

Interactive comment on “Variation in total electron content with sunspot number during the ascending and maximum phases of solar cycle 24 at Birnin Kebbi” by Aghogho Ogwala et al.

Anonymous Referee #1

Received and published: 4 September 2018

Short description.

The article contains a description of the daily dynamics of the total electron content (TEC) averaged over month and over season. The data obtained from a single equatorial GNSS receiver (Birnin Kebbi, Northern Nigeria).

Regular effects in the equatorial ionosphere are investigated in many works, and are summarized in reference ionospheric models (Advances in Space Research, Special Issue, Volume 60, Issue 2; DOI: 10.1016/j.asr.2018.01.009). In addition, the ionospheric effects are widely modeled and investigated (doi: 10.1016/j.jastp.2017.06.020). Therefore, from my point of view, at the present time, the presentation of any statistical

[Printer-friendly version](#)

[Discussion paper](#)



experimental data in this field without comparison with the existing models of TEC behavior at these latitudes, without comparisons with the data of other tools and without explanation of the found differences make a paper incomplete.

The novelty of the results obtained in the work in comparison with other papers is not clearly distinguished. Perhaps the only new effect that leads the authors is the differences in seasonal variations of TEC with other researchers (lines 237-238). However, this effect has not been studied in detail by the authors, numerical estimates are not given, its statistical significance has not been evaluated, its detailed comparison with the models and data of other researchers and instruments has not been carried out, and its explanation has not been given. In this case, the title of the paper also does not correspond to the main result of the work.

The paper contains principal errors in the basic formulas.

From my point of view, the paper does not represent a complete study. The novelty and practical significance of the work are not clearly distinguished, not thoroughly investigated and not obvious. Some of the results stated in the work are either insufficiently investigated and not proven in the paper, some are sufficiently known.

From my point of view, to correct these problems, a complete rewriting of the paper and additional analysis is required, in current state i would recommend to reject the paper.

Detailed description.

1. The most part of the article contains known facts. Some of the text do not have a direct relation with the data presented in the paper (lines 10-19; 56-105; 266-277).

2. The paper contains gross mistakes:

a) The basic formula (3) is incorrect: Slant TEC (STEC) and Vertical TEC (VTEC) in the thin ionospheric layer model are proportional to each other with a factor of the cosine of the radiowave propagation zenith angle (DOI: 10.1029/97RS00451, eq.1), but not with a factor of 1, as suggested by the authors (in equation (3)). This causes doubts

[Printer-friendly version](#)

[Discussion paper](#)



about the validity of all the results presented in the paper.

b) The SPIDR website <http://www.ionosonde.spidr.com> (line 152) is unavailable from 2016, so the submission of the link in the paper looks strange.

3. The ionosphere is a dynamic medium, especially during the period of the active Sun studied by the authors. This leads to significant day-to-day variations of TEC. The authors did not estimate the comparative magnitude of the effects they believe to be new, in comparison with the level of day-to-day variations.

4. In the most part of the paper, the authors describe known regular effects found at the GNSS receiver, with references to the papers in which they are interpreted (lines 170-182, 217-219, 225-229, 232-234, 247-249, 257-258). This makes the feeling that most of the results presented in the paper are not novel.

5. The results of the paper, summarized in the conclusion, are not sufficiently investigated in the paper:

result 1. The obtained values are not explained, it is not explained are they statistically significant and is not compared with models or data obtained by others;

result 2. Numerical values and their statistical significance are not given, is not compared with models or data obtained by others;

result 3. Apparently, the new result, but it is not investigated and not explained in the paper in details. In particular, it is not proven that this effect is statistically significant and is not described by existing ionospheric models, for example by IRI. In addition, the work does not mention anywhere the ionospheric height (h_{eff}) in equation (4). Its changes can lead to changes in the VTEC value (DOI: 10.1007/s10291-018-0715-0) and can produce results different from presented in the paper;

result 4 and paper title. The increase in the average TEC with the increase of the solar activity (average number of sunspots or f10.7 index) is a well-known fact (for example, 10.1051/swsc/2016023 (fig.1), DOI:10.1051/swsc/2013047 (fig.2)), is associated with

[Printer-friendly version](#)

[Discussion paper](#)



the processes of plasma ionization by solar radiation, and from my point of view is not novel result.

Some of the acronyms are not explained in the paper: VTEC(line 118), STEC(line 136), EIA (line 106). Sometimes spaces between words are missing (line 20, some other places)

Interactive comment on Ann. Geophys. Discuss., <https://doi.org/10.5194/angeo-2018-95>, 2018.

Printer-friendly version

Discussion paper

