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Atmospheric Measurement Techniques Discussions

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Interactive comment

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.

Anonymous Referee #2

Received and published: 1 September 2017

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-

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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; Garcia-Fernandez, 2003).

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.

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

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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.

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.

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 electron 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).

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?

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.

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.

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.

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

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 ∼25% 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?

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.

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 pseudo-layer 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.

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

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