

Interactive
Comment

Interactive comment on “Interannual variability of upper tropospheric and lower stratospheric (UTLS) region over Ganges–Brahmaputra–Meghna basin based on COSMIC GNSS RO data” by Khandu et al.

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.

Major points:

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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.
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.

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.

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.

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- Page 9405, line 8: CDAAC maintains data from many, or most, RO missions, but not “all”.
- 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.
- 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.
- Page 9407, line 6–7: I am not sure what “Therefore” relates to.
- 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).”
- 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 \approx 7.5–14.2 km) and (b) LS (70–30 hPa or \approx 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.
- 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).
 - 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.
 - 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 %”).
 - 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.
 - 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?
 - Page 9415, Sec. 4.2: The section contains comparisons with radiosondes, this should be reflected in the section title.
 - 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

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were also included in the analysis.”

I am not sure what “thus” relates to. I do not understand the sentence.

- 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.
- 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.
- 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.

- 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.
- Page 9417, Figure 9c: Please state clearly in the plot and/or caption that this plot shows MERRA–COSMIC.
- 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

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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.

- Page 9418, line 25–26: I think it should be: “indicating a decrease in temperature during the warm ENSO phase”.
- Page 9418, line 23–29: The negative correlation of tropospheric temperature and ENSO deserves some comments and references.
- Page 9419, line 5: What do you mean by “relationship between IOD and UTLS”?
- 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?
- 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.
- 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

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COSMIC are surprisingly large—this should be discussed in more detail, and more (and newer!) references should be stated.

3 Technical corrections

- Page 9400, line 6: “has overcome” instead of “have overcome”.
- Page 9400, line 16: You use “interannual” everywhere else in the document.
- Page 9401, line 13: “Much of these temperature changes” instead of “Much of this temperature changes”.
- 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.
- Page 9402, line 7: “The primary observables are” instead of “The primary observable are”.
- Page 9402, line 18: “The number of RO profiles has” instead of “The number of RO profiles have”.
- Page 9404, line 4–5: Change the last sentence to present tense.
- Page 9406, line 26–28: Sentence has several grammatical issues.
- Page 9409, line 1–2: Remove the superfluous “climate”.
- Page 9410, line 9: “calculated” instead of “carried out”
- Page 9411, equation 4: Both l and t are used as the temporal index.

- 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.”
- Page 9414, line 13: I think it is “negligible” and not “negligent”.
- Page 9417, line 21: Please update the link.
- Page 9417, line 22: Please introduce the acronym.
- Page 9418, line 10–11 “+ive” and “-ive”: Please use “positive” and “negative”.
- Page 9420, line 4: “were” instead of “wwere”.

References

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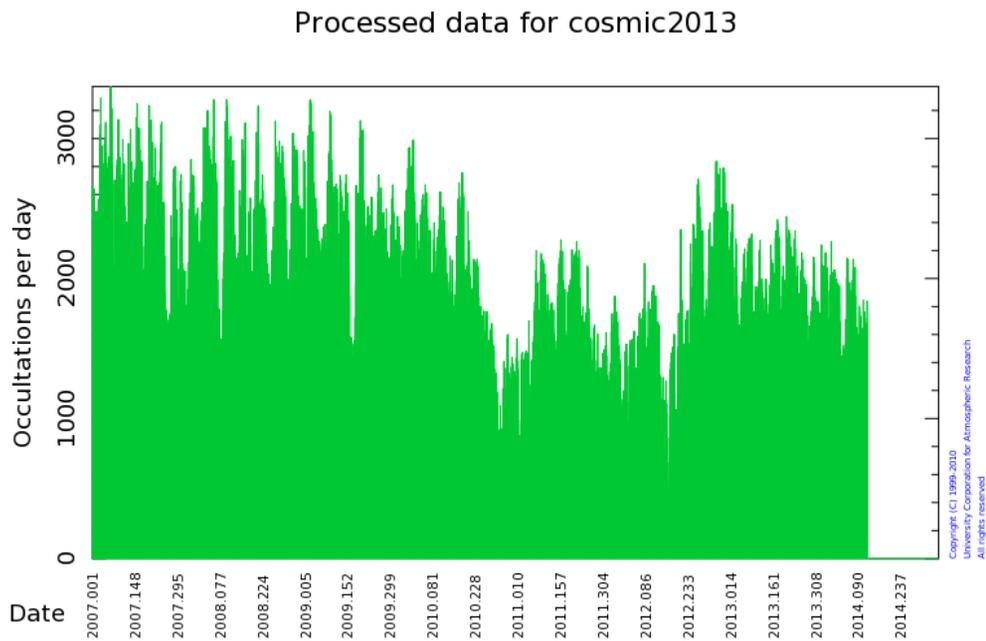


Fig. 1.

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