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Interactive comment on "Meteorological information in GPS-RO reflected signals" *by* K. Boniface et al.

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I have one general comment and several minor comments / remarks described below: General comment: Page 1202, section 1 "Introduction", lines 8ff: "The main objective of the study is to assess the potential of [GPS radio occultation] signals that rebound off the ocean surface." The authors base their assessment on the analysis of only one COSMIC reflection event. I suggest to extend the processed data set and perform a thorough statistical analysis of the retrieval results.

This was indeed a poor choice of words, as the objective was to assess if further work is justified, rather than to present a comprehensive study. We will modify the wording of the manuscript, especially the introduction and conclusion. We explore the potential, to determine whether there is geophysical information that can be accessed

and be potentially useful. We do conclude that there is geophysical information, that it can be accessed, and we outline a procedure. It was not the manuscript's intent to state that this is the best way, or to present a comprehensive analysis of extracted data, including the merging of direct and reflected data, and the statistical verification against independent data. This work is currently underway, and will be the subject of future reports.

Comments and questions:

Page 1200, "Abstract", line 14-15: "The methodology is applied to one reflection case." It appears that two observations are discussed: Fig. 2 shows COSMIC observation C001.2007.100.00.29.G05 2007.3200, Fig. 5, on the other hand, is based on C001.2006.227.00.46.G02 2006.3200.

We agree that the use of two different RO profiles for presenting the methodology may appear confusing. We will modify the manuscript to keep only one GPSRO profile in the present study. The modified manuscript keeps the COSMIC observation C001.2007.100.00.29.G05 2007.3200, as in Figure 1. By contrast in the new version of the manuscript, Fig 5 will be changed to the same 2007's profile.

Page 1206, section 3 "Observables: [...]", lines 18ff: How sensitive is $B_R(t)$ (Eqn. 6), in particular its phase (plotted Fig. 4), to the particular choice of the mask R(red parallelogram in Fig. 2)?.

We are exploring a case where the two signals are clearly different, with the power clearly separated into two carriers, with very small power (close to noise level) between them. Therefore the exact shape of the mask is not critical, as long as it separates the two clearly defined signals. The only location where the mask is difficult to place is the upper boundary of the parallelogram, near zero frequency, where the two signals merge. However, at zero frequency separation, the two signals do not turn with respect to each other, and do not accumulate a relative phase, which is the observable here.

This clear case is very frequent, and there was no particular effort of selection of a good case. Cases do exist where the two signals are not as clearly separable. Our focus, however, was to explore the clear cases to determine whether there is valuable information currently not being used, before exploring how to handle the difficult cases. We will modify the discussion in the paper along the line of this answer.

Page 1210, subsection 4.1 "Ray path determination" and subsection 4.2 "Optical path length variation [...]": I think these two extended, rather theoretical sections could and should be shortened. The ray tracing equations (page 1213, line 4) and their derivation are discussed in the literature; I recommend to revise subsection 4.1 and 4.2 and add suitable references.

We will shorten sections 4.1 and 4.2. We will in particluar:

- In part 4.1 “Ray path determination” page 1209, line 15 : we could remove the sentence : “The corresponding total optical length, if the propagation took place through $n_0(\overrightarrow{x})$ would be:” and the following formula.
- Next, we can replace on page 1209, line 18 :“L_0 will differ from L ..” by “L_0 the total reference optical length will differ from L ..”
- For the rest of parts 4.1 and 4.2.1 we found that the explanations are not in excess especially for a reader who is not familiar with ray tracing. These paragraphs detail the methodology that has been applied so we believe they can be helpful.
- For part 4.2.2 we propose to reduce the variational principle paragraph. The first paragraph (p1212, line 15 to 22) would become : “Following the Fermat principle and calculus of variations defining precise extremals (Caratheodory (1937)) : we know that the optical path length of any ray is a minimum. Therefore, the path of a ray can be found minimizing the optical path length. Details on ray tracing analysis and its derivations can be found in the literature (Gelfand and Fomin

(2000); Born and Wolf (1980); Aki and Richards (1980)).”

Page 1218, subsection 5.3 "Ray tracing examples [...]": If I understand correctly this subsection serves as an illustration of the OAT ray tracer’s performance, it does not provide results of the inversion procedure. I suggest to merge this subsection into section 4 "Ray tracing analysis".

