**Annales
Geophysicae**
Discussions

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Interactive comment on "Comparison of quite time ionospheric total electron content from IRI-2016 model and GPS observations" *by* Mulugeta Melaku and Gizaw Mengistu Tsidu

Anonymous Referee #1

Received and published: 28 May 2019

The paper evaluated IRI-2016 model using GPS-TEC observations during the solar minima 2008 and maxima 2013. I understand, the authors have spent a lot of time and effort on model evaluation and paper writing. However, this paper is not organized in a logic way. The authors listed many figures in their paper but I find it difficult to understand which aspect of IRI2016 (for example: the performance in the EIA/the performance in hemispheric symmetry) they wanted to validate even though I have read their figures and the corresponding statements. I suggest the authors to rewrite it and emphasize the aspect you want to evaluate. Besides, I have some comments that need addressed in your revised version: Major 1. You used the TEC data extracted at a grid resolution of 5 latitude by 5 longitude from IGS. What is your time resolution? What

are the error distributions (spatial-temporal) of IGS TEC? How about the system error? I don't think the conclusions are convinced if the errors of the GPS-TEC are not clear. 2. The ability to simulate the diurnal variation of TEC is very important for a model. I suggest the authors to display some result about this performance of IRI2016. 3. Page 10, Section 3.1.2. You display some figures at 4 selected longitudes. But you didn't say anything about the difference between different longitudes. This may be an interesting work to do. 4. Page 8, Line7-11: I think it is not appropriate to conclude that the performance during high solar activity is poor just according to the large RMSE. As we know, the TEC is larger under high solar activity than under low solar activity. When the RMSE is analysed, the background value should not be ignored. The same situation should be considered when the RMSEs of different seasons and longitudes are analysed. 5. The authors should include a brief summary of the comparison between their conclusions and that of other recent publications in this field such as "Liu, Z. et al. (2019)" and "Acharya, R., & Majumdar, S. (2019)". Additionally, a literature review of the recent publications. Liu, Z., Fang, H., Weng, L., Wang, S., Niu, J., & Meng, X. (2019). A comparison of ionosonde measured foF2 and IRI-2016 predictions over China. Advances in Space Research, 63(6), 1926-1936. Acharya, R., & Majumdar, S. (2019). Comparison of observed ionospheric vertical TEC over the sea in Indian region with IRI-2016 model. Advances in Space Research, 63(6), 1892-1904. Minor 1. You used "S" and "O" to stand for simulated and observed data in Section 2.2.1. In Section 2.2.2, you used "SIM" and "OBS" for them, please confirm this and make them in accordance. 2. Page 7, Line 16: QPOD, not "POD". 3. Figure 4, the total number of points should be indicated. I usually do not like this kind of scatter plots that does not allow to appreciate the distribution of data. I prefer figures where the data density is more evident (e.g. using a color scale and binning the data in ranges).

Interactive comment on Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2019-44, 2019.