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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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> This study discusses the use of an empirical electron density model with traditional > radio occultation processing packages. Also presented is a "scintillation" proxy index > for the identification of highly variable electron density profiles. Unfortunately, I see > this work as the combination of two separate studies that are each incomplete. Based > on the following reasoning, I believe that the present study is insufficient to warrant > publication and would require very significant modification to take it to a point where it > would be suitable for publication. I recommend that the authors undertake a significant > revision of the study and re-submit the study as two separate papers. >

> > > In the following, I will address the empirical electron density model portion and scintil > lation proxy separately. > > > Empirical Electron Density Model: > > In this section of the study, the authors present the Separability-Hypothesis as the basis > for their electron density profile inversion and discuss quality control and screening > processes that they have undertaken in order to ensure the quality of the tested data > set. My comments and concerns are the following: > > 1) While the inversion process could be discussed in more detail, the focus on data > quality control and handling is very appreciated and sets a strong foundation for the > proceeding study. > > 2) The Separability-Hypothesis technique has been extensively documented in previous > studies by the author and their co-authors (Hernandez-Pajares et al., 2000; GarciaFernandez, > 2003).

ANSWER:

The authors appreciate these 2 remarks and comments

> 3) Despite the claimed focus on neutral atmosphere inversion, the neutral atmosphere > is only mentioned once outside of the introduction and conclusion of the study. Given > the title and abstract, I expected to see a diligent discussion of the implications that > their technique has with respect to neutral atmospheric inversion (comparison to other > techniques, demonstration of improvement over standard techniques, etc. . .) but no > such discussion took place.

ANSWER:

The authors agree on the fact that the neutral atmosphere topic is not the main point throughout the paper as it is essentially focused on the ionosphere. For this reason, the authors have changed the title of the manuscript.

The authors have also made a major rework of the abstract, introduction and conclusions to address the comment of the reviewer. Its new version focus more on the aspect of ionospheric modeling and how this has been done.

> 4) The study is perhaps somewhat misleading. The abstract discusses the development > of an empirical electron density model based on previous occultation mission > data (i.e. something like the International Reference Ionosphere) that could be used in > neutral atmospheric inversion from future radio occultation missions. While it is clear in > the study that the authors intend to use IONEX TEC maps to represent the horizontal > spatial variability of the ionosphere, there is no discussion of what is done to account > for the variability of the proposed shape functions method. I understand the method > by which the shape functions are generated and that these functions will be used to > represent the vertical structure of the ionosphere; however, the method by which these > shape functions are parameterized for generalized use in a model framework is not discussed > (i.e. method by which to specify how the vertical structure changes in time and > with horizontal location). The method of undertaking this parameterization is integral > to the creation of an empirical electron density model and can, in fact, be considered > the most challenging component of such a model. Now, while this is an interpretation > of the study, another interpretation could be that the authors intend to use measured > shape functions from the ionospheric delay inversion to act as an empirical electron > density for the subsequent neutral atmospheric inversion (i.e. invert electron density > and then use that electron density in the neutral atmospheric inversion). The fact of > the matter is, without a more detailed methodology, I am not certain which of these > interpretations are correct.

ANSWER:

In order to clarify the issues mentioned by the reviewer, the following changes have been made

- Removed "selection" from title Section 2.2 - Title of section 2.4 changed to "Definition of representative scenarios" - Added a new Section 2.5 that describes the method in which the profiles are assigned to each occultation (the reviewer is referred to the new section in the manuscript (highlighted in paper) - The following paragraph has been added in the introduction:

"Note that the purpose of this work is not to develop a generic climatological model such as the International Reference Ionosphere (IRI). Rather, this work aims at developing a data-driven methodology to provide realistic ionospheric data set (also referred to as "model" in this article). This data set might also become useful for an improved understanding of the ionospheric impact on neutral bending angles, as well as an input source for simulation studies of RO data. "

> 5) This portion of the study is incomplete. The authors detail a series of scenarios > for which they will assess the presented methodology but then do not discuss those > scenarios or any results of neutral atmospheric inversion using this technique. > To summarize this section, the technique used has already been extensively studied, > no comparison was made to other techniques, there is virtually no discussion of the > implications of this method on neutral atmospheric inversion, and there are insufficient > details regarding the methodology of the model to clarify the authors' intent or understand > what the authors are proposing. Based on this, I see this portion of the study as > incomplete and merely a discussion of planned research rather than results. I feel that > this is insufficient to warrant publication (see previous answer).

ANSWER:

The main goal of this work was to offer a new feature to a current software to use a more realistic iono model to retrieve atmospheric profiles from RO: as of today limited to analyitic functions. Note that the basis of the proposed technique has been already validated multiple times in previous works. In order to elaborate on the reviewers concerns, the abstract, introduction and conclusions have been modified in an effort to clarify the focus of the paper.

> Scintillation Index: > > In this portion of the study, the authors present what they are referring to as a scintillation > index based on radio occultation electron density profiles. My comments/concerns > are the following: > > 1) This method shows promising results as a quality assessment tool for users of in > verted radio occultation elec-

tron density profiles, which is particularly important given > the tendency for RO data providers to not provide error values for their RO electron > density profiles (CDAAC COSMIC, for example).

ANSWER:

THe authors appreciate the comment of the reviewer

> 2) The OSPI method, akin to the ROTI method, is essentially a phase scintillation > index, which will be influenced by both refractive effects (real variations in electron > density, changes in propagation path, higher order ionospheric terms in the phase > delay equation, etc. . .) and diffractive effects. Amplitude, S4, is largely an indication of > these diffractive effects and is not sensitive to larger scale variations that may cause > strong variations in phase. We don't expect these indices to necessarily agree. How > do your results compare to, say, sigma phi (since you have S4, I assume you should > be able to also calculate sigma phi)? What is the advantage to using OSPI over sigma > phi? What physical information can be inferred from OSPI, other than just inversion > quality information?

