contained in the figures should be discussed in more detail.

In the light of major concerns from the reviewer, we have performed a major revision focusing entirely on the interannual variability of temperature in the UTLS region. Most of the concerns raised by the reviewer have now been addressed in this updated version.

2 Specific comments

• Page 9403, line 27–29: “The use of retrieved parameters such as refractivity, temperature, and water vapour from COSMIC RO data accounts for the overall error budget of the GNSS RO technique.” I do not understand this sentence. Could you please elaborate, or remove the sentence. Thanks for

pointing this out. Refractivity, temperature and water vapour profiles retrieved from GNSS RO are considered to be end products of RO process that inherits various errors during the RO process such as random, systematic and sampling errors, which may be termed as a total error. The sentence has been removed.

• Page 9405, line 8: CDAAC maintains data from many, or most, RO missions, but not "all". Thanks for the correction, which has been updated in line 177.

• Page 9405, line 18–19: "dry Profiles" is the term for atmospheric parameters derived from refractivity and neglecting the humidity term (e.g. "dry temperature"). The refractivity itself is not a "dry Profile". CDAAC also provides a modified refractivity resulting from their 1D-Var analysis, in addition to their "wet profiles". Please correct the sentence accordingly. Thanks for clarifying this. We have modified the sentences in lines 189-200.

• Page 9406, line 5–6 and Figure 2a: I am a bit surprised about the timeline of COSMIC profiles as shown in your Fig. 2a. The onset/early times look OK, but the number of occultations should go down around 2010 due to an increasing number of problems with some COSMIC satellites. This can be seen e.g. in Fig. 1 in this review, which I have created using the online tool on the CDAAC web page. Do you have any explanation why your Fig. 2a is not showing this? Please check Fig. 2a of the manuscript. We thank the reviewer immensely for noting this error. While we have filtered the RO at three different stages, it is still surprising to see that such errors have occurred. To identify this error, we have downloaded the data again to do the analysis as we could not identify the error. It should be also mentioned that we have slightly extended the study area (see Figure 1) to the south (by 4 degrees) to help identify the data issues. Figure 2 is now updated.

• Page 9407, line 6–7: I am not sure what "Therefore" relates to. Sentence removed.

• Page 9408, line 16–18: I do not understand the meaning of this sentence: "While radiosondes are sparse in the GBM basin (see, Fig. 1), the MERRA reanalyses products serve as complementary data source in the lower atmosphere in addition to the existing reanalyses products (e.g., ERA-Interim)." Sentence removed.

• Page 9410, line 13–16: "The COSMIC RO data must be gridded for each month over the region to provide a uniform spatial pattern and to provide a fair comparison to MERRA data. The monthly gridded temperature data of COSMIC were averaged for the two regions: (a) UT (400–150 hPa or _ 7.5–14.2 km) and (b) LS (70–30 hPa or 18.7–24.0 km). The mean temperature data for the two regions were then interpolated to a spatial resolution of 0.5 x 0.5 using the "ordinary kriging" method . . . " I am not sure what the "monthly gridded temperature data of COSMIC" is. I suppose that in fact the single profiles are averaged to UT and LS, and then these averages are interpolated? Please clarify. We agree with the reviewer that the temperature profiles from COSMIC RO were first averaged to UT and LS before interpolating to a monthly grid initially. However, we found that this averaging method makes significant difference while computing correlation with atmospheric-ocean indices. Therefore, we plotted them separately for each level and not average it. See Figure 5 and 7 in our revised version.

• Page 9412, line 4–5: I recommend updating the list of references for the comparison of radiosondes with RO. I would consider Ho et al. (2009) and Hajj et al. (2004) of minor relevance here (and recommend removing them here), and recommend adding a few other relevant references instead: Kuo et al. (2005); He et al. (2009); Ho et al. (2010); Sun et al. (2013); Ladstädter et al. (2015). Thanks for providing the above relevant citations. We have included some of them in the relevant sections (see, e.g., line 183).

• Page 9412, Sec. 4.1: Is there any previous work concerning the quality of the updated IMD sondes? I found at least Kumar et al. (2011) and Ansari et al. (2015) which should be referenced. We have included Sun et al (2010) in our manuscript. Thanks for providing the two important references. We have also included Gupta et al. (2005) in addition to Kumar et al. (2011) and Ansari et al. (2015). See line 48.

• Page 9413–9414, Sec. 4.1.2 and Figure 6: I suggest showing the humidity differences in percent for easier interpretation. You also mention percentages in the text (e.g. "of up to 30 %"). Thanks for pointing this out. However, we maintain as it is to highlight their absolute differences. Our focus is now on the interannual variability.

