Anonymous Referee #2

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Summary: This paper uses COSMIC data from 2006 to 2013 to investigate the accuracies of radiosondes in the Ganges–Brahmaputra–Meghna (GBM) basin region. Reliability of radiosonde networks in the region have come under question, which is corroborated in this study. A major focus is the interannual variability of the upper troposphere, lower stratosphere (UTLS) over GBM. Correlations of tropopause temperature and height with geophysical indices such as the El Niño Southern Oscillation (ENSO) and Indian Ocean Dipole (IOD) are presented, while stratospheric temperature anomalies are compared to periods of major Sudden Stratospheric Warming (SSW) events. The temperatures and tropopause heights from the Modern-Era Retrospective Analysis for Research Application (MERRA) and ECMWF-Interim reanalysis are also compared with COSMIC RO data.

Summary of Review: The paper treats an interesting topic and in many ways uses a sound approach to address the topics mentioned in the summary. However, major revisions are required due to significant concerns regarding interpretation of the data. These concerns include the statistical comparisons and conclusions reached regarding the observations' correlation with geophysical indices based on ENSO, the IOD and SSW. We find that the conclusions lack statistical justification and rigor. Certain conclusions of the data analysis are also insufficiently described or are unclear. Detailed comments that the authors can address are below.

We would like to thank the reviewer for finding our topic interesting and also for providing very useful comments on the manuscript, which has been carefully considered to improve the quality of the manuscript. Following comments of Reviewer 1, we have moved objective 1 (previous version), i.e., to compare radiosonde and COSMIC RO to the appendix (i.e., as a Supplementary Material) and focus only on the "interannual variability of UTLS temperature in the GBM river basin". This will provide us more opportunity to present and discuss the key aspects of interannual variability including their relation to ENSO/IOD and QBO, and also keeping in mind the statistical methods used to analyse them.

Detailed Comments:

p. 9406, Line 8: in Figure 2, mention that the left panel contains monthly totals. Corrected.
p. 9409, 3: On what basis is it stated that the reanalyses are largely independent of the radiosondes? Please provide a reference. Thanks for the comments. Radiosonde observations provided by IMD were generally not accepted by the data assimilation systems such as the reanalysis due to their poor quality. The following references were included to support the statement:
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.
M. Das Gupta, S. Das, K. Prasanthi and P. K. Pradhan, Validation of upper-air observations taken during the ARMEX-I and its impact on the global analysis-forecast system. MAUSAM, 56 (1): 139-146, 2005.
p. 9411, equation (4): There is something incorrect about this equation. Arguments of transcendental functions cannot have dimension (these arguments have the dimension of time). Thanks for the comments. This is a generally accepted equation with the vector x(l,j) indicating a single time series subtracted by the modelled time series (annual and semi-annual terms). However, due to differing seasonality along different pressure levels, we removed the time mean from each month to compute the interannual anomalies instead of applying the multilinear regression. The two methods yield same results at the tropopause level.

Table 2 and p. 9413, 1: The values reported in the table are over a height range and involve integrating across sonde biases that vary with altitude. The low value reported here (ShangE sonde) is based on such bias calculation, but uses of the sonde data are most often at individual pressure levels, so a bias metric that integrates across altitude is not particularly useful. Other possible quantities that represent bias error could be mean absolute deviation, so there is no bias cancellation with altitude. Thanks for the comment and suggestion. We will consider them in our future works.

p. 9414, 1: Water vapor certainly leads to temperature errors, but not refractivity errors directly since water is simply a component of the refractivity. Is the refractivity bias due to super-refraction intended here? This should be clarified. Yes indeed. We have specified them in the supplementary material.

p. 9414, 5: Kumar 2010 seems like a non-standard reference for what data are assimilated into ECMWF. Could another reference be used? We have included e.g., Kumar et al. (2011), Gupta et al. (2005). See supplementary material.

p. 9414, 10: Representing water vapor error in this way is misleading because of the large variation with altitude. It could appear that all sondes do very well at higher altitudes (low bias) where in fact the water vapor error might be very large in a fractional sense. A better way to represent this error is as a fraction of a climatological value for each altitude, thus compensating for the rapid drop in water vapor with altitude. We could represent the error as a fraction of climatological value at each altitude as pointed out but we would also like to see the error in the same units as vapour pressure. Nevertheless, we will consider in our future works.

p. 9415, 10: Please clarify the reasoning behind this sentence, or its expression. 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 been removed.

p. 9416, 17: "The temperature pattern in the stratosphere (above 18.5 km) was relatively stable without much seasonality but exhibited sudden fall in temperature during the early months of 2008, 2009, and 2013." I do not reach the same conclusion from the figure. Could you please clarify? Thanks for pointing this out. We have reanalyzed the data again and updated our plots to also show their interannual anomalies. The drop in temperatures can now be clearly seen at various pressure levels in the above mentioned years. See Figure 6 as well as discussions in lines 357-384.

p. 9418, 27: Please explain your interpretation of these correlation values. (Similar comments could be made at several points in the text). What makes values of 0.62 or 0.44 "very good"? What is the null hypothesis or level of confidence associated with these values? By visual inspection of this time series, the correlation with, e.g. the ENSO index, appears not so good except in the time period 2009-2011. During other years, the correlation appears much worse. A more rigorous test is in order here. We have analyzed the data again to conclude that the correlation between ENSO and UTLS temperatures are high and significant. The correlations were computed at different levels and also plotted them as time-series. See Figure 7 and Table 1 as well as the updated interpretations in 394-430. In general, most of them analysis and discussions are updated.

p. 9419, 12: Why are correlation values of 0.5 considered high? What is the magnitude of the stochastic component of these time series? We mean to mention significant. See the above response.

p. 9420, 9: Please indicate latitude in Figure 12 (latitude on vertical axis). Done.

p. 9421, 17: Similar comments to before: why is -0.38 considered a high correlation? This has been addressed as mentioned above. See, also Table 1.

Note that the implication here is that ENSO and IOD indices track each other well. Is there a reference to this? (It need not involve GPS RO data). The two geophysical signals are a result of sea surface temperature variations in the equatorial eastern Pacific ocean (ENSO) and equatorial Indian ocean (IOD) and are now thought to be independent of each other even though the two events may simultaneously occur (usually in opposite phase) . The following reference is included:

Ashok, K. and Saji, N. H.: On the impacts of ENSO and Indian Ocean dipole events on sub-regional Indian summer monsoon rainfall, Natural Hazards, 42, 273–285, doi:10.1007/s11069-006-9091-0, 2007.

p. 2421, 28: Attribution of height effects due to SSW versus the indices (ENSO, IOD) is without a rigorous basis. The data are consistent with this, but there is no basis for concluding this explains the data. Thanks for the comments again. We endeavoured to address it based on previous occurrence of SSW events and evidence from data analysis. The impact of SSW events on the lower stratospheric

temperature and tropopause can be clearly seen during 2008/2009 (major) and 2010/2011 (two minor) winters. See reference text (e.g., lines 525-543).