

***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**

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We thank the two reviewers for their time and contributions towards the improvement of the manuscript. We have opted to respond to their comments and suggestion in a single response. Part 1 of the response deals with comments and suggestions of anonymous Reviewer #1 and Part 2 addresses those of Reviewer #2.

Part 1: Reviewer #1 Reviewer comments 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

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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.

Response The paper evaluated the performance of IRI-2016 in reproducing TEC observed by IGS GPS observations globally using various statistical metrics. The common statistical metrics for continuous variables and categorical statistical metrics are employed. The later is intended to assess the skill of IRI-2016 at the extreme ends of TEC distribution, which is often neglected, based on quantile-based TEC categories. The scientific community has neglected this aspect primarily due to the fact that IRI model is climatological model and is not expected to reproduce extreme tails of the TEC distribution. However, in the paper, we want to investigate how good is this assumption in view of the continued improvement of IRI models over years. Therefore, we have employed categorical statistical measures to assess the skill of IRI-2016 in reproducing TEC observations at the margins of the observed distribution. In this process, we have observed a number of features from the analysis. The peculiar model skill over the EIA region and the hemispheric asymmetry in the model skill are a few of the features that we found and worth noting. Therefore, the two aspects mentioned by the reviewer are just features that came out as a result of the analysis, not the main focus on their own. In short, the study in this paper addresses the over all skill of the model across the range of TEC distribution. However, we have noted the reviewer concern and have rewritten the manuscript to improve the clarity including the aspect mentioned by the reviewer.

Major Reviewer comment 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.

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Response The time resolution of IGS TEC is two hours. The IGS TEC has estimates of TEC error at each grid. In the revised version of the manuscript, the estimated error in TEC is included and assessment is made to determine whether the difference between model and IGS TECs are within the $2\text{-}\sigma$ error margins of IGS TEC or beyond.

Reviewer comment 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.

Response This is well taken and the manuscript now includes monthly averaged diurnal model and IGS TEC at selected longitude sectors.

Reviewer comment 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 analyzed, the background value should not be ignored. The same situation should be considered when the RMSEs of different seasons and longitudes are analysed.

Response We agree with the reviewer that RMSE is sensitive to large model errors as compared to metrics such as mean absolute error (MAE). This may be harmful in the presence of outliers in the data sets. In particular, when the size of the data set is small, the use of MAE is preferred. Therefore, we have included MAE and skill score based on MAE. The skill score is evaluated with respect to reference measurements. In this case, we used long term climatological mean as a reference observations. In this manner, we hope to remove possible artifacts that may arise due to difference in solar activity, seasons and longitudes as indicated by the reviewer.

Reviewer comment 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

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"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.

Response We accept the suggestion and have incorporated them in the relevant section of the revised manuscript. Details will be submitted along with revision.

Reviewer comment 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).

Response We have color coded the latitudinal bands as suggested by the reviewer in the revised manuscript.

Part2: Reviewer #2 **Reviewer comment** The article considers the real problem, namely the validity of the IRI-2016 ionosphere model, which has become the most popular model up to date. The subject is suitable for the *Annales Geophysicae*. However, I recommend the major revision and resubmission in order to improve the results. The following shortcomings were found: 1. In the abstract and in the first chapter the authors stated the requirement of the ionosphere for the correct forecast of the radio wave propagation. However, the altitude range covered by IRI is suitable only for HF-UHF forecast. Moreover, TEC allows to predict only integral attenuation, as far as no profile properties can be derived from TEC.

Response The statements made in the abstract and the introduction are intended as

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motivation and are not results of this study. It is true that TEC is a measure of integral attenuation but it is also a basis from which profile information can also be derived (e.g., using tomography techniques from slant TEC). Any improvement in understanding of TEC is critical for obtaining accurate profile informations. Therefore, we are afraid that we did not understand the shortcoming suggested by the reviewer. But we have reworded the statements for better clarity.

Reviewer comment 2. The motivation for the article is not clearly stated. IRI has been verified intensively for years, using ionozondes, satellites and GPS receivers as well. What is the novelty of the work? What is the hypothesis to be checked using GPS observations and IRI calculation? Please state it clearly!

Response We believe that we have emphasized the gap in the past studies (see also the first response to the first reviewer's comment) and how we plan to address it. Moreover, we have made some reorganization in the introduction to make this aspect notable.

Reviewer comment 3. When the authors deal with 5x5 gridded TEC values they do not work with evidence. Instead they work with the results of an IGS computer model (some kind of Kalman filter and gridding technique). Thus, the title becomes wrong – you compare one model with another model. If they want to validate IRI model then exactly 422 sites must be used, with further gridding and mapping if necessary.

Response We are afraid that the conclusion made by the reviewer is rather simplistic in reducing gridded observations to a model simulations. It is obvious that gridded observations will have additional errors due to gridding techniques on top of the observational errors (both random and sytematic (e.g., receiver bias error)). Therefore, such equivalence is inaccurate. In addition, the reviewer is suggesting to us to do our own gridding to create maps if necessary. We do not see any good reasons to do our own gridding when well investigated and quality checked gridded TEC along with associated uncertainty are readily available. Moreover, the IGS TEC is publically

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accessible and the authors are not aware of publically accessible individual GPS station TEC for the whole globe except over USA. Therefore, in our opinion, comparing IRI-2016 against commonly used IGS TEC is justifiable. However, out of curiosity and respect of the reviewer suggestion, we have taken good time to derive TEC from about 200 GPS receiver sites globally and made comparison with IRI-2016 simulations. The results are presented in the form of average within latitude bands (see Fig. 1). The results have been incorporated and consistence between gridded TEC and individual GPS site TEC is indicated in the revised manuscript.

