

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 significantly 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.

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.

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

-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.

-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.

-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](https://doi.org/10.1029/RS023i006p00968)

Enell C-F, Kozlovsky A, Turunen T, Ulich T, Valitalo S, Scotto C, Pezzopane M (2016) Comparison between manual scaling and Autoscalca automatic scaling applied to Sodankyla™ Geophysical Observatory ionograms. *Geosci Instrum Method Data Syst* 5:53–64. doi:[10.5194/gi-5-53-2016](https://doi.org/10.5194/gi-5-53-2016)

Bamford RA, Stamper R, Cander LR (2008) A comparison 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](https://doi.org/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

-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).

-at page 6 replace “(with $i=1,2,\dots,n$)” with “(ei , $i=1,2,\dots,n$)”

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

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

-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.”

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

-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”.

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

-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.

-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.