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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global temperature estimates in the troposphere and stratosphere: a validation study of COSMIC/FORMOSAT-3 measurements" *by* P. Kishore et al.

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"general comments"

The paper contributes to the assessment of the quality of the COSMIC temperature profiles in the upper troposphere/lower stratosphere (UTLS) as well as the uncertainty in our knowledge of temperature in the UTLS. The paper contains some interesting comparison results in comparing COSMIC profiles with 3 global analyses/reanalyses and several other satellite and radiosonde measurement data sets. As a validation of COSMIC temperatures, the comparison with these other data sets, the success of the validation of COSMIC depends on and is limited by the knowledge of the accuracy of

the comparison data sets. The authors don't really indicate what the expected errors of the comparison data sets are. As a result, it is not clear how much each of the data sets involved is contributing to the observed discrepancies and it is not clear to what extent the other data sets are being validated by COSMIC. When there are outliers amidst several comparisons, one can certainly identify likely suspects as being the least accurate.

The term, "model", should not be used interchangeably with the word, analysis. Model implies an atmospheric model run with some initial and boundary conditions. An analysis is the result of a data assimilation process that has combined information from a model with the information from observations in some hopefully optimum way. The CEP, JRA-25 and UKMO data come from analyses that have assimilated a great deal of observations. They are not free running models and should just be called "analyses" or "reanalyses" (if they are in fact reanalyses). It is interesting to see how COSMIC data compares to other data sets and what the vertical and horizontal dependence of the discrepancies looks like.

Along these lines, the phrase, "good agreement" or "very good agreement" is a bit ambiguous and context dependent. GPSRO is purported to provide climate level data quality and perhaps SI-traceable data quality in orbit. In the climate monitoring business it is now recognized that the ability to accurately measure changes of the order of 0.1 K/decade is required. (At least we hope the climate is warming that slowly). In this context, a 2 K discrepancy between two data sets is quite large. When using such a phrase about agreement, the authors should establish the context or drop the phrase and simply state how large the discrepancy is in terms of degrees K.

There is some sloppiness in the discussion that needs to be cleaned up. For instance, the use of "cold bias" is either wrong or misleading. When COSMIC temperatures are colder than the those in the analyses in regions of the Arctic, such that COSMIC-JRA and COSMIC-MetO differences are negative, the authors say "In the Arctic region the JRA-25 and MetO data sets show cold biases up to 4.5K". This sounds as though the

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analyses are colder than the COSMIC temperatures. However, the opposite is true. The JRA and MetO analysis temperatures are warmer than the COSMIC temperatures in Arctic winter conditions. Similarly, the text indicates the MLS has a distinct cold bias. However, it appears that MLS is warmer than COSMIC. It is not clear if the authors have mislabeled figure panels as COSMIC-JRA when they should read JRA-COSMIC or if this is a misinterpretation of the meaning of the sign of the difference or an ambiguous choice of English that needs to be clarified.

The authors show a single mean profile comparison of specific humidity for the 20S to 20N which reveals a large spread in the 20S-20N average humidity results shown in Fig. 9. The authors should either drop this and address this in a separate paper or give it far more attention and expand the scope of this paper.

"specific comments"

The focus of the paper is validation of the COSMIC temperature profiles based on comparisons with NCEP and JRA-25 reanalyses and UKMO analyses. Do the authors have some estimate of the accuracy of these 3 temperature analyses? Are there any references on the expected accuracy of analyses? This raises the question of whether the validation is a validation of the GPSRO or of the analyses.

8331: The authors state the vertical resolution of GPSRO is 50 m in lower trop and 200 m above. I have seen the 200 m claim. 50 m sounds too good. They need to provide a reference on this.

8332: It would be worth stating that the vertical resolution of the analyses is in the UTLS region to contrast it with that of COSMIC. It is stated that the resolution of the JRA-25 reanalysis is 120 km in the horizontal but the resolution is also stated to be 2.5 degree which is more like 280 km. Which is correct? Are the UKMO data analyses or reanalyses? What is the time resolution of each of the analyses? Is temporal interpolation done in extracting profiles at the times and locations of the comparisons?

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8335 It would be useful to provide some indication of the number of profiles in each 5 degree bin to get a sense of how much averaging is occurring in each bin and to what degree the limited number of COSMIC profiles is contributing to the discrepancies.

Note that the extent of the 194K contour is far larger in the NCEP than in the COSMIC data. Also the locations differ with the NCEP spanning from the northwest Indian ocean down to a region near the SPCZ whereas the COSMIC 194K contour is more broken into smaller spatial regions with the biggest one spanning the equatorial Indian Ocean.

"technical corrections"

8334 thus temperature retrieval is "incomplete"? What does "incomplete" mean?

The authors should remove the word "performed" from in front of analyses.

How large is large when the authors say "these differences are expected to be significantly reduced when large data sets are averaged". The number of COSMIC profiles seems fairly large. Is it not for the purposes of the authors?

A sentence says "The warming is larger in the upper troposphere than in the lower troposphere". What is meant by "warming" in this context?

8335 The tropical minimum temperature is slightly colder than 194K in the COSMIC data and NCEP reanalysis. It is warmer in the other two analyses. The location is the tropical western Pacific, not the eastern Pacific as the authors indicate. The sentence is also ambiguous because it is not clear if the authors are referring to COSMIC or one or all of the analyses.

8337 In Figure 7, COSMIC-NCEP values in the Arctic show slightly positive values, not the -1.5 to -2.5K values the authors indicate

8338 The authors show a single mean profile comparison of specific humidity for the 20S to 20N which reveals a large spread in the 20S-20N average humidity results shown in Fig. 9. At 2 km, the average of the 6 datasets is about 6 g/kg whereas the

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peak to peak spread is about 3 g/kg. This behavior needs more attention than is given here. I would suggest that this be dropped from the paper and addressed in a separate paper.

8349 In figure 3, there appear to be significant latitude and height dependent differences through many of these vertical profiles. DO the authors have any suggestiosn as to what is causing these. At low latitudes, there is a sizeable upper troposphere/TTL temperature bias where COSMIC is warmer. Since COSMIC temperatures are purported to be quite accurate for temperatures colder than ˜230K were the water begins to be important, this suggests the three analyses have problem in this altitude range. COSMIC is significantly cold biased at lower altitudes at all latitudes in Figure 3. Are the COSMIC temperatures in Fig. 3 the so-called dry temperatures that assume no water vapor is present? If so that would explain the apparent cold bias.

8352 Fig 6. 100 HPa pressure level needs to be indicated in the caption

8354 In figure 8, there are 8 different curves which are hard to distinguish. Can you use dashed lines or something in addition to colors to help separate these? Large spread in temperatures. MLS is distinctly larger than

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