Reviewer comment 4. It is not clear, whether the authors used IRITEC subroutine, or they calculated vertical electron profiles and integrated them manually.

Response IRI has several options of providing outputs. We opted for TEC output over manual integration. We have indicated this in the revised manuscript.

Reviewer comment 5. The monthly basis can suffer from biases. It is obvious to use 27 days periods corresponding to Bartels rotation cycles. 6. Short remark about (4). The correlation coefficient (4) makes sense only for stationary processes and for the processes that have normal distribution. No tests are presented that prove the aforementioned requirements. If they are violated then the results have no sense.

Response This is indicated in the revised version.

Reviewer comment 7. Figures 2 and 3 in Mercator projection are awful and unreadable. I see the authors want to prove that they have calculated everything declared. But at that scale it is impossible to make difference between Canada and the US. It is much better to choose a couple of the most interesting frames and print them at large scale. For high latitudes the orthogonal polar projection must be chosen.

Response We have improved the readability of the figures by reducing the number of figures to allow for large scale figures.

Reviewer comment 8. The style of the presentation in the article can be accepted only

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if the authors used real F10.7 and Kp indices (or IG index) from the database. But even in this case I recommend to improve the results in the following way: a. Use only 422 sites with GPS data b. Use estimations of TEC, namely if error is larger than 20% of TEC the data must be discarded c. Present the results as a function of Solar Zenith Angle and Magnetic Local Time. That will be compact and informative, and there will be no necessity to plot tens of filled contours. I think that all simulated data have been stored thus it won't take a lot of time to reduce and remap the results.

Response As indicated earlier in the response to one of the reviewer's comments, we have included GPS site based analysis to strengthen results obtained from comparison of IRI-2016 and gridded GPS TEC (e.g., see Fig. 1). We do not want to abandon the gridded IGS TEC altogether since it is widely used standard data sets in several studies. Moreover, as indicated by the reviewer, the individual IGS site TEC may have large error far exceeding the 20% of TEC in some cases. As a result, screening of bad data should be done and it is subjective in most cases (e.g., the 20% threshold suggested by the reviewer is rather rule of thumb) which suggests that model validation should be performed with standard data sets. In view of these shortcomings, we preferred to complement our studies with station-based analysis.

Summary We have noted that the comments from the reviewers are useful and helpful to improve the manuscript. As a result, we have made major revision that includes processing of GPS observables to determine TEC at about 200 stations for solar minimum 2008 and maximum 2013 for March, June, September and December months. The new processed data are then used in the comparison along with IGS gridded TEC. The details of the changes we have made in the response to the reviewers' comments are incorporated in a different letter that accompanies the revised manuscript.

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

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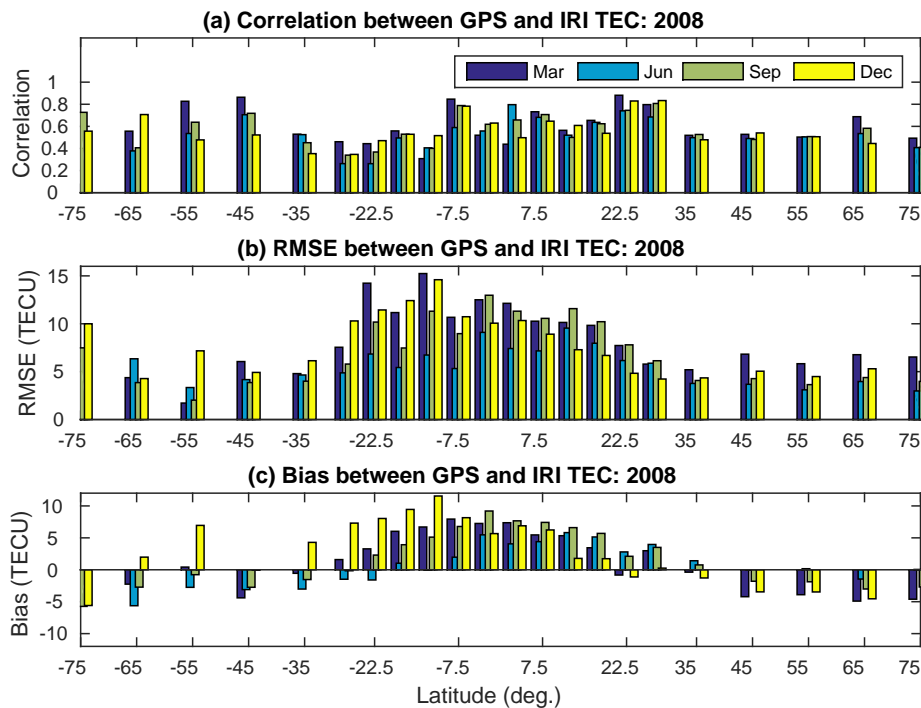


Fig. 1. Correlation, RMSE and bias of TEC from IRI-2016 with respect to TEC driven from over 200 GPS receiver sites.

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