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Interactive comment on "Ionosonde Total Electron Content Evaluation Using IGS Data" *by* Telmo dos Santos Klipp et al.

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Reviewer #1 Comments:

Report on the paper "Ionosonde Total Electron Content Evaluation Using IGS Data" by Telmo dos Santos Klipp et al.

The paper considers a time window of two years to compare the ITEC (ionospheric total electron content) measured by some ionosondes to vTEC (vertical total electron content) given by IGS maps. The authors say that ITEC is significantly lower than vTEC and uses the adapted α-Chapman analytical representation of the topside proposed by Jakowski (2005) to fill the gap.

My major concern is about the novelty of the work. It is well-known that ITEC is signifi-

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cantly lower than vTEC and it is somewhat expected that introducing for the ionosonde a topside representation extending till 20000 km the gap is reduced.

To increase the scientific content of the paper I invite the authors to compare at least two different topside analytical representations, in order to evaluate which one could be considered the most reliable for the region under study. For instance, the authors might consider the following paper

M. Pezzopane and A. Pignalberi (2019), The ESA Swarm mission to help ionospheric modeling: a new NeQuick topside formulation for mid-latitude regions, Scientific Reports 9:12253, doi:10.1038/s41598-019-48440-6

which has been recently published, consider the new analytical topside formulation proposed by the authors and make a performance comparison between this and the one proposed by Jakowski (2005). Even though the paper by Pezzopane and Pignalberi (2019) is focused on mid latitudes, the authors have recently given a presentation at the IRI workshop held in Nicosia (Cyprus) from 9 to 13 September 2019 in which they have shown that the new Nequick topside formulation is really powerful also at low latitudes.

Authors: We would like to thank the reviewer for the very important comments and suggestions. The modifications in the manuscript (included in supplement material) were included in red color.

We are deeply grateful to the reviewer for bringing us this up-to-date suggestion to substantially improve the novelty of our paper. We have read the suggested paper (and references therein), and also got in touch with the authors (Dr. Pezzopane and Dr. Pignalberi) who provided the 2 two-dimensional grids (numerically) derived from Swarm satellites. Then, we were able to implement this topside reconstruction technique using ionosonde data - the NeQuick topside formulation, based on a semi-Epstein layer with modeled scale height as a function of a corrected version of the Ho empirical parameter, and the manuscript was significantly improved. The section “3 Experiments and

results" was divided into 3 subsections: "3.1 Adapted α-Chapman", "3.2 NeQuick topside formulation", and "3.3 Comparative evaluation". Figures 9 was updated including NeQuick formulation and divided into 2 figures (figures 9 and 10), to better present the results. Abstract and conclusion section were substantially altered.

Other issues: -when talking about the total electron content until 1000-2000 km measured by an ionosonde they usually talk about ITEC (Ionospheric TEC) and not vTEC.

Authors: We adjusted the correspondent references to Ionospheric TEC (ITEC).

-kilometer has to be written as "km" and not as "Km".

Authors: We corrected the manuscript.

-the following sentence "....and the adjustment in the plasmaspheric basis electron density was based on differences to IGS data" at page 2 is unintelligible, please rearrange.

Authors: The sentence was modified to avoid misunderstanding and to reflect the major changes in the manuscript.

-at page 2 the authors write: "..to produce a vertical electron density profile (ionogram)." This is incorrect. An ionosonde records an ionogram and, after applying an inversion process on the ionogram trace, a vertical electron density profile is obtained.

Authors: We changed the manuscript to avoid this inaccurate explanation.

-at page 3, concerning the citations made by authors about the evaluation of autoscaling systems, I suggest to cite also the following papers: Gilbert JD, Smith RW (1988) A comparison between the automatic ionogram scaling system ARTIST and the standard manual method. Radio Sci 23(6):968–974. doi:10.1029/RS023i006p00968 Enell C-F, Kozlovsky A, Turunen T, Ulich T, Valitalo S, Scotto C, Pezzopane M (2016) Comparison between manual scaling and Autoscala automatic scaling applied to Sodankyla ÌÍĹ Geophysical Observatory ionograms. Geosci Instrum Method Data Syst 5:53–64. doi:10.5194/gi-5-53-2016 Bamford RA, Stamper R, Cander LR (2008) A comparison

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between the hourly autoscaled and manually scaled characteristics from the Chilton ionosonde from 1996 to 2004. Radio Sci 43(1):RS1001. doi:10.1029/2005RS003401 M. Pezzopane, V. G. Pillat, and P. R. Fagundes (2017), Automatic scaling of critical frequency foF2 from ionograms recorded at Sao Jose dos Campos, Brazil: a comparison between Autoscala and UDIDA tools, Acta Geophysica 65, 173-187, doi:10.1007/s11600-017-0015-z

Authors: The suggested references were included in the section "1.2.1 Ionogram quality" subsection, which was improved following also reviewer # 2 suggestions.