ANSWER:

The following comment on the rationale for 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 interaction with Earth's magnetic field, ...)"

Unfortunately, we cannot add a comparative plot of the OSPI vs. sigma_phi due to lack of available data to the authors. A comment in this direction has been added at the last paragraph before the Conclusions:

"No comparative plots between OSPI and σ_ϕ could be performed due to the lack of available data"

> 3) The OSPI threshold seems somewhat arbitrary, as the distribution of "scintillating" > events in Figure 8 is largely flat. If I, personally, were to use this as a filter to remove bad > profiles, I might have chosen a more aggressive threshold of 0.0012. This threshold > largely seems to depend on what you are interested in identifying: do you want to > be certain you have "non-scintillating" profiles, or do you want to be certain that your > sample contains only "scintillating" profiles? Both of these regimes have very different > thresholds.

ANSWER:

The following text has been appended at the 3rd paragraph of Section 3.1 to address this question:

"This (rather loose) OSPI threshold of 0.0031 ensures a selection of profiles (for Scenario 4) that would include some profiles affected by scintillation and also some others without."

> To summarize this component of the study, the index provides a novel method to assess > the quality of radio occultation electron density profiles; however, it is not made evident > what other uses this index may have, how this index can be used to evaluate physical > phenomena, or how this index can be used to compliment standard indices. With some > expansion this section could make for an interesting study on its own but may, perhaps, > be better suited for a publication such as Radio Science or JGR, which would be more > suited for the stronger radio propagation/ionospheric focus of this work.

ANSWER:

The authors appreciate this comment from the reviewer.

> Other Significant Comments: > > 1) Please define what you mean by "wave-like"

structures (page 9) and how you determined > a profile was "scintillating" by eye.

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."

> 2) Please provide a reference for the “COSMIC RO GNSS raw data check and editing” > mentioned at the start of section 2.2.

ANSWER

The start of first paragraph of Section 2.2 has been changed as follows in order to clarify this question:

"After a basic data check, editing and cycle-slip detection using the dual- frequency observables of the FORMOSAT-3/COSMIC RO GNSS data, [..]."

> 3) Page 4, line 7. You state that the contribution from the topside and plasmasphere > above the satellite altitude is limited to a maximum of âĹij25% of the TEC; however, > 25% is a large error, especially considering that this error will likely produce a 25% > error in peak electron density and could be systematic. Have you considered using a > plasmaspheric model to mitigate this impact? What steps have you taken to mitigate > or assess this impact?

ANSWER

The following comment has been appended at the text of the second bullet of the list at section 2.2:

"Despite this fact, this is mitigated by taking the POD data of the LEO to assess the topside content as well as making an extrapolation of the profile by means of the Vary-Chap model, as proposed in Hernandez-Pajares et al 2017"

Also, a new reference (Hernandez-Pajares et al 2017) has been also added

> 4) In section 2.2, page 4, line 11, please explicitly define what you considered “reasonable” > for hmF2 altitudes. You state that it must be above the E and D layers, but I > presume an altitude threshold was used, unless you have a method of identifying the > altitude and presence of coincident E and F layers.

ANSWER:

The following text has been added a the location pointed-out by the reviewer:

"Reasonable values for hmF2 are considered between 200km and 400km, as supported by Figure 1 of HernaÌĄndez-Pajares et al. (2017)."

> 5) Page 5, line 10: Shouldn’t these profiles with a “pseudo D-layer” also be removed > from the data set, rather than simply omitting that region? The presence of this pseudolayer > error seems to be indicative of accumulated errors from above that altitude and > could be considered a sign that there are issues above as well.

ANSWER:

The following text has been at the end of the location indicated by the reviewer:

"As it is known, the iterative nature of the Abel inversion implies that the error accumulates at the lower heights of the profile, but this does not necessarily imply that the complete profile is incorrect. For this reason, those profiles with suspicious D-layer were left (without its bottom part) rather than completely eliminate them. Also, some of these suspicious profiles correspond to some occultations where lower observations start at a region with low electron density, but then end up in high activity areas.

> Minor Comments: > > > 1) Please include locations and times in the captions of all RO electron density profile > figures. > 2) Figure 2: Please describe each curve here in detail in the figure caption (what each > color means). > 3) Author affiliations, 1), “Spaim” -> Spain > 4) Abstract: EUMETSAT and ROMSAF, please expand these abbreviations. > 5) Abstract and elsewhere: “peel onion” - > onion peeling > 6) Page 2, line 18, rearrange end of sentence to “software would then be tested”. > 7) Page 3, line 1, “mechanization” -> method > 8) Page 4, line 4, the E-region is generally located between 100km and 120km, except > sporadic-E layers which may form at higher altitudes, this is below your 150km lower > boundary. Please correct or explain. > 9) Page 9, line 20, “automatizing” -> automating

ANSWER:

Many thanks, these comments have been implemented.

(1) Details have been added in Figures 4 and 7 (2) Ok, done. Caption has been expanded (3) Ok, thanks! (4) Acronyms expanded (5) Ok three instances modified (6) Ok, changed (7) Ok, changed (8) E-layer removed (9) Ok, corrected

The authors would like to thank the reviewer for his valuable comments

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2017-217/amt-2017-217-AC2-supplement.pdf

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-217, 2017.