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Atmospheric Measurement Techniques Discussions

Interactive comment on "Consistency and structural uncertainty of multi-mission GPS radio occultation records" *by* A K. Steiner et al.

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Many thanks to the reviewer for the positive reception of our paper and for the constructive comments and questions. We will revise the manuscript accordingly. Please find our responses to all comments below.

**Specific comments

Comment 1.

Page 2, Line 2: Suggest inserting "in situ" before obserservations, i.e. => " in situ observations" because it could be argued that there are many satellite radiances available.

Response 1.

We prefer to keep this formulation as it is a general statement and not specific to in-situ observations.

Comment 2.

Page 2, Line 15: the accuracy requirements, 0.1 K for climate and 1 K for NWP, need further explanation. The 1 K for NWP is presumably a random error for a given observation, but what is the definition of the 0.1 K requirement? Further, on Page 3, line 25 it says the observational uncertainty estimate for an individual RO observation is 0.7 K near the tropopause. Is this consistent with the 0.1 K climate requirement? Please clarify. Similarly, clarify "measurement uncertainty of 0.5 K" on Page 2, line 19.

Response 2.

The accuracy requirement numbers in line 15 were "just" given as an example by Trenberth et al. (2013) to explain that accuracy for climate observations needs to be more stringent than for weather observations as changes in temperature over decades are small compared to daily variability. The established numbers for climate monitoring are defined in GCOS (2016), which we cite in the same paragraph. To avoid confusion, we removed the numbers from line 15. The observational uncertainty stated on page 3, line 25, is given for individual RO profiles, and not for monthly averaged climate fields. We further clarified the metrological terms in the revised manuscript on page 2, lines 15–21 and included the following reference. Reference: JCGM (2012), International vocabulary of metrologyâĂŤBasic and general concepts and associated terms (VIM 3rd edition), Tech. Rep. JCGM 200:2012, Joint Committee for Guides in Metrology, Office BIPM, Paris.

Comment 3.

Page 5, Line 18: "Two coherent carrier signals ...". This sentence may give the impression that the ionospheric correction is in phase space. Please clarify.

Response 3.

We rephrased this sentence to: "Two coherent carrier signals are transmitted, in case of the U.S. Global Positioning System (GPS) at wavelengths of 0.19 m (L1 signal) and 0.24 m (L2 signal) (Hofmann-Wellenhof et al., 2008; Teunissen and Montenbruck, 2017), which enables removing contributions due to Earth's ionosphere in a later retrieval step. "

Comment 4.

Page 6, Line 2: It probably should be noted that no centre is currently trying to correct residual ionospheric errors using, for example, techniques such as those in Danzer etal (2015). Although there is still work required to demonstrate this approach (Danzer etal, 2019 submitted), it should be noted that residual ionospheric errors are a potentially a common error at all the centres.

Response 4.

We thank the reviewer for pointing to this. We included the suggested references and added a sentence on residual ionospheric error on page 6, first paragraph (line 3–4): "Current research aims at further minimization of the residual ionospheric error (Danzer et al. 2015)." Reference: Danzer, J., Healy, S. B. and Culverwell, I. D.: A simulation study with a new residual ionospheric error model for GPS radio occultation climatologies, Atmospheric Measurement Techniques, 8(8), 3395–3404, doi:10.5194/amt-8-3395-2015, 2015.

Comment 5.

Page 6, Line 9: Some NWP centres have moved away from Smith and Weintraub (1953) to potentially more accurate formulations including both updates to the assumed C02 concentration and non-ideal gas effects. This is mainly as a result of work by Dr Aparicio. See Appicio and Larosche (2011) and references therein, Cucurull et al (2013), Healy (2011). The NWP implementations should be noted.

Response 5.

We updated the respective sentence and included all suggested references: "... and is given by the Smith Weintraub formula (Smith and Weintraub, 1953) or updated formulations (Aparicio and Laroche, 2011; Healy, 2011; Cucurull et al., 2013)."

Comment 6.

Figure 2: The Metop bending angles for WEGC at _15 km seem to be an outlier. Any reason for this?

Response 6.

We thank the reviewer for pointing to this. We can confirm that some outliers are the reason. For a better statistical representation, we consistently recomputed all monthly statistics using the median. We included the revised plots in the manuscript.

Comment 7.

Figure 3: The JPL and UCAR appear to have almost equal and opposite bending angle biases. Please discuss.

Response 7.

It appears that JPL and UCAR have almost equal and opposite bending angle biases, because the centers are plotted with respect to the all-center mean. If one center has a larger deviation, this is counter-balanced by the other centers. This is a limitation of the comparison to the all-center mean. A better identification of which data sets have a larger deviation than others is possible in Fig. 9 (top) for trends. In the concrete case, the JPL bending angle deviates more whereas the other centers show more overlap. We added a sentence in the revised manuscript text to emphasize that the plots are with respect to the all-center mean, in section 4.1 at the end of the first paragraph: "Note that deviations of one center are counter-balanced by other centers due to referencing to the all-center mean."

Comment 8.

Page 10, line 21: "Above this altitude, WEGC ...". It might be worth adding that the WEGC dry-temp and temperature differences above 16 km shown in Figure 3 are because of different all centre mean values.

Response 8.

We added the following explanation in the revised version of the manuscript text : "Above this altitude, WEGC dry and physical temperature are the same. However, in Fig. 3, differences are shown with respect to the all-center mean, and the latter is different for dry and physical temperature.“

Comment 9.

Section 4.2, Page 11, Lines 9-10. "Larger variability ..." for JPL is likely due to bending angle extrapolation? Why is extrapolation relevant here?

Response 9.

Actually it is not relevant here because the “raw” bending angle is shown in Fig. 4a. We therefore removed this part of the sentence in the revised text.

Comment 10.

Page 14, line 16. When quoting the uncertainty in the trends , e.g. "0.06 %", include "per decade".

Response 10.

We included “per decade” throughout the revised manuscript text.

Comment 11.

Page 14, line 26. The bending angles are found to be consistent up to 50 km because they are less sensitive to a priori information. Ringer and Healy (2006) suggested monitoring the climate in bending angle space for this reason, although the interpretation

of bending angle trends is more complicated. Consider adding this reference.

Response 11.

According to the reviewer’s suggestion, we added the following statement in the revised manuscript text in the last paragraph of section 4: “Bending angles are found to be consistent up to 50 km because they are less sensitive to a priori information and thus useful for climate monitoring (Ringer and Healy, 2008).”

**Technical suggestions

Comment 12.

The text on many figures is still very difficult to read. Figure 4b, 5b, 6b, 7b, 8-18km dry temperature time series. The vertical ranges/axes could be expanded. Figure 6a, 7a. Better vertical ranges could be used in these figures.

Response 12.

We revised Figures 4 to 7 and enlarged the fonts. We removed the inserts in the subpanels except for the global differences. The difference trends are described in the manuscript text anyway. Regarding changing the vertical axes for some sub-panels, we decided to keep the range of the vertical axes the same for all panels. The main purpose of the differences time series plots is to give an overview on the consistency of the data for different altitude layers and over all satellite missions. Keeping the same axis ranges makes it better comparable and gives the reader and data users an overview on the performance and accuracy of the different RO missions. It is not about zooming into the details in each difference time series but conveying the big picture.

Comment 13. Suggested References

Cucurull, L., Derber, J. C., and Purser, R. J. (2013), A bending angle forward operator for global positioning system radio occultation measurements, J. Geophys. Res. Atmos., 118, 14-28, doi:10.1029/2012JD017782.

Aparicio, J. M., and Laroche, S. (2011), An evaluation of the expression of the atmospheric refractivity for GPS signals, J. Geophys. Res., 116, D11104, doi:10.1029/2010JD015214.

Healy, S. B. (2011), Refractivity coefficients used in the assimilation of GPS radio occultation measurements, J. Geophys. Res., 116, D01106, doi:10.1029/2010JD014013.

Ringer, M. A., and Healy, S. B. (2008), Monitoring twenty-first century climate using GPS radio occultation bending angles, Geophys. Res. Lett., 35, L05708, doi:10.1029/2007GL032462.

Response 13.

We included all suggested references.

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