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Atmospheric Measurement Techniques Discussions

Interactive comment on "Empirical model of the ionosphere based on COSMIC/FORMOSAT-3 for neutral atmosphere radio occultation processing" *by* Miquel Garcia-Fernandez et al.

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> General comments âĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤ- > > > Overall this is an interesting paper, which provides a new 3D model of the ionosphere > derived from RO excess phase delay data. It also offers a new measure of ionospheric > scintillations, derived from these electron density profiles.

ANSWER:

The authors appreciate this comment

> I find the paper to be a little short on detail as regards the generation of the electron

> density profiles. Likewise the nature and detection of the 'wave-like' structures which > they are sometimes said to exhibit.

ANSWER:

Regarding the wave-like structures, the following text has been added to the second paragraph of Section 3:

"Wave-like events comprise profiles that might include small scale Travelling Ionospheric Disturbances (TID), with local maxima very different from hmF2."

For the scintillation selection, the following text has been added at the end of paragraph 4 of Section 3.1:

"The manual labelling of scintillating events was based on identifying significant jitter in smooth profiles."

> I do not find the usefulness of the new 'OSPI' scintillation index to be overwhelming, > but the authors are clear about the evidence in support of it, so that readers can draw > their own conclusions.

ANSWER:

In order to clarify this issue to the reviewer, the following comment on the usefulness of OSPI has been added in Section 3.1

"The main advantage of the OSPI index compared to e.g. σ_{ϕ} is that, as the ROTI index, can be computed with low sampling rate data (e.g. 1Hz rather than 50Hz) and, additionaly, OSPI is computed using topside data, which gives a more localized indicator of scintillation events. This is specially relevant for LEO RO data because measurements with low impact parameters traverse various ionospheric regions (height, different line-of-sight geometries at low and high latitudes with regard to the Earth's magnetic field, ...)"

> With a few exceptions the English is clear and direct. The figures are generally

clear, although > some of the writing is very small on my pdf version. The paper is commendably > brief. I recommend it for publication, subject to some minor corrections and additions.

ANSWER:

The Figures have been increased in size to avoid this problem

> Specific comments âĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤ- > > > P1, L7; and P11, L7: I'm not sure it's been proved that the availability of electron density > profiles will definitely increase the accuracy of inverted bending angle profiles. Perhaps > you could say " ... in the hope of increasing the accuracy ...". Or give a reference to > support the original contention.

ANSWER:

Thanks, agreed. A major rework has been done in the abstract to put more focus on the methodology rather than the quantitative aspects of the potential imrpove. The reviewer is referred to the new abstract.

> P2, L10; P6, L1: The Zorro formula is for bending angles, not phase delays. (And it's > an approximation to a numerically calculated integral, not an analytic evaluation of it.)

ANSWER:

Clarification texts have been added in the abstract:

"This formula (called Zorro formula, see [Culverwell and Healy (2015)]) is the aproximation of the numeric integral along the line of sight considering that the vertical profiles of electron density follow a Chapman layer model."

and in P7 (start of Section 2.4):

"So far, the ROPP software computed the bending angle using an analytical integration

of the Chapman function:"

> P2, Sec 2.1: Please say a little more (or give some references) for the way the excess > phase data are turned into (separable) electron density distributions. Please also give > some details or references for the LMS method.

ANSWER:

The following clarification has been added in Section 2.4 to address this comment:

"With this new option, for a given radio occultation event, a shape function is selected and scaled with the appropriate VTEC value so that an electron density profile is obtained. This profile will represent the ionospheric electron density for the footprint of the occultation, which is then needed for the integral of the bending angle.

> P7, L5: Please explain what you mean by 'wave-like structures'. For example, where > are the wave-like structures in the green curve in Fig 6?

ANSWER:

The reviewer is referred to the first answer provided by the authors.

> P9, Sec 3.1: OSPI is defined as the normalised standard deviation of (ne(h+dh) - > ne(h)), where h is the height of the samples and dh is their separation. Have you > examined the sensitivity to dh? What value did you choose? You say 'ca 1 to 3 km'. > A different value might have led to a more convincing split between the 'clean' and > 'scintillating' profiles in Fig 8, from which a less arguable choice for the threshold value > might have followed. Please say a little about this.

ANSWER:

No analysis on the dh sensitivity was made. The chosen dh is the one provided by the profiles: between 0.5 and 1km. The reason not to change the dh values is due to the fact that the same approach was used by the naked-eye analysis: the original sampling rate of the profiles.

This has been addressed in the manuscript with the following text at the start of Section 3.1:

"The sampling rate used to compute these differences is the original sampling of the profile so that it is consistent with the naked-eye analysis"

> P11, L2: Please quote the correlation coefficients between OSPI and S4, and between > OSPI and SNR.

ANSWER:

The following text has been added in the caption of Figure 10.

"The correlation coefficients of the OSPI vs S4 and OSPI vs SNR are 0.42 and -0.38 respectively, which correspond to a linear correlation with positive and negative sign, consistent with the plots."

> Technical corrections âĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤ > > > Typos: > P1, L10: You haven't yet defined separability hypothesis, so perhaps it should be in > quotes? > P1, L12; P3, L1; P11, L13: 'peel onion' –> 'onion peeling'. > P2, L5: 'over impact parameter' –> 'as a function of impact parameter'. > P4, L5: 'inf' –> infinity symbol in integral. > P4, L14: 'One of the main advantages of this criteria is that it can be applied in an > automated way' –> 'One of the main advantages of these criteria is that they can be > applied in an automated way'. > P5, L8: 'allows guaranteeing' –> 'guarantees'. > P5, L8: 'reducing at a minimum' –> 'reducing to a minimum'. > P7, L11: 'top latitudes' –> 'high latitudes'. > P10, L10: 'electron density' –> 'peak electron density'. > Figures: > They are all a bit small, which makes the writing hard to read (e.g. the OSPI values in > Fig. 7). Some of the figures are also a little 'unpolished', e.g. they include the filename > in the title (Fig. 4, Fig. 7).

ANSWER:

These corrections have been applied. The FIgures have been increased in size and the ones with the reference have been polished. following the reviewer suggestions

The authors would like to thank the reviewer for his/her comments

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2017-217/amt-2017-217-AC1-supplement.pdf

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