Indeed, the subsection was presented to illustrate the OAT ray tracer’s performance. We agree and find it more coherent. As a consequence we move part 5.3 at the end of section 4. Also, Figure 7 becomes Figure 5 to be at the right place.

Page 1220, subsection 5.3 "Ray tracing examples [...]": "In the next two figures, we show the result of the two procedures [...]" I assume this paragraph refers to Figs. 8 and 9 which are missing in my copy of the paper.

As we detailed in our answer to Reviewer 1, the labels were incorrect. We will fix them, leaving this paragraph as an introduction of part 5. There are no other figures after the 7th. The comment was related to both approaches shown : the pertubation approach and the inversion method. We will modify the sentence to avoid the ambiguity.

Page 1220, section 6 “Conclusions”, lines 18-19: “[...] indirect phase [...] indirect signals [...].” I assume that ’indirect phase’ and ’indirect signals’ refer to ’reflected phase’ and ’reflected signals’.

True. We used indirect phase to refer to the reflected component. In this case the two are synonymes. We will use a consistent expression in the new version of the manuscript.

Fig. 1, page 1225: I suggest to plot the signal amplitudes in units of V/V instead of $0.1V/V$.

We used the same units as are delivered by UCAR. However, for our purpose, these are arbitrary units, as they are a combination of emitted signal power, antenna gain and receiver sensitivity, none of which we are modeling. We thus made no effort to interpret them as any other than arbitrary units. We will modify the caption to mention that units are arbitrary.

Fig. 2, page 1226: It would be interesting to know the geographical location of the reflection event shown here and in Fig. 5.
We have represented hereafter on Figure 1 the geographical location of the reflection for the event (we will redraw the figures to show one single event). It is shown in this answer, and will be added to the manuscript.

Fig. 6, page 1230: I assume that Fig. 6 shows the result for COSMIC event 'C001.2006.227.00.46.G02 2006.3200'. Why does the profile derived from the multidimensional solution search end at a height of about 1 km ? For occultation event 'C001.2006.227.00.46.G02 2006.3200' the COSMIC data centre CDAAC provides a refractivity profile down zero height. Furthermore, I suggest to show the fractional refractivity error $(N^{RO} - N^{ECMWF})/N^{ECMWF}$ derived from the standard RO analysis as well.
As mentioned by the reviewer, the choice of showing two different RO profiles is confusing. Here in fact, Fig. 6 depicts the result for COSMIC event 'C001.2007.100.00.29.G05 2007.3200'. We will homogenize the results, and only work with the RO-profile from 2007.

The profile is not shown below, as the solution was not well constrained by data. Immediately above, the error bars are also big, and then some portion of the profile was better constrained. Indeed, the objective was to find if the data were able to constrain the solution at least somewhere, not that a full profile can be extracted. We will mention it in the interpretation of Figure 6.

Fig. 6, page 1230: I assume that ΔN denotes the difference $N^{retrieved} - N^{ECMWF}$. Typo: in the caption "refraction index ($\Delta n/n$)" should read "refractivity $(\Delta N/N)$".
Sorry for the confusion. Yes, this is true.

Fig. 7, page 1231: For the vacuum propagation case (dotted lines) the plots show zero interferometric phase between 57 and 80 s implying identical paths of direct and reflected ray for a time period of 23 s. Most likely the corresponding event ends at 57 s (tangent point reaches the surface) and the vanishing phase for $> 57s$ shown in the figure is an artefact of the plotting program. Please check.
The zero threshold obtained for the interferometric phase difference between the direct and reflected signals does not represent any artifact of the plotting program. The existence of an obstacle when the signal approaches the tangent point at the Earth surface does not prevent the propagation. By diffraction, the signal would propagate around the obstacle. However, the two distinct channels would have merged into a single one. This is true for all cases shown, although it is more evident in vacuo, as the tangent point touches the surface several seconds before.

References

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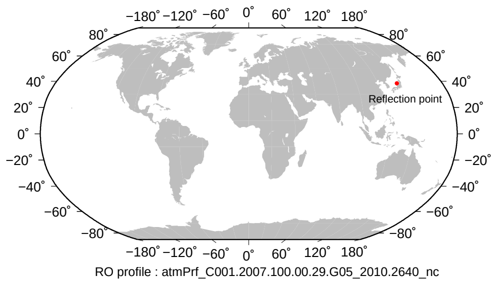

Fig. 1. Geographical location of the GPSRO event on 10 April 2007 at 00:29h.