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Atmospheric Measurement Techniques Discussions

Interactive comment on "Processing of GRAS/METOP radio occultation data recorded in closed-loop and raw-sampling modes" *by* M. E. Gorbunov et al.

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Main comment:

Please explain down to what height the RO signals were used for inversions. It is known that GRAS/METOP data contain a significant amount of data gaps both in CL and RS modes. How were the RO signals with data gaps handled? This needs to be explained.

We added the following expanation: *GRAS/METOP data contain a significant amount of data gaps both in CL and RS modes. Gaps in CL mode mostly occur when RS*

data are present. For each gap its length is evaluated. It the gap length does not exceed some pre-specified threshold, the deviation of the phase excess from the phase model and the amplitude inside the gap are linearly interpolated to the uniform time grid between the two surrounding points where the signal is present. This fill-in procedure introduces some additional uncertainty. However, if the gap length threshold is chosen small enough, the uncertainty will also be insignificant. In this study the threshold was 0.04 second.

Other comments:

Page 1062, lines 4-5, "600-700 measurements per day." Better to say "...occultations..."

We corrected this.

Page 1062, line 25 -page 1063, lines 1-2, "From the processing view point, these modes are similar to phase-locked loop (PLL) and open-loop (OL) modes implemented in COSMIC". There is no full similarity. COSMIC OL uses frequency and delay models without feedback. GRAS/METOP RS uses the frequency model and the delay-locked loop (DLL). The DLL results in data gaps in RS mode (Bonnedal et al., GRAS on MetOp: Instrument Characteristics and Performance Evaluation, Presentation at OPAC-4 Int. Workshop, Graz, 2010). From the processing view point, a user needs to decide how to handle data gaps (see main comment above). This introduces an additional uncertainty.

See our response above.

Page 1064, lines 4-6, "This makes the internal demodulation of the navigation bits easier, because the phase variation between signal samples is much smaller than for the 50 Hz sampling rate". I am not sure this is so trivial. Yes, the signal phase lapse *is 20 times smaller for 1 kHz than for 50 Hz sampling. But there is another competing reason: additive noise. Down-sampling from 1 kHz to 50 Hz (by aligning the integration intervals with the navigation data chips) reduces the noise sqrt(20) times. In other words, for a strongly fluctuating signal with low noise or for a smooth signal with high noise the 1 kHz or 50 Hz may be advantageous for distinguishing the navigation data phase flips.*

We agree. We updated the text as follows: *This results in much smaller variations of the phase between signal samples as compared to 50 Hz sampling. The signal phase variation for 1 kHz sampling is small enough to perform the internal demodulation without employing externally supplied navigation bits. There is, however, a trade-off between the decreased phase variation and the increased additive noise level which for 1 kHz is $\sqrt{20}$ times greater than it is for 50 Hz. The 50 Hz sampling rate is insufficient for the correct internal demodulation of signals from the lower troposphere in the tropics (Sokolovskiy et al. 2009). On the other hand, 1 kHz may be too high. The choice of the optimal sampling rate should be addressed in the future research.*

Page 1065, lines 14-17, "The bending angle profile below 2 km is not related to the atmosphere, because it is obtained from the phase model used to fill in the area where the receiver was unable to track the signal. This part of the profile is discarded in the inversion". Was the receiver completely unable to track the signal below the impact height ~2 km or is there a data gap? In any case, if this part of the profile is discarded from the inversion, then the reason for applying the phase model should be explained. These questions should be considered part of the main comment above.

The receiver was completely unable to track the signal, because there is no direct nor reflected rays in this area which belong to the shadow. The phase model is applied at an early stage of the signal processing where it is still unknown where the shadow area is. This does not create any problems because the shadow zone is identified and excluded later in the framework of the CT2 processing algorithm. The text has been

updated.

Page 1066, lines 17-19, "The choice of the background atmospheric model used for the statistical optimization (Gorbunov, 2002a) is referred to as the initialization" so the initialization is defined as the choice. In Gorbunov, 2002a: "The use of background ... instead of measurements at big heights ... is referred to as the initialization" I recommend to avoid unnecessary ambiguities in terminology.

We updated the text as follows: *The statistically-optimal use of the background atmospheric model at large heights where the signal is noisy (Gorbunov 2002a) is referred to as the initialization.*

Page 1066, lines 21-22, "... differences and standard deviations GRAS–ECMWF are very close to those of COSMIC..." is sloppy, please revise.

We revised this as follows: *The systematic differences and standard deviations of GRAS-retrieved refractivities from the ECMWF analyses are very close to similar characteristics of COSMIC retrievals...*

Page 1066, line 29, "... data have does not penetrate ...", please revise.

We removed word “have”.

Page 1067, lines 8-9, "[GRAS] raw sampling mode ... allows for accurate measurements of wave fields..." I recommend moderation of this statement; with the data gaps, the measurements of wave fields cannot be accurate.

We removed word “accurate”.

Page 1067, line 27, "... the COSMIC lifetime is expected to be a matter of one or two *more years". I recommend providing either a reference or justification of the expectation or, in the absence of both, removing the statement.*

Page 1068, lines 4-5, "The GRAS instrument with its raw sampling mode meets the high standards de.ned by COSMIC". I don't believe that the "high standards" is a relevant expression and recommend being more specific. If retrievals of data from two instruments are compared in terms of differences with a reference model, it should be written explicitly what has been found.

This paragraph has been removed upon the request of Axel von Engeln, who wrote a Short Comment.

Page 1069, lines 25-29, I did not find the referenced paper on the AMT website.

The paper was withdrawn by the authors. We reference their OPAC-2010 presentation instead.

Page 1075, "Fig.5. [S]tatistical comparison..."

In the original file we don't see this mistake. We will pay more attention to this while converting the final version of the text to PDF.

Pages 1075 and 1076, Figs. 5 and 6. What are the dates and for those figures? What is the sample number for Fig. 5: the same as for Fig. 4 or Fig. 6? Apparently, Fig. 6 is aimed at demonstrating the difference between CL+RS and CL processing in the LT. Concurrently, it shows substantial differences in the UTLS and above compared to Fig. 5. Those differences cannot be related to RS. If they are related to different samples used for Figs. 5 and 6, this reduces confidence in the found differences in LT. This needs to be explained.

The RS+CL statistics is based on the whole month, while the CL statistics is only based on one day. In the text we state: *The comparison is based on 506 RO events observed*

during September 30, 2007, in particular there are 183 tropical events. This explains the (small) difference between RS+CL and CL in the UTLS. However, this amount of events is enough to establish large tropospheric difference due to signal tracking failure in CL mode.

The updated version of the paper is attached.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/4/C316/2011/amtd-4-C316-2011-supplement.zip

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 1061, 2011.