-at page 3 the right citation for the QUALSCAN algorithm is McNamara, L. F. (2006), Quality figures and error bars for autoscaled Digisonde vertical incidence ionograms, Radio Sci., 41, RS4011, doi:10.1029/2005RS003440. and not Galkin et al. (2013).

Authors: The suggested citation referencing QualScan algorithm was included to the sentence.

-at page 6 replace "(with i=1,2,...,n)" with "(ei, i=1,2,...,n)"

Authors: We did not find the text "(with i=1,2,...,n)" on page 6. Assuming the reviewer meant to replace "(ei, i=1,2,...,n)", the text was corrected in that way.

-concerning Fig. 4, on the x axis add also the local time.

Authors: We can not simply add local time to x-axis, since the brazilian ionosondes used are located at 2 different time zones (UTC-3 and UTC-4).

-concerning Fig. 4, the legend is confused, there are two "11" and two "22", please check.

Authors: In fact, Fig. 4 shows the number of occurrences of ionograms C-level 11 (blue curve), 22 (green curve), and we also included a red curve to show the number of occurrences of both 11+22 (basically the sum of 11 and 22 curves), which represent the data actually used in this work. We included the comment "see red curve in Fig. 4"

to better clarify this issue.

-at page 6 remove the sentence “Such coherence has been well explained by Klipp et al. (2019). These authors have analyzed the IGS TEC for the equatorial, low and mid latitudes and also for the same period as presented in this work. It was noticed seasonal TEC dependence with maxima during equinoxes for equatorial and low latitude sectors, but modulated by an overlay effect of the solar flux.”

Authors: The sentence was removed.

-Figure 5 is useless, I invite the authors to remove it.

Authors: The first paragraph of section 3 “Experiments and results” bring us interesting evaluations of figures 5 and 6, with reasonable explanations about differences in the plots of Figure 5, which strengthen the authors decision of working only with C-level 11 and 22 data in the experiments.

-at page 8 the authors write “Figure 7b shows the ionosondes peak of plasma frequency (foF2).....” but I cannot understand how these values have been calculated. Have these mean values been calculated by considering data coming from all the ionosondes? Please, clarify. The same issue stand also for the “Maximum altitude” and the “Plasma Frequency at the Maximum Altitude”.

Authors: Yes, the values presented in Fig. 7a, 7b, 7c and 7d are calculated as the daily mean, considering data in all ionosondes. The text was improved and the caption of Fig 7 was updated to clarify that.

-at page 9 the authors talk about 22,000 km but in the next page they talk about 20,000 km. Please, clarify.

Authors: For different GNSS satellite constellations, the satellites altitudes are not the same but also are not too different. We believe it is appropriate to mention “approximately” 20,000 km as the satellites altitudes, and to use the same 20,000 km for the experiments. The text was corrected that way.

-at page 10 the authors write “Different values for the proportionality coefficient K were examined, and Fig. 8 shows the correspondent TEC differences to IGS in terms of total RMSE.” Again, I cannot understand how RMSE values have been calculated? Have these mean values been calculated by considering data coming from all the ionosondes? Please, clarify.

Authors: Yes, the RMSE values presented in Fig. 8 are the global mean for the whole period evaluated, considering all ionosondes. We changed the sentence to better explain this.

-at pages 11-12 the authors write “Yet, we could observe the matching between ionosonde and IGS TEC seems worse during low ionization periods, mainly nearby June solstice,.....” but looking at Figure 9 it does not seem that during June solstice the matching is worse.

Authors: The conclusion section was completely reviewed to consider the new results, and the mistake was corrected.

Please also note the supplement to this comment:
https://www.ann-geophys-discuss.net/angeo-2019-131/angeo-2019-131-AC1-supplement.pdf

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