• Page 9414, line 22–23: ". . . ShangM sonde was also better than those from India and Bangladesh." I would not say from Figure 7 that ShangM is better than the RS92 or "unknown" sondes from

Bangladesh. Thanks for the observation. While it may not be that ShangM is relatively better than RS92 and those from Bangladesh over the whole altitude range, it certainly showed better results above 150 hPa (mean) and 250 hPa (standard deviation).

• Page 9415, line 1–3: I am not sure what "Thus" relates to. I can see that the refractivity errors are related to the warm bias in Fig. 5a, but where does the conclusion that these errors might be related to errors in pressure measurements come from? Thanks for pointing this out. "Thus" may be irrelevant here as well as those comments about the relation between refractivity error and pressure measurement. However, we do believe that refractivity error may be related to pressure measurement as it is a component in equation (1). See Page 9407 in our previous version.

• Page 9415, Sec. 4.2: The section contains comparisons with radiosondes, this should be reflected in the section title. Thanks for the observation. This section is summarized and taken to the supplementary material.

• Page 9415, line 10: "The year 2012 was adopted arbitrarily in order to confirm the statistical results of radiosondes in the region, and thus, radiosonde observations were also included in the analysis." I am not sure what "thus" relates to. I do not understand the sentence. Thanks for the comment. The sentence was included in order to justify the reasons for choosing that particular year, i.e., 2012. This section has now been removed.

• Page 9415, line 17–24, Figure 8: Figure 8 is hard to read; is there any specific reason why you chose a skew-T/log-P type of plot? I recommend simply plotting the differences similar to Fig. 5. The figure has now been excluded.

• Page 9416, line 4–8: As far as I understand you use ERA-I and GFS data interpolated to the position of the GPS RO profiles. The radiosonde profiles on the other hand are collocated to the RO profiles with the given collocation criteria. It should be noted that this difference will induce an additional error for the RO/RS comparison because of imperfect collocations. Thanks for pointing this out. We acknowledge the reviewer's concern. This section has been removed.

• Page 9416, line 13–16: "The strong seasonality gradually diminishes and became nearly constant in the tropopause layer between 150 and 70 hPa (14–18.5 km) indicating a much wider tropopause belt in the region. This supports the view that tropopause is no more a thin layer" What do you mean by "much wider" (wider than what?), and by "no more a thin layer" (has that changed?). I suppose you want to point to the layer-like characteristics of the tropical tropopause layer. Please rewrite these sentences to clarify. We have removed the sentence.

• Page 9416, line 21–24: In this context, Foelsche et al. (2008) seems to be of minor relevance and I recommend removing this reference here. Removed.

• Page 9417, Figure 9c: Please state clearly in the plot and/or caption that this plot shows MERRA-COSMIC. We did not show the differences in our revised, see also Figures 4 and 6.

• Page 9417, line 1–7, and Figure 9: I am surprised by the large and systematic differences between MERRA and COSMIC, and I would strongly recommend discussing these differences in much greater detail in the paper. I am not convinced that these differences are the result from "low resolution background" as stated in the manuscript. The differences are in the order of the difference between physical temperature/dry temperature at these latitudes, and the 1D-Var should only improve things (resulting in smaller differences). An easy way to check this would be to simply plot dry temperature instead of physical temperature in Fig. 9c. If the differences look similar, at least in upper troposphere, then they are not caused by the background. You could also use additional datasets (e.g. ERA-Interim or ECMWF analyses) to see where these differences come from. I would be surprised if they stem from COSMIC. Could it be that your method of calculating these climatologies introduces such effects? Further investigation and discussion are certainly needed. Thanks for pointing this out and also providing valuable suggestions. We have investigated this large difference with respective to dry temperature profiles and ERA-Interim and found that both datasets showed similar patterns that were found to be consistent over time. Thus, the relative differences were of the order ±0.5°C in the UTLS region. We have clarified these in lines 339-345.

• Page 9418, line 25–26: I think it should be: "indicating a decrease in temperature during the warm ENSO phase". Thanks for the observation. We have corrected it (see e.g., lines 390-391).

• Page 9418, line 23–29: The negative correlation of tropospheric temperature and ENSO deserves some comments and references. This comment is in coherent with Reviewer 2. We have discussed it in Lines 380-406

• Page 9419, line 5: What do you mean by "relationship between IOD and UTLS"? It is mean to be relationship between DMI (i.e., the IOD index) and UTLS temperature". We have amended the language where necessary.
• Page 9419, line 11–13: What do mean by a correlation of "0.5 to -0.5"? Why is this correlation coefficient not listed in Table 4? The correlation between QBO with average temperature of UT and LS was not very clear as it followed a cyclic pattern due to the QBO pattern. In the revised version the correlation coefficient between QBO and UTLS temperature was computed for four pressure levels and are shown in Table 1 together with ENSO and IOD.
• Page 9419–Page 9420, Figures 12 and 13: These figures are hardly discussed in the manuscript. What about the semi-annual variations, what do we learn from them? What are the related uncertainties? Are MERRA and COSMIC able to resolve an amplitude of 150 m? I am not sure what information we gain from these two plots, and I think the reason to include them should be discussed in more detail. Thanks for the comments. Figure 12 and 13 intends to show the annual and semi-annual variations, which in a way only show the large-scale variations. The semi-annual variations were negligible and were also quite similar to the annual patterns. Thus, we have plotted the time mean of temperature (and height) in Figure 8 (and Figure 9). Their uncertainties were not displayed and were found to be ±0.2°C and 0.01 km.
• Page 9420, line 28–Page 9421, line 2: Indicate which of the two datasets has warmer and lower tropopauses in the text. The differences between MERRA and COSMIC are surprisingly large—this should be discussed in more detail, and more (and newer!) references should be stated. Thanks for the suggestion. Indeed the differences are quite large between MERRA and COSMIC and we agree that there needs to be more discussion about it. We have discussed this issue better in our revised manuscript. See Lines 470-480.

3 Technical corrections

The minor corrections below have been addressed in the revised manuscript.
• Page 9400, line 6: "has overcome" instead of "have overcome". Done
• Page 9400, line 16: You use "interannual" everywhere else in the document. Corrected as "interannual".
• Page 9401, line 13: "Much of these temperature changes" instead of "Much of this temperature changes". Sentence removed.
• Page 9401, line 15: What is meant by "recent amplification of global and regional vertical structure of the troposphere and the lower stratosphere"? Please rephrase. Modified.
• Page 9402, line 7: "The primary observables are" instead of "The primary observable are". Done
• Page 9402, line 18: "The number of RO profiles has" instead of "The number of RO profiles have". Done
• Page 9404, line 4–5: Change the last sentence to present tense. Maintained as it is.
• Page 9406, line 26–28: Sentence has several grammatical issues. Modified.
• Page 9409, line 1–2: Remove the superfluous "climate". Done
• Page 9410, line 9: "calculated" instead of "carried out" Done
• Page 9411, equation 4: Both l and t are used as the temporal index. Removed.
• Page 9414, line 3: "which also contains radiosonde observations and is hence not fully independent." instead of "which also contains radiosonde observations and hence not fully independent." Done.
• Page 9414, line 13: I think it is "negligible" and not "negligent". Corrected as "negligible".
• Page 9417, line 21: Please update the link. Done
• Page 9417, line 22: Please introduce the acronym. Done
• Page 9418, line 10–11 "+ive" and "-ive": Please use "positive" and "negative". Done
• Page 9420, line 4: "were" instead of "wwere". Removed.

References

Thanks for providing relevant references, of which we have included the following:

M. I. Ansari, R. Madan, and S. Bhatia. Verification of quality of GPS based radiosonde data. MAUSAM, 66(3):367–374, 2015.

U. Foelsche, M. Borsche, A. K. Steiner, A. Gobiet, B. Pirscher, G. Kirchengast, J. Wickert, and T. Schmidt. Observing upper troposphere-lower stratosphere climate with radio occultation data from the CHAMP satellite. Climate Dyn., 31:49–65, 2008. doi: 10.1007/s00382-007-0337-7.

G. A. Hajj, C. O. Ao, B. A. Iijima, D. Kuang, E. R. Kursinski, A. J. Mannucci, T. K. Meehan, L. J. Romans, M. de la Torre Juarez, and T. P. Yunck. CHAMP and SAC-C atmospheric occultation results and intercomparisons. J. Geophys. Res., 109:D06109, 2004. doi: 10. 1029/2003JD003909.

G. Kumar, R. Madan, K. C. Saikrishnan, S. K. Kundu, and P. K. Jain. Technical and operational characteristics of GPS radiosounding system in the upper air network of IMD. MAUSAM, 62 (3):403–416, 2011.

Y.-H. Kuo, W. S. Schreiner, J. Wang, D. L. Rossiter, and Y. Zhang. Comparison of GPS radio occultation soundings with radiosondes. Geophys. Res. Lett., 32:L05817, 2005. doi: 10. 1029/2004GL021443.

B. Sun, A. Reale, S. Schroeder, D. J. Seidel, and B. Ballish. Toward improved corrections for radiation-induced biases in radiosonde temperature observations. J. Geophys. Res., 118: 4231–4243, 2013. doi: 10.1002/jgrd